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# PROCEEDINGS

OF THE

# ROYAL SOCIETY OF MEDICINE

EDITED BY  
J. Y. W. MACALISTER  
UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

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**VOLUME THE NINTH**

SESSION 1915-16

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## PART I

OCCASIONAL LECTURES                      SECTION OF ANÆSTHETICS  
BALNEOLOGICAL AND CLIMATOLOGICAL SECTION  
SECTION FOR THE STUDY OF DISEASE IN CHILDREN  
CLINICAL SECTION                      DERMATOLOGICAL SECTION  
ELECTRO-THERAPEUTICAL SECTION



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GENERAL REPORTS



LONDON  
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1916

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# GENERAL REPORTS.

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# The Royal Society of Medicine.

President—Dr. FREDERICK TAYLOR.

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(October 8, 1915.)

## A Lecture on Wound Infections and their Treatment.

*Delivered (with Demonstrations) at the Opening of an Exhibition of Fracture Apparatus, held at the Royal Society of Medicine from October 8 to 14, 1915.*

By Colonel Sir ALMROTH E. WRIGHT, C.B., M.D., F.R.S.<sup>1</sup>

GENTLEMEN,—In undertaking to deliver an introductory lecture on such an occasion as this I feel myself under a very heavy disability. While I am impressed—very profoundly impressed—with the ingenuity and value of the mechanical devices and appliances which have been brought together within these walls, I am, as you know, quite lacking in that expert knowledge required for the proper appraisement of such things. There is, however, one—and it is, I would urge, by far the most important—aspect of wounds which does fall within my particular department of study. It is this that emboldens me to address you on this occasion.

I would, at the very outset, put it to you that the distinction between *sick* and *wounded* is from the point of view of science an entirely improper one. Those who are classed as wounded are as universally as, perhaps even more universally than, those classified as sick suffering from bacterial infection.

Ever since the days when Lister demonstrated that sepsis in surgical wounds was avoidable, very little study has been devoted to

<sup>1</sup> A Consultant Physician to the Expeditionary Force in France. (From the Research Laboratory attached to No. 13 General Hospital, Boulogne-sur-Mer.)

the bacterial infections which here come into consideration. For the surgeon has set before himself as his goal, not the successful treatment of septic infections of wounds, but their avoidance. And who will say that he is wrong? Less excusable, as it seems to me, was it for him to think that he had in reserve—if occasion should ever require it—an effective ready-made method of treatment for septic infection of wounds. After a year of war there are on that point very few illusions left.

There has in the meanwhile, I hope, been growing up a conviction that we shall not arrive at an effective treatment of wounds without strenuous study of the infecting microbes, the conditions in the wound, and the therapeutic agents which we employ, and the defensive operations of the organism.

Let me, drawing upon the research work which my fellow-workers and I have been doing in France, try to put before you, as briefly as may be, in connexion with these questions such points as seem to me most deserving of attention.

The wounded man seen just after he has received his wound is a man seen in the *incubation period* of his infection, just after the microbes have been implanted—these being, as you know, carried into the wound, upon particles of infected skin and clothing.

The problem presented by the patient when first seen accordingly shapes itself thus: What is here the chance that the defensive mechanism of the body will prove adequate for the destruction of the microbes; and may we reasonably look to see the infection aborted? And in the event of the infection not being extinguished, what kind of course will it shape?

The extinction or non-extinction of the infection will depend upon the amplitude of the microbial implantation, and the favourable or unfavourable physiological conditions in the wound.

#### RIFLE WOUNDS TRAVERSING ONLY SOFT PARTS.

The first type of wound which we have to consider is the perforating rifle wound where the bullet traverses the soft parts without touching bone. Here, nearly always, the bullet will cut into the wall of blood-vessels. As a result there will be hæmorrhage from the wound, and when the blood coagulates the track will become plugged with clot. Later, when the patient is moved, and the wound thereby disturbed, oozing may take place from the track.

Here we have a very light microbic implantation—only small

numbers of microbes having been left behind by the missile on its passage through the soft tissues. Further, the walls of the track will have been scoured by the outflowing blood. And finally such microbes as are here left are enveloped in clot—in other words, there will be brought to bear upon the residual microbes the full bactericidal power of the volume of blood which occupies the lumen of the wound. We have, in fact, exactly the same conditions as in experiments conducted in capillary tubes with a very small number of microbes implanted into a unit volume of blood.

As a sequela, as indicated above, we may have oozing. It is not, as might be supposed, blood which escapes from the mouth of an oozing wound. It is a thinner incoagulable fluid containing only a small proportion of red corpuscles. It is, to come to the point, serum expelled from the clot by the *vis a tergo* of blood pouring into the upper reaches of the wound. There attaches, as I think, to this oozing a therapeutic significance. It is, in point of fact, equivalent to a reinforcement of the clot in protective elements.

You have already seized the point that when oozing occurs the serum of the clot—and that serum will, by contact with microbes, have lost some of its antibacterial power—is replaced by blood fluids fresh from the vessels. And you also appreciate that the outflowing stream of serum will tend to wash microbes out from the clot and convey them out of the wound. But it might perhaps be overlooked that when oozing takes place the clot is reinforced by red, and—this is important—by white, corpuscles.

What, I take it, is occurring in the oozing wound can be brought quite easily before the eye in the capillary tube. And let me describe the experiment in detail, for I want you here at the outset to have before you a perfectly clear idea of the method of working in capillary tubes with *washes* and *mural implantations*.

#### DESCRIPTION OF A REINFORCING EXPERIMENT.

We take a capillary tube pipette fitted with a teat, place upon the stem a fiducial mark, fill in with a 15 per cent. solution of citrate of soda, blow this out—leaving thus what we may call a *wash*<sup>1</sup> on the walls of the pipette—fill up with a unit volume of blood, and then mix

<sup>1</sup> A series of weighings made with the form of balance used by Bang in weighing drops of blood on filter paper for sugar estimations have brought out the fact that a “wash” of watery fluid in a capillary tube corresponds to between a twentieth and a thirtieth of a unit volume.

by moving the blood with its wash of citrate up and down in the stem of the tube.

We now, leaving in the distal end of the stem an air space somewhat greater than that occupied by our unit volume, draw up into our pipette a suspension of microbes and blow out again, leaving a wash of microbes on the wall. We follow on now with a unit volume of



FIG. 1.

(A) In the neck of the capillary pipette we have the reinforcing unclotted blood, and in the distal end of the stem the blood-clot which has been implanted with microbes, and connexion has been made between the two by the introduction of a very fine glass stylet.

(B) We have here a more magnified image of the clot showing the creeping down of reinforcing blood along the stylet, and, below the clot, the expelled serum that previously filled in its meshes.

blood and incorporate into this the microbes left on the wall (this is what I mean by a *mural implantation*). Finally, we carry our volume of uncoagulable and our volume of implanted coagulable blood and intervening air-bubble up some little distance away from the mouth of the pipette, and then seal up our tube. The implanted blood now clots, giving us an ordinary *unreinforced* clot. Such a clot is shown diagram-

matically and on an enlarged scale in fig. 1, B. It consists of a meshwork with spaces shown as half filled in with corpuscles and half with serum—we have in the blood, it will be remembered, almost equal volumes of corpuscles and serum.

To imitate the conditions in the oozing wound we have now to feed the incoagulable blood in the upper part of our tube into the coagulum in the lower part of our tube. We do this by taking a fine filament of glass (made by drawing out a capillary tube in the by-pass flame of a Bunsen burner) and passing it down through the unclotted into the clotted blood (fig. 1, A). Contact having in this manner been established, both the fluid and the formed elements of the uncoagulated blood creep slowly down along the capillary filament into the clot (fig. 1, B). Arrived there they occupy all the unfilled spaces, and expel the serum from the clot (fig. 1, B)—driving it out of the tube if we have broken off the tip of the pipette.

In this way we not only carry out from the clot blood fluids which may be exhausted of their antibacterial elements, but we fill up the clot with red and white corpuscles, bringing to the leucocytes of the clot, which, of course, are the elements which count, a reinforcement of 100 per cent.

Going back now to our rifle-bullet wound, we have to consider what will happen if the implanted microbes are not killed off. In this case, we shall presently find an infected purulent fluid coming down along the walls of the track; and very soon the blood-clot will disintegrate and come away, and we shall have a surface infection all along the track. We now pass to consider other types of wounds.

#### RIFLE WOUNDS WHERE THE BULLET HAS COMMINUTED BONE.

Where a projectile comes up against a bone it will shatter it and make a large exit wound by blowing out the fragments and splinters, at the same time scattering the charge of microbes, which would otherwise have been sown only on the walls of the track, far and wide through the tissues.

We have here obviously very unfavourable physiological conditions. For not only is the circulation in the wound completely disorganized, but the wound will contain bone and soft tissues cut off from their blood supply and bound to necrose and slough. And deep in the tissues we have an implantation of microbes which no outflowing blood will wash away. And even more important, the outflow of lymph from the



wounded surface—an outflow which meant the continuous replacement of invalidated by potent lymph—is going to be arrested by desiccation of the external wound surfaces. The wound, in other words, will, if left to itself, become *lymphbound*. And, finally, the wound is, by virtue of its large exposed surface, going to lie open to all manner of after-infection from without.

These same unfavourable physiological conditions are associated also with shrapnel, shell, and bomb wounds. We shall consider them further in connexion with these.

#### SEVERE SHRAPNEL, SHELL, AND BOMB WOUNDS.

The essential feature about this class of wound is that we have here blunt or flat missiles, and that by consequence, as compared with bullet wounds, we have less penetration and perforation, and proportionately more bruising; and at the same time larger portions of infected skin and clothing are carried in by the missile. In other words, we have in the wound much worse physiological conditions; and along with this a heavier microbial implantation.

It will be a convenient arrangement to consider, in connexion with the severer types of wound, the effects of the bruising and the cutting off of the blood supply and the implantation of microbes into devitalized tissues; and then, in connexion with the lighter types of wound, to deal with the effects of the bacterial implantation into tissues not devitalized.

The disorganization and the shutting off of blood supply which is the feature of all severe wounds is followed, of course, by mortification; and thereafter the necrotic tissues fall a prey to every type of microbe: serophytic and sero-saprophytic, aerobic and anaerobic, non-sporing and sporing. And all these influences, working in combination, will cause the tissues to turn black and putrefy and disintegrate; giving, where the wound is allowed to desiccate, a condition of *dry gangrene*. Under the black gangrenous coating there will, if the infection fails to spread to the underlying tissues, and if things are not composed otherwise by the growing out of the tetanus bacillus in the necrotic tissues, gradually be formed a line of demarcation. And finally, the gangrenous layer will be exfoliated, leaving underneath a granulating membrane and a surface infection. Essentially the same sequence of events will supervene if the wound is kept moist. Only here we shall have *moist gangrene*, and the necrotic tissues will be converted into sloughs; and there will be earlier and more profuse suppuration.

SLIGHTER "PUNCHED-IN" WOUNDS PRODUCED BY SHRAPNEL,  
SHELL, AND BOMBS.

We come now to the lighter wounds which are inflicted by blunt missiles, to the wounds which we may call *punched-in wounds*. They are comparatively superficial wounds with steep sides going down to a floor sunk below the level of the surrounding skin. Here the microbes have been carried in over an area corresponding to the superficies of the wound, and they are implanted into the walls and the floor. What is important in connexion with this implantation is, that it is made not into an open track from the walls of which the microbes might be washed off by outflowing blood, nor yet into effused blood, which is up to a point a very uncongenial culture medium for microbes; but into lymph standing in lymph spaces. Now the lymph in such spaces is only under very low pressure—a pressure as low as, or lower than, that of the capillaries—and by consequence when lymph spaces are broken into we have nothing resembling the outflow of blood from a wounded vein or artery. There will at most be a little weeping of lymph, and the conditions will be comparable with those produced if, after dealing a heavy blow with a hammer upon the bark of a tree, there followed a little exudation which afterwards dried, forming an impermeable coating. It is clear that we should then have, instead of a washing away, an embedding of microbes in the outflowing fluid, and an incorporation of these into the subjacent tissue. The conditions in the punched-in wound are, as you will now see, very unfavourable. They will be unfavourable, first, because the antibacterial power of the lymph with which the microbes are brought in contact is bound to be quickly exhausted; and further, because with the arrest of the outflow there will no longer be any renewal of the lymph. Again, the conditions are unfavourable also in the respect that the emigrating leucocytes coming, as they do in such a case, only tardily into a zone which is already poisoned by microbes, cannot press home their attack, and will effect nothing but a choking up of the tissues round the bacterial infection.

In this way we get in the course of a very few days all round our punched-in wound a hard infiltrated edge, margined towards the healthy skin by a zone of pale pink; and in the indurated walls of the wound there confronts us an *imprisoned infection*. That infection will now extend, and—if its way outwards to the surface is too solidly obstructed—it will spread inwards; giving rise, according to circumstances and the character of the microbic infection, to *cellulitis* or *gas*

*gangrene*. In the ordinary course, however, the infection will manage to break through to the surface, and then again we have an *infection of flowing discharges*.

It is important to appreciate that the processes that have just been described take place not only in the punched-in wound, but everywhere where we have microbes implanted into lymph spaces, and afterwards an effusion of lymph which desiccates and seals up the wound.

And precisely the same sequence of events as follows upon the original implantation of microbes will, just so long as the microbes still maintain themselves in the walls, recur if at any moment the wound is allowed to desiccate and become lymphbound. A set-back of this kind will, for instance, almost inevitably follow when, by the transporting of the patient from hospital to hospital, the outpouring of lymph from the infected walls of the wound is interrupted.

#### NATURE OF THE MICROBIC INFECTION MET WITH IN WOUNDS.

We now pass to consider very briefly the nature of the microbes which are carried into wounds from the soiled skin and clothing of the soldier. These microbes may, as I pointed out in a previous lecture, be classified—and the classification is important for treatment as well as for the understanding of the mode of infection and of the evolution of the wound—into two main classes, a class of *serophytes* which (presumably because they find ready-made pabulum in the blood fluids) can live and multiply in serum; and a class of *sero-saprophytes* which, so far as we know, can develop in the blood fluids only when these have lost their antitryptic property—the property in question being that which inhibits those digestive processes which would be capable of converting the native albumens of the serum into pabulum for microbes. Intermediate in character between the serophytes and sero-saprophytes is a class of microbes which cannot grow in the serum when we make only a small implantation, but which, no doubt owing to the fact that they bring into operation powerful digestive ferments,<sup>1</sup> succeed in establishing

<sup>1</sup> The suggestion here made wins support from the fact that the streptococcus, which we may take as the type of a true serophyte, does not, when growing in clear serum, effect any reduction in its antitryptic power, whereas both the *Bacillus proteus* and the bacillus of Welch do this. And it will be remembered in this connexion that the streptococcus does not liquefy gelatine, while the *Bacillus proteus* and the bacillus of Welch rapidly digest albuminous substances (even coagulated white of egg) and gelatine, and, as the case may be, fat, urea, and other substances.



themselves when we make a heavy implantation. We may call these *imperfect* or *secondary serophytes*.

To the category of *serophytes* belong the streptococcus and the staphylococcus—the latter being far inferior to the former with respect to its power of multiplying in unaltered serum. To the category of *imperfect serophytes* belong the *Bacillus aerogenes capsulatus* of Welch (*Bacillus perfringens*); the *Bacillus proteus*; its close congener, the *Bacillus pyocyaneus*; and the wisp-shaped diphtheroid bacillus commonly found in foul suppurating wounds. To the class of *sero-saprophytes* belong the larger number of microbes found in such wounds.

It will suffice here to bring out a few of the more important points in connexion with the serophytes and imperfect serophytes found in wounds.

The microbe most universally present is a streptococcus. It differs in very many respects from the classical *Streptococcus pyogenes*, which is met with, though much more rarely, in wounds. In film preparations of pus the streptococcus here in question shows up nearly always as a diplococcus. As obtained from agar and broth cultures, the elements of the diplococcus are lancet-shaped, and they are bent into an angle. To follow the French description, they resemble a circumflex accent or take the form of saddle-bags (*formes en besace*). In broth cultures we have interspersed with these a few short chains. The colonies as they grow upon agar are more opaque, less sharply margined, and somewhat larger than those of the *Streptococcus pyogenes*. Instead of being as colourless as glass and severely discrete, they show up as very faintly grey-green, and, when planted closely, tend to run together. As compared with the ordinary *Streptococcus pyogenes*, growth is also much more rapid—luxuriant cultures being obtained at 37° C. on broth and agar in four or five hours. Moreover, growth is obtained, not only at 37° C., but also at the temperature of the laboratory bench.

The most remarkable characteristic of this streptococcus is, however, the freedom with which it grows out in normal serum, and also upon agar when transplanted in blood. When we implant into blood in emigration tubes, putting the tubes directly into the centrifuge, and from this into the incubator, we obtain after three to five hours with a moderate implantation a growth in the form of diplococci and short chains permeating the whole white clot; or, with very light implantation, a growth in the form of colonies clearly visible to the naked eye and consisting of typical convoluted chains made up of indefinitely numerous elements. In the case where we implant into blood and then implant

the blood culture on agar, we have very opaque white convex colonies which may be as much as  $\frac{1}{2}$  cm., or even 1 cm. in diameter; and which, except for the fact that they are rather moister, closely resemble staphylococcus colonies. These are made up of lancet-shaped diplococci which might easily be taken for pneumococci. The surrounding blood is not hæmolyzed.

There will be no doubt in the mind of anyone who has studied descriptions and illustrations of the *enterococcus* and its mode of growth on ordinary media as given in French bacteriological text-books that the streptococcus here in question is the enterococcus of the French authors. Moreover, it may be taken as assured—for we have compared our cultures of streptococci from wounds with a series of cultures of streptococci obtained by Professor Dreyer and his colleagues from the stools of patients who were being searched in the ordinary way for typhoid and paratyphoid bacilli—that the streptococcus we are here considering is the ordinary streptococcus of the fæces. And assurance is made still more complete by the fact that when searching normal fæces by the *fæco-sero-culture method*<sup>1</sup> my fellow-worker, Lieutenant A. C. Inman, invariably obtained from the fæces in his after-washes a pure culture of a streptococcus which was, in all the above-mentioned morphological and biological characters, indistinguishable from that which is practically invariably present in the wounds. We may therefore take it as unquestionable that the streptococcus which is commonest in wounds is of fæcal derivation, and both our *fæco*- and *pyo-sero-cultures* show that if the smallest possible implantation of this microbe is made, in no matter what bacterial admixture, into serum, it will immediately grow out there.

With regard to the presence of staphylococcus in wounds, it may be pointed out that, by reason of its wide distribution in the skin and its serophytic properties, it is bound to be present in practically all wounds. We shall, however, presently, in discussing the results of our *pyo-sero-cultures*, appreciate that its growth in the wound is very quickly restricted by changes produced by the immunizing responses of the patient.

Like the staphylococcus and the *Streptococcus fæcalis* which we have just been discussing, the *bacillus of Welch*—which is also, of course, a constant inhabitant of the fæces—is implanted, one may take it, into every wound. This microbe, be it noted, is only an imperfect sero-

<sup>1</sup> This method is modelled in all respects upon the *pyo-sero-culture method* presently to be described.

phyte. In point of fact, as my fellow-worker, Captain d'Este Emery, has succeeded in showing, the serum exerts upon the bacillus of Welch a very considerable bactericidal power; and it is therefore in serum implantations of this microbe only the survivors which grow and multiply, and in blood implantations only those which elude destruction by phagocytes and resist the action of the serum. All this means—and clinical experience amply bears this out—that if we can bring the blood fluids and leucocytes to bear on Welch's bacillus, we have very little indeed to fear from it.

What little requires to be said about the *Bacillus pyocyaneus* and *Bacillus proteus* and other members of the class of imperfect serophytes may for the moment be reserved.

Before passing on something may appropriately be said about methods of cultivation, and I may limit myself to the description of methods of obtaining cultures in serum. For it is only by implantation into serum that we learn what microbes threaten danger, and how far the body is protecting itself against these. And again it is only by the method of serum culture that we can, when dealing with a complicated mixture of microbes, choose out from among these those which we ought to employ as vaccines.

#### METHODS OF SERUM CULTURE IN CAPILLARY TUBES.

What we want for these purposes is a cultivation method which will enable us to make first a moderate implantation and then a series of smaller and smaller implantations.

The *wet-wall method*, or as we may also call it when implanting microbes the *method of mural implantation*, gives us what we here want, and there are two different procedures by which we can obtain the progressively diminishing implantations we require. We may call these respectively the *wash and after-wash* and the *wave-wash* methods.

*Wash and After-wash Method.*—Here taking a capillary pipette fitted with a teat and furnished with a fiducial mark, we fill it up to that mark with such a mixed bacterial culture as is furnished by pus, sputum, or fæces, and then expel this volume, leaving behind a *unit wash* on the walls of our capillary stem. Into the tube thus primed we now aspirate a *unit volume* of serum, then an air-bubble, and then nine or more unit volumes of serum separated by air-bubbles. In this way we obtain in successive volumes of serum a series of smaller and smaller implantations. If we want, as we may often do, a series of

implantations in which the number of microbes falls off more steeply,<sup>1</sup> we can, after filling in the first unit volume of serum and carrying it some little distance up the stem, resect this just above the level of our fiducial mark, completing after that in the ordinary way the filling in of the pipette. Or we can, leaving the stem unresected, arrive at the same result by making each unit volume, as we fill it in, pass several times up and down over the implanted portion of the tube, thus washing off the microbes more effectively.

*Wave-wash Method.*—Here we place two fiducial marks on the stem of our pipette and then, using the proximal mark for the serum and the distal for the air-bubble, fill in with unit volumes of serum separated off from each other by measured instead of, as in the ordinary way, with unmeasured volumes of air. Then at the end of the series we fill in up to the proximal mark with the diluted bacterial suspension which is to serve as our implanting fluid. Retaining this accurately in place, we now place further fiducial marks on our capillary stem, marking off in this way the distal end of the last, and penultimate, of our volumes of serum. We now, relaxing the pressure of our fingers upon the teat, carry the whole system of volumes and intervening bubbles upwards in our capillary stem, arresting the movement as soon as the distal end of the implanting fluid comes level with the first, or, if we prefer it, the second, of the fiducial marks we have just inscribed. We may call this implanting movement a *wave wash*, and may speak of a *one-unit* or *two-unit wave* according as the implanting fluid is carried up into the segment of tube belonging to the next or the next but one volume of serum. We make a series of these wave washes (generally a series of six or twelve), making with these not only an implantation of microbes from the implanting fluid into the neighbouring volume or volumes of serum, but also at the same time implantations from the implanted, into the previously unimplanted, volumes of serum.

<sup>1</sup> It may be well to bring out at this point that though one could hardly, when dealing with a solution of a chemical agent, ask for a steeper series of descending dilutions than that furnished by the wash and after-wash method—for we go down here by steps of 25,625, 15,625, 400,000 and 10,000,000—quite a different standard comes into application when diluting a microbial culture. For in the first case we are dealing with dissolved chemical agents, and imponderable quantities do not come into the account; and in the second case we are dealing with particulate matter, and quite imponderable quantities come very seriously into reckoning. And in point of fact, experiment shows that in pipettes implanted with a unit wash of pus and then filled in by the wash and after-wash method with unit volumes of serum we implant in many cases as far as the fifteenth and in some cases as far as the twentieth after-wash.



*Anaerobic Cultivations.*

When we want to cultivate under anaerobic conditions we proceed as follows, employing, according as we want to cultivate only one volume of serum or a series of volumes of serum, the first or the second of the procedures now to be described.

*Method of making a Single as distinguished from a Multiple Anaerobic Culture in a Capillary Tube.*—Taking a capillary pipette fitted with a teat we thrust the stem deep down into a rubber tube

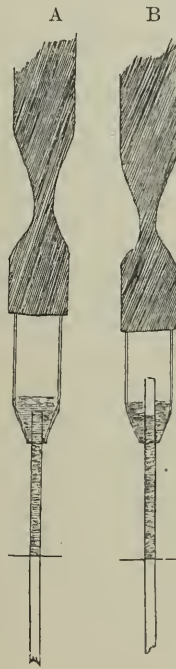


FIG. 2.

We have here in each case a rubber delivery tube, compressed as it would be between thumb and finger, and inserted into this a glass end-piece open at the end. The glass end-piece contains, in its upper portion, gas and, in its lower portion, serum. Into this last is thrust the distal extremity of the capillary cultivation pipette.

delivering hydrogen or ordinary coal gas, and now, alternately compressing and relaxing pressure upon the teat, expel the air, and fill teat and pipette with unmixed gas. This done we withdraw the end of the pipette from the delivery tube, keeping as we do so the orifice of the pipette directed downwards, and then, after expelling a little of the

gas, draw up from a drop of implanted serum placed ready to hand a convenient quantity into the stem of the pipette, keeping the serum close down to the orifice of the pipette, so as to prevent any entrance of air; and finally we once more thrust the capillary stem deep into the gas delivery tube, and draw in sufficient gas to give us comfortable space at the end for sealing up the orifice in the by-pass of a Bunsen burner. We complete operations by burning through the tube capillary stem just below the barrel of the pipette.

*Method of making Anaerobic Cultures of a Series of Volumes of Serum implanted by the "Wash and After-wash" or the "Wave-wash" Methods.*—We begin by fitting to our gas delivery tube the barrel of a capillary pipette which has been truncated by resecting the stem just below the neck. We sterilize this by flaming and let our hydrogen or coal gas run through it sufficiently long to expel all admixture of air. Then directing the orifice downwards we turn off the gas and compress the rubber tube between the forefinger and thumb of the left hand so as to expel a little of the contained gas. Then we dip the orifice of the glass end-piece into a large drop of serum placed ready to hand and draw this up, and then by regulating the pressure with our fingers keep the serum closely applied to the open orifice (fig. 2). We now take into our right hand a capillary pipette furnished with a teat and a fiducial mark, and implanted, if we are using the wash and after-wash method, with a wash of a microbial suspension. The tip of the pipette is now introduced into the serum in the mouth of the glass end-piece; and we now draw up into it first, as in fig. 2, A, a unit volume serum, and then, pushing, as in fig. 2, B, the point out into the gas which is trapped by the serum, a bubble of gas; and so on in series until we have filled up the stem of our capillary pipette.

#### RESPONSE OF THE WOUNDED MAN TO HIS WOUND INFECTION, AND BLOOD CHANGES INDUCED IN HIM BY AUTO-INOCULATION.

In wound infections, as everywhere where bacterial toxins are elaborated and absorbed into the blood, the machinery of immunization is after a time called into operation, and as a result the blood is put into a better condition for defence. And then begins a serious conflict between the invaded organism and the invading microbes.

It is in connexion with septicæmic invasions, such, for instance, as typhoid fever, gradually coming to be understood that it is by the event of this conflict that the issue is decided; and that the physician

in attendance is not following, much less directing, events. But it is as yet a quite unfamiliar thesis that the wounded, like the typhoid, patient is reacting to his infection with a systemic immunizing response; and that the changes so induced in the blood exert a quite decisive influence on the course of the infection, while the surgeon who is dressing the wound and making local applications is only in a subordinate way helping or, as the case may be, hindering the curative procedures of Nature.

It will be well before considering the surgeon's task to take cognizance of what the immunizing responses of the patient are doing in the matter of fortifying his blood.

We have, of course, in connexion with this only very imperfect knowledge, and we shall therefore have to keep very close to our experimental data. It will be convenient to marshal our facts under headings supplied by our methods of blood testing.

*Data furnished by Measurements of the Antitryptic Power of the Patient's Serum.*—Reference has already been made to the antitryptic power of the blood fluids. In point of fact, the antitryptic power of the serum acts as a check upon all microbial growth.<sup>1</sup> In the case of *sero-saprophytic bacteria* it completely balks their efforts to establish themselves in the serum. In the case of *imperfect serophytes* it places a very formidable obstacle in the way of growth. And in the case of *serophytes proper*, as a comparison between sera of low and high antitryptic index, and between sera which have, and sera which have not, received an addition of trypsin shows, it also determines whether the culture in serum<sup>2</sup> shall be scanty or luxuriant.

It will be obvious from the above that an increase of the antitryptic power of the blood fluids would operate in restraint of any blood invasion, and that it would also, so far as the blood fluids came into application unaltered, restrain the growth of all forms of microbes, both in tissues and in the wound. In other words, an increase of antitryptic power would operate as a non-specific factor in immunization. Now we have, as already indicated in the lecture I delivered here

<sup>1</sup> The antitryptic power of the blood fluids represents, be it noted, much more than merely a power of inhibiting microbial growth. With the antitryptic power go lost also the complementing, opsonic, bactericidal and coagulating powers of the blood.

<sup>2</sup> Where instead of implanting streptococci into serum free from leucocytes we implant into blood containing leucocytes these are broken down, with the result that the antitryptic power is diminished, and we have then, as already described in connexion with emigration tube cultures, always after a sufficient interval, luxuriant growth.

six months ago,<sup>1</sup> in every, or practically every, wounded man a notable increase in the antitryptic power of his serum. Already within thirty-six hours or less after the infliction of the wound the antitryptic index has risen far above the level of the normal; and reckoning the antitryptic power of the normal serum as *unity*, antitryptic indices of four and five are very common in the patients under treatment at the base.

More than that, we have found a quite similar but smaller increase in the antitryptic power of the blood after inoculating ourselves—one of us with typhoid vaccine, another of us with streptococcus vaccine, and a third with staphylococcus vaccine.

And let me here recall to you that attention has, in connexion with inoculations against plague, typhoid fever, and pneumonia, been time and again called to the probability that these vaccines give some protection against diseases other than the particular disease which the inoculation is designed to ward off. Particularly convincing in this respect are the results in the form of *diminished incidence of "other diseases"* which were obtained in South Africa by the inoculation of pneumococcus vaccine upon the Premier Mine.<sup>2</sup> And I think all those who have had much experience of vaccines will have seen cases where therapeutic effects lying quite outside the range of the particular vaccine employed, and therefore, as we thought, not quite creditable to science, have been obtained by vaccine-therapy.

#### DATA FURNISHED BY PYO-SERO-CULTURE.

The method of pyo-sero-culture was described in my last lecture<sup>3</sup> (Section I, Sub-section I). We may, however, substitute for the lengthier procedure there set out the wash and after-wash method (*vide supra*).

Having by this method introduced into pipettes similarly implanted with the patient's pus: into one, a series of unit volumes of the patient's blood; and into the other, a series of unit volumes of a normal serum; and having incubated these for from six to twelve hours, we now follow the procedure portrayed in fig. 3, A, *infra*. Beginning with the volume nearest the distal end of the stem which

<sup>1</sup> *Vide* Section I, Subsection of Section 3, last paragraph.

<sup>2</sup> *Vide* Appendix II in my treatise on "Preventive Inoculation against Pneumonia in the African Native," Constable, London, 1914.

<sup>3</sup> *Lancet*, April 10, 1915, p. 737.



has received the smallest implantation, we blow out our successive serum cultures in separate drops on to the surface of a sterile slide; and then with a platinum needle or glass filament stroke out these in succession upon an agar plate, making in this way a series of linear implantations. On incubating our plates we then obtain with the serum of a wounded man who is uninfluenced by his infection exactly the same results as with the normal serum. In the case of

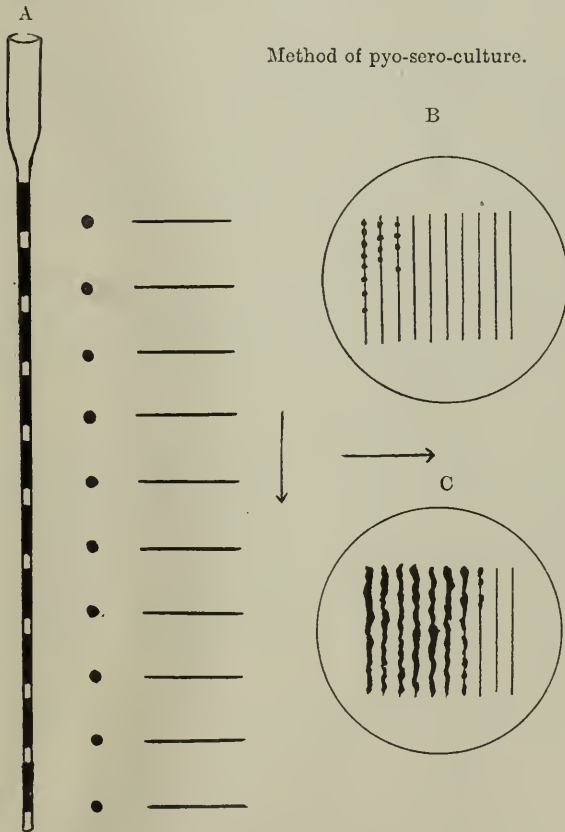


FIG. 3.

(A) Pipette which has been implanted by the wet-wall method and has then been filled in by the wash and after-wash procedure with unit volumes of serum. By the side of the pipette to the right are ranged a series of disks representing the series of unit volumes of serum blown out in order from the pipette, and, finally, to the right of the disks are a series of lines representing linear implantations made upon agar.

(B) Results of the series of linear implantations made with the unit volumes of the patient's serum.

(C) Results of the series of linear implantations made with the unit volumes of the normal serum which was used as a control.

a wounded man in the negative phase of his infection, we obtain cultures containing microbes (in particular, members of the class of imperfect saprophytes) unrepresented in the cultures made with the normal serum; and, moreover, the cultures made with the patient's serum are, as compared with the cultures made with normal serum, more luxuriant and extend into higher dilutions. In cultures made with the serum of a wounded man who is making satisfactory immunizing response—and the illustration reproduces the results actually obtained, by implanting the pus of such a patient into his own serum, and into that of a normal man—we have in both series of pyo-sero-cultures only serophytes, staphylococci and streptococci; but while the cultures made with the patient's serum (fig. 3, B) furnish only a few discrete colonies, and these only in the first wash and two after-washes, we have with the normal serum (fig. 3, C) in the first seven plantings massed colonies, and in the eight, discrete colonies.

The significance of such data will, perhaps, best be appraised if we compare the method of sero-pyo-culture—considered as a procedure for the appraisement of the clinical condition and prospects of the wounded man with the method of *pyo-culture* which Delbet has quite recently proposed to use for these same purposes.

The real value of the method of *sero-pyo-culture* lies in this, that it tells up to what point the blood fluids of the patient would, without aid received from the leucocytes, be capable of protecting themselves against an implantation of microbes, and specifically against an implantation of microbes from the wound. Now merely to tell us this is not to tell us how the microbes are going to fare in the wound. For we are not entitled to assume of the blood fluids that they will in the wound obtain unrestricted access to the microbes; nor of the leucocytes that they will not die in the wound, converting thereby the effused lymph into a favourable culture medium for microbes. Still, evidence of satisfactory immunizing response obtained by the method of pyo-sero-culture is, so far as it goes, evidence of favourable import, always with the proviso that the wound be treated in a rational manner.

The method of *pyo-culture* aims at arriving, by an idyllically simple procedure going straight to its goal, at a trustworthy forecast of the future of the wound. It is suggested that by merely incubating the pus and then examining microscopically to see whether the number of microbes has increased or remained stationary we shall be able to foretell the future, and to decide, where the clinical appearances seem

to prescribe amputation or other radical surgical procedure, whether operation can be avoided. In connexion with this proposed method of investigation, the first point to note is that the pictures which are furnished by it are generally the very reverse of clear, and that we are not in any way helped by controlling, as we are invited to do, our culture in pus by cultures made by implanting pus into bouillon. In point of fact, this particular method of controlling our results could only mislead, for the common pyogenic micro-organisms, and in particular the coliform organisms, would, no matter how sparingly they might be present in pus, always grow out luxuriantly in the bouillon culture. But let us suppose for a moment the case that we have been able to satisfy ourselves that the micro-organisms in our pyo-culture have increased, or, as the case may be, that they have not increased; and let us then ask ourselves what prognostic value would attach to the one or the other finding.

There cannot be any doubt as to the answer to these questions. A multiplication of the microbes in the pyo-culture would, of course, indicate an unsatisfactory condition in the wound; but we should not know whether to interpret this as a consequence of a default in immunizing response on the part of the patient, or as a consequence of the surgeon having suffered the wound discharges to accumulate and become corrupt in the wound. On the other hand, if the microbes definitely failed to grow out in the patient's pus, this would clearly suggest that the patient was making good immunizing response; and that, in correspondence with this, the wound would, given requisite attention, be likely to progress favourably.

DATA FURNISHED BY MEASUREMENTS OF THE OPSONIC POWER  
OF THE PATIENT'S SERUM.

The changes in the serum which gradually render it a more and more uncongenial culture medium for microbial growth have their counterpart in changes in the serum which render the microbes an easier prey for phagocytes. These changes in the opsonic power of the serum are exactly similar to those which manifest themselves in all other forms of localized infection; and we find, when we look for them, in connexion with wounds, all the phenomena of auto-inoculation with which we are familiar in other bacterial infections. And every day it becomes clearer that every displacement or movement of a fractured and infected limb—such displacements, for instance, as are

associated with the transport of the wounded man, or the sagging of his limb when it is dressed, and also all those unguarded passive movements which the surgeon or orderly may inflict when the patient is under an anæsthetic, operate as auto-inoculations; and are followed by that sequence of negative and positive phases which we are accustomed to witness after the stirring up of a focus of infection by active exercise, massage, or the application of Bier's bandages.<sup>1</sup>

#### DATA FURNISHED BY A STUDY OF THE EMIGRATION RESPONSE OF THE PATIENT'S LEUCOCYTES.

As was already brought out in my last lecture, comparative experiments made with the method of testing emigration there described show that we have in patients who have made immunizing response to their wound infections or to the inoculation of streptococcus vaccines, an emigration response to streptococcus implantation which is strikingly greater than that of the normal man. I do not, until the method of estimation shall have been further improved, desire to be more detailed on this question. I prefer to pass on to consider the bactericidal power of the whole blood, for we have in this in some sort the resultant of all those factors in immunity which we have been separately considering.

#### DATA FURNISHED BY A STUDY OF THE BACTERICIDAL POWER OF THE WHOLE BLOOD.

The experiments on the bactericidal power of the whole blood which I undertook in connexion with work on pneumonia in South Africa<sup>2</sup> constituted a first attempt to make a complete evaluation of the bactericidal power of the blood fluids and leucocytes working in conjunction and reinforcing each other.

Let me lead up to what I have to say upon this matter by reminding you in connexion with these experiments with the pneumococcus that the blood fluids exert no bactericidal effect upon that microbe. And let me also very briefly go back over the procedure which I employed.

That procedure was to make by the wet-wall method implantations of the pneumococcus into blood drawn direct from the finger, or, as the

<sup>1</sup> *Vide* in this connexion the auto-inoculation curves set out in the author's "Studies on Immunization," Constable, London, pp. 377 *et seq.*

<sup>2</sup> "Preventive Inoculation against Pneumonia," Constable, London.



case might be, into washed corpuscles suspended in serum, decalcified blood, blood kept liquid upon ice, and into what I will, to distinguish it from *exvascular* blood, venture barbarously to call *excoagular* blood. I mean by *that*—the red and white corpuscles and serum which come out from the blood clot when it contracts. The experiments conducted with all these different varieties of blood gave, as perhaps some of you will remember, very remarkable but undeniably paradoxical results. With blood drawn directly from the finger, a very striking bactericidal effect—an effect which was upon occasion equivalent to a killing off of 600,000 to as much as 1,000,000 pneumococci per cubic centimetre of blood—was achieved. But it was not in every instance achieved.

With none of the other varieties of blood was any bactericidal effect achieved; or if there was any effect, it was very insignificant and very inconstant. The reason for these diverging results did not at that time appear. I made some guesses which fell wide of the mark.

This was the position of the question when I again, in connexion with the streptococcus, addressed myself to the study of the bactericidal power of the whole blood. And the situation to be dealt with was here essentially the same as in the case of the pneumococcus, saving only in the respect that the serum is a much better culture medium for the streptococcus than for the pneumococcus.

During many months of work only very disappointing results were obtained. The blood of normal men, the blood of wounded men who were progressing favourably, and my own blood and that of two of my colleagues after we had been inoculated with streptococcus vaccine, alike gave when implanted with streptococcus in a graduated manner as good as no evidence of any bactericidal power. Or at any rate only very inconstant results were obtained.

This was even a more violent paradox than that encountered with the pneumococcus, for the blood that failed to kill streptococcus was not blood which had in any way been tampered with, but blood drawn directly from the finger; and it is, in view of the favourable event of most streptococcus infections, and the striking therapeutic effects obtained by streptococcus vaccine, difficult to doubt that streptococci are killed in the blood.

At the end a possible explanation of the contradiction between the results obtained *in vivo* and *in vitro* suggested itself. I suddenly woke up to the fact there was a flaw in the technique I was employing.

The nature of this flaw will be understood on referring to the diagrams. In my experiments the implantation of microbes into the

blood was made by what I have called the method of mural implantation, and the particular form of mural implantation employed was the *complete mural implantation* (fig. 4, B).

Now what follows in the case where we implant into a blood which afterwards clots is brought before the eye in fig. 5. When the clot contracts the space which was at the outset filled with unclotted blood, and then with coagulum (fig. 5, A), divides itself up into two regions (fig. 5, B): a region occupied by clear serum and a region occupied in

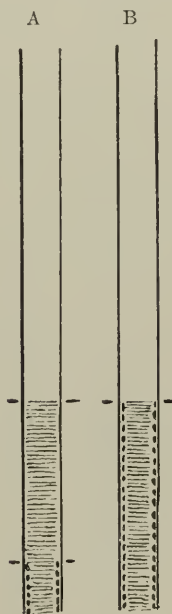


FIG. 4.

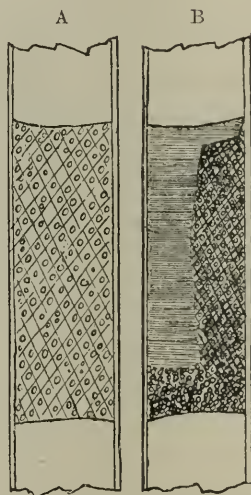


FIG. 5.

FIG. 4.—The figure shows two forms of mural implantation into blood—in A a “wainscot,” in B a “complete mural” implantation. In A the distal end of the capillary stem has, as a first operation, been fitted on as far as the upper fiducial mark with blood. An air-bubble was then drawn in; after this the tip of the tube was dipped into a microbial suspension, and then was allowed to run in as far as the distal fiducial mark. After this the microbial suspension was expelled and the blood was then, to receive its “wainscot implantation,” driven down to the orifice of the tube. In B, where the blood receives a “complete mural implantation,” an essentially similar procedure was followed.

part by the clot and in part by the corpuscles which have fallen out from the clot and have ranged themselves round its base. The *first* of these regions is one in which streptococci—supposing they had once managed to find their way here—would be quite safe from phagocytic

attack, and would find conditions favourable to their growth. In the *second* region, inasmuch as the phagocytes would be crawling about and fossicking for microbes in the clot and escaped red corpuscles, there would, at any rate, be a chance of the leucocytes extinguishing the infection. *Mutatis mutandis*, this would apply also in experiments conducted with the different varieties of incoagulable blood. In each case the corpuscles would settle to the bottom, and there would be left above them a stratum of clear nutrient fluid in which microbes would be quite safe from phagocytic attack.

It will be obvious that these quite elementary considerations provide a very simple explanation of all the blank results obtained in our bactericidal experiments. They do more than that. They point the way to a better technique. For clearly, if instead of making a *complete mural implantation*, we now limit ourselves to what I may call a *wainscot implantation* (fig. 4, A) we shall be planting all our microbes into a region of the blood where they will have to run the gauntlet of the white corpuscles; and there will no longer be for them, as there was with the complete mural implantation, any direct way of escape into a region of safety and nourishment provided by the serum.

It will suffice to say in connexion with this improved technique that it has answered even beyond expectation, and that when we limit ourselves to a wainscot implantation, and make implantations with graduated dilutions of streptococcus, we obtain not only with exvascular blood but also with excoagular and citrated blood conclusive evidence of bactericidal power.

Let me, however, make the following point plain. Even with this improved technique the demon of chance has not been exorcised from our experiments. And since chance will always, in our experiments, play a part in the ordering and disposition of leucocytes, it will be impossible in the case where the bactericidal power of the blood is centred upon these, and so operates only where gravity permits, to get regular quantitatively accurate results, such as are attainable when the bactericidal power is centred in the serum, and so operates uniformly through the medium.

I would ask you to note this point. It is necessary to emphasize it here where we are considering the blood changes produced by immunizing response to auto-inoculation, and it will be necessary to emphasize it again when I come to speak of vaccines and their employment in connexion with wound infections.

But we have not, in the matter of the leucocytes and the factors

which determine the bactericidal effect they exert, got quite to the end of our story. You will find the following experiment very full of instruction, and it has, as we shall presently see, very important applications in connexion with the treatment of wounds.

We take a couple of looped pipettes, such as are shown in fig. 7, and we proceed as follows: In a first operation we fill into the cultivation chamber in the barrel of the pipette a nutrient medium which will reveal by a colour change any growth of streptococcus (we have such a medium in cane-sugar litmus-broth). We then fill up to a fiducial mark on the stem with a 15 per cent. solution of citrate of soda, blow this out again, and then fill in with a unit volume of blood from the sterile finger. We now, after carrying the blood several times up and down the capillary stem so as to mix thoroughly with the citrate, seal up the end of the tube. This done we place our pipettes in a test-tube rack—the one in the upright position, the other with the tip turned upwards—and we let the blood sediment. The red corpuscles will now, in the upright pipette, be brought to the distal; and in the inverted pipette, to the proximal end of the volume of decalcified blood (fig. 6, A and B). And in each case there will on the top of the red, be a layer of white corpuscles, and above this a layer of clear plasma. We now, taking, of course, the pipettes in hand one after the other, make into each a minimal implantation of streptococcus. We do this by just dipping the tip of the tube into a streptococcus suspension and then very cautiously bringing the corpuscles, or, as the case may be, the plasma, into contact with the infected inner surface of the tip of the tube. We now incubate the pipettes for three or more hours—the one in the upright, the other in the inverted position. And finally, using the procedure as described in my text-book of technique,<sup>1</sup> we carry up the implanted blood into the nutrient fluid provided in the incubation chamber.

We obtain in experiments of this sort always one invariable result. In the case of the pipette which is kept upright—i.e., in the case where the implanted streptococci have to run the gauntlet of the corpuscles—the microbes are all killed off, and in consequence our nutrient medium undergoes no change. In the case where the pipette is inverted and we implant directly into the plasma the streptococci survive, and our nutrient fluid changes from blue to red.

And we can vary our experiment in all sorts of ways and still

<sup>1</sup> "Technique of the Teat and Capillary Glass Tube," Constable, London.



each time get the same result. For instance, we can, very easily, cut the red corpuscles out of our experiment and work, as in the wound, only with leucocytes and plasma. Or we can at the outset invert our pipettes, and then, after implanting into the plasma, dispose them upright, thus raining down our leucocytes upon the microbes and aborting the infection.



FIG. 6.

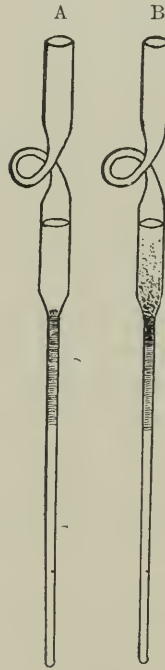


FIG. 7.

FIG. 6.—The figure shows a magnified view of the distal ends of the capillary pipettes represented in fig. 7, filled in with citrated blood, which is disposed, with respect to microbes positioned at +, in A in the *agathotropic*, and in B in the *kakotropic* arrangement. In A the pipette has been placed upright, in B in the inverted position, and in each case the blood has settled down in three layers, the red corpuscles below, a layer of white corpuscles above them, and at the top clear plasma.

FIG. 7.—In A and B the elements of the blood had before implantation been allowed to settle down respectively into the *agathotropic* and *kakotropic* arrangement, and we have represented here the final result, the nutrient medium remaining in the former case sterile, while it shows in the latter a microbial growth.

All this shows, as I think, that we have arrived at a generalization which must be important for practice. Let me formulate it for you thus :—

Where the leucocytes are in the front rank and the blood fluids behind, the elements of the blood are marshalled in that order which is most favourable for combating streptococcic infections. I will venture to call that arrangement the *agathotropic arrangement*; but I should have no quarrel with any lover of superlatives who might feel moved to call it the *aristotropic arrangement*; nor yet with any grammatical purist who might prefer to call it the *eutropic arrangement*; I would only insist that where we set out to extinguish a streptococcic infection we ought to place the leucocytes in the forefront of the battle.

When we have realized what is the favourable arrangement of the elements of the blood for the combating of streptococcus infections, there will have come home to us also that there is an unfavourable arrangement. And we have seen that the unfavourable arrangement is that in which the blood fluids are ranged up in front and the leucocytes are behind. I would ask you to let me call that the *kakotropic*, but there is no reason why you should not call it the *kakistotropic* or the *dystropic arrangement*. With this I bring to a conclusion the preliminary portion of our subject-matter, and let me say that of the lessons that can be drawn from it many will only appear later.

From the introductory studies which have up to this occupied our attention, I pass to the subject-matter proper of this lecture.

#### THE TREATMENT OF MICROBIAL INFECTIONS OF WOUNDS.

I would propose, despite the fact that this will sometimes carry me over ground traversed in my last lecture, to discuss with you, one by one, the various therapeutic procedures which have been employed or suggested for use. And then at the end it will perhaps be possible to draw up something in the nature of a general programme for the treatment of infected wounds in their different stages.

Let me tell you—for you will then see our road ahead—what are the therapeutic procedures we shall be dealing with. These are—and I enumerate them in the order which I shall follow—*treatment by antiseptics*; *treatment by surgical procedures*; *treatment by “physiological,”* or, as I would now for the sake of clearer definition wish to call them, *“phylacagogic” methods*; and, lastly, *treatment by vaccines*.

## I.—TREATMENT BY ANTISEPTICS.

That one ought, where the organism is unable to deal effectually with an infection, to resort to the use of antiseptics seems to many minds to be perfectly clear *a priori*. Now when a proposition of this sort is said to be evident *a priori*, what this really means is, not that the proposition is established every day by clinical experience, but that it has been demonstrated in the past by laboratory experiments; and that we may confidently take those laboratory experiments as our guide.

But, of course, everything will here hinge upon the question whether the laboratory experiments which we trust accurately reproduce the conditions in the wound. Now the earlier experiments on antiseptics—and it is always upon antiquated experiments that current doctrines rest—did not even aim at reproducing the conditions in the wound. The antiseptics came into application, not as in the wound upon microbes enveloped in albuminous fluids, but upon microbes simply suspended in water. And again it passed as an axiom that if *in vitro* all the microbes were destroyed, then also *in vivo* all the microbes would be destroyed; and there was no thought that the microbes in the cavity and the recesses of the wound—and, above all, the microbes in the infiltrated or granulating wall—might escape destruction.

We have lately in the matter of theoretical requirements made satisfactory advance. We now ask of laboratory experiments with antiseptics that they shall conform more closely to the conditions in the wound; and we ask in particular that it shall be kept in view that we deal in the wound with microbes enveloped in albuminous fluids; and that antiseptic solutions cannot come into direct contact with all the microbes which we wish to destroy. In other words, we now ask in connexion with every antiseptic for a testimonial of *bactericidal efficacy in albuminous fluids*, and for a testimonial of *penetrating power*. And in accordance with this we find inserted into the prospectus of every new antiseptic this double certificate of character. It would plainly be beyond the scope of this lecture to look into all these certificates; but it will perhaps be useful to explain in a general way what sort of an attitude one ought to take up when traffic is made with such credentials.

*On Certificates of Character which testify to the Bactericidal Efficacy of Antiseptics in Albuminous Fluids.*—It is familiar matter that the ordinary antiseptics (I shall return later to the case of some

very special antiseptics) form chemical combinations indifferently with all native albumens, and not specifically with those which compose bacterial protoplasm. It is a corollary to this that whenever we have in an experiment an antiseptic, and albuminous substances, and microbes, it will, if we propose to sterilize our albuminous fluid, be necessary to cut down very severely its content in albuminous substances. And, again, it will follow from this that every promoter of antiseptics will, if he is conducting his experiments with any of the albuminous fluids of the body, find it to his advantage to employ serum rather than whole blood, and whole blood rather than pus. And, again, when he is operating with serum he will find it to his advantage either to operate with very little serum, or—and this amounts to the same—to use a large number of volumes of antiseptic to every volume of serum. This particular device is, one can see, legitimate or illegitimate according as the experiment is designed to elicit what strength of antiseptic is required for flushing out and irrigating a wound; or to create the impression that the circle has been squared, and that an albuminotropic antiseptic can do its office efficiently in the presence of albuminous substances. Let us reflect that if our ordinary antiseptics, instead of combining as they do with the albuminous substances of the blood, combined only with microbial protoplasm, it would be a perfectly proper policy to administer them intravenously in septicæmia.

Let us, however—for the above may be thought to savour of theoretical discussion—come down to the hard fact. It is, we have found, practically impossible, when employing as material pus from foul, suppurating wounds, to sterilize it by addition of any ordinary antiseptic.

Let me with the aid of these rough coloured diagrams (reproduced with shading instead of colour in fig. 8) indicate what here stands in our way. In fig. 8, B, I have a representation of a test-tube full of thick pus, and I have represented the thick albuminous pus by a uniform coating of deep yellow, and the contained microbes by a stippling of orange. In fig. 8, A, I have represented in a very rough way by strokes and cross strokes of blue chalk the effect that would be obtained by an addition of antiseptic. You will appreciate that no matter how long I go on imposing blue strokes upon my yellow ground, there will, because my cross strokes will never fuse and run into each other, still remain over small islands of yellow stippled with specks of orange. Now we have here a rough representation of what occurs when we add an antiseptic to pus and shake it up. What that gives us is a

system made up of islands of pus intersected by channels of antiseptic—the sphere of action of the antiseptic being in each case limited by confining banks of coagulated albumin.

When we dilute pus with very many volumes of an antiseptic solution, as we do in washing out a wound, we, of course, effectively sterilize.

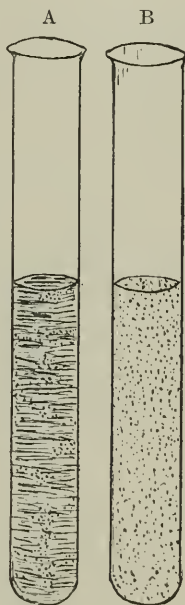


FIG. 8.

*Certificates of Character attributing to the Antiseptic Penetrating Powers such as would render it Efficacious in the Sterilization of the Wound.*

It will be well in embarking upon the discussion of this question to place it clearly before ourselves that when we speak of an antiseptic having *penetrating power*, we mean that the active agent will diffuse out into, and come into operation in, any contiguous fluid.

In other words, if I may illustrate exactly what I mean by the aid of this diagram (fig. 9), we say that a chemical agent has penetrating power when applied in a fluid *a*, it passes out into, and produces its effect in, a contiguous fluid *b*—and let me, for the sake of facilitating exposition, call *a* the *disbursing fluid*, and *b* the *recipient fluid*.

Having recognized that penetration is only diffusion under another name, it will be plain that we shall be able to consider the question of



penetration without travelling outside familiar ground. We know in connexion with chemical agents generally that their rate of diffusion, i.e., the rate at which they will pass out from a disbursing into a recipient fluid, will—exclusive of the influences exerted by temperature, extent of surface of contact, and such-like—be determined by the concentration of the chemical agent in the disbursing fluid and the receptivity of the recipient fluid—in other words, it will depend upon the respective *plenitude* and the *avidity* of the contiguous fluids.

Now, in the case of the ordinary antiseptics which are employed in wounds we have chemical agents which, owing to their toxicity and escharotic properties, are of necessity brought into application only in dilute solutions; and, moreover, many of these chemical agents are not very soluble in water. By consequence, when dealing with ordinary antiseptics, we are confronted with conditions which are unfavourable to diffusion. They will resemble those roughly indicated in the diagram on the blackboard (reproduced with stippling for colour in fig. 9, A), where we have a dark blue disbursing fluid superposed upon a colourless recipient fluid, and have below the line of contact of the fluids a comparatively narrow zone of paler blue representing a slight carrying over of colour by diffusion.

But in reality the conditions in the wound, where our recipient fluid is instead of water a concentrated albuminous fluid, much more nearly resemble those depicted at c (fig. 9, c). Here we have, as before, our blue disbursing fluid, but there has been substituted for the colourless watery recipient a deep yellow fluid stippled with orange, representing pus with scattered microbes. And you see what we get. Instead of the narrow blue zone which diffusion gave us in fig. 9, A, we have now a much narrower zone which is—for yellow quenches blue—nearly colourless. I think you will recognize that under conditions such as are here postulated, the prospect of getting any penetration which would be worth while may be given up.

But perhaps you will suggest that one ought not to place too much confidence in demonstrations conducted on blackboards with coloured chalks, and you would wish the experiment transposed from the blackboard into the real world in order to see whether it would also apply there. Let me suggest such an experiment.

I will suppose that we have as our disbursing fluid a dilute solution of nitrate of silver standing over a recipient fluid consisting, in the one case, of water derived from a fresh-water pond, and, in the other case, of sea-water—both peopled with living organisms. You will discern



what would then happen. In the case of the pond water, the nitrate of silver will slowly diffuse into it, killing, as it is carried in, all the living organisms it encounters. In the case of the sea-water, the effect would be quite other. Where the two fluids meet, the nitrate of silver would be thrown down and rendered inert. And below, the marine life would go on absolutely unaffected.

But you will wish for actual experiments conducted with an anti-septic, and blood fluids, and leucocytes, and microbes such as we have in the wound. Let me therefore show you here in the form of a rough

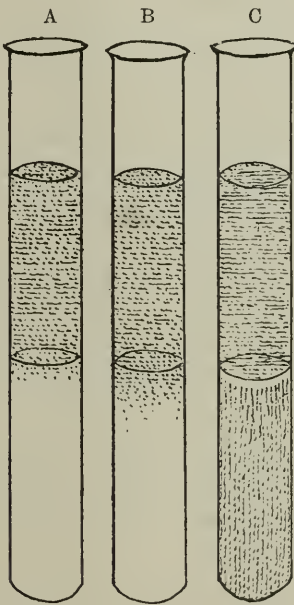


FIG. 9.

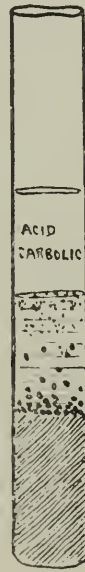


FIG. 10.

Fig. 9.—In A is represented a test-tube containing : above, a watery solution of methylene blue, and below, clear water. In B is represented a test-tube with a watery solution of methylene blue above and strong salt solution below. In C we have represented above a deep blue, and below a deep yellow fluid.

diagram the results of typical experiments—experiments carried out with blood implanted with streptococci, and then centrifugalized and allowed to clot in emigration tubes, and then covered in with carbolic acid in strengths of from 1 in 50 to 1 in 20 (fig. 10). You will see that we have here in our white clot, and—I want you to note—quite close up under the covering layer of antiseptic, colonies of streptococci derived from those we have implanted. And below—as you discerned would

happen in our sea-water—you have the living elements going about their avocations quite unaffected. The leucocytes have, as you see, emigrated into the white clot; and they have, in point of fact, been hunting down and ingesting the microbes. And I see no reason to doubt that the results would have come out just the same if we had in our experiments used instead of carbolic acid any other of the antiseptics which are employed in wounds.

With this, I think we have, in connexion with the sterilizing and penetrating power of ordinary antiseptics, got before us all the really vital points in the evidence furnished by laboratory experiments. And let me now—for you will bear in mind that the advocates of antiseptics rest their case almost exclusively on the data of laboratory experiments—try to summarize for you what these experiments really teach.

And there are here, as I see the matter, two distinct issues. The *first* has reference to the effect exerted upon the microbes of the pus which is removed by washing; the *second* to the effect exerted on the microbes which are left behind in the wound.

In connexion with the first of these issues, it has been shown that while it is all but impossible to sterilize undiluted pus by any addition of antiseptics, it is quite practicable, by diluting and mixing the pus thoroughly with many times its volume of an antiseptic fluid, to kill all the microbes.

But, note, to kill these microbes is simply a work of supererogation, for they are in any case destined to go down the drain. And, moreover, pus is not in the same case with typhoid stools, which we might hesitate to put down the drain unsterilized.

Coming, then, to the microbes we have really to combat—those that washing does not remove from the wound—it will be clear that these will be killed off only so far as the antiseptic may possess penetrating power. And we have seen how the case stands with regard to this. We have appreciated that our antiseptics are incapable of penetrating through anything more than the very thinnest film of pus. They will therefore fail to kill the microbes which washing does not dislodge from the blind passages and culs-de-sac and pockets of the wound. And *a fortiori* the antiseptic will not come into operation upon the microbes in the solid barriers of albuminous substance provided, in the early stages of the wound, by exposed muscle and connective tissue, and, in the later stages, by infiltrated walls or granulating membrane.

And what holds true of the suppurating wound would clearly hold true also of that filled with blood-clot. The antiseptic, if it exerts any

influence at all, will exert it here only at the particular spot to which it is applied.

I am not at all sure that the kind of evidence I have just tried to summarize—or, indeed, any array of laboratory experiments—could convert from his beliefs the man who knows with confident *a priori* knowledge that antiseptics must be useful in wounds. With regard to that type of man I feel that, despite the fact that his faith in antiseptics is really based upon laboratory experiments, he will, now that the old laboratory experiments have been invalidated by the new, desire to appeal from these to the tribunal of clinical experience. And he will, I take it, desire to discover by cross-examination whether clinical or bacteriological investigation has not furnished data which might tend to show that the course of wound-infections has sometimes been favourably modified by treatment with antiseptics.

On the important principle of logic here raised—i.e., on the issue as to how far therapeutic measures ought to be adjudicated upon by laboratory methods, and how far by an appeal to clinical observation—I would here say only this. What we are, in difficult issues such as that in which we are here involved, concerned to arrive at is a verdict based on unambiguous evidence; and I feel confident that clinical observation cannot safely adjudicate upon a therapeutic measure unless it happen to give either outstandingly good or outstandingly bad results. Where the results are neither brilliantly successful nor the reverse—and antiseptic treatment of wound infections is precisely an instance in point—I submit that we shall be well advised if we guide ourselves always, when this is unambiguous, by the verdict of laboratory experiments.

With this much by way of protest and explanation, I will try to put before you the clinical evidence as I see it; and to resume for you the data of our bacteriological examinations of wounds treated with antiseptics.

I can put what I have to say in a very few words. I have not myself come across—and I have the permission of all my fellow-workers to say that they also have not come across—any satisfactory clinical or bacteriological evidence of the utility of antiseptics as employed in infected wounds. In connexion with this, let me say that working as we have been for the last year in the largest base hospital in Boulogne, we must, I think, have watched and examined bacteriologically very many thousands of wounds—to say nothing of our having conducted the bacteriological examinations in no fewer than four long

series of clinical experiments undertaken to put to the test the confident prognostications of reputable promoters of this or that antiseptic. And let me also say here that the opinion I have just expressed—i.e., the opinion that, judged by its clinical results, the antiseptic treatment of infected wounds is of quite doubtful utility—is that which is, I believe, held by the very large majority of those who have had prolonged practical experience in this War.

I am well aware that also contrary opinions have been expressed. I would, however, submit that these should be very largely discounted. For the more part they are opinions expressed by persons possessing a very limited experience, and unfamiliar with the course the wound follows when left to itself. Moreover, the observers here in question were, as I think, deceived by the fallaciously favourable appearance of the wound in the stage that precedes the advent of suppuration. Coming to those favourable opinions which have, as I hear, been expressed by a few experienced and very competent observers, I would submit that it is possible that credit may have been given to the antiseptic where it ought really to have been given either to the menstruum in which it was applied, or to incidental circumstances attendant upon the treatment. Thus, for instance, continuous irrigation practised with antiseptic solutions would effectively wash out the wound and keep it constantly moist; a hypertonic menstruum used as an excipient for the antiseptic would act as a lymphagogue; peroxide of hydrogen would effectively assist in the evacuation of the pus; carbolic acid or other antiseptic used in the form of hot fomentations would bring heat and moisture to bear; and no doubt many different varieties of antiseptic would induce temporary hyperæmia. I think it will be seen that favourable results occasionally manifesting themselves after antiseptic treatment might quite well be interpreted in the sense here suggested.

So far we have considered in connexion with the clinical data only the question whether the antiseptic has had any beneficent effect in the wound. Let us now turn the leaf, and see whether there are entries also on the debit side; and let us here consider the *effects produced in the wound itself*, dealing afterwards with those produced on the healthy skin in its neighbourhood. In connexion with this, let me first emphasize the fact that disastrous results—I am thinking more particularly of the development of gaseous gangrene—supervene when antiseptics made up in viscid or semisolid excipients are introduced into the wound. For this result the excipient, which confines the discharges and imprisons the infection, would seem to be primarily responsible.



It is, however, not only antiseptics in unsuitable menstrua which arrest the outflow of lymph and imprison the infection. With perhaps the exception of carbolic acid employed in concentrated form, all antiseptics in their degree, and strong antiseptics in particular, would seem, by coagulating the albuminous substances on the surface of the wound, to exert this prejudicial effect.

Coming now to the *application of antiseptics to the skin surface in the neighbourhood of the wound*, we have, of course, here a procedure which has exactly the same aims as the preparation of the skin for operation, except only that it is here foreseen that the wound will remain open.

The usual procedure for the sterilization of the skin in field ambulances is to paint strong iodine all round the wound, and then—plant for sterilizing dressings not being available so far to the front—to cover in the wound with cyanide gauze. This in many cases is followed by results like those which used to be obtained when carbolic fomentations were used as a prelude to surgical operations. There is produced—and this applies quite generally in connexion with every application which “irritates the skin”—first, reddening of the epidermis; then, effusion and blistering; and then there develops in the blisters a luxuriant growth of serophytic microbes. In other words, by our misdirected energy we cultivate at the very doors of our wound, upon the very area of skin which we intended to keep free from microbes, a rich harvest of streptococci and staphylococci.

This, however, has been of the nature of a digression, and I must now, before saying something about a fundamentally different variety of antiseptics, come back and try to make clear what is the general conclusion which emerges from the clinical study of the effects of antiseptic treatment. It can be put into a sentence. The data of clinical observation, such as they are, not only confirm the verdict of the laboratory as to the inefficacy of antiseptic treatment, but they suggest that such treatment may sometimes be not only useless but prejudicial.

*Antiseptics which are, instead of Indiscriminately Albuminotropic, specifically Bacteriotropic.*

I have now to say just one word about a kind of antiseptic which concentrates its chemical energy upon the microbe, instead of, like the ordinary antiseptic, expending it wastefully upon the albuminous substances in which the microbe may be embedded. Those of you



who have followed the development of "chemotherapy" will immediately perceive that I refer to *salvarsan* and ethylhydrocuprein hydrochlorate<sup>1</sup> (now known as *optochin*), and will appreciate that, if these chemical agents can be shown to exert a powerful bactericidal effect upon pyogenic organisms, and in particular upon serophytes, they might perhaps usefully be applied both directly in the wound and also by the channel of the blood, reaching in these ways all the microbes in the body with the exception only of those embedded in dead spaces and infiltrated tissues.

We have made in connexion with this subject-matter as yet only preliminary researches; and I do not here propose to say anything more than that my fellow-workers, Captain S. R. Douglas and Lieutenant A. C. Inman, working the one with *salvarsan* and neo-*salvarsan*, and the other with *optochin*, have found that these kill off the streptococcus in serum, and that *optochin* in particular kills the streptococcus in serum in very high dilutions.

## II.—TREATMENT BY SURGICAL PROCEDURES.

A long series of surgical procedures, all having reference to the treatment of wound infections, might quite properly come up for consideration here if this were a treatise instead of an introductory lecture. Let me, however, just run over the list, indicating in connexion with each the place that it would take in the treatment of the wound infections. We have, *first*, the opening up of the wound and the incision of the tissues undertaken to get drainage for imprisoned infections; *secondly*, the washing out of the wound and the irrigation of the walls with therapeutic fluids; *thirdly*, the immobilization of the limb for the avoidance of auto-inoculations; *fourthly*, the ablation of the heavily infected and infiltrated walls and floor of the wound; and, *fifthly*, the secondary suturing of the wound for restricting the area of exposed and infected surface, and closing the wound when the infection has been overcome.

I have already treated of the first of these procedures. Its rationale and the methods of making it really effective were considered in a Memorandum on Wounds which was officially circulated to the Medical Service of the Army, and which was published in the *Lancet* of April 24, 1915, p 873.

<sup>1</sup> In connexion with this drug, *vide* the author's treatise on "Pharmacotherapy and Preventive Inoculation applied to Pneumonia in the African Native," Part I, and Appendix I.

The second and third procedures—those for the irrigation of wounds and the splinting of fractured limbs—form the subject-matter of the demonstrations which are to be given in this exhibition.<sup>1</sup>

The principles involved in the so-called “excision” and “secondary suture” of wounds I propose here very briefly to consider.

*Ablation of the Heavily Infected and Infiltrated Lining of the Wound.*

The customary procedure in wounds is to remove only those portions which are sloughing, and to amputate, where a limb is involved, only that portion which is deprived of its blood supply or quite hopelessly infected. To Colonel H. M. W. Gray is due the credit of pointing out and enforcing by a long series of cases successfully treated under his supervision in the military hospitals at Rouen, that it is possible in suitable cases to abort wound infections by the excision of the infiltrated wall and floor, the operation being completed by sewing up the wound immediately, or after-treatment with hypertonic salt solution. I may explain that the kind of cases which Colonel Gray selects for treatment by excision are scalp wounds and what we may call “gouged out” and “punched in” wounds of soft tissues.

The theoretical considerations which commend the procedure of Colonel Gray are that it removes heavily infected portions of tissue which, though they could be brought back to a healthy condition, are for the moment incapable of contending successfully with infection. And, above all, the method holds out a prospect of immediate victory over the infection, and, of course, practically immediate convalescence.

There, however, suggests itself in connexion with the procedure the plaguey question as to what amount and kind of microbial infection we can allow ourselves to shut up inside a wound. This is the issue which confronts us everywhere in connexion with secondary suture.

*Secondary Suture of Wounds.*

That a deep wound when once infected must be allowed to granulate up from the bottom, and that a surface wound must be allowed to close by the growing in of skin from the sides, are maxims which would be in their proper place in a system of surgery which

<sup>1</sup> See the Memorandum by the author on “The Employment of Bandages for the Irrigation of Wound Surfaces with Therapeutic Solutions, and the Draining of Wounds,” *Lancet*, October 16, 1915, p. 879.

aimed at doing all it could to delay healing, and keep the patient in hospital for the longest possible time.

On the other hand, it might quite well be imprudent to embark upon an indiscriminate policy of sewing up microbes in wounds to accelerate healing.

This is the kind of dilemma which sooner or later confronts us in every wound. To resolve it we must sit down and take thought, and ask ourselves what precisely are the risks we have to take; and whether there are not precautions to be adopted: and whether we really are, in the matter of the closure of the wound, irretrievably condemned to a policy of inertia and inaction.

Let us place before ourselves first these facts. An *infection of subcutaneous tissues* is always of more serious import than a mere surface infection. There will always be something unfavourable either in the general condition or in the histological conditions when microbes succeed in maintaining themselves in the tissues. Here at least the leucocytes and the blood fluids ought to be able to hold their own. Exactly the contrary holds true of *surface infections*. On denuded surfaces, no matter how healthy the condition, the microbes, though they may sometimes have to struggle, will always find means to maintain themselves. We may, I think, attribute this to the circumstance that on an exposed surface the leucocytes are subjected to every kind of uncongenial condition—to aerobic surroundings, to desiccation, to a tryptic or diminished antitryptic environment, and—who can tell?—perhaps also to conditions of famine.

We now see that where we have an infection of tissues—i.e., a form of infection which would, if conditions had been favourable, have been driven out and been converted into a mere surface infection—we shall be taking a serious risk if we sew up the wound. For that would be to follow leisurely and with deliberation the disastrous practice of those who, in the earlier period of the War, in the hospitals at the front, precipitately sutured infected wounds, or securely sealed up imprisoned infections by resorting to escharotic or gluey antiseptic applications.

On the other hand, we see that, if we are dealing with a purely surface infection, it will be possible, by bringing the opposing surfaces of the wound into intimate contact, to procure for the leucocytes and blood fluids those more favourable conditions which prevail in the interior of tissues; and, by procuring these, to contribute to the destruction of the microbes.

But clearly our decision as to whether we may, or may not, bring the

edges of the wound together ought not to be determined solely by whether we are dealing with an imprisoned or a surface infection. There will in connexion with surface infections be other factors to be taken into account.

There is, without doubt, a great deal of sound common-sense hidden away in the maxim that an infected wound which goes down into the depths must be allowed to granulate up from the bottom. The lessons which that precept enforces are, first, that if we allow dead spaces to develop in closed wounds we must expect to find the infection go ahead therein; and, secondly, that, pus gravitating downwards, dead spaces will tend to develop at the bottom of the wound, while the upper part unites, converting the dead space into an abscess sac. Of all the mechanical conditions which have to be secured in secondary suturing of wounds, the most important will clearly be the avoidance of such dead spaces.

There is another important consideration to be kept in view—and this time it is not a mechanical consideration. We must expect success or failure in secondary suturing to depend in large measure on the condition of the surfaces and the number of microbes on those surfaces. Where we have very few microbes and a profusion of active leucocytes we may confidently look forward to success. Where we have a large population of microbes and disintegrated leucocytes it will be only reasonable to expect to fail.

This much by way of general principles. And let us now turn and inquire how these can be applied in practice, and by what methods we ought to proceed in deciding whether a wound is in fit condition to be closed down. We shall have first to certify ourselves that we are dealing only with a surface infection; and then we shall require a method to tell us what is the condition of the leucocytes and the number of microbes on the surface of the wound.

The first of these points is resolved by a mere clinical examination. As long as the floor and walls of the wound are still indurated we can be sure that we are still dealing with an imprisoned infection. On the other hand, when we have lying naked before us in the wound a system of lymph spaces communicating freely with one another, filled in with clear fluid, and fed from full capillaries, we then know that we have got rid of the imprisoned infection; and that if any infection still lingers it is a purely surface infection.

There remains the question as to what number of microbes are lying on the surface of the wound, and what force of active leucocytes



we have there at disposal. It will probably in the case where the walls and the floor of a wound have been freshly resected—provided always that this has been done in a workmanlike way—be unnecessary to insist upon such inquisition. But where we are dealing with a granulating wound whose edges have not been refreshed it will assuredly be required.

The method of obtaining the information is quite simple. We first prepare a number of cover-glasses by coating them on one side with a layer of agar or serum (serum is preferable) and then letting them dry. We then bring the coated side down upon the surface of the wound. And we make in this way (using, of course, a number of cover-glasses) a series of impression preparations. We now fix and stain either by the method of Gram or with carbol thionin. Microscopic examination then tells us whether we have few or many leucocytes; and whether the leucocytes are degenerated or well preserved.

And a concluding word may now be said on the technique of closing in the wound and on the procedure to be followed.

Except possibly in cases where we pass directly from the excision of the wound to secondary suture it will be well always to preface the operation by phylacagogic treatment conducted upon the lines laid down in the next section of my lecture, and controlled, of course, by the making of impression preparations.

Where we are dealing with a deep and extensive wound it will be well to undertake the work in stages and to content oneself at first with bridging and subdividing the wound by bringing the walls into application by sutures passed in from the skin from each side, deep into the tissues. And I may add in connexion with such sutures that they ought, in order to prevent extension of the microbes along their track, to be withdrawn at the earliest possible moment. Where bridging operations have been undertaken, the interspaces between the connecting bridges ought still to be irrigated and drained. Where it is a question of the closure of large, gaping, "gouged-out" wounds, it will be well to choose for the patient such a position as will minimize the gaping of the wound and the strain upon the sutures. Where large surfaces of skin have been carried away, grafting ought to be resorted to, or the skin ought to be undercut and the liberated flaps drawn over the wound by strips of adhesive plaster. Where a limb has been amputated by the flapless operation, the sleeve of skin ought to be slowly drawn down over the stump by an extension apparatus.



## III.—TREATMENT BY PHYLACAGOGIC METHODS.

When I was discussing with you the clinical results obtained by the antiseptic treatment of wounds, I drew your attention to the fact that with this a variety of physiological stimuli were brought into application, and that the good results which were upon occasion witnessed might quite well be credited to these.

Now, of course physiological stimuli are applied in wounds, not only incidentally to antiseptic treatment, but also of set purpose as therapeutic agents. Dry or moist heat is brought into application; ether is introduced into the cavity of the wound; stimulating ointments and lotions are applied: or, as the case may be, the wounds are uncovered to air, they are exposed to sunlight; or electricity and radium are exploited: each of these measures being resorted to with a more or less vague idea that that particular form of stimulus will directly or indirectly assist in combating the wound infection.

So far as I can discern, all these are forms of treatment—and the same holds true also of the surgical procedures we have considered above, of the methods of Bier, and, of course, of saline solutions and vaccines—which are capable of bringing the blood fluids and leucocytes into application in the wound. And I would submit that they can be useful only so far as they serve as—let me suggest the word—*phylacagogic* agents.

We ought therefore to give up employing these agents as fetishes, and ought to prelude their use by a careful study of their physiological action.

This field of study being so endlessly wide, I propose here to confine myself to the study of strong and weak salt solutions. We shall see that we have in these *phylacagogic* agents: in hypertonic saline solution a *lymphagogic*; and in physiological saline solution a *leucocytagogic* agent.

*Action of Hypertonic and Physiological Salt Solutions.*

Hypertonic salt solutions exert three kinds of effects: a physical effect; a physiological effect; and an effect on the condition of the wound and the bacterial infection. Let me deal first with the physical action; for it was this which I had chiefly in view when I suggested the employment of hypertonic solutions in the treatment of infiltrated tissues.

*Physical Action of Hypertonic Saline Solutions.*—I can perhaps most conveniently introduce what I have to say with reference to the physical action of strong salt solutions if I take up again the discussion of the phenomena of diffusion at the point where I left off when I was discussing with you the penetrative powers of antiseptics.

What we were there considering was the conveyance of the chemical agent outwards by diffusion from the disbursing into the recipient fluid; and we did not then need to concern ourselves with the return movement from the recipient into the disbursing fluid. But in connexion with every diffusion there is always a process of barter and exchange; so that when we have, as in fig. 9, A, as our disbursing fluid, water containing a colouring material; and as our recipient fluid, clear water; there is, in point of fact, not only a passing out of colour into the recipient fluid, but also a return flow of water: and, as a result, a certain dilution of the disbursing fluid.

It will subserve the purposes of exposition, and perhaps help to call up in the mind clearer pictures, if, speaking as we do of the conveyance of the chemical agent into the recipient fluid as *diffusion*; we may call the conveyance of the water (or other excipient) into the disbursing fluid *infusion*.

Let us now look—for this will carry us a step further—from fig. 9, A, to fig. 9, B. Here, while the coloured fluid in the upper part of the test-tube remains as before, strong salt solution has at the bottom of the tube been substituted for clear water; and we have now a double process of diffusion—a diffusion of colouring matter downwards into the salt solution, and a diffusion of salt upwards into the coloured water; and along with this we have also infusion into each fluid. This will give, on either side of the surface of contact, a broader zone of mixture; and you have in fig. 9, B, below the junction of the coloured and colourless fluids, a broader band of stippling representing colour.

We can by the aid of a very simple experiment satisfy ourselves that there is a much greater indrawing with a strong salt solution than with a weak solution or simply water. We take a capillary pipette, seal it at the end, fit on a rubber teat, and then squeezing this with the fingers so as to get up a positive pressure in the interior, introduce a small portion of the wall, about half-way down the stem, into the flame of a by-pass. The glass will, as soon as it softens, blow out here and give us a lateral opening. We now resect our capillary stem at some little distance above, and again some little distance below, this opening. Having provided ourselves with such tubes, we fill in one

with strong salt solution and another with water, and then pour a little watery solution of methylene blue into a Petri dish or other shallow, flat-bottomed vessel. This done, we take up first one, and then the other, of our capillary tubes. We seize it in the middle with a pair of forceps, hold the central orifice uppermost and the two limbs horizontally, and in this position immerse for a matter of a second or two into the coloured fluid. We then wash the outsides and compare the two tubes.

In the tube of water there will be three narrow, and in the tube of salt (for there is here more indrawing) there will be three very broad bands of blue; one occupying the middle and the others the ends of the tubes.

And let me show you also another experiment which brings clearly before the eye the drawing power of salt. I have here two test-tubes filled in each case to a depth of 3 or 4 cm. with a watery solution of methylene blue; and standing in these test-tubes I have two pieces of glass tubing, open at both ends, and packed with moist cotton-wool<sup>1</sup>; and, lastly, in the one tube I have at the top of the cotton-wool a layer of salt. These pieces of tubing have been standing, as they are standing now, since last night. I now lift them out, and you see that here again the methylene blue has been drawn far up into the tube where we have the salt, while it has in the companion tube been carried in only a very short way.

With this we have come some little distance on the way to an understanding of the physical action of hypertonic salt solutions applied in wounds. This further series of test-tube experiments (reproduced in fig. 11) will, I think, show you exactly what happens.

I have here a tube of water-agar (fig. 11, A). That is to say, I have dissolved in the distilled water, which is here going to serve as a recipient fluid, a sufficiency of agar to set it into a firm jelly. And I have added also a little trace of nitrate of silver. Into the next tube (fig. 11, B)—a precisely similar tube—I introduced some twelve hours ago a cube of sodium chloride, imposing it upon the surface of the just moist jelly. And you here see the result. The sodium chloride has diffused into the water-agar to a considerable depth, discolouring the silver salt. But the important thing to note is the counter-movement. Water has been drawn out from the jelly, and we have now round what remains of the pellet of salt, as you see, a layer of fluid a centimetre or a centrimetre and a half in depth.

<sup>1</sup> The actual procedure employed was to fill in the cotton-wool, to immerse in water, and then to remove the superfluous fluid by centrifugalization.

This is the particular effect which I set out to get when I suggested the employment of hypertonic solutions as a dressing for infected surfaces. I expected to get that combination of diffusion and infusion which we have under our eyes here—in other words, a process of barter in which salt and water should be exchanged, not in volumetrical equivalents, but, as you see here, in the ratio of very many volumes of fluid for one of the solid.

Let me try to make plain another point. The whole question of diffusion and infusion is to the ordinary medical man and biological student complicated by ideas connected with osmosis, his mind being taken up in particular with the idea that crystalloid substances can, and albuminous substance cannot, traverse a so-called vegetable or animal membrane. In fact, some have come to believe that it is the interposition of a membrane between the recipient and discharging fluid which calls into existence the attractive forces of salt for water and all the phenomena of infusion; or, to put it in other words, that it is the interposition of the sieve which confers upon the salt the power of drawing water to itself. It is this confusion of thought which has inspired the criticism that while hypertonic solutions may be competent to draw fluid out from the walls of a wound, the fluid thus extracted will by theoretical necessity be a fluid deprived of all its albuminous substances, and with these of all its antibacterial powers.

The test-tube I now show you demonstrates to the eye that the theoretical deduction here in question is quite unfounded. You see here (fig. 11, c) a tube of water-agar into which I have incorporated a considerable amount of blood. I have dealt with it in precisely the same way as with the tube of silver nitrate agar; that is to say, I imposed upon it yesterday a cube of sodium chloride. You will see for yourselves that the salt has here drawn out from the agar a very considerable amount of a turbid blood-stained fluid; and when I take a sample of this and boil, you will see that we obtain a very heavy precipitate of albuminous substances (fig. 11, d).

I think we may feel reasonably confident that this is exactly what occurs in the wound. And at any rate we may be sure that neither in the earliest stage of the wound, where the tissues lie exposed, nor yet in the later stage, when the walls are infiltrated, have we in the wound, any more than we have here in this test-tube, anything in the nature of a membrane which could filter out albuminous substances.

Arising directly out of the physical properties of concentrated salt solutions is another point which has a very important bearing on the



therapeutical action of these agents. We have seen in connexion with antiseptics that inasmuch as they have as good as no penetrative power, they will exert their effect only at the immediate point where they are applied; and, in the case where the antiseptic marches with an albuminous fluid, only on the face where the two fluids come in contact. In other words, the antiseptic will not diffuse into, and come into application in pus held up in the upper reaches or recesses of the wound, nor even in islands of pus encompassed by antiseptic fluid.

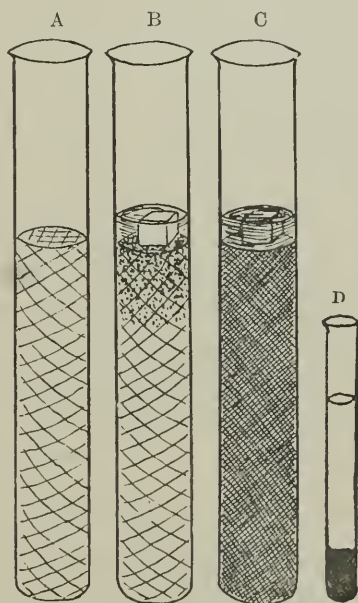


FIG. 11.

- (A) A test-tube filled with water-agar serving as a control to B and C.
- (B) A test-tube filled with silver nitrate water-agar upon which has been imposed a cubical block of sodium chloride, showing the precipitation of the silver salt in the interior of the agar, and on the surface of the agar the fluid which has been drawn out.
- (C) A test-tube containing water-agar in which blood has been incorporated on the surface of the agar; a cube of salt and the turbid blood-stained albuminous fluid which has been extracted by the action of the salt are seen.
- (D) A small test-tube showing the albuminous precipitate thrown down on boiling a sample of the fluid obtained from C.

Now the exact contrary of this holds true of strong saline solutions introduced into the wound. No sooner are they introduced than the sodium chloride—for it is, of course, highly diffusible and is not quenched by albuminous substances—will radiate out by diffusion through all the



fluids of the wound. (In an experiment in which a tube full of water-agar was inverted in a shallow receptacle of 4 per cent. salt solution we found that diffusion carried the salt up  $4\frac{1}{2}$  cm. in a night.) We may therefore picture in our mind's eye, in connexion with strong saline solution applied to the mouth of a wound, that the salt will very soon, in the lower reaches of the wound, attain a high concentration and act as a lymphagogue; and that afterwards it will come into operation in the upper reaches in a dilute, but perhaps still therapeutically valuable, form.

But let us note that this will apply only if the concentration of the saline solution is maintained throughout. Where the salt solution is applied in the form of a damp pad, and is then very rapidly diluted by the exudation, we shall get, but only very temporarily, a hypertonic solution in the lower reaches of the wound, and after that we should obtain only the same effects as with an isotonic salt solution.

Finally—and this is a point which may upon occasion have a practical interest—let us remember that 4 per cent. salt solutions have a specific gravity equivalent to that of the serum; and more concentrated solutions of course a higher specific gravity. It will therefore be theoretically possible by availing ourselves of gravity to get strong saline solutions directly to the bottom, not perhaps of a wound filled with pus, but certainly of a wound whose lumen is occupied only by blood-clot and serum.

#### *Physiological Action of Hypertonic Solutions.*

In connexion with the physiological action of hypertonic solutions—and I have in view those containing about 5 per cent. of salt—we have to consider primarily the effect produced upon leucocytes. And we have to distinguish between the case where the strong salt comes into direct application, and the case where it comes into operation from a distance.

In the former case—both in pus and blood—the leucocytes are broken down; and, in pus, evidence is obtained of a setting free of trypsin. In other words, strong salt solutions acting on a medium containing leucocytes will promote auto-digestion, and provide a nutrient substratum which will, as soon as the excess of salt is removed, favour the growth of microbes.

To study the effect of strong salt solutions acting from a distance we centrifuge blood in an emigration tube or cell<sup>1</sup>; let it clot; superpose

<sup>1</sup> The cell referred to is a special form of cell devised by my fellow-worker, Captain d'Este Emery, and made by luting together two pieces of glass slide with paraffin wax.

our 5 per cent. salt solution; allow time for the salt to diffuse; and then incubate at  $37^{\circ}\text{C}$ . Fig. 12, A, shows the picture that we then get; and I add for comparison fig. 12, B, showing the picture obtained when we impose upon the blood instead of 5 per cent. 0.85 per cent. salt solution. In each case we have: below, the red corpuscles; and above these, the layer of leucocytes; and above these again, the white clot. In A, where the 5 per cent. salt solution has been imposed we have practically no wandering out of the leucocytes into the white clot. We have instead a tightly packed and sharply margined band of leucocytes intervening between the red and white clots. Below this we see a good many leucocytes which have, as it seems, travelled back from the leucocytic layer into the red clot. In other words, we have here, instead of positive chemotaxis as in B, paralysis and presumably also negative chemotaxis. There would seem to be little doubt about the

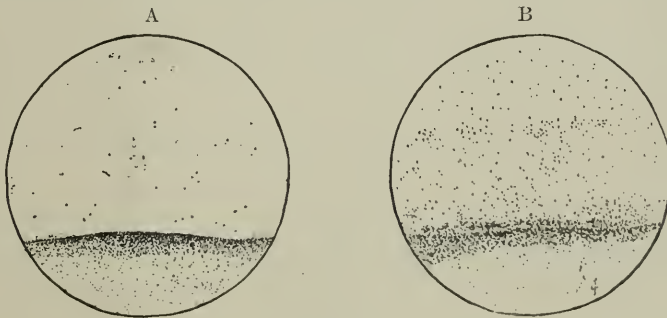


FIG. 12.

We have represented in A and B the lower portion of the white and the upper portion of the red clot obtained by centrifuging blood in one of Emery's cells. The white corpuscles only are shown in the figure. In A, strong salt solution has been imposed upon the free surface of the white clot. In B, physiological salt solution has been imposed. The appearances are described in the text.

negative chemotaxis, for in companion tubes or cells, treated in every other respect than the salt solution as exactly alike, the white corpuscles have by the centrifugalization all been brought to the surface of the red. It will be appreciated in connexion with fig. 12, A, that the elements of the blood have, first by the mechanical force of the centrifuge, and secondly by the chemical stimulus of the strong salt solution, been disposed in what I have called the "kakotropic" arrangement, i.e., the blood fluids are in front and the leucocytes behind. The import of this in connexion with the conduct of the wound will presently appear. For the moment let us merely put it down upon our tablets that it

would seem to follow from what we see in fig. 12, A, that we have in strong salt solution an agent which would be capable of arresting all suppurative processes.

*Effect of Hypertonic Solutions on the Condition of the Wound  
and the Microbic Infection.*

The effect of strong salt solution on the wound will, of course, be the resultant of its physical and physiological effects; and there will be in addition an effect exerted directly upon the microbes.

This last may very quickly be dealt with and disposed of. Percentages of 2 per cent. will begin to inhibit, and 5 per cent. solutions will completely arrest the growth of pyogenic microbes. In other words, undiluted 5 per cent. salt solution will by itself—and this is to be borne in mind in connexion with proposals to combine this with carbolic acid—prevent any growth of microbes in the wound.

Let us, however, turn—for this is of more moment—to the effect exerted by the hypertonic solution on the condition of the wounds. And we may take the case of hypertonic solution applied to a sloughing or indurated wound. Now coming as it will here into direct application upon leucocytes, the strong salt solution will break these up and set free trypsin, and favour auto-digestion; and, coming into operation at the same time on the walls, it will promote an outflow of fluid. The effect of this will be to loosen and separate the sloughs; to disperse the induration; and finally to give us, always provided that the concentration of the salt is maintained, a wound in which muscles and connective tissue lie before us as bare and clean, and as free from pus, as meat exposed on a slab at the butcher's. Moreover, impression preparations will show that the surface lymph is practically free from leucocytes; and that the sero-saprophytic microbes have disappeared, leaving behind only a few staphylococci or streptococci. This complete result is, as a matter of fact, only rarely seen—for applied, in the ordinary way on a pad of lint or gauze, the hypertonic solution is by the outflow of lymph very rapidly diluted.

But whether we have before us the complete, or only the incomplete, result, we have a wonderful improvement upon the state of the wound before treatment. None the less, the prospect for the future is not quite reassuring. In point of fact, we have here, lying open and naked before us, all the lymph spaces; and, moreover, the elements of the lymph are disposed in the "kakotropic arrangement"—the fluid

elements which furnish a favourable culture medium in front, and the leucocytes, which would be capable of combating the streptococci, behind. And, at any rate so far as the superficial lymph spaces are concerned, there would now seem to be nothing to prevent microbial invasion save only that we have here an outflowing current.

But if the salt which has been absorbed into the tissues were no longer carried away, or if the saline solution outside were now to be diluted, the tide might easily turn and flow inward. In short, we see that we have made a successful advance; but that we must, before we can congratulate ourselves, advance a great deal more.

Our experiments in connexion with the bactericidal power of the whole blood tell us what our next step must be. If we want to keep the streptococci at bay, and destroy them, we must get the elements of our lymph into the agathotropic order; in other words, we must now bring forward our leucocytes. Our studies on emigration tell us how to proceed. We shall have to substitute for our 5 per cent. 0·85 per cent. saline solution.

#### *Effect of Physiological Salt Solution.*

It will in connexion with physiological salt solution be unnecessary to go much into detail with respect to its physical and physiological action. All that requires to be emphasized is that 0·85 per cent. salt solution, which we call normal or physiological, is normal and physiological only with respect to its tonicity. It has, of course, a much higher content in sodium chloride than the blood fluids; and will, when placed in contact with these, send into them sodium chloride, exchanging this against other salts. It is to this different salt content that we have to ascribe the physiological action which 0·85 per cent. salt solutions exert when superimposed upon centrifuged blood. Fig. 12, B, has shown us what then invariably happens. The white corpuscles—and in particular the polynuclear white corpuscles—are carried forward by a chemotactic movement in the direction of the free surface upon which the physiological solution has been imposed. And precisely the same occurs in the wound. After the lapse of a few hours the perfectly bare wounds which are obtained by treatment with strong salt solution begin to clothe themselves with a fine grey film. And when we make impression preparations we find on the surface of our cover-glasses a layer of beautifully well preserved polynuclear white corpuscles. In other words, the elements of the lymph are now in the



agathotropic arrangement; and microbes, if found at all, are only very few in number.

This marks another great step in advance. It is, however, as we shall presently see, one that falls very far short of a final conquest over the infection. But for the moment let us turn aside and see what will happen when hypertonic salt solution is applied to a clean, or, as the case may be, a suppurating wound, and is then diluted. We shall see our new data dovetailing in perfectly with the old.

To study the sequence of events where a hypertonic salt solution is spontaneously diluted with outflowing lymph, we carefully clean the surface of a granulating wound. We then bring down upon it—and I owe this piece of technique to my fellow-worker, Lieutenant H. H. Tanner—a *lymph cup* such as is shown in fig. 13, fastening it down securely upon the skin by strips of adhesive plaster. We now introduce a

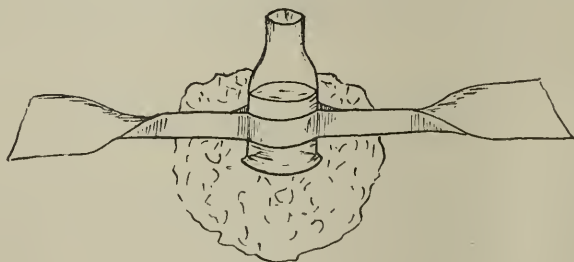


FIG. 13.

measured quantity of hypertonic solution into our lymph cup, and then from time to time re-measure and sample the fluid. During the first hour or more only a *lymphagagic effect* is produced. The fluid in the cup increases in quantity—in an experiment I have in mind the original 3 c.c. of 5 per cent. salt solution increased in three hours to 5 c.c.—but the fluid remains quite limpid. After that a *leucocytagogic effect* begins to manifest itself. First a few, and then more and more leucocytes emigrate into the fluid or the lymph cup; and the streptococci, which in the first phase of the experiment may have been fairly numerous, gradually decrease until it is difficult to find them.

To study what will happen when 5 per cent. salt solution is brought into operation upon a suppurating surface and is afterwards diluted, we take a sample of pus, mix it with 5 per cent. salt solution, and then place it in the incubator. We then dilute with sterile water until we have brought down the content in salt to 0.85 per cent., and then we



incubate for a further period of hours. After the expiration of that period we find—and we may put this down to the setting free of trypsin from the cells broken down by the strong salt solution—a much more luxuriant growth of microbes than in a control sample incubated for the whole period with physiological salt solution.

Reverting now to our wound which was treated with physiological salt solution, you will remember that we had there a position won but not consolidated. And if events were now allowed to take their course, the leucocytes which had been drawn up to the surface, and in particular, perhaps, those which had done service in the destruction of microbes, would not long maintain their vitality. For the conditions which prevail on denuded surfaces are, as we have seen, uncongenial to leucocytes. Now just as soon as the first leucocytes die and break up and set free their trypsin all those in the neighbourhood become involved in the same ruin, providing for any microbes that have survived, or that may come in from outwards, an eminently favourable culture medium. After this we have very soon upon our wound surface, in the place of an almost extinguished infection, a luxuriant culture of microbes. And though, no doubt, it might be possible by instituting continuous irrigation with physiological salt solution, still to hold on, our position cannot really be consolidated by anything except the restriction of the infected surface and the final closure of the wound. In other words, as soon as we have practically conquered the infection surface, we ought, as I said in an earlier section of this lecture, to proceed immediately to secondary suture of the wound.

#### IV.—TREATMENT BY VACCINES.

Treatment by vaccines would have hardly any meaning in connexion with antiseptic treatment; but it is, as a moment's consideration will make plain, very intimately linked up with phylacagogic treatment. For once we have made up our minds that an infection will have to be fought by the protective powers of the organism, it will be reasonable to look round and see whether the protective powers of the organism generally cannot be reinforced. And it will also occur to the mind that not only the blood fluids could be reinforced in protective substances, but also chemotactic sensibility of the leucocytes could be so altered as to give a more vigorous emigration response. If this were secured, the leucocytes would not only come more rapidly and more effectively into action, but they would come into action in the whole theatre of infection

instead of, as in the case where salt solutions are applied, only in those regions into which the salt is conveyed by diffusion.

Having seen where vaccine therapy fits in, we have next to consider how and upon what principle we shall select from the bacterial flora of the wound those microbes which ought to be combated by vaccines. The principle here is quite clear: we ought to direct our attention to those microbes which are least easily killed by the protective elements of the blood. That will mean that we ought to put into our vaccine primarily the streptococcus and the staphylococcus, for these belong to the class of serophytes; and in the second instance the bacillus of Welch and the *Bacillus proteus*, for these belong to the class of imperfect serophytes, microbes which will, with a sufficiently heavy implantation, establish themselves and then grow luxuriantly in the blood. Let me explain that whenever in the following I speak of a vaccine for bacterial infection of wounds I shall have in view either (a) a vaccine containing streptococci and staphylococci obtained from wounds, or (b) a vaccine which contains in addition to these the bacillus of Welch.<sup>1</sup>

The question of the vaccine settled, the next question is at what stages in the wound infection we can find specially favourable opportunities for the use of vaccines. The *first* of these specially favourable opportunities will present itself immediately after the wound has been received. The patient is here just entering upon his incubation period; and it may quite well make all the difference to him if he is now, before the microbes which have been carried into his tissues grow out, enabled to kill them. I hold very strongly—and there would be no difficulty at all in arranging for this—that every wounded man should be inoculated as soon as he reaches the first-aid post. A *second*, and also very favourable, opportunity of achieving definite good by inoculation would present itself at a later stage, in those cases where the streptococcus, or the bacillus of Welch or the two acting in conjunction, establish a footing in the tissues and begin to spread there. I am thinking, of course, of the early stage of erysipelas, cellulitis, or gaseous emphysema, where we have, as the case may be, the beginnings of an inflammatory

<sup>1</sup> Both varieties of vaccine are available in all military hospitals, the first under the designation "*antisepsis vaccine*"; the other under that of "*antigangrene*" vaccine. If an autogenous vaccine were to be made for a particular patient one would determine the microbes which menaced him by taking a sample of his pus and making, as described in the first portion of this lecture, progressively smaller implantations of this into his serum or blood by the method of pyo-sero-culture, or pyo-hemo-culture, and then using for the vaccine those microbes which grew most readily in the blood.

blush on the skin; or the beginnings of induration; or, as the case may be, that purplish mottling of the skin which heralds gaseous gangrene.

And the employment of vaccine would also be specially opportune when we address ourselves to the partial or complete secondary suture of the wound. For the conditions are here analogous to those which obtained when the original wound was received—analogous in the respect that we have in both an implantation of microbes into the depth; and analogous also in the respect that success or failure will hinge upon the rapidity and effectiveness with which the protective elements of the blood are brought to bear upon the implanted microbes.

But to all this it may be objected that we have here merely plausible theoretical considerations; and that what is required is direct evidence establishing the utility of inoculation.

Let me therefore consider with you how far it would be possible here to procure probative evidence; and for the sake of those who attach superior authority to clinical evidence, let us begin with this.

A comparison of what would be procurable in connexion with antiseptis inoculation with what has been procured in connexion with antityphoid inoculation will here clear up certain points for us.

The first of these is that once a man has been inoculated against typhoid, events will take their course unaffected by outside interference. In other words, if an inoculated man comes into contact with infection, and the typhoid bacillus effects a local lodgment in him, nobody comes and interferes between the organism of the patient and the invading microbe. Moreover, when the invading microbes are successfully disposed of, there will not remain any open door, or continuous opportunity for re-infection.

The exact reverse will hold of antiseptis inoculation. Here there will be all sorts of interference, useful or, as the case may be, harmful, at the point where the microbes have effected a lodgment; and the wound will remain open to re-infection from outside; and in particular it may readily be further infected in the course of operative interference. Hence, if the wound becomes septic, no one can tell whether the microbes originally implanted have survived, or whether these were killed off, and we are in presence of an after-infection. Enough will have been said to show that it will be quite out of the question to procure in the case of antiseptis inoculation trustworthy statistical data such as are available in connexion with typhoid.

Precisely the same will hold true in the case of antiseptis inoculation

undertaken as a preliminary to secondary suture of the wound. For here everything will depend on the operative skill of the surgeon and on the scientific preparation of the cases. And the only judgment which will have value will be that of the surgeon who has actually followed the cases; and, again, his opinion will have value only if he have experience also of cases of secondary suture undertaken without inoculation.

There remains the third class of case: that where vaccine is employed to abort an incipient infection of tissues. Here, again, statistical proof will be unattainable. When, however, a sufficient number of separate observers shall have experimented with the vaccine, using controls; and shall have formed each his own experiential judgment; we shall have evidence which will really have probative value. But experiential judgments of that kind are not yet available in sufficient number.

For the present, therefore, there is not—nor will there ever be, except only in connexion with the last class of case—anything to carry conviction to that type of man who turns aside from what he calls “theory,” and demands everywhere proof and certainty.

But while we have in these matters no certainty—and let us not regret it overmuch, for certainty is like the stick without the dog to the blind man who wants “to get a move on”—we have here all that an exacting mind could require in the way of inferential evidence of the value of immunizing response in wound infections. Such evidence is furnished by the fact that patients who make only very indifferent immunizing response suffer from long-continued pyrexia; and their wounds heal slowly; and they not infrequently develop serious complications, in particular in the form of spreading infections and metastatic infections of joints. The direct reverse holds of patients who are making satisfactory immunizing response. Their wounds heal rapidly, and they never give cause for a moment’s anxiety. Now such immunizing response as we see here safeguarding the patient is, in the case of healthy men—and I reckon as healthy men all who are not suffering from auto-inoculations and fever—readily obtainable by the inoculation of vaccines.

I have now completed what I have to say about the different ways of treating wound infections; and you will have appreciated that, while antiseptic treatment stands apart, the three other methods are closely interlinked—forming together a complete system of phylacagogic treatment. Let me now draw the threads together, and put before you



in outline a scheme which might serve as a basis for the treatment of wound infections.

Before that, however, let me just pause to express my gratitude and indebtedness to my fellow-workers in these researches on the treatment of wounds, and in particular to Captain S. W. Patterson, of the Australian Medical Service, to Captain d'Este Emery, and to Lieutenant A. C. Inman and Lieutenant H. H. Tanner. And I have at the same time to express my grateful acknowledgments to the authorities of the Army Medical Service and to Colonel W. l'Estrange Eames, C.B., of the Australian Hospital, and, above all, to the Medical Research Committee for placing the services of these officers at my disposal.

#### SCHEME FOR THE PHYLACAGOGIC TREATMENT OF WOUND INFECTIONS.

It will be convenient for the ordering of this scheme of treatment to follow the wounded man from the "First-aid Post" back through the whole system of hospitals.

*Treatment in the First-aid Post.*—Here, after hæmorrhage has been arrested and the wound has been cleaned and bandaged, and splints have been fitted; it will be well to give a prophylactic injection of "anti-gangrene vaccine," this being, as already explained, a vaccine containing the streptococcus and staphylococcus and bacillus of Welch. For this injection nothing in the way of apparatus would be required beyond a syringe in a metal case and the vaccine in a rubber-capped bottle. From this last the vaccine would be drawn off as required through the rubber cap sterilized with lysol or other antiseptic. To sterilize the skin at the point inoculated is, in connexion with injection of vaccine, a work of supererogation. There would follow upon the inoculation a rapid immunizing response, which would, one is entitled to anticipate, in a bullet wound perforating only tissues, extinguish the infection, and would in other wounds do the same in those regions where the physiological conditions were not too unfavourable.

*Treatment in the Field Ambulance.*—I would suggest that the work of the Field Ambulance, which is—except in the case of the wounded who are too ill to move—restricted to mere washing and sterilizing operations, to the application of clean, dry dressings, and to the injection of tetanus antitoxin, should be extended to the performance of simple operations for the excision of the projectile and foreign bodies, and the procuring of efficient drainage. And I would suggest that all wounds, with the exception of those which promise to get well of



themselves, should be treated with hypertonic salt solution. These suggestions are dictated by the consideration that where a wound will, if left to itself, certainly become septic, we ought, at the earliest possible moment, both to remove extraneous substances which contain microbes and provide for a free outflow of lymph from the whole internal and external surface of the wound.

Concerning the operative procedures for the procuring of drainage it will suffice to say that they ought to give sufficient access to permit of loops of sterile bandage, previously steeped in a solution of 5 per cent. sodium chloride and 0·5 per cent. sodium citrate, being carried down into the wound to serve as wicks. The introduction of the bandages ought to be prefaced by a syringing out of the wound with the aforesaid solution, and the free ends of the bandage ought to be carried out from the wound to be inserted between plies of lint well soaked in the same solution, and folded over so as to form a thick pad. Finally, one or two "tabloids" of salt ought to be planted in between the back layers of the pad, and over the top of all ought to come a layer of impervious protective tissue. This method of dressing ought to be applied also to the open funnel wound. In the case of a "gouged-out" or "punched-in" wound of soft tissues, the bandages would, of course, be dispensed with.

This will be the place to elucidate two points which come up for consideration everywhere when hypertonic salt solutions are used as lymphagocic agents. The first has reference to the concentration of the salt, the second to the addition of citrate of soda.

In connexion with the concentration of the salt, all strengths from that of sea-water—corresponding to about 2·5 per cent. of sodium chloride—to a saturated solution—corresponding to 30 to 40 per cent. of sodium chloride—have been used in the wound; and the stronger the salt solution the greater the lymphagocic effect. But as the concentration of the salt increases, treatment will be more and more painful until with saturated and nearly saturated salt solutions we get escharotic effects with intolerable burning on the wound surfaces; while on the skin there is produced severe irritation followed by bacterial infection. The strength of the salt solution must therefore be kept within limits. Where we have freshly cut edges of skin, and, as in the flapless amputation, nerve-fibres exposed in the wound, it will be unjustifiable to employ more than 5 per cent. of salt; and even then it will be well to protect the cut edges of the skin with a thin coating of vaseline. When, on the contrary, we are dealing with comparatively

insensitive granulation tissue, or with quite insensitive sloughing surfaces, we may, with a view to achieving more rapid results, employ somewhat stronger solutions. But even with sloughing wounds it will be better not to go beyond 10 per cent. of salt. Again, when we use a wet pack of salt solution, it will be well to protect all the skin in the neighbourhood with a coat of vaseline. And finally, when we use, as suggested above, "tabloids" of salt to keep our salt solution up to strength, in spite of outflowing lymph, care must be taken to prevent the solid salt coming into contact with the wound surface.

Passing now to the question as to when citrate of soda should be employed in combination with hypertonic salt solutions, it may be explained that the purpose of the citrate is to prevent the lymph coagulating in the siphon bandages and on the walls of the wound. I may point out, in connexion with this, that while 5 per cent. salt solution will prevent clotting, as Alexander Schmidt has already shown, it will not, as my fellow-worker, Lieutenant A. C. Inman, has shown, prevent clotting in blood that is mixed with pus. Such blood is, however, prevented from clotting by 5 per cent. salt mixed with 0.5 per cent. of citrate of soda. The practical rule, therefore, will be to add citrate of soda when we are confining the discharge and may be dealing with pus; and to omit the citrate as unnecessary when we are irrigating and washing away any pus.

*Treatment at Casualty Clearing Stations.*—As the patient is transported farther and farther back from the front, and X-ray and other equipment becomes available; it becomes possible to undertake more extensive operations in order to procure drainage and for the removal of the projectile and extraneous substances, as well as for resection of hopelessly infected tissues. But we have always in connexion with operations upon a patient who has to be transported farther, to consider not only the practicability of the operation, but also that of keeping the patient under close observation sufficiently long after we have operated. It must be remembered here that the wound infection is always the important factor to keep in view; and that the period which is occupied in transport will generally represent for the patient a period of retrogression; and the set-back will generally be proportionate to the duration of the journey. During transport, drainage will be interrupted unless this can be prevented by wet salt packs applied under protective, the wound may become "lymph-bound," and then cellulitis or gas gangrene may supervene. Or again, if the wound has arrived at the suppurative stage, the discharges will be confined and become tryptic and corrupt.

This will, of course, apply retrospectively as well as prospectively, so that in every hospital which receives patients from the front its work will consist largely in efforts to regain for the patients ground lost upon their journey. In particular the induration of the walls of the wound will require to be dispersed, and relief will have to be obtained for the wounds that are lymph-bound.

*Treatment at Hospitals at the Base.*—Operative work will here be called for in connexion with two classes of cases. First there will be cases of spreading infection in the tissues—cellulitis and gaseous gangrene—which have manifested themselves during transport. These will require treatment by free incision supplemented by hypertonic salt solution. Further, there will be operations postponed from the casualty clearing stations—postponed either because of pressure of work or for the patient's sake, to give him the benefit of special equipment or special operative skill available at the base, and in conjunction with this the longer hospital stay which is there permissible.

The longer hospital stay will, when it can be secured, allow of quite important progress in the conduct of the wound infection. It will then be possible to embark upon what I may call the full programme of phylacagoc treatment.

In this programme we first irrigate with a hypertonic salt solution until we have restored the tissues to their natural condition, and are dealing with a mere surface infection, and only with serophytic bacteria. We then irrigate with physiological salt solution till we have extinguished, or all but extinguished, the surface infection. And we then undertake in one, or in a series of successive operations, secondary suture of the wound.

The essential features of these three linked procedures have already been explained, but the details remain to be worked out. The following practical points may, however, be emphasized.

*Lymphagoc Irrigation.*—The irrigation and drainage of the wound may with advantage be conducted by the methods which form the subject-matter of the demonstrations already referred to.<sup>1</sup> The solution which I would suggest for use is a 5 per cent. solution of sodium chloride, boiled, and kept at a temperature of 37° to 40° C. Irrigation with this solution ought to be continued *de die in diem* until the desired result is obtained. If we fail in this we have probably some retention of pus in inaccessible spaces, and these ought to be opened up. Whether

<sup>1</sup> Loc. cit., *vide* footnote, p. 37.

irrigation ought to be continuous or whether it ought to be continued for only a few hours at a time remains to be determined.

*Leucocytagogic Irrigation.*—Our therapeutic solution will here normally consist of boiled physiological salt solution. It, however, remains to be determined whether in those cases where secondary suture is for any reason impracticable, it may not be better to substitute for physiological salt solution some such fluid as Ringer's solution or Locke's solution (without the sugar), whose salt content will more nearly resemble that of the blood; such a fluid, though as a leucocytagogue not so good as physiological salt solution, might quite as well prove more favourable to the growth of connective tissue and epithelium.

There remain over now two general questions with regard to which an explanatory word must be said. The *first* relates to infections of joints and serous cavities which are shut off from the exterior; the *second* to the employment of antiseptics and antiseptic precautions in connexion with operative interference and the various manipulations and procedures undertaken in the wound.

*Treatment of Infections in Unopened Joints and Serous Cavities.*—The complete programme of lymphagogenic treatment, leucocytagogic treatment and secondary suture is, of course, a programme for open wounds. But lymphagogenic treatment will be superfluous where we have a purely surface infection in a closed cavity. And the question of secondary suture will not come up unless we have first opened up widely—a method of treatment which is, at least in infections of the knee-joint, of very doubtful wisdom. There remains accordingly of our programme only the irrigation with physiological salt solution, and in combination with this effective mechanical drainage. In the particular case of a knee-joint infection the fluid could be supplied through one needle and be carried off through another. Or the fluid might be carried in through a fine rubber tube inserted in a very small incision. And the fluid might be allowed to find its own way out, or it might be carried out by a siphon of sterile bandage inserted through a small counter-opening and carried down to a vessel of water standing on the floor.

*Employment of Antiseptics and Resort to Antiseptic Precautions in connexion with the Wound.*—The employment of antiseptics and the taking of antiseptic precautions in connexion with all operations and manipulations in wounds is governed by the consideration that the patient will have made immunizing response only to those microbes which the projectile carried into the wound and to those contained



in the vaccine—these being *ex hypothesi* those present at the outset in every wound. To all other microbes the patient will, except only so far as the increase of his antitryptic power may protect him, be fully susceptible. From this it follows that we shall, if we neglect any antiseptic precaution, be running the risk of superadding another infection to the already subsisting infection; and in hospital we shall be running the risk of transferring infections from patient to patient. Herein, then, lies the justification for prescribing the prophylactic employment of antiseptics and antiseptic precautions, and in particular the use of sterile instruments and sterile gloves in connexion with the dressing of patients, and in association with this the sterilization of all bandages, dressings, and salt solutions used in the wound.

#### CONCLUDING REMARKS.

I pass in conclusion to the problem as to whether it would not be possible for the researches I have detailed to bear fruit in a wider sphere than that represented by the present audience and those who will perhaps read this discourse.

You see that my mind is here set upon those questions of organization concerning which I ventured to say something when lecturing here some six months ago.<sup>1</sup> Let me to-day again break ground for you by indicating that there are included in the Army Medical Service in reality three different services—a Service of Administration, a Service of Sanitation, and a Service for the Treatment of the Sick and Wounded. And let me again, in connexion with the last-mentioned, ask you to remember that it is staffed almost exclusively by medical practitioners joining for the War; and that the civil profession is by consequence specially responsible for its efficiency. Now, if that is so, it then must be for the civil profession a duty of special obligation to see that whenever, in the practically unexplored field of wound infections, any new knowledge is gained, that new knowledge shall be brought into application in all military hospitals.

At the beginning of the War the outlook so far as it related to the wounded was, I think, somewhat as follows:—

It was realized that all sorts of surgical operations in which life would be at stake would have to be undertaken; and that these would, under the conditions that existed, fall to new-joined junior medical

<sup>1</sup> *Lancet*, May 1, 1915, p. 898.



officers with very little operative experience. There were therefore sent out to the theatre of war a certain number of eminent surgeons.

Here at the very outset we see recognition of the fact that the rank and file of new-joined medical officers will require—and, moreover, will very gratefully accept—skilled help and supervision.

There were two alternative ways of supplying help and supervision.

The one was to associate the eminent surgical experts with the Medical Service of the Army in the capacity of Consulting Surgeons. To do so was not to follow the precedent of civil life, for in civil life the operating surgeon is not consultant and adviser to the operating general practitioner. Nor was it to follow the precedent of the Army. But, at any rate, that was the course that was adopted.

The other and, I think, the better alternative would have been to have made those who now function as Surgical Consultants integral elements in the Army machine; to have entrusted to them the selection of the operating staff in the hospitals in their administrative unit; and then to have made them directly responsible for all operative treatment in those hospitals. All this touches, but only indirectly, the particular point in organization I want you to consider.

What seems to me specially required is the organization of all that department of treatment which lies outside the sphere of the operating surgeon, or, at any rate, the sphere of the operating theatre. Before the War it was generally held about this department of treatment—let me call it for short “the conduct of the wound infection”—that all that would be required would be: to drain the wound by making an opening at the most dependent point; then to wash out with antiseptics; and, finally, to apply dressings. And it was held—and no doubt correctly—that for the proper carrying out of the antiseptic washings and dressings, any doctor who was on the Medical Register, or any trained nurse would suffice.

Now the wind has swept this all away, and it has come home to everybody that every wound is infected; and that the infection is the really serious element in wounds. Coming on the top of this, practically everybody has become aware that the antiseptic system has—so far as the treatment of the wound infection is concerned—completely broken down. So finally it comes to this, that the progress of knowledge has filched away from the ordinary medical officer everything, other than the knife, which he was relying upon for the treatment of bacterial infections of wounds.

And though here and there he may have substituted hypertonic

salt solution for the antiseptic; that is, as we have seen, only to take one item out of the programme of bringing the protective elements of the body to bear upon the infection; and what is required is the complete programme. Now to carry out the complete programme it will be necessary to stop every moment and think; and it will be necessary also to verify every step; and to make new departures as occasion requires. Now to stop every moment and think is not given to everybody; and to verify and, as occasion may require, rectify one's course in combating a bacterial infection involves thinking in terms of microbes and protective substances and auto-inoculations and vaccines, and involves also a certain acquaintance with laboratory technique.

So that I have the conviction that a newly joined medical officer, supposing him left completely without supervision and help, would more easily perform successfully all the surgical operations that might be required of him, than conduct satisfactorily the treatment of a wound infection.

If that is so—and it is for you to judge whether it is so—what follows is: that it will be necessary to provide in connexion with the conduct of the wound infection the same kind of aid and instruction as is provided in connexion with operative surgery.

And what holds true with respect to the organization of the one, will hold true also with respect to the other. For the conduct of the wound infection, which we are here considering, one would wish to have, in each large administrative unit, a responsible head disposing of a staff of men with both laboratory and clinical experience, who would be deputed one to each hospital to exercise supervision as “a physician in charge of wounds,” over all that department of treatment which lies outside the sphere of operative surgery.

And I would emphasize that it would be just as necessary in the case of the “physician in charge of wounds” as in the case of the “surgical specialist” to confer such rank as would make it possible effectively to direct the operations of the medical officers working in the wards.

## APPENDIX.

THE EMPLOYMENT OF BANDAGES FOR THE IRRIGATION OF  
WOUND SURFACES WITH THERAPEUTIC SOLUTIONS, AND  
THE DRAINING OF WOUNDS.

THE irrigation of wounds has the following advantages over the application of wet dressings:—

We can bring our therapeutic solution continuously into application, maintaining its concentration unaltered. I have elsewhere in my lecture emphasized that this, in treatment with saline solutions, is of fundamental importance.

We can, with proper arrangements, apply our therapeutic agent to all the internal and external surfaces of the wound, and at the same time obtain ideally effective drainage.

I propose now to describe a series of simple arrangements which, I think, satisfy all requirements.

In the ordinary arrangement in which the irrigating fluid is supplied in the form of a “drip” falling into the wound—and it is the funnel-shaped wound I have in view—the fluid makes its way down along the side of the wound in a runnel, and we thus treat only one small sector of the wound. We have, in fact, the conditions of a rock valley or cave, where a thread of water flows to the bottom in a single channel, leaving all the rest dry.

Let me show you that by an arrangement of bandages the water can be led into the wound where we require it, that it can be distributed so as to wash down all the walls, and that it can then, without any leakage into the bed, be carried away.

It will be convenient to begin by considering the properties of bandages and strips of gauze as conduits for carrying water. They may be regarded as conduit pipes with porous walls—pipes which will convey water uphill by capillary attraction, downhill by gravity, and first up and then down hill when arranged as siphons.

We may, as a matter of fact, confine our consideration to bandages, for the ordinary bandage is, as comparative experiments bring out, a more effective water carrier than a strip of gauze cut to the same width.

The first point which comes up in connexion with the employment of bandages in suppurating wounds is their liability to be obstructed by the lodgment of particles in the meshes. The method which is employed when the dependent opening of a wound is plugged with a strip of gauze would seem specially designed for the purpose of compassing such obstruction. Here gravity carries the undiluted pus directly down into the wick, and we obtain instead of a drain an impermeable bung which effectually confines the discharges.

The arrangements shown in the models (figs. 1 and 2) show how these difficulties can be circumvented and the wick be kept free from obstruction. We have in these—we may call them “mud-funnel” experiments—funnels filled in with a glutinous mixture of flour and water, and in each case a bandage arranged as a siphon. In the first model (fig. 1) the siphon is a simple loop of bandage carried over the rim of the funnel. We have in addition in this model a wick carried up into the stalk of the funnel. In the second model (fig. 2) we have a rubber tube introduced through the stalk of the funnel and standing out above the surface of the fluid. This rubber tube is threaded with a bandage, and its end has been cut into two strips which hang down into the muddy fluid. The siphons are here, as you will appreciate, working in what would correspond to wounds standing full of pus, and they have, as you see, in each case carried over a very considerable volume of muddy fluid. And if they were continually refilled they would—for only the distal ends of the ascending limbs of the siphon bandages can here be obstructed and put out of functioning—continue to siphon over indefinitely through the upper segment of each ascending limb. You will also note that the wick in model No. 1 has become obstructed and has carried out only very little fluid. Finally, in connexion with model No. 2, I would like, in passing, to call your attention to the principle that we can always, as occasion may require, reinforce the one or the other limb of the siphon; and that in this model we have in the two strands of bandage which compose the ascending limb of the siphon two streams of fluid which are tributary to the descending stream.

I pass on to certain further points having also a general application. You will appreciate that though water will, when it has a clear fall, run down a bandage without leakage, it will, as soon as contact is made with any surface, flow away over that surface. Again, it will leak out at any point when we constrict the bandage, or at any point where we loop up the bandage. For water refuses to run quickly uphill.



To prevent trouble from these sources we must, where our irrigating fluid has to be carried through obstructions in the wound, or uphill, substitute rubber tubing for bandage. If, however, we have to resort to a siphon arrangement for drawing off our irrigating fluid from the reservoir, it will be of advantage to thread a bandage into the ascending limb of the siphon, and to carry it sufficiently far in to pass down some little way into the descending limb. It will also be advisable to leave in position the wire which we have used for threading in the bandage, and to bend it round into the form of a narrow U, so as to prevent the end of the rubber tube kicking itself out of the water. The great advantage we get from the use of a wick in our delivery tube is that we cut down the delivery from what the rubber tube would carry (which

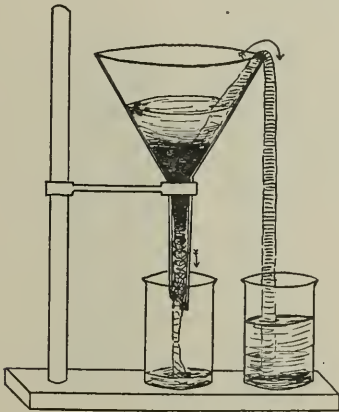


FIG. 1.

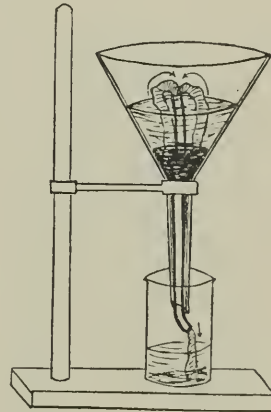


FIG. 2.

would be excessive) to what the wick will carry (which will be amply sufficient). Moreover, the siphon, when furnished with a wick, will start itself automatically, and also restart itself when the reservoir comes to be refilled.

I pass on to consider the factors upon which the efficacy of bandage siphons depends. The volume of fluid delivered is determined (*a*) by the height to be climbed in the ascending limb; (*b*) by the length of, and weight of water in, the descending limb; and (*c*) any obstacle to delivery encountered at the distal end of the descending limb.

In addition to these the rate of delivery will be affected (*d*) by the nipping of the bandage when it rests upon a knife-edge, this factor coming seriously into account in experiments with a long length of descending limb heavily weighted with water.



The experiment here set up (fig. 3) shows the influence of factors (b) and (c); these being the factors we have to take special note of in connexion with the drainage of wounds. It will be seen that where other conditions are the same it is always the siphon with the longer descending limb which gives the more efficient drainage. Again, it will be seen that, other conditions being the same, a larger volume of water is delivered from a bandage whose end is immersed in water than from a bandage suspended in air. In the case of water dripping off from a bandage into air, consideration will show that the bandage

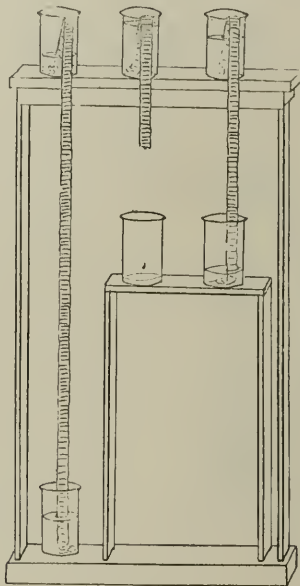


FIG. 3.

will, just before the drop falls, be over-full of water, and that the inflow into it will for that moment be suspended, to be accelerated as soon as the drop has fallen off. As a result, we have in the bandage which is dripping off into air a cyclically interrupted flow. In contrast to this we have, where the descending limb of the bandage goes down into water, a continuous unobstructed delivery.

The practical application of this in connexion with wounds is that the siphon bandages which carry off the washings from wounds ought to pass down from the patients' beds into water in vessels placed on the floor. And if any bandage happens to be too short to dip into the water, the drip from it will be accelerated if it is torn into tails.

Where only little water is flowing off through bandages draining a wound, evaporation must be guarded against, for this will, by diminishing the load of water, retard the outflow into the descending limb.

Up to the present we have considered only siphons formed from a single loop of bandage—that is, drainage carried out through conduit-pipes very much smaller in calibre than the vessel they are draining.

One may in many cases—in particular in wounds where the bandages may be obstructed by pus—desire to provide an ampler outflow. This

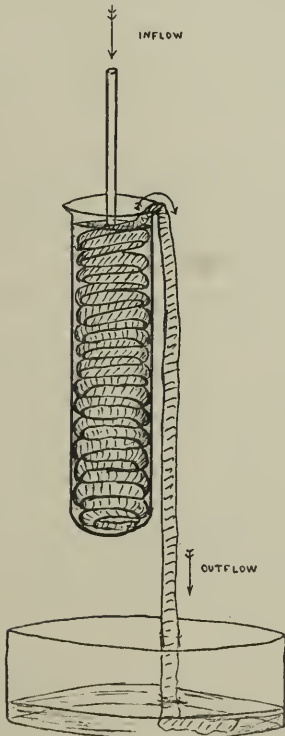


FIG. 4.

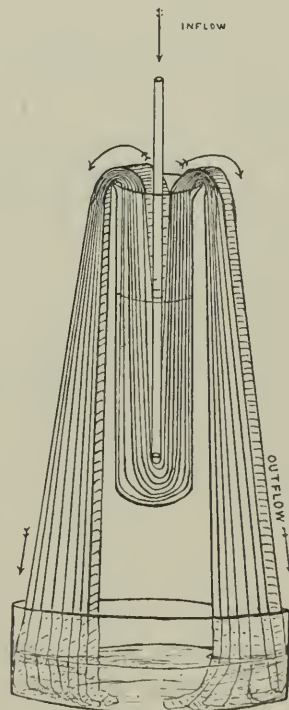


FIG. 5.

can be done by packing the wound full of bandages, but there is here a right and a wrong way.

In this model (fig. 4) I have packed a test-tube in the ordinary method by taking a strip of bandage and coiling it up into the wound, and I have then taken the loose end of the bandage and, leading it out of the test-tube, have employed it as the descending limb of my siphon. Side by side with this I have packed a test-tube (fig. 5) by taking a number of loops of bandage, laying them one above the other over the

mouth of the tube, and then pushing them down so as to form with the strips outside a sort of rough letter M. We may call this the *multiple loop method of packing*. When I now turn on the water and fill up these companion test-tubes you see that while the one which is packed with a single strand of bandage coiled up inside it empties itself only very slowly, the one which is packed with multiple loops empties itself almost as fast as I can fill it in. You will note in connexion with the first system of packing that it is quite effective, really more effective than the second so far as relates to the conveyance of fluid along the tube. In point of fact, the contiguous coils of bandage fuse together, and give us all that a full calibre wick could. The real fault of the system lies in the outflow. It is as if we had a large main running out into a small pipe.

Holding these principles before us, we may now consider how we can in each particular type of wound arrange for feeding in the therapeutic solution, for distributing it all over the walls, and effectively draining the wound.

Wounds may for our purposes be classified into *cylindrical wounds disposed horizontally*; *funnel-shaped wounds with the funnel opening disposed upwards, downwards, and laterally*; and finally *ascending or descending cul-de-sac wounds*. We may take these in order.

*Arrangement for Irrigating a Cylindrical Wound which Perforates a Limb Horizontally.*—Here, as shown in the model (fig. 6), the irrigating fluid is conveyed into the interior of the wound in a narrow rubber tube. The wound is drained by two leashes of loops, which go down on either side into vessels of water. These are pulled into position in each case by a strand of bandage fastened, as shown in the figure, round the bight of the loops.

In actual practice, when we want to verify that the irrigation is working properly, we do so by the following devices:—

We test the effluent by drawing a line across one of the bandages with a stylographic pen, and note that the ink is carried down in streaks by the current. We test the inflow by piercing the rubber tube with the needle of a hypodermic syringe and driving in a bubble of air just above the point where the glass union provides an inspection chamber. Then, holding the piece of glass tubing horizontally, we see the bubble of air carried along. Where we want to test the condition in the interior of the wound, we aspirate into the syringe while pinching the rubber tube proximally to the insertion point of the needle. Where we want to see the effect of the irrigation in conspectus we cut transverse strips

from the bandage at different levels, and prepare microscopical preparations from these by streaking them out one by one over the surface of the microscopic slides. We then, if the wound is getting progressively cleaner, see the microbes falling off, and if the wound were getting dirtier should see the microbes increase, as we travel inwards towards the wound.

*Arrangement for Irrigating the Inverted Funnel Wound.*—The kind of wound here in view (fig. 7) is that produced when a projectile, entering the thigh from in front, fractures the femur and drives it before it, making an extensive wound of exit.

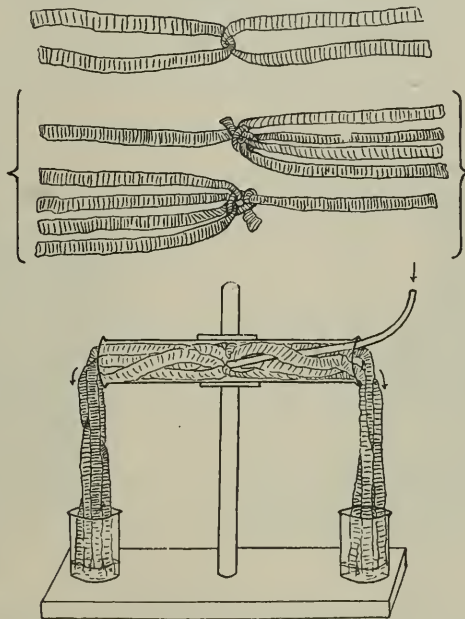


FIG. 6.

A strip of bandage tied loosely round the bight of a leash of bandages is passed up from below through the stalk of the funnel. This is now drawn tight, and then the strips of bandage hanging bunched up in the centre are one by one looped up over a framework fitted just below the mouth of the funnel. The irrigating stream is now carried through the upper bandage into the neck of the funnel, and here divides up into a whole system of separate streams which together irrigate the whole interior face of the funnel. In connexion with the actual wound we should loop up the bandages, not as in the model through a circular framework of wire, but as in fig. 8 over a light aluminium cradle placed astraddle upon the limb.



*Arrangement for Irrigating the Upright Funnel Wound.*—The wound here in view (fig. 9) is that seen in connexion with a compound fracture of the femur with wound of entrance behind and the wound of exit in front. Instead of dividing after entering the wound, the irrigating stream must here divide outside the wound and enter in a number of separate streams. We can get this by dividing up the end of a bandage into a number of tails and planting these about the wound.

The arrangement shown in the model embodies improvements suggested by my fellow-worker, Lieutenant H. H. Tanner. It will be seen that we have here fitted to the delivery tube which conveys the fluid into the wound the upper end of a test-tube. This serves as a receptacle for the bandage. Fitted to the mouth of the test-tube we have a rubber cap traversed by a number of small rubber tubes. Into

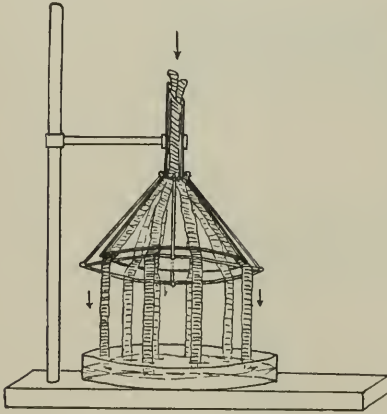


FIG. 7.

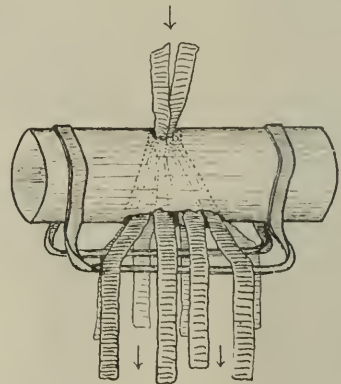


FIG. 8.

each of these is inserted one of the tails of the bandage. Finally, each of the rubber tubes is stiffened with wire. This enables us to direct its stream in any direction and to ensure the tubes maintaining any position which we give them in the wound.

*Arrangement for Irrigating a Funnel Wound which is disposed Horizontally.*—Except only in the detail of the disposition of the irrigating tubes and the provision for carrying off the washings, the arrangement is the same as in the upright funnel wound.

*Arrangement for Irrigating an Ascending or Descending Cul-de-sac Wound.*—Both these wounds are irrigated by the arrangement shown in fig. 5. In other words, the irrigating fluid is conveyed in a fine rubber tube upwards or downwards to the blind end of the wound, and the washings are carried away by multiple loops of bandage.

*Method of Preventing the Irrigating Fluid running away over the Patient's Skin or Clothing and Soaking into the Bed.*—It has already been emphasized that bandages resemble tubes with porous walls, and that fluid leaks out whenever on entering or leaving a wound they make contact, as they inevitably must, with the external surface of the body.

We need not here concern ourselves with leaking from the inflowing stream. We have already provided against that by feeding the irrigating fluid into the wound through rubber tubes. There remains the leakage from the bandages which carry the outflowing stream. It is a form of leakage which is always liable to occur except where the wound of exit occupies, as the patient lies in bed, the most dependent portion of his

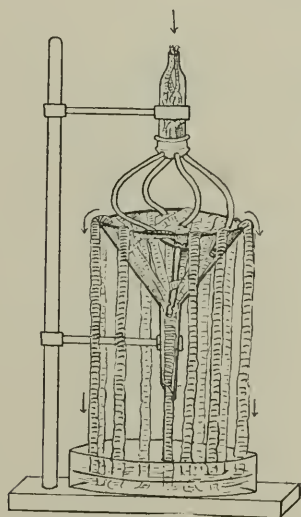


FIG. 9.

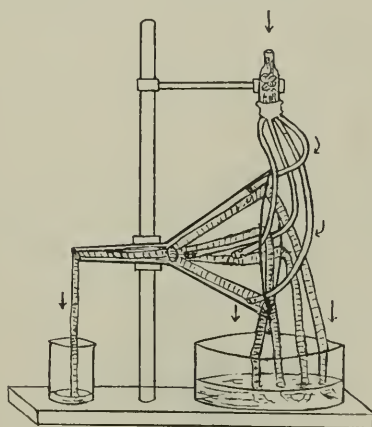


FIG. 10.

body surface. The only method of dealing with this very serious inconvenience is by damming back the water which escapes. We can do this by means of what I may call *irrigation flanges*. Where we are dealing with a wound situated on a limb, what we require is a *ring-flange* above or below the wound (fig. 11); or, better, irrigation flanges both above and below. Where we are dealing with a wound opening on the outer aspect of the trunk or limb—with, for instance, a wound on the lateral aspect of the shoulder—we require a *horseshoe flange* (fig. 12) round the opening of the wound.

Such flanges are built up upon the patient's skin in a very simple manner. We first prepare some formalin gelatin. We do this by dissolving 20 grm. of gelatin in 100 c.cm. of water—or better, so as to have plenty, twice that quantity in double as much water.

We now, pouring the gelatin solution out into a bowl, add to it one-tenth of its volume of the ordinary 40 per cent. formalin. Then taking a number of short lengths of bandage, previously laid ready to hand, we immerse these in the formalin gelatin. This done, we take a roll of cotton-wool and encircle the limb with it, or, as the case may be, bend this round in the form of a horseshoe and then apply it, with the opening of the horseshoe disposed upwards, to the skin round the wound. Going back then to our strips of bandage lying in formalin gelatin, we take them one by one from the bowl, paste one end down on the skin, carry the middle over the ridge formed by the roll of cotton-wool, and then

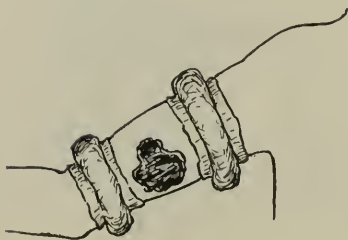


FIG. 11.

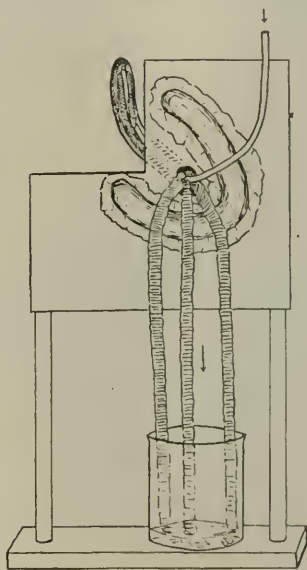


FIG. 12.

paste down the other end on the far side, taking care always to overlap one strip of bandage by another. When we have covered in our roll of cotton-wool we have completed our task, and we have now, as soon as the formalin gelatin sets—and it sets in a few minutes—a light stiff watertight confining dam firmly fixed down upon the patient's skin.

Let me say in conclusion that I am indebted to my fellow-worker, Lieutenant H. H. Tanner, for setting up and drawing the models, and also for valuable assistance in elaborating the above system of irrigation and drainage.

## The Royal Society of Medicine.

President—Dr. FREDERICK TAYLOR.

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(December 20, 1915.)

### A Lecture on the Treatment of Dysentery.

By Lieut.-Colonel Sir RONALD ROSS, K.C.B., F.R.S.<sup>1</sup>

MR. PRESIDENT AND GENTLEMEN,

My object in this lecture is not to detail my own experiences and opinions upon the very large subject of the treatment of dysentery so much as to elicit the experiences and opinions of the many medical men who are at present treating cases in Britain, in order that we may be able to compare notes and, if possible, to consolidate our methods in this important branch of practice. Fortunately, for some centuries past Britain has remained almost entirely free from this serious disease and probably entirely free from the amoebic form of it, except in regard to patients who have been infected elsewhere. The result has been that medical men in England have not been able themselves to collect experiences, and now perhaps find themselves called upon to treat large numbers of cases suffering from a malady with which they are not familiar. But we must remember that nothing in the whole range of clinical practice requires greater experience, skill and judgment than the treatment of dysentery in all its stages. In fact, this subject has been almost the principal pre-occupation of physicians in Egypt, Greece, and Italy, and other warm climates for thousands of years. In classical times the treatment, of course empirical, consisted largely of the use of oils, essential oils, wine and dieting, not to mention some of the curious remedies which our predecessors were apt to advocate. In what may be

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called the first work on tropical medicine, namely, that curious book "The Cures of the Diseases in Forraigne Attempts of the English Nation," 1598, and recently republished by the Clarendon Press and attributed to one George Wateson, who apparently gave his manuscript to Richard Hakluyt, we find the following instructions: "To cure Las Camaras, or Camaras de Sangre, which is the Bloodie Flux. With the more expedition, that medicine is ministred to the diseased of Camaras de Sangre, Laxativenes, or Bloodie Flux, there is the more possibilitie it should prevaile. And detracting it, the Pacients often die suddenly, without feeling much grieffe. For speedie and assured remedie thereof, the Pacients bodie must bee clensed of the slimines, engendered in the passages of the nutriments; before anie sustenance can remaine in his bodie. And for that purpose, give to purge him in the morning, halfe a pint of white Wine coold, wherein  $\frac{1}{2}$  ounce of Rubarb hath been sodden, being small cut; putting in some Suger Candie to sweeten it. And Immediatly after he hath so purged, keepe at his navell Rosemarie sodden in strong Vineger, applied in the morning and evening verie hot, untill it be stayed: giving him often Quinces brused and rowled in Marmelet like Pils, which hee should swallowe whole, and none of the Fruits, or meates before recited, nor any more white wine, but red wine of any sort. And if it be on Land, the livers of Goates (especiallie) Sheepe, or Bullocks roasted: not willingly permitting the Pacient to eate any other meate. And if at Sea, Rice only sodden in water, rather then any thing else usuall there, untill the infirmitie is perfectly asswaged." Sydenham used to employ venesection followed by purgation and laudanum administered in a cordial, his treatment being based upon his pathogenical hypothesis that dysentery is "a fever turned inwardly on the bowels." Early last century calomel was much employed. Morehead, in the middle of last century, still supported the use of blood-letting and calomel, but added ipecacuanha, and, of course, used blisters and fomentations.

The most important event in the history of the treatment of dysentery was the employment of ipecacuanha, which was first advocated in 1600 and then described by Piso and Marcgraf in 1648. Later in the same century the Dauphin, son of Louis XIV, was successfully treated by Helvetius with ipecacuanha, which he had obtained from Brazil, and Helvetius obtained a great reputation by the use of the drug, which he kept a secret, though he was ultimately prevailed upon to disclose the secret for a sum of a thousand louis d'or. Since then the fortunes of ipecacuanha have varied from time to time, just as did

the fortunes of cinchona bark. At times vaunted as specifics, and at times condemned as useless, probably both drugs suffered much from being given in quite inadequate doses, and for periods which were too short to compass complete cure. The use of large dosage seems to have been revived by Balmain at the beginning of last century, just as the use of large dosage of quinine was revived by Maillot some years later. Balmain prescribed up to 2 dr. of the powder, with 60 drops of laudanum, and claimed that one or two of these doses was often sufficient to establish complete cure. I am not sufficient of a medical historian to know whether the credit was due to him or to Docker, but it is often maintained that the latter was the first really to claim the rank and title of specific for ipecacuanha in dysentery—which was something to have done, and for which he received a special pension from the Government of India.

In the year 1881, when I entered the Indian Medical Service and went to Netley, ipecacuanha was certainly in high favour, and I remember the fine lectures on the subject given by Surgeon-General Maclean. No one more strongly emphasized the wonderful powers of ipecacuanha, not only in dysentery but as a preventive of hepatic abscess, even after the pus had begun to form. I believe that all of his hearers (young men at the time) followed out his teaching in India afterwards. He insisted upon massive dosage in spite of the frequent vomiting caused by it, and, so far as I remember, gave the drug the last thing at night, on an absolutely empty stomach (if possible) and on a previous dose of opium. At any rate, this may also be described as the usual Indian treatment till the days of emetine. I suppose that there are few medical men in Indian practice who do not, every day of their lives, have under treatment at least half-a-dozen cases of acute or chronic dysentery, so that our experience was certainly large. Personally, I am a convinced believer in the specific value of ipecacuanha. When I was Staff-Surgeon at Bangalore I saw many cases of severe dysentery in officers returning from Burma and the Mofussil, and never dreamed of treating such without this drug. I remember particularly one such officer who was, we all thought, dying when brought in. The first dose of ipecacuanha powder was vomited, but the second was retained, and the patient's recovery began next morning, and was complete. We used to repeat the ipecacuanha at least once a day for about a week or more in spite of vomiting, but, of course, patients often became habituated even to large doses in three or four days. Later on I used to prefer giving opium every night, and the dose of ipecacuanha in bolus

with a single mouthful of milk at about four o'clock in the morning, if a nurse to give it was available, and the patient was not allowed anything either to eat or to drink until about 9 a.m. I thought that, with this method, one daily dose sufficed. Later in the case we used to combine 5 gr. or 10 gr. of tannic acid with the ipecacuanha, especially in cases of "running dysentery," but I believe practice was generally opposed to the use of astringents until the disease had first been mastered by ipecacuanha. Of course, in those days we did not know whether the dysentery was amœbic or bacillary, and gave ipecacuanha in both; and up to the present I am not by any means convinced that ipecacuanha is of no benefit in bacillary dysentery. The extraordinary specific effect which it has upon the mucous membrane of the stomach and colon especially suggests that it may be at least partially destructive to the vegetable as well as to animal parasites. I do not think that we used bismuth much in those days, nor calomel, nor the sulphates of sodium and magnesium. And many of us were certainly opposed to enemata, at least in very new cases, as we thought that they were likely to have an irritating effect similar to that often produced by catheterization. Here, I admit, opinion differed largely, and I remember many cases in which enemata certainly did not kill the patient, although in acute cases they undoubtedly cause much depression and considerable pain. I also remember a few cases in which I thought that ipecacuanha had stopped the progress of commencing hepatic abscess—and also some, I fear, in which it failed to do so. In cases of chronic dysentery—cases which we often found extremely difficult to treat, and, indeed, which have often become chronic simply because the patients refuse the treatment advised by medical attendants—ipecacuanha was often given daily for some weeks, was then remitted for another week or so, and commenced again, and so on. There were many retired officers in Bangalore suffering from this disease and from sprue, and the difficulty of the physicians almost always lay in dealing with the patient as much as in dealing with the disease, and everyone knows the state of irritability and impatience to which chronic dysentery and sprue reduce their victims. In *chronic* dysentery, enemata were, of course, certainly much more used than in acute dysentery, and nitrate of silver was almost a classical drug for the purpose. We heard nothing in those days of appendicostomy, but dieting was a science in itself, requiring not only the greatest skill of the clinician, but an obedience in the patient which was seldom met with.

So far as I can ascertain, it was chiefly the work of Kartulis, in

Alexandria, which first definitely incriminated intestinal amœbæ as pathogenic organisms, partly in consequence of his admirable and long-continued clinical studies, partly by his discovery of amœbæ in the sterile pus of hepatic abscess, and partly by his experiments on kittens. This work was commenced about 1880, and was already progressing when Koch visited Alexandria in 1883. The previous studies of Lewis and Cunningham had led these workers to think that these organisms were merely pseudo-parasites in men, though they (or rather the latter) thought they were true parasites in cattle; and Celli and Fiocca and other workers seemed inclined to support them. As the result of my own studies in Bangalore, both on amœbæ and on the intestinal flagellates which abounded there, I felt almost convinced that all these organisms are, or at least may be, injurious to the host—and still think so. It appears to me to be a very rash statement to make that any parasite is quite innocuous. One might as well say that a grain of dust in a watch is harmless, because it does not always stop the mechanism. Certainly amœbæ may remain comparatively innocuous for considerable periods, let us say in the pockets of small and few ulcers in the intestines, or even in its lumen; but we never know whether they may not at any moment produce serious lesions, or relapses of old lesions, or aggravate the lesions caused by other organisms. And it still seems to me to be a safe teaching that we should consider all intestinal amœbæ and flagellates to be potentially dangerous. However this may be, the rôle of certain amœbæ in dysentery has now been almost generally accepted. Captain (now Colonel) Patrick Hehir, C.B., I.M.S., and myself certainly recognized both amœbæ and flagellates in India in cases of dysentery and diarrhœa as early as 1896 (*see*, for example, my paper in the *Indian Medical Gazette* of May, 1897). Sir Leonard Rogers also recorded amœbic dysentery in India in 1901 (p. 16 of his book); and he has also won immense distinction by his elaboration of the treatment of dysentery by emetine hydrochloride. Emetine was first separated from ipecacuanha by Pelletier and Magendie in 1817, and was, I believe, first recommended for dysentery by Bardsley, of Manchester, in 1829, while Tull Walsh gave it by the mouth in 1891. But it was the elaborate studies of Rogers which first established the use of it in 1912 and later—so that it has now been before the profession for more than three years. I believe that it is accepted everywhere, not perhaps as being essentially superior to ipecacuanha powder, but because the hypodermic injection of it is much less troublesome to the patient. Personally, I think that the hypodermic use brings the



drug more directly into conflict with the amœbæ at the bottom of the ulcers, while ipecacuanha does more to attack amœbæ on the surface of the mucous membrane and in the lumen of the intestine, so that I am inclined to favour the use of both at the proper stages in the treatment. But as an immediately applicable routine treatment the hypodermic injection of emetine has certainly become a great boon to humanity, for which we owe a debt to Sir Leonard Rogers which can never be repaid. I must, however, again mention the use of the sulphates of magnesium and sodium, apparently originated for dysentery by the French physicians in the middle of last century, and which use has now become almost universal in bacillary dysentery, so that many think that we have specifics against both forms of the disease. According to many, ipecacuanha and emetine are powerless against bacillary dysentery, while the saline purgatives are powerless against amœbic dysentery.

I now come to the almost unique experience which we have recently had in connexion with cases of dysentery among soldiers. As was to be expected, both forms of dysentery have occurred largely among the troops near the Mediterranean and the Red Sea. Apparently bacillary dysentery prevailed up to midsummer of last year, but after that there was certainly an epidemic of amœbic dysentery in July and August. I was then on duty in Alexandria as Consulting Physician for Tropical Diseases, and had most unusual opportunities for studying the practice of the physicians in all the hospitals. On October 17 we held a meeting at Alexandria, the Principal Director of the Medical Services being in the chair, to discuss the treatment of acute dysentery, and no fewer than sixteen papers were read on that occasion. Subsequently we held another meeting on the pathology of dysentery, which the able pathologists of the hospitals had been carefully considering; and I think that altogether we could claim to know something about the subject, at least as regards the forms of dysentery and diarrhœa, pure and simple, or complicating the typhoids which had been occurring up till then; and I should now like to compare notes with officers who have all this time been practising in England on such of the cases as have been invalided home.

Certainly up to June, amœbæ had not been easily found in Egypt, but towards the end of July they began to be recovered without difficulty in a large number of cases, and this state of things continued till the end of September or later. After this, however, whether due to the large use of emetine in early cases or to the natural decay of the epidemic, amœbæ began to become more scarce, and I gathered from

men who have worked long in Egypt that amœbic dysentery is likely to be supplanted by bacillary dysentery in the winter. This, of course, is the almost general rule, for amœbic dysentery is especially a disease of hot climates, and one which tends to be always prevalent more in the summer than in the cool season, and more in low latitudes than in high ones. Unfortunately, however, amœbic dysentery tends to be such a chronic complaint that the treatment of it continues to interest us long after the commencement of epidemics, and we have to guard not only against the chronic form of it but against the terrible hepatic abscesses which so frequently follow it.

As soon as we recognized the presence of amœbic dysentery among the troops, emetine hydrochloride began to be employed by, I believe, every officer in charge of wards on the medical side; and, indeed, the Principal Director issued urgent orders to use it in every case of suspected dysentery without waiting for a definite diagnosis as to the exact nature of the case. In my opinion this order was absolutely demanded, and proved to be very beneficial. I think that all those who have had experience of dysentery will agree with the first sentence which I have already quoted from that old writer George Wateson, to the effect that the sooner that treatment is commenced in dysentery the more it is likely to prevail. Once the amœbæ have begun their destructive trenching and mining under the mucosa there is apparently in many cases no natural limit to their activities, and they will soon end—as we unfortunately saw in autopsy after autopsy—by removing up to 90 per cent. or more of the surface. Every hour therefore counts at the beginning of the infection, and the delay of a few weeks often means almost irreparable ruin to the colon, even if the patient survive. It appears to me to be useless to wait for laborious laboratory examinations as to the exact nature of the infection. Emetine is so benign a drug when given subcutaneously that it may well be administered on the chance that the infection is amœbic—just as it is the universal practice in the Tropics to give quinine in nearly all tropical fevers, without waiting for a pathological diagnosis, at least if this is likely to be delayed for more than a few hours. As in fever so in dysentery, we strike our preliminary blow on the chance that it will destroy the enemy whose presence we have reason only to suspect. The results were remarkable. Of course, owing to many difficulties the order referred to could not take full effect for several weeks; but, after that, I was informed by everyone that the newer cases were not of so grave a type as the earlier ones.

I will now digress for a moment to emphasize a point which appears

to me to be of great importance. The large bulk of the troops came from Britain, Australia, and New Zealand, and were therefore men who had never had amœbic dysentery before. Many of the cases were at first exceedingly grave—as grave, in fact, as I have ever seen among European troops freshly arrived in India. Many of them had died before reaching the base hospitals, and those that survived had frequently reached the condition of “running dysentery,” which always suggests to me that a large proportion of the mucosa is already destroyed. But, on the other hand, it was remarkable that among the Indian troops, who are exposed to amœbic dysentery from childhood, there was not nearly the same frequency of severe cases. I was, in fact, informed at the Indian hospital that they did not suffer much from the disease, and that the cases which occurred were comparatively slight. From this and other considerations I am therefore led to believe that in amœbic dysentery, as in malaria and many other diseases, there is likely to be a certain amount of immunity acquired during childhood in localities where such dysentery is endemic; and that, conversely, there is no such acquired immunity among the inhabitants of non-endemic areas. I think, then, that among the European and Australian troops we had to deal with acute amœbic dysentery in its most unmodified and malignant form. This point must be remembered in connexion with the results of emetine treatment. The studies of Rogers and of many Indian physicians were carried out mostly upon Indian patients—which probably explains the very rapid action of the drug; and we could hardly expect that we should have equally rapid results among the European troops in the Mediterranean.

Nevertheless, we were all convinced of its great and indeed remarkable efficacy. As a general rule, doses of 1 gr. a day of the hydrochloride were prescribed, either given in one subcutaneous injection or given in two doses morning and evening. Practice differed considerably as to the number of doses administered. In one hospital the rule was that of three days on and three days off, and in another hospital the rule was five days on and five days off; while many practitioners preferred to give it daily, even for some weeks, without intermission. Indeed, the practice in India has frequently been to administer it not only for weeks but for many months, and I have heard of cases in which altogether 100 doses or more have been given. But opinion soon began to be divided upon this point in rather an abrupt manner. In one hospital three cases of dysentery died without our being able to find any very obvious cause of death, and some

suspicion was aroused that the death might have been due to heart failure owing to cumulative action of the emetine. These cases were admirably reported by the medical officers in charge, but on considering them carefully I doubted much whether the fatal result could in any way be attributed to the drug. Two of the cases died, indeed, about a week after the last dose of emetine, and both these patients had mysterious rises of temperature for some days before death, these suggesting that death was really caused by some secondary infection which could not be traced clearly at the autopsy. The third patient died four days after the last dose of emetine, though he was convalescent and had been allowed out of bed. Even in this instance the case against emetine was extremely weak, because the total amount of emetine which the patient had received from the beginning scarcely amounted to what Rogers has calculated is a single lethal dose, namely, about 15 gr. for a man. There was fatty degeneration of the heart in all these cases, but no more than is frequently found after any grave illness. On the other hand, numerous patients had received emetine for more than thirty days without exhibiting any bad effects which could at all be traced to the drug, and Indian officers especially were not inclined to admit that emetine is at all cumulative or mischievous in its results. Nevertheless, practice gradually crystallized into the formula that, unless there was very strong reason for continuing emetine, it should be remitted, at least for a time, after about ten days, especially if it had ceased to do good. And on the whole I think that this is the best practice in acute dysentery.<sup>1</sup>

As was to be expected, about 10 or 20 per cent. of the cases failed to be cured by emetine. I do not mean that all of these patients died, but that the dysentery gradually merged into the chronic form. Most but not all of these cases were patients who had not received emetine early in the infection, and in whom, therefore, the mucosa was probably destroyed over a large surface before the treatment was commenced.

<sup>1</sup> Since this was written an important preliminary note has been published in the *Brit. Med. Journ.* of December 18, 1915 (p. 895) by Dr. H. H. Dale, F.R.S., stating that experiments on cats and rabbits showed that hypodermic injections of emetine (? hydrochloride), continued from three to ten days in doses above those usually administered in cases of dysentery, caused diarrhœa, emaciation, and other symptoms with fatal results. Dr. Dale thinks that these experiments suggest that "emetine can produce cumulative poisoning of serious importance." I presume that he means rather that the effect is cumulative than that the drug itself is cumulative—another thing. If one dose can cause intestinal hyperæmia I presume that too many and too large doses can exaggerate this effect without the drug itself being actually cumulative. We await further experiments, but the few already made fully justify the note of caution sounded by those to whom I have just referred.



And this introduces a very important factor in treatment. Emetine can only destroy the parasites in the ulcers, it cannot heal the lesions themselves. Now it is almost or quite certain that in most cases the original trench-work of the amœbæ is followed by an extensive bacillary invasion of some kind. At our pathological meeting in Alexandria, difference of opinion was expressed as to whether these secondary organisms were always *Bacillus dysenteriae* of any strain, or others, including the normal flora of the intestine; and prolonged researches will be required before this point is settled. We must, of course, understand (though I have not stated it clearly up till now) that complex infections were exceedingly common. For obvious reasons it is quite impossible for me to give figures, but I gathered from continual visits to the hospitals that this was the case. Thus both the amœbic and the bacillary forms of dysentery occurred together in a certain proportion of cases, and in others both forms complicated the typhoids, or were complicated by obstinate diarrhœas, many of the latter being probably due to the flagellates. Now in all such cases we could hardly expect emetine to do more than its own proper work; it destroyed the amœbæ, but the patient was then obliged to deal with the secondary or complicating maladies. The text-book picture of simple amœbic dysentery was therefore not the only one seen. It frequently happened that cases which were brought in for the typhoids showed dysentery in convalescence, and even died from it. We were at first inclined to attribute these cases to hospital infections, but later came to the conclusion that the dysentery infection had probably been present from the beginning. I became very convinced of this from the remarkable results of Lieutenant G. B. Bartlett, R.A.M.C.(T.), pathologist to one of the hospitals. He found dysenteric lesions in thirty-three autopsies, and has kindly given me a preliminary classification of these, pending further studies which he will, I hope, publish in detail. Out of the thirty-three cases, fifteen died from exhaustion, two from typical tropical abscess of the liver, two from perforation of the dysenteric ulcers, two from hæmorrhage, two from typhoid or paratyphoid, and no fewer than ten from other causes such as wounds, empyema, septicæmia, cirrhosis of the liver, &c. Now it is remarkable that in many of these cases dysentery had not been diagnosed at all during life, although post mortem the dysenteric lesions were found sometimes to be extensive and severe. Out of these thirty-three cases, moreover, sixteen were found to be purely amœbic, three were amœbic with typhoid ulceration, and in fourteen there was

evidence in the intestine (or rarely in lymph glands, spleen, liver, or kidneys) of a secondary infection on the top of the amœbic ulceration. We are thus forced to infer that the amœbic ulceration may not always declare itself during life in the form of atypical dysentery which can be diagnosed by the well-known signs. In fact, I personally think that much mischief may be caused by the amœbæ without any signs whatever, and that in many cases the typical disease does not begin to show itself until some secondary intestinal invasion has taken place on the top of the trench-work of the amœbæ. How long this larval form of amœbic dysentery can continue I cannot say; but in many cases it had continued through the whole course of an intercurrent typhoid infection. Indeed, I was much inclined to recommend, on the strength of Dr. Bartlett's figures and of other observations, that emetine should be given to all hospital patients as a preventative, just as we give quinine to almost all hospital patients in a very malarious locality. But the recommendation was hardly necessary, since most officers in charge of wards had already become very much alive to this danger, and were giving the emetine very generally in consequence.

While I was in Alexandria pure bacillary dysentery was not very common, though we saw a fair number of cases. There were also, of course, a proportion of cases which certainly did not belong to either group; and we had the usual difficulties in the way of diagnosis. The pathologists scarcely had time at first to make very minute studies, and everyone knows that amœbæ may be easily overlooked unless plenty of time is given to the search. I will not say more under this head, as a Central Laboratory has now been established and will deal much more thoroughly with such questions in the future.

Returning, then, to treatment, our cases practically divided themselves into two groups—those in which about one week's emetine treatment practically resolved the dysentery, and those in which it failed to do so. Of course, the latter cases were by far the most troublesome, and they generally resulted in what I have always known as running or even incontinent dysentery. I fear that some of our best clinicians admitted their failure in dealing with these, which, however, form only a small percentage of the cases. Post mortem they always showed most extensive lesions and, indeed, an almost entire destruction of the mucosa. Many of the cases died, but, as I have already said, others resulted in chronic dysentery and were invalided, and are probably now under treatment in this country.

Now regarding the sulphates of sodium and magnesium, these have long been recognized as invaluable for the treatment of bacillary dysentery. Their purgative action is not due to peristalsis, but to the increase of the secretion of fluid and at the same time to the retardation of its absorption. According to the researches of Hay, the dose produces increased secretion of fluid as it passes downwards, and the salts do not purge when given intravenously. But the more recent experiments of Cook and Schlesinger suggest that the drugs are first absorbed from the stomach and then cause a rush of fluid from the mucosa of the large intestine; and these results certainly accord more with the fact, often noted, that the salts purge within half an hour after administration. It was also shown that bismuth meals administered at the same time as the sulphates succeeded in travelling downwards a very little distance before the fluid began to collect in the large intestine—which proves the same thing. These points are of great importance in connexion with the treatment of dysentery. I infer that the salts act in this disease simply by flushing out the large intestine and washing away the parasites, amœbic or bacillary, by a strong current *from within*, that is, from the blood itself. In fact, they probably produce the same, or better, effects than would be produced by energetic washing of the surface from without. That is to say, as long as these salts continue to be given the dysentery-producing organism cannot easily get a foothold within the mucosa. Of course this reasoning is hypothetical, but, taken in connexion with the proved efficacy of the drugs, it encourages us to use them in both forms of dysentery. Indeed, the practice of administering the sulphates together with emetine hydrochloride in the earliest days of a dysentery is now becoming almost universal; the emetine kills any amœbæ that may be present, and the salts wash both them and injurious bacteria out of the follicles. In further advanced cases—say those of several weeks' duration—the use of the salts becomes more problematical in the case of amœbic dysentery, and one must be on guard against exhausting patients who have been extremely purged by the disease itself, especially if this has already taken the form of running dysentery. On the other hand, I admit that many practitioners gave the salts in almost all cases, of whatever duration.

The treatment of acute running dysentery is the great problem in most cases, and, I regret to say, one which cannot always be solved—as admitted by some of the most experienced practitioners in all countries. Here the mischief has been already accomplished, and we are probably in the presence of an almost denuded colon. We tried

antidysenteric sera in many such cases on the mere chance that they might destroy the specific bacteria if present, there being usually not sufficient time to make minute pathological studies; and the treatment was said by many to be remarkably beneficial in a certain proportion of cases; but others were not so favourably inclined to it, and others, again, argued that an ordinary horse serum would probably have been just as useful. I have suggested that polyvalent sera against other intestinal flora than the specific might be put on the market, and hope that such will be tried before long. Failing sera, we had recourse to medicinal treatments. I always urged the bismuth treatment elaborated by Deeks in Panama, and consisting of a heaped teaspoonful of the subnitrate three or four times a day. The utility of this medication is supposed to depend not only upon the astringent value of bismuth, but also upon a chemical change, and lastly upon the mechanical action of the powder, which is likely to injure amoebæ by attrition. At all events, bismuth in these and smaller doses is very generally given in dysentery, was largely employed throughout the hospitals in Alexandria, and certainly, I think, proved distinctly useful. It was generally administered continuously, beginning a day or two after the initial treatment with emetine was commenced. I have always liked tannic acid in conjunction, say up to 10 gr. each dose. In fact, tannic acid has been used for a long time, and was often administered together with ipecacuanha powder, because it tends to check the vomiting as well as because of its astringent effect. But it is a good general rule not to use any astringents too early, that is, until the specific remedies have had time to take effect. After that happens, however, opium, bismuth, tannic acid, catechu, and other astringents do often succeed in terminating the attack very rapidly. I regret to say, however, that many became cases of established running dysentery in spite of all treatment.

Collapse is another very difficult condition to deal with. Lieutenant-Colonel A. H. Lister, of one of the hospitals in Alexandria, often used the subcutaneous infusion of hypertonic salines together with 10 minims of adrenalin solution, 1 in 1,000, every four hours, injected hypodermically, with very good results, and strongly recommends the adrenalin.

Opium is, of course, an invaluable drug throughout the treatment, and, though some practitioners appear to be nervous about it, I think that the use of it is really general at all stages.

We now come to a very vexed question, namely, the use of enemata in acute dysentery. There is no question of their utility in chronic



dysentery, especially of low-down ulceration; but some of us felt that they are contra-indicated in very early cases. The disease is itself complicated with purging, and if the sulphates are being given at the same time, there is a continual washing out of the bowel from within. In such cases I really cannot understand the rationale of using massive lavations, which depress the patient and may even rupture the very thin walls of the ulcers—and we never know how old and how deep the ulcers may be. Others object to them on the ground that they may engender secondary infections from without, and I have a feeling that the use of enemata is somewhat too often followed by running dysentery. Others, again, adopt enemata as a standard treatment. Such a great authority as Professor Kartulis is confident that tannic acid enemata are most beneficial from the first. Rogers often recommends quinine lavations. Colonel Lister is an advocate of 3 or 4 oz. of warm olive oil containing a drachm of ichthyol, preceded by a morphia suppository half an hour before the injection. Nitrate of silver has always been employed in chronic dysentery, and I know many cases where it has certainly effected a cure; but nowadays we should use protargol or similar salts, and these were employed with fairly good results in many of our fresh cases. If lavations are employed at all, I like warm water without salts, as it swells up and destroys the amœbæ, or warm and weak permanganate solution, or warm and weak solution of quinine. But I still do not much favour this form of medication in acute dysentery.

I was able to follow up only one case of acute dysentery in which appendicostomy was tried, and cannot say that the result was very brilliant. There does not appear to me to be much *a priori* argument in favour of it. The bowel can be washed out from within by means of the sulphates, and I am by no means convinced that washing from without gives as much benefit in the case of the large intestine as it would do in the case of a superficial ulceration of the skin, while the operation itself and the wound add a severe trial to patients who are already sorely tried by their malady. At our discussion on dysentery at Alexandria Colonel Tubby expressed himself as opposed to appendicostomy for acute dysentery, and even for chronic amœbic dysentery, but was less opposed to it for bacillary dysentery. Irrigation with oil through the appendix appears to me to be more promising than that with other fluids, and it was tried, but with doubtful results, in the case which I watched.

Regarding subsidiary items of treatment, I need scarcely insist

upon the general use of cholera belts or warm applications to the abdomen. In India I frequently used very hot hip baths, which I am certain give great relief, often lasting for several hours, and these baths can be repeated several times a day if the patient is not in too weak a condition. We could not try such baths much in Alexandria, but I would suggest them for the use in the cases in this country. Personally, also, I have always been much in favour of oils by the mouth. Olive oil is an old remedy for dysentery in Italy. A dessert-spoonful of it with a pinch of salt may be given several times a day, but the patient soon sickens of it, and when that happens I would suggest the non-purgative medicinal paraffins, which, indeed, may be given from the first. I have often used these, and have observed that they appear in a saponified form in the stools, a form which is likely to be very injurious to all kinds of organisms and in fact to smother them. Indeed, this effect of oils is, I believe, well recognized.

Innumerable drugs have been supposed to be curative of dysentery. Kartulis told me that he had had very good results from a drug called uzara (difficult to obtain in this country), and Dr. Fingland recommended, in the *Lancet* of August 15, 1903 (p. 456), the use of *Aplopappus baylahnen* (Parke, Davis and Co.), on the results obtained from three cases only. The evidence in favour of all such drugs is generally very weak, and they are scarcely to be tried unless all other measures have failed. In this paper I have confined myself to the broader lines of treatment used by the majority of clinicians.

Regarding diet in acute dysentery. I must say that, after comparing the very various diets given by many medical men, I am beginning to be almost sceptical as to whether any particular form presents any real advantage. I have seen cases which improve and cases which die with liquid diet, bland diets, and ordinary diets; and, in fact, cases which are not given the great advantages of perfected hospital dietaries are often those which suffer the least. We are too apt to think that the form in which food is presented to the stomach is also the form in which it appears in the colon. For example, bread is a solid diet for the stomach but almost a liquid one for the colon, and the converse holds very often with milk. To be frank, I scarcely dare speak upon this subject owing to the strong but very opposite opinions held by medical men. Of one thing, however, I am almost sure—namely, that large bulks of fluid diet are injurious in very running dysenteries. It seems to me that such bulks of fluid are rapidly absorbed from the stomach, and increase the blood-pressure, with the result that fluid rushes out again from the

ulcerated surfaces of the intestine. Whatever the explanation may be, we can observe that such large bulks of fluid are followed, sometimes within ten minutes, by copious evacuations which by no means benefit the patients. The case is much the same as that of large bulks of fluid in the treatment of catarrhs; and, personally, I prefer a comparatively dry but varied dietary in running dysenteries. Perhaps many medical men would be almost appalled by this hypothesis, and would never dream of presenting the patient with anything but whey, milk, broth, and so on. Certainly in typhoid a premature return to solid diet notoriously sends up the temperature; but the large intestine is further away from the stomach, and I have not seen much evidence of a similar effect in dysentery. At the same time, one officer told me that when he wished to diagnose the presence of dysenteric ulceration in a convalescent case of enteric he gave a meal of oatmeal porridge.

I now come to the most important point of all—the prevention of hepatic abscess. And this especially is the leading proposition before us here in England. Some months ago, we in Alexandria heard from several sources that you in England had discovered only bacillary dysentery, and still, by many accounts, amœbæ are rarely found here. This divergence of experiences can be explained in several ways, either on the hypothesis that the amœbæ have become scarce during the lapse of time or that they have been destroyed in the majority of the cases by emetine, or that the epidemic of amœbic dysentery lasted only for a very short time. Possibly also amœbæ were not found because they were not searched for often enough in each case. By no means do we always discover the amœbæ in the first sample. I remember an acute case in which I discovered them only on the fourth trial, though the previous trials had been very carefully conducted; and it stands to reason that we cannot give negative diagnoses on the strength of such extremely small samples as we usually examine. On the whole, I think that, in view of Lieutenant Bartlett's post-mortem figures, there was a vast amount of amœbic dysentery in the Mediterranean, even among persons in whom the disease was never diagnosed. Now we must remember that hepatic abscess follows in a large proportion of amœbic infections, whether these infections have produced dysenteric symptoms or not. On taking the histories of cases of hepatic abscess we find no history of previous dysentery in something like a quarter of the cases—the probable truth being that the amœbic infection of the liver takes place from small intestinal pockets in which amœbæ had remained without causing widespread dysenteric lesions. In fact, I have

often thought that abscess follows the milder cases of dysentery more frequently than the severe ones.

Again, from some figures quoted by Rogers in the *Indian Medical Gazette* for November, 1912, we learn that the British Army in India during the decade 1901-10 had one case of liver abscess for every seven cases of dysentery—more than 14 per cent. If a similar proportion will be found to hold for Mediterranean dysentery, we can expect a very large number of cases of abscess. We must remember that the abscesses do not only appear a few months after attacks of dysentery, but continue to appear for years afterwards. Now this seems to me a very grave danger, and I would strongly urge that the only way to avoid it is to give courses of emetine or ipecacuanha in convalescent cases every month or so. I would suggest emetine hydrochloride in grain doses for three days every month until the patient is finally returned to duty, and think that this should be administered whether any symptoms of dysentery remain or not. As long as any amœbæ still live in the intestine so long will the danger of abscess be present—or, at least, that is the safest hypothesis in practice. And I do not care whether a positive diagnosis of amœbic dysentery has ever been made or not made. If I myself had been attacked with dysentery I would certainly submit myself to this discipline without fail. Acute dysentery is a grievous malady, but it is only the first act of the tragedy; chronic dysentery is the second act, and is still worse; but hepatic abscess is the third act and is worst of all. We are too apt to look upon amœbic dysentery as being only a temporary affair, easily cured by a few doses of the specific.

Some clinicians (such as Colonel Robinson, in Alexandria) think that ipecacuanha is still useful in the later stages of dysentery, and especially as a prophylactic, and I am inclined to agree with them. Emetine destroys the parasites in the mucosa, but I doubt whether it can really have the same effect upon those which are living and perhaps abounding in the lumen. For these, perhaps, the powder is still useful, and, I think, can be administered in much smaller doses after emetine has been given than would be useful without emetine. For example, 5 gr. each of ipecacuanha powder, Dover's powder, tannic acid, and sulphate of quinine can be administered the last thing at night on an empty stomach, without prefatory opium, and, if given properly, will not cause vomiting, at least after the first dose or two. But this is a matter of personal opinion, and I think that as long as ipecacuanha is given in some form, the same result will be achieved. Of course, if abscess is beginning to manifest itself in any way, return



to full doses of emetine hydrochloride should be enjoined, combined, of course, with suitable diet. Most people urge entire abstinence from alcohol, but three of the worst cases of abscess ever seen by me in India occurred in teetotalers, and abscess is fairly frequent among Indians, who never touch alcohol.

I have now kept you long enough, although I have practically dealt only, and that cursorily, with the treatment of acute dysentery. Fortunately, I think, we are not yet in the presence of much of the chronic dysentery which will probably remain over from the epidemic. The treatment of chronic dysentery is itself a very large subject requiring special consideration, and we can hardly deal with it now.

Two points must be mentioned before I close. The first is that we thought of a method of diagnosing dysenteric ulceration by means of the X-rays, and this method was ably worked out by Lieutenant Glasson, of one of the Alexandrian hospitals, whose paper will shortly appear in the *Proceedings* of this Society. I show some photographs of his. A bismuth meal must be given and then cleared by means of oil. Some of the bismuth remains in the ulcers and is then shown by X-ray photographs. Secondly, I should like to call attention to the excellent "Notes on the Treatment of Diarrhœa and Dysentery," recently written by Lieutenant-Colonel Andrew Balfour, C.M.G., which will be useful to all concerned.

In conclusion, I hope that we may hear their views on the treatment of dysentery from some of the many medical officers who are now treating cases in England.

## DISCUSSION.

Dr. C. M. WENYON : My reason for wishing to make a few remarks on the subject of dysentery is that for about a couple of months I have been examining for evidence of protozoal infection invalids who have returned from Gallipoli. I have made about 1,500 examinations in 500 different cases, and in this way have obtained a good idea of the type of protozoal infection contracted by these men. It must not be forgotten that most of these men have been treated with emetine or some other drug, so that the number of infections I have encountered must be considerably below the number of infections actually occurring on the Peninsula. I have found that 60 to 70 per cent. of the men I have examined have some protozoal infection, and it is very important to distinguish the type of infection, because on this diagnosis depends the subsequent treatment of the case. I have recently published in the *Lancet* a short account of the protozoa which occur in the human intestine, and I have mentioned there some of the results I have obtained. The commonest infection one finds is due to *Entamæba coli*, a perfectly harmless parasite. I want you to realize that in the human intestine there are several kinds of amœbæ. The *Entamæba coli* is non-pathogenic, whereas *Entamæba histolytica* is the cause of amœbic dysentery and liver abscess. There are also other small amœboid organisms, resembling the free-living *Amœba limax*, which are occasionally found in the human intestine and which are again quite harmless. Of the 60 to 70 per cent. of protozoal infections which I have mentioned, 50 were due to *Entamæba coli* and 10 to the pathogenic *Entamæba histolytica*. The remainder were infections with flagellates or coccidia. In other words, 10 per cent. of all the men examined were harbouring *Entamæba histolytica* and were carriers of amœbic dysentery. Such men are liable at any time to relapse into acute dysentery or contract liver abscess, and they are also dangerous in that they may be the means of spreading amœbic dysentery among other people. It is therefore very important to separate these carriers of *Entamæba histolytica*, and they can only be discovered by examination of the stool microscopically. It must be fully realized that, it is only rarely in such cases that active, crawling, free-living amœbæ are seen ; more commonly one finds the encysted forms, and these are just as diagnostic of an amœbic infection of the gut as an ankylostomum egg is diagnostic of an infection of the gut with the adult worm. There occur encysted forms of both *Entamæba coli* and *Entamæba histolytica*, and it is very important to distinguish between the two kinds of cyst. I have carried out observations on the amœbæ in the human intestine for a number of years, and I have kept under observation, sometimes for three or four years, people who have had infections with *Entamæba coli* or *Entamæba histolytica*. I have found that the amœbæ always remain true to type, and one form never changes over into the other—a man who is passing cysts of one of these amœbæ

continues to do so, it may be for years, till his infection is got rid of. Such a person may carry *Entamæba histolytica* without showing any dysenteric symptoms at the time, but the infection can be recognized by the finding of the characteristic cysts in the fæces. On several occasions I have been able to produce typical amœbic dysentery in cats by administering cysts of *Entamæba histolytica* obtained from cases showing no signs of dysentery. These carrier cases may at any time lapse into acute dysentery or liver abscess, and the fact that 10 per cent. of the cases I have examined have this type of infection is a highly important one. I do not say that an epidemic is likely to occur in this country, as the conditions may not be favourable, but I have seen cases which have contracted infection in this country almost certainly from carrier cases returned from abroad. Recently I saw a case in a man who was employed at the London Docks. He had never left this country, but he contracted amœbic dysentery with liver abscess, and died. Numbers of characteristic *Entamæba histolytica* and the cysts of these were found in his fæces. He had small ulcers in the large intestine, and it must have been through these that invasion of the liver had taken place. I am shortly going to the Mediterranean, and I hope to obtain some information as the extent of the amœbic infection amongst the troops in that part of the world. The percentage of infections will probably be higher than 10, for many of the men returning to this country have already been treated with emetine. Of the other protozoal organisms of the human intestine the pathogenicity is doubtful; but on those occasions on which they occur in such numbers that the field of the microscope is filled with them as a mosaic, it is difficult to doubt their pathogenicity. Finally, the point I want particularly to emphasize is that the carriers who return to this country should be isolated and treated with emetine, for the drug, even if it does not always get rid of the infection, will usually do so, and to a large extent prevent relapses and the spread of infection to other people.

Dr PENFOLD: We (Ledingham, Penfold and Woodcock) have been examining stools from cases of convalescent dysentery in the King George Hospital, and there we have had a different experience from that of Dr. Wenyon. In 103 cases we found about 47·5 per cent. gave evidence of previous bacillary dysentery. We tested the serum of these cases against Shiga's bacillus, and we got that percentage of positive agglutination. We established the upper normal limit of agglutination against twenty-three controls who had never been out of the country, so that at least 47·5 per cent. were found to have suffered from dysentery. Amongst the negatives we had one man in whose stools we isolated Shiga's bacillus, so possibly many of the negative cases may have had Shiga's dysentery, and may have lost the positive agglutination reaction. We have not yet had time to classify these cases according to the month of their infection, but a rough analysis of the cases leads one to suppose that a large percentage of them had bacillary dysentery. We have isolated from subacute and convalescent

cases about twenty strains of *Bacillus dysenteriae* (Shiga) and two strains of *Bacillus dysenteriae* (Flexner). These positive agglutination results and the frequent isolation of Shiga's bacillus have greatly impressed us with the importance of bacillary dysentery in the sick returning from the Mediterranean. The search for entamœbæ in the pathological material from the convalescents has been conducted by Dr. Woodcock, parasitologist to the hospital. Active forms of *Entamœba histolytica* have been seen in two cases only—once in the fæces of a convalescent and once in material from a liver abscess. *Entamœba histolytica* cysts have not been found in any case up to date. These negative entamœbic results are in consonance with the positive bacteriological findings. [Sir RONALD ROSS: I would like to ask Dr. Penfold if he knows at what time of the year these cases had become infected, and whether all the cases examined were from the Mediterranean?] They were all from the Mediterranean. They became infected, I think, in May and June, some of them, but the bulk in August and September. I have taken the histories of the whole 103 myself, but I have not yet classified them in groups of months.

Dr. CROPPER: The subject of dysentery is new to me from the experimental point of view, but for some years I have been examining free-living amœbæ of the non-pathogenic type, and judging from what Sir Ronald Ross says about the treatment, I think my work may have some bearing on the subject. I will first explain the methods on which the experiments were based. We used the jelly method devised by Dr. H. C. Ross some years ago, which was originally intended as a new method of staining. But since then its application has been extended to examining the life-history of various organisms, particularly of amœbæ. I was associated with A. H. Drew in this work. The first thing we aimed at explaining was the reason why amœbæ encysted, and then, subsequently, why they came out of their cysts. We came to the conclusion that the amœbæ were greatly influenced in this respect by the bacteria which are always growing along with them. That suggested to me that possibly the explanation of the use of the different substances which Sir Ronald Ross has mentioned in treatment is that they act as intestinal antiseptics, and prevent the amœbæ multiplying and becoming encysted, and in that way bring about an alleviation of the symptoms. If anyone else knows of other antiseptics useful in dysentery, it might help to settle the matter.

Dr. A. NEWSHOLME, C.B.: Although my practical acquaintance with dysentery is almost *nil*, I have been greatly interested in Sir Ronald Ross's paper, and I have learned much from it. I hope that so far as this country is concerned, the opportunities for physicians generally to treat dysentery will remain very few. At the present time, however, these opportunities are numerous, and the information which has been given by Sir Ronald Ross will be of great value to physicians in this country in their treatment of this difficult and obstinate disease. I therefore hope his paper will be published



very shortly. I am more interested in dysentery from the point of view of prophylaxis than of treatment, and I was particularly interested in Sir Ronald Ross's statement as to the possibility, judged by past experience, that one-seventh of the total cases of dysentery may hereafter develop hepatic abscess. If that be so, then, speaking as one particularly interested in prophylaxis, I would emphasize the importance of the advice given by Sir Ronald Ross that emetine should be pushed for all the value that it possesses. I cannot help thinking that, from the standpoint of prophylaxis in this country, not only that of hepatic abscess but of dysentery itself, there is ample scope for a further paper—possibly from Sir Ronald Ross—on the question of the prevention of spread of this disease, and as to whether it is likely in the next few years to spread materially in this country or not. In past times there has been much dysentery in England, but for many years we have had little or none of it outside lunatic asylums, and I hope the present importation of the disease will not lead to any change in this state of matters. But it would be interesting to hear from Sir Ronald Ross what are the possibilities in that respect; and if he can give any further guidance as to the means for preventing these possibilities from becoming realities we shall be grateful.

Dr. F. J. WETHERED: I have listened with great interest to the discussion. It so happens that I have seen a large number of Australian officers who have returned from the Dardanelles, having suffered from dysentery. Some of those officers belong to the Australian Medical Corps, and I questioned them particularly as to results of treatment by emetine. With regard to bacteriological findings, they were unable to give me any information, but their opinions rather differed as to the results of using emetine. The majority, however, seemed greatly to favour it. Some of them gave emetine in all the cases, and I was particularly interested in Sir Ronald Ross's statement that when the diagnosis as to the form of dysentery is doubtful emetine should be given, somewhat on the lines of a doubtful throat affection, in which, before a bacteriological examination is made, a dose of antidiphtheritic serum should be given in order to decide whether it is tonsillitis or diphtheria. And, at the other end of the story, as Sir Ronald Ross said, when the patient is convalescent, if it has been an amœbic case, the emetine should be continued, with the object of preventing liver abscess forming. I would like to ask Sir Ronald Ross a question or two: whether the means of spread is a composite one, whether there are several modes, or whether it is chiefly fly-borne, or due to diet?

Major T. R. BRADSHAW (R.A.M.C., T.): I am sure we have all listened with very great interest, and I hope a great deal of profit, to Sir Ronald Ross's admirable account of the work he and others have been carrying on. Coming from one of the base hospitals in order to get some help in my work, I have been disappointed in gathering that Sir Ronald Ross does not wish this discussion to deal with the treatment of chronic dysentery. Of course, the cases which come to us at the base hospitals in the country are practically all chronic; in fact, beyond the statement that they have had dysentery,

there would be little to show that they were not merely suffering from some form of chronic diarrhœa. There are many of these cases, and they are somewhat troublesome, because the diarrhœa does not tend to stop very readily, and they have become still more a source of anxiety to us since the order from the War Office to the effect that no more cases of dysentery are to be discharged from hospital or transferred to another until three bacteriological examinations, spread over a period of a month, prove negative. One is very anxious to know what is the best treatment to carry out in these cases which are reported to be dysentery, in which some mucus and possibly a trace of blood are present, and which do not tend to get well. In my own ward, at the First Western General Hospital, I have had a number of these cases, but in only one of them has a positive bacteriological finding been recorded. A young private, the son of a doctor, has come in very anxious to get home. The first bacteriological examination was negative; the second also was negative, and when I was just preparing to send him home, a third examination was made, and the pathologist reported that there was a large culture of Shiga's bacillus. Now, according to the regulations, I suppose he is fixed in hospital for another month. The fourth and fifth examinations have been negative. Of course, if there is one more negative report he will go home. I had a personal experience of acute dysentery of a very intimate kind many years ago, when I was ship surgeon in South America. It was a case of very intense dysentery. The patient, a ship surgeon, was well in about a fortnight, after being given massive doses of ipecacuanha, in very much the way Sir Ronald Ross has described. That was thirty years ago, and there has been no return and no abscess formed. But I want to know what is the proper treatment where men are returned invalided from the Dardanelles—not, perhaps, seriously ill, but with the dysentery hanging fire, so to speak. I may mention what my own treatment is: it is a very old-fashioned remedy, and, strangely, it has not been mentioned to-night. It is treatment with castor oil. I do not think it is generally known that castor oil was early employed—I think by the missionaries in Barbados in the eighteenth century—not as a purgative at all, but as a treatment for dysentery. I came across the original paper among some old books in the Northern Hospital in Liverpool; it was most interesting, telling a great deal about castor oil, but without mentioning its purgative properties; it was stated to be a remarkable specific for dysentery. I have treated these cases of persistent diarrhœa with small doses of castor oil, guarded or not, as the case might require, with a little opium, and the results have been most excellent. But I should like to know if anyone can tell me what is the prognosis in these cases, presuming them to be amœbic or bacillary dysentery. How am I to know that they are free from the germs? And how long ought they to be kept in hospital? Are they any material danger to people whom they move amongst? If a man's excreta have been demonstrated to be free from the germs, is it safe to allow him to go home to his ordinary life? These are points on which we at the base hospitals would be very glad of information.

Dr. DUNN: I have been treating dysentery for some years, and I would like to say a few words with regard to the treatment of the disease in this War. First, I would urge that patients should not, unless unavoidably, be changed from one doctor to another. It is unfortunate that in some hospitals there may be a doctor who has had experience of dysentery with no cases of dysentery under his care, while cases of that disease are being treated by men who have never seen dysentery before. Another thing I would suggest is that great care should be taken that at least one of the doctors on each hospital ship should be experienced in the treatment of dysentery, for it is of the utmost importance that these men who are brought straight from the East should be under the care of men who know the disease. Many doctors do not take enough care to ensure absolute rest for their patients. Many of these poor fellows are allowed to get up and go to the lavatory day after day. Perhaps it cannot be helped when there is only one orderly to every ten or fifteen beds: but I think this practice often causes relapses, and many men come back with diarrhœa as a result, I believe, of their having been allowed to be up and about much too soon. As to diet there are great differences of opinion, and it is only possible to touch on the question. Personally, if I get a case early I starve him for the first few hours, giving him nothing but water, with perhaps a little brandy. If he rapidly improves, I get him on to small solid meals as soon as I can, such as a lightly boiled egg and a piece of rusk, because that compels movements of the tongue and jaw, and only the pulped food enters the stomach. Paraffin I have found very useful in dysentery, both in the acute and the chronic stages. I have seen only one acute case recently—an orderly on a hospital ship. It was fairly mild: he had ten bloody stools in twelve hours, and suffered a good deal of pain. I gave no treatment beyond starvation and small doses of paraffin; the paraffin I ordered myself at each visit after deliberating whether he ought to have it or not. Hot applications to the abdomen are most important, with absolute rest. I think it most desirable to warn all the medical men in the British Isles, Canada, Australia, and New Zealand to look out for cases of abscess of the liver in our men, for it occurs in every part of the Empire. It is a wise precaution to give those who have recovered occasional doses of emetine, and I hope this suggestion will be supported by the leaders in our profession; for otherwise many practitioners who know nothing of liver abscess will not be on the look-out for it. It must especially be remembered that the abscess may occur in a man who has only had slight diarrhœa and who will deny that he has ever had dysentery.

Professor SIMPSON: Sir Ronald Ross mentioned that Tull Walsh was giving emetine in 1891; but I think one can go further back. In 1869 Mr. Eccles, in the Bombay Presidency, treated twenty-two cases of dysentery with emetine and morphia, with very satisfactory results. There is this to be said with regard to the use of emetine: it should be employed with care, for there appears to be a certain amount of risk attendant on the administration of large

doses or of small doses given for prolonged periods. I was reading, not long ago, the Medical and Surgical Report on the American Civil War, in which there is a very good résumé of the drugs which were used for dysentery. It points out that Magendie, in 1817, proved by experiments on dogs that enteritis was produced by large doses of emetine, and that this was produced whether the drug were given by the mouth or by intramuscular or intravenous injections. It also details a number of experiments by other investigators, and always with the same result. The general conclusion arrived at was that with very large doses there was produced an affection of the nervous system and collapse, while with small continuous doses enteritis was caused. The warning given by Sir Ronald Ross when referring to Dr. Dale's important investigations, reported in the *British Medical Journal* of this week,<sup>1</sup> regarding the administration of this drug should receive careful attention. I have not noticed any bad effects in cases under my care treated with emetine, but still, I think it is right that one should be careful and watchful when employing the drug, otherwise it is possible that a chronic colitis may be set up.

Dr. R. MURRAY LESLIE: I have recently had under my care about 100 cases of dysentery, and among them were five cases of well-marked jaundice—three who arrived with, and two who had already had, severe jaundice. I wish to ask Sir Ronald Ross whether these cases with jaundice are more likely to develop liver abscess than the others. I should like to take this opportunity of strongly emphasizing what one or two of the speakers have already referred to—namely, the value of liquid paraffin in dysentery. Having had a good deal of experience with petroleum in cases of mucous colitis, I at once administered liquid paraffin in the recent cases of dysentery under my care at the Hampstead Military Hospital, with extraordinarily good results. Cases with fairly profuse diarrhoea and much mucus, and in some instances blood also, have been so greatly benefited that in two or three days the blood ceased and the quantity of mucus soon diminished and gradually disappeared. I have already charted a series of these cases. It may be that the paraffin interferes with the nourishment of the amœbæ by coating the mucous membrane of the bowel, while its lubricating property prevents the food exerting an irritative action. I strongly advocate the use of liquid paraffin in cases of subacute dysentery. In severe cases rectal lavage along with the petroleum is useful; either simple saline douches or solutions containing silver nitrate may be employed.

The PRESIDENT: There are one or two interesting historical points which I may mention. There was a time when, in some of the hospitals of London, dysentery was frequently seen. When I was a student and junior assistant physician, there was no school of tropical diseases as there is now at Poplar. We had at Guy's Hospital, then, a chance of seeing cases of dysentery, though they were not, as a rule, severe cases. They were sailors who arrived at the

<sup>1</sup> *Brit. Med. Journ.*, 1915, ii, p. 895.



Port of London suffering from mild forms of dysentery, and ipecacuanha in doses of 5 gr. three times a day used to cure them promptly. In those days Sir William Gull—Dr. Gull as he was then—was one of our best clinical teachers at Guy's Hospital, and his treatment of dysentery was comprised in the following: rest, warmth, and ipecacuanha. It is interesting to note that Dr. Dunn after his experience should lay similar stress upon rest and warmth. It is also of interest that ipecacuanha, after so many years, should still, either as the drug itself or its alkaloid, be regarded as the most practical remedy for one form of dysentery. But at one time, in consequence of the recognition that ipecacuanha caused so much vomiting, an attempt was made to cure dysentery with the residue of the ipecacuanha after the emetine had been removed: and it is not surprising, from what we know now, that that attempt failed, and that ipecacuanha in its proper form had to be retained; and that, because the emetic agent and the anti-amœbic agent are one and the same. My own experience of dysentery recently has been small. At the Seamen's Hospital, Greenwich, we have had some cases over from the Dardanelles. In the few that came under my own observation, no amœbæ were found; on the other hand, the identity of the bacillus was not established. None of them were very bad cases. Most of the patients were already much better and they were sufficiently treated by astringent methods.

Sir RONALD ROSS (in reply): I should like to have dealt more fully with the subject; but one hour is too short a time in which to deal even with acute dysentery. And to my friend, Dr. Bradshaw, I would say that I would very much like to discuss what he calls chronic dysentery, but which I think are probably merely old cases; chronic cases will be met with about five years hence. You will be having such for the next ten years; you are only just beginning now with old cases.

Dr. Wenyon pointed out that there are many amœbic cases in this country, and Dr. Penfold said he had examined 103 cases, out of which 47 per cent. were due to the Shiga bacillus. This discrepancy is not at all uncommon. First, Dr. Penfold's total of cases (103) was very small, and the possible statistical error, as doctors will forgive me for mentioning, is very large with such a number. In some of our hospitals in Alexandria some did not find the amœba as frequently as those at other hospitals did; there is the personal element. There is also a margin of about 20 per cent. error in finding the amœba and in making the diagnosis; and I should not wonder if in some hospitals the figure were found to go up to 50 per cent.

With regard to the treatment by emetine, many of the cases which come to England may have had doses of it over and over again. And there is a remarkable association, I fear, between the *Bacillus dysenteriar* and the *Entamoeba histolytica*. We found the two diseases together in a large proportion of cases—too large a proportion for it to be a matter of mere chance—so that I feel sure there is such association; we have not yet got to

the bottom of it. I hope Dr. Ferguson's central laboratory in Alexandria, just started, in which he has some admirable workers who are tackling this problem, will obtain some information about it. So we had better leave that for the moment.

Dr. Newsholme asked me how we are to prevent dysentery. I have not the remotest idea, and I do not know that anybody has, as to how dysentery is carried. It is an immense subject, and I cannot deal with it in connexion with treatment; I have not entered into the pathology, nor attempted to deal with prevention. There is the method of fly carriage, and there is the old story about water. We have found the *Amœba limax* attached to granules of sand in the desert of Africa, out in the burning sun, and at the bottom of the catacombs. They had been living there hundreds of years, for all I know. If the pathogenic amœbæ live in the same way, they will be liable to be carried down our throats in dust. The dysentery in Egypt is called "sand dysentery," and the dysentery in Gallipoli was attributed to the sand; so that this may be indeed a method of carriage. There is another possible method which has been mentioned, and which, I think, is a very likely one: it is off the seats of latrines. And there are other methods that I could mention. There was one case I came across (and I got the Surgeon-General there to call the attention of sisters and nurses everywhere to it) in which the infection was probably carried via the enema syringe. And it seems possible that the amœbæ may get on to the skin and crawl across it anywhere, and may enter the anus and other orifices. Hence I regret that I cannot answer Dr. Newsholme definitely. But owing to the kindness of the Medical Research Committee of the Insurance Fund, we have been able to start research on the very point he mentions at the Marcus Beck Laboratory, where Dr. Cropper is now assisting me; and we hope to have another worker on it also. And, if you will allow me the opportunity, I would like to say that after Christmas I would like to see as many cases as possible, especially of the kind which Dr. Bradshaw referred to. We do not only want to get material; we want to follow up the cases, or some of them, at any rate.

Dr. Dunn has made some very apposite remarks, which I am very glad to hear; I shall call attention to them in certain quarters, especially the point that cases should not be changed about from doctor to doctor; that is very important. That at least one doctor on each hospital ship should be experienced in dysentery is an absolutely important point, though the hospital ships of the present day are magnificently provided in every respect. In respect to another point of Dr. Dunn's, one medical man rather *objected* to absolute rest in dysentery; he says that it allows accumulations in the bowel, and so he wished his dysentery patients to move about, and he is a man of considerable experience. So there is a difference of opinion on these points.

Both Dr. Dunn and Dr. Murray Leslie have emphasized the use of liquid paraffin, and I am grateful for that. Hot applications to the abdomen are also important.

I thank Professor Simpson for the information he has given me on historical matters, and I agree that emetine should be used with care. I did not know about the work of Mr. Eccles. I thank the President also for his remarks. I do not think I need detain the meeting any longer, though I have much more which I could say.

## The Royal Society of Medicine.

President—Dr. FREDERICK TAYLOR.

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(*March 8, 1916.*)

### AN OCCASIONAL LECTURE.

**Personal Experiences on the Gallipoli Peninsula and in the Eastern Mediterranean while a Member of the War Office Committee for Epidemic Diseases and Sanitation.<sup>1</sup>**

By LEONARD S. DUDGEON, F.R.C.P.

EARLY in July of last year I left England to act as a member of the Advisory Committee to the Mediterranean Expeditionary Force on Epidemic Diseases and Sanitation. We were attached to Surgeon-General Babbie, V.C., Principal Director of Medical Services for the Mediterranean Expeditionary Force. Our Committee consisted of Colonel William Hunter, A.M.S., and Lieutenant-Colonels G. S. Buchanan, Andrew Balfour, of the Royal Army Medical Corps, and myself.

The work commenced on our arrival in Egypt, so I think it will be necessary to refer to certain medical arrangements in Alexandria and Cairo before proceeding to the conditions farther East, because Egypt is the principal base for the Gallipoli Peninsula, although this is not always sufficiently realized. It meant, however, that considerable organization in Egypt was necessary to equip and maintain the

<sup>1</sup> It is necessary to state at the outset that this paper contains only an account of personal experiences for which I alone am responsible, and is not in any way intended to act as a report from the Committee to which I was attached. All such reports were private, and were submitted to Surgeon-General Babbie, V.C., Principal Director of Medical Services for the Mediterranean Expeditionary Force.



necessary hospital accommodation for the sick and wounded who had to be brought from the Peninsula to Egypt on hospital ships—a journey of some forty to forty-eight hours at the best. Troops arriving from England, Australia, and New Zealand were often quartered in Egypt before proceeding to the Gallipoli Peninsula.

#### HOSPITALS IN ALEXANDRIA.

There were a large number of hospitals here, but the chief, with accommodation for 1,000 beds or more, were No. 21 General, No. 17 General, No. 15 General, and No. 19 (the German Deaconesses' Hospital, well situated, and a most excellent building), and the two Indian Hospitals at San Stefano. At Glymenopoulos, a tent hospital, for surgical cases especially, was extremely well arranged on the sand along the sea-shore under the direction of Dr. Arthur MacCallan, who also acted as the chief surgeon; this hospital is now transferred to Cairo for the winter months.

My own personal experience was more closely associated with the two hospitals first mentioned, which were constantly visited. No. 21 General Hospital, under Lieutenant-Colonel Robinson, R.A.M.C., would be difficult to improve upon as a hospital. It is situated facing the sea, and is most excellently arranged, while it is here that infectious fevers are concentrated as far as is possible. Lieutenant-Colonel Sir Ronald Ross, while acting as Consulting Physician on Tropical Disease in Egypt, carried out his laboratory work here, together with his assistant, Captain David Thomson. No. 17 General Hospital, in the extreme opposite direction on the sea-front, is under Colonel Healy, and is another model example of how a hospital should be run.

#### *Convalescent Hospital at Montaza.*

This convalescent hospital lay beyond Victoria College on the road to Aboukir. The main building was the Khedival Palace, which belonged to the late Khedive and is situated in most beautiful grounds. It had accommodation for 600 patients, with a view to immediate expansion up to 1,000.

#### *Bacteriological Laboratories.*

Each of the chief hospitals already referred to had its own laboratory attached, with a bacteriologist in charge. It was decided, however, for various reasons to form a large central laboratory in Alexandria to be

attached to No. 21 General Hospital, more especially with a view to the concentration of bacteriological investigations of infectious diseases as far as was practicable. It also permitted a more thorough investigation of infectious and other diseases by experienced bacteriologists and protozoologists. Major Fergusson (Professor of Pathology at the School of Medicine at Cairo) was appointed Director, with a staff of trained men working with him, while Lieutenant John Thomson carried out both routine and research work in protozoology, more especially in relation to amoebic dysentery, and excellent work was being done in the ordinary laboratory for this hospital by Lieutenant Bartlett, of the Pathological Department of the London Hospital. By this means special investigations on the infectious diseases could be carried out as required; further, this laboratory was in close touch with the central laboratory at Cairo under Dr. Charles Todd and with the central laboratory at Mudros under Captain Archibald, of the Royal Army Medical Corps. It is important to mention that all these central laboratories were employing methods of investigation which were as far as possible common to all, so that reliable comparative information could be obtained; and, further, every possible effort was being made to isolate the organism in cases of typhoid and paratyphoid fever and in bacillary dysentery, although serum agglutination tests were employed as additional agents for the differential diagnosis of typhoid and paratyphoid fever. In Cairo, the Public Health Department, under the Director-General Sir David Semple, was giving every possible help, and, as already mentioned, the central laboratory for the military cases in Cairo was situated here under the control of Dr. Charles Todd, Bacteriologist-in-Chief to the Public Health Department of Egypt.

Before concluding these few remarks on Alexandria, I should like to pay tribute to the excellent medical meetings which were organized here owing to the energy and enthusiasm of Lieutenant-Colonel Sir Ronald Ross. I was able to attend two meetings out of the three and can testify to their great success.

#### THE ISLAND OF LEMNOS.

It may be an advantage to refer very briefly to certain points concerning the Island of Lemnos, more especially to Mudros, which acted as a base for the Peninsula, chiefly because of its magnificent harbour, which is some 3 miles in length and  $2\frac{3}{4}$  miles in breadth at the extreme limits. In this harbour certain hospital ships, such as

the *Aquitania*, having been filled with our wounded and sick, returned direct to England. East and West Mudros were connected by motor sardine boats which ran at irregular intervals—a most unpleasant journey in rough weather. All round the harbour the hills sloped down to the water-side over rocky and extremely irregular sandy ground. Across this tract of country the wind at times would blow with terrific force out to sea and occasionally in the reverse direction. The water supply is derived from local wells, while the village of East Mudros has its own supply from the hills. The chief water for our troops, however, was an oversea supply from Alexandria, Port Said, and also from Millwall Docks, brought to shore in barges from water-ships lying in the harbour; this was chlorinated in the water-carts after the final shipment. The largest of these vessels which we inspected was all iron, and its massive tanks served well for the storage of water. I frequently stayed at East Mudros in a tent suitably pitched on the edge of the cliff except when the wind blew forcibly towards the sea. At East Mudros there was a large tent hospital for typhoid and dysenteric cases under Major MacMunn, R.A.M.C. It was really remarkable that such thoroughly sound work could be accomplished under such trying conditions; violent sand-storms, which often lasted three days and nights, would sweep across this portion of the island, and if the sanitary arrangements in the hospital had not been admirably arranged, wide dissemination of diseases must have occurred. It was the practice here to treat the infectious excreta at once with some recognized disinfectant, and then to complete the sterilizing process by boiling in special tanks and barrels constructed at the hospital for the purpose. The cases were brought here from the Peninsula in hospital ships, and cases of typhoid and dysentery arising locally were admitted here.

Different opinions were often expressed as to whether the sand-storms were worse at Mudros than on the Peninsula. To my mind it was merely a question of where you were at the time of the sand-storm; during such a gale sand was included in every article of diet, and it was blown everywhere in dense clouds. It was thought probable that sand-storms would increase the number of cases of diarrhœa and other intestinal infections, partly by giving rise to "sand colic," such as that from which horses are well known to suffer; but although many inquiries were set on foot it is doubtful if any positive evidence of such mechanical irritation was established.

Difficulties in the arrangement of satisfactory latrines were far

greater here than may appear to those unacquainted with this island; but the unsatisfactory character of this rocky ground, the terrible dust-storms, the fly pest, and the necessity for constant supervision of the various labour parties of Greeks, Maltese and Egyptians, added considerably to the already expected troubles of those in command. The improvements which were made here were largely due to the enormous energy and experience of Lieutenant-Colonel Bray, Assistant Director of Medical Services for East Mudros.

Reference should also be made to the fact that troops on their way to and from the Peninsula were quartered both here and at West Mudros. I stayed at East Mudros off and on from August to December at the Stationary Hospital, which is under the care of Major Lewis, R.A.M.C., to whom I am indebted for various kindnesses on many occasions.

*The Laboratory at East Mudros.*

This was situated in a portion of ground between the two stationary hospitals not far from the sea and sheltered as far as was possible from the wind by the rising ground all round. It consisted of wooden huts, and was furnished with the necessary equipments from home. All necessary bacteriological investigations were carried out here under Captain Archibald, R.A.M.C. (although at present attached to the Egyptian Army), and two assistants, together with the usual laboratory personnel. An enormous amount of valuable work was done here on dysentery, fever of the *enterica* group, and all other investigations relating to the diseases present in the various hospitals, as this was one of the central laboratories which were established for the purposes already referred to. It is impossible to over-estimate the value of the work that Captain Archibald accomplished under conditions which were most trying, for it was extremely hot in the summer months at Mudros, so that in these laboratories, covered only by a wooden roof, the heat was terrific; further, the amount of work was unlimited. During the dust-storms, which lasted, in my experience, from twenty-four hours to five days and possibly longer, no bacteriological investigations which required the use of culture media were possible, as the dust penetrated everywhere. Reference will be made to this fact later, since it had a very important bearing on the bacteriological diagnosis of many infectious diseases for which an urgent examination was necessary.



*Nature of Diseases arising Locally.*

Quite apart from the cases conveyed to Mudros from the Peninsula, there were also certain diseases arising locally, such as amœbic dysentery, paratyphoid and typhoid fever, jaundice, and some bacillary dysentery.

Dr. Condonikos, an old inhabitant of Castro, on the Island of Lemnos, informed me that "dysentery" associated with the passing of blood and mucus had occurred in his experience all over this island and the neighbouring islands every year during the summer months; but during 1915 it was especially prevalent among the inhabitants of the various villages.

In the Indian Hospital I saw a case diagnosed clinically as malaria, but found by Captain Archibald to be relapsing fever, as the specimens of blood contained very large numbers of spirochætæ. The patient had returned from the Peninsula, but his was the only proved case of this disease in the Eastern Mediterranean with which I am acquainted. It was thought probable that undulant fever might be a source of trouble among the troops quartered here. In August, while in Castro, a coast town some eighteen miles from Mudros, across the mountains, I learned that there were several cases of anæmia with pyrexia among the local inhabitants, but the samples of blood which I procured from some of them failed to agglutinate the *Micrococcus melitensis*. The villagers here drink goats' milk, and eat butter and cheese made from such milk.

## THE GALLIPOLI PENINSULA.

The portions of the Peninsula occupied by our troops, and known as Cape Helles, Anzac and Suvla, all presented such distinctive features that it was difficult to realize that they were only a short distance apart. In many questions of sanitation entirely different considerations were necessary, while the same remark would even apply to the various portions of the chief areas.

I first visited Cape Helles, and it was this extremity of the Peninsula with which I was best acquainted. A casualty clearing station, situated on the cliff at Cape Helles, in the base area known as Lancashire Landing, was the chief hospital for all cases coming from the various divisions before leaving the Peninsula for Alexandria, Mudros and elsewhere. In November, a stationary hospital was established above and to the left of Lancashire Landing, so that patients

could remain for a certain number of days for complete examination and for treatment, and thus prevent the evacuation of a large number of cases which could be suitably dealt with on the Peninsula. This hospital was mostly tented, but wooden huts were being erected. The field ambulances and dressing stations which did such excellent work on the Peninsula were sometimes buildings of great art, and were formed by deep excavations, which afforded complete protection from the weather, and were constructed by the ambulance men often under difficulties impossible to imagine.

#### *Bacteriological Laboratory.*

At the end of August I carried out some work on the type of dysentery which was then prevalent on the Peninsula. The only laboratory which we possessed had been erected for some time at Cape Helles, but this had not been used. It was built in the wall of the cliff immediately below the 11th Casualty Clearing Station at Lancashire Landing, and was constructed of sandbags, with a series of sandbag steps leading from the sea-shore to the laboratory; it had a sandbag roof and two windows and a door, while the size of the laboratory just permitted two people to work in it at the same time. The outlook might have been good, but unfortunately the front window looked directly on to a "sea latrine." It was, however, the temperature which rendered working in this laboratory so difficult, as the heat was terrific, and in addition a steam sterilizer had to be kept in constant use. Both windows and the only door were closed as far as possible to avoid flies and dust, both so abundant and yet so appalling in view of the type of work which was in progress. Further, it was of extreme importance to prevent flies entering this laboratory, as many samples of fæces which were being investigated contained infectious organisms in exceptional numbers. Early in September Lieutenant Campbell arrived to take charge of the laboratory and carry out whatever routine work was required of him. He, perhaps, will be best known as one of the co-workers with Lorrain Smith in the preparation of the much-discussed antiseptic eusol.

At Sedd-ul-Bahr the French had very good working laboratories in underground rooms in the Turkish village. Daylight did not enter these laboratories, but artificial light was obtained by means of oil lamps. The Director was Dr. Sarrailhé, and his co-workers were Major Clunet and Dr. Richet, the son of the eminent French scientist.

Dr. Armand Delille also worked here, but was subsequently appointed Director of the Laboratories for the Levant Army at Salonica, where I met him in December. They were extremely well equipped with all possible necessities sent from the Pasteur Institute in Paris, including culture media already prepared and immune sera for testing bacterial cultures. In addition, a full supply of antitoxic sera was included. The chief objections to the excellent arrangements referred to were the



The Bacteriological Laboratory at Cape Helles. (From a photograph taken by my colleague, Colonel Andrew Balfour, C.M.G.)

absence of natural light and air, and although there was no sun nor dust to give trouble, rats were over plentiful. Further, the walls were old and made of brick and plaster, and they contained enormous numbers of the *Phlebotomus papatasi*, which promptly attacked visitors, as I know to my cost, for I was bitten on my first visit and developed a form of phlebotomic fever which lasted several days, the only illness from which

I suffered during my six months in the Eastern Mediterranean. After I had been bitten Dr. Sarrailhé showed me several of these flies which escaped from the old plaster and had produced similar attacks among the workers in these laboratories. The "sand-fly" nuisance, however, did not deter me from visiting here on every possible occasion, as so much valuable work was accomplished, and every possible assistance was always offered and freely given. An inter-communication was established between this laboratory at Sedd-ul-Bahr and ours at Cape Helles and at East Mudros.

*General Questions regarding the Water Supplies and Sanitation.*

The duties of the Commission to which I was attached were considerable, but sanitary arrangements in the camps and trenches, the water supplies, and various questions concerning prevalent diseases on the Peninsula required most consideration. The inspection of the sanitary arrangements in the trenches was laborious work as it necessitated many miles of difficult walking, while, as many of the trenches were low, and were only raised to what was considered a satisfactory height by sandbags, the close proximity of the enemy could not be lost sight of, for in some areas the Turkish and our trenches were but ten yards apart. Yet I am convinced that there was almost an equal chance of being killed or wounded while walking on the beach, especially at Anzac and Suvla, or even while bathing. In a small portion of the Anzac area, which I was visiting one afternoon in November, no fewer than fifty odd shells fell during a period well within the hour, and this area was some distance from the firing line.

The water supply for the Peninsula was obtained from overseas in ships, which conveyed it from Alexandria and Port Said; from existing Turkish wells, some of which were good, as, for example, those on the extreme left at Anzac; and from wells made by our troops. The cartage of water, as on the steep slopes at Anzac, was one of the many forms of heavy work which fell to the lot of our men.

*Sanitation.*

While the comparative merits of latrines are possibly a subject of little interest in this country except to those engaged on questions of sanitation, yet it was probably the most important question in relation to the health of the troops on the Gallipoli Peninsula. There is little



doubt that those officers who were careful to observe that the latrines under their control were efficient and were unattractive to flies did the most valuable work in maintaining the health of the troops and in reducing the loss from the fighting force. The latrine arrangements, however, gave rise to enormous labour on the Peninsula, as, owing to the non-porous state of the soil, shallow latrines were quite useless—only deep excavations being satisfactory. The deep trench latrines had a still further advantage in preventing the escape of “butterflies,” and thus the spread of what may be infectious material all over the ground. In the beach area at Anzac special deep bottle-shaped latrines were being made by the British sanitary section under Lieutenant Young which had the advantage of using a large area of the deep soil and much less surface soil. In the trenches and firing line, buckets and biscuit tins were commonly in use, while efforts were being made to erect special fly-proof latrine boxes of wood for the trenches and firing lines, and also elsewhere on the Peninsula. The commonly used disinfectants were so-called “chloride of lime,” a form of kerosene oil, and more recently solution “C.” I noticed in July that a special form of kerosene oil, known as solar oil, was widely used with great success all along the Suez Canal defences by the Indian Medical Service. I was, however, greatly impressed by the disinfectant known as solution “C,” introduced by Dr. Graham Smith, of Cambridge. It kills all flies within the immediate area of the sprayed fluid, while those at a distance affected by the vapour fly in circles with extraordinary rapidity, and finally settle and die. They would not approach material sprinkled with solution “C,” and this fact alone is of considerable importance, for if you can prevent flies approaching infected material much may be hoped for in the way of checking infection. Decomposing organic matter, whatever the nature and however malodorous, was rendered “possible” if solution “C” was freely sprayed over it. The solution mixes with fresh but not with sea water, but the advantage probably rests with the pure solution, especially in the prevention of putrefaction, as it is obvious that the addition of water to solution “C” must counteract the chief effect of this preparation when employed for such purposes. The sanitary arrangements in some of the trenches were remarkably efficient, considering the enormous difficulties with which many officers had to contend. While some of the greatest efforts to maintain the health of the troops were often made under conditions most antagonistic to progress, yet an explanation was readily obtained when one came in contact with certain Assistant

Directors of Medical Services and their co-workers on the Gallipoli Peninsula.

A Medical Society was formed at Anzac for the purpose of discussing questions which concerned prevalent diseases on the Peninsula, and on Sunday afternoon, November 7, the first meeting was held, at which between seventy and eighty medical officers from the various divisions of Anzac attended. The first and only President was Colonel Howse, V.C., of the Australian Army Medical Corps, Deputy Director of Medical Services for Anzac. Colonel Howse was an old student of the London Hospital—a most striking personality, and one of the most delightful men with whom I have been associated. The first discussion was on lice and diseases conveyed by them, and was opened by Major Butler, D.S.O. (an old Cantab), of the Australian Army Medical Corps, and I had the privilege of being the first visitor to take part in a discussion unique in this area of the globe.

*Diseases occurring on the Peninsula.*

The prevalent diseases undoubtedly were diarrhœa, dysentery, paratyphoid fever and camp jaundice. It will be understood, therefore, that the diseases which were common to Cape Helles, Anzac, Suvla and to the French at Sedd-ul-Bahr were of intestinal origin. What are the important causes of such widespread infection by the intestinal tract which require consideration? They are: (1) Innumerable flies, (2) dust-storms, (3) contaminated water. The physical resistance of the majority of the men, however, was lowered, owing to the cramped space, great heat, excessive hardships, and inability to secure complete relaxation, even when at rest, and this gave the infective agents every possible advantage. The diseased condition of the teeth and gums in many cases no doubt also played an important part in these intestinal diseases.

*Diarrhœa* was certainly extremely common; in fact, it was a rare exception to meet either an officer or a private who had spent any time on the Peninsula and had failed to contract an attack of diarrhœa, while it was quite common to learn that it had commenced the actual day of landing on the Gallipoli Peninsula. On the other hand, it was common experience to observe a rapid improvement among those suffering from this complaint almost directly they were comfortably in bed on a hospital ship.

*Camp Jaundice.*—One of the most interesting conditions met with in the Eastern Mediterranean was so-called camp jaundice. Jaundice apparently first appeared at the Chatby cavalry camp, which was pitched on the sand at Alexandria. I saw many cases here in July and August with Lieutenant-Colonel Roger Bullock of the Warwickshire Yeomanry. The majority of patients had had previous sickness and diarrhœa, others constipation, a feeling of slackness and ordinary malaise, associated with slight pyrexia, and followed subsequently by jaundice, often slight, but sometimes intense. The liver was often enlarged and in some cases the spleen. Now many of these men rapidly improved to a certain point under ordinary treatment, but were unable to perform their full military duties owing to general weakness. Mention might here be made of the fact that during a discussion on jaundice, in Alexandria, organized by Lieutenant-Colonel Sir Ronald Ross, Lieutenant Gunson, of the 19th General Hospital, stated that in his experience enlargement of the right side of the heart was common among the jaundiced soldiers admitted to this hospital from Chatby Camp, while Colonel Willcox has drawn attention to this point in a paper on camp jaundice recently published in the *Lancet*. Previous experience has shown that jaundice may appear among soldiers camped on ground which had been occupied by horses, but so far as the health of the animals was concerned in the ætiology of jaundice, Colonel Bullock informed me that there had been very little sickness among the horses at Chatby. It may be of interest to refer here to some remarks made by Professor Kartulis on the infectious jaundice of Alexandria at the meeting previously referred to. This disease, which was unknown before 1870, has a high death-rate and may terminate fatally between the tenth and sixteenth days, and is associated with enlargement of the liver, severe jaundice, a hæmorrhagic tendency and nephritis, and is probably transmitted by mosquitoes. Therefore it may be said to differ in most respects from the jaundice at Chatby Camp. Jaundice commenced on the Peninsula on August 10, among the sappers of a company of French engineers at Sedd-ul-Bahr, and soon became a widespread disease. It may be of interest to learn that malingering was first suspected, and picric acid was searched for in the urine of the first cases, but without results. However, jaundice soon began to appear as a widespread disease amongst the French Expeditionary Force and reached its maximum in October. The disease started amongst the British troops at Cape Helles some fourteen days after it had been first noted among the French at Sedd-ul-Bahr—that

is to say, on that portion of the Peninsula occupied by us which was in direct contact with the French—while from here it extended to Anzac and Suvla. It also occurred on the Islands of Imbros and Lemnos, attacking men who had never visited the Peninsula. Dr. Sarrailhé and Major Clunet, at the French Headquarters at Sedd-ul-Bahr, informed me that there was no case of jaundice amongst the Turkish troops, which, if correct, is a most remarkable fact, considering that the conditions must have been similar for the Turks and the Allies, and that the opposing forces were in very close contact. I saw some of the French soldiers suffering from jaundice and large numbers of the men of our own forces on the Peninsula. In the vast majority of cases it commenced and was associated with a gastro-intestinal disturbance with pyrexia, generally slight, but sometimes more marked, enlargement of the liver and sometimes also of the spleen. The jaundice varied from the palest lemon colour to a deep orange, but the slighter grades of pigmentation were more common. The French observers considered that many of the cases were also associated with some degree of cholecystitis. When I was in Anzac and Cape Helles, in November, it was quite common to find men doing full active work with evidence of jaundice still present. The French bacteriologists at Sedd-ul-Bahr in November sent me a private communication on the interesting results they had obtained by means of blood cultures in cases of jaundice, and these have now been published in full.<sup>1</sup> From June 10 to December 20, 606 blood cultures were made; of these 294 were positive. However, the most interesting facts are the presence of a particular strain of an organism isolated by them on 112 occasions from the blood-stream and the close relationship between the first appearance of the jaundice and the isolation of this bacillus. This organism is closely allied to the paratyphoid group, but differs in certain details, more especially as it does not agglutinate with true paratyphoid sera although reacting, often strongly, with the sera of the jaundiced patients. This organism has been entitled by them provisionally as species "D," or *paradardanellensis*, pending its full investigation at the Pasteur Institute in Paris under more favourable conditions, and cultures have also been handed to me for investigation.

<sup>1</sup> "La Jaunisse des Camps et L'Épidémie de Paratyphoïde des Dardanelles," par MM. A. Sarrailhé et J. Clunet; extrait des *Bull. et Mém. de la Soc. méd. des Hôp. de Paris* (Séance du janvier 21, 1916) and *Lancet*, March 25, 1916.



*Typhoid and Paratyphoid Fever.*—The diagnosis of cases of pyrexia on the Gallipoli Peninsula was in most instances only provisional and made with a view to direct the attention of others to the most probable infection. It is necessary to recollect that such a diagnosis was often hurried, had to be made under most unfavourable circumstances, and generally rested only on clinical evidence. There can be no doubt that it is unwise to accept a diagnosis of typhoid or paratyphoid fever among the military cases unless it is confirmed by the isolation of the bacillus, or, if this is impossible, by accurate agglutination reactions. There was, however, no laboratory at Anzac or Suvla, so that suspected cases of enterica were investigated at the base hospitals. Bacteriological investigations were made at Cape Helles from September onwards, but for reasons which need not be given in detail the diagnosis of suspected cases of typhoid and paratyphoid fever even here could not be a routine practice; if dust-storms were in force, for instance, such work was impossible. Bacteriological investigations by means of blood cultures, examination of fæces and urine, and testing blood sera, at the three central laboratories at Alexandria, Mudros and Cairo, showed that infection with the paratyphoid group was much more common than with true typhoid fever, while similar results were obtained by the French observers at Sedd-ul-Bahr. Further, it was noted that there were a great many cases of paratyphoid "A" infection, while among cases occurring locally in Cairo, Dr. Todd found that the total number of "A" cases was almost as great as those of "B."

*Dysentery.*—In August I had an exceptional opportunity for the study of dysenteric cases direct from the trenches. Lieutenant-Colonel Humphrey, R.A.M.C., Officer in Command of the 11th Casualty Clearing Station at Cape Helles, most kindly placed at my disposal full hospital accommodation, and the pathological investigations were carried out in the laboratory situated on the side of the cliff below the hospital. The cases were brought from the various divisions of the Helles area, and those with a definite history of dysentery were retained for examination; a complete history was taken, more especially as regards the following details: length of illness, area at which they were stationed, whether they had previously been in Egypt, whether they had had a previous attack, and whether they had received injections of emetine. The intestinal contents of each man, passed into a suitable receptacle in the hospital, were then examined by the naked eye, and samples were taken for microscopical investigations in the laboratory. Large

numbers of cases were examined by this means, but only fifty were reported on in which every detail was fully recorded, and the results may be tabulated as follows:—

Total cases reported upon	...	...	...	50
Total cases in which amœbæ were found to be present on one examination	...	...	...	30
Total cases in which blood, mucus, or blood and mucus were the chief constituent of the fæces	...	...	...	34
(And of these amœbæ were present in no fewer than	...	...	...	29)
Previously stationed in Egypt	...	...	...	22
(And of these amœbæ were found in	...	...	...	11)
History of previous attack	...	...	...	12

The fæces in the typical cases (thirty-four out of fifty) consisted of small deposits of thick tenacious greenish or yellow mucus, often blood-stained, but a large quantity of blood was not met with. Amœbæ were often abundant in this mucus, and in such instances the polynuclear leucocytes and epithelial cells were apparently unaltered, while bacteria were extremely few in the film preparations. The amœbæ generally showed these characters: active motility, prominent processes, usually a clear ectoplasm, while ingestion of red cells was quite a constant feature. This work was undertaken spontaneously, so that special staining methods were out of the question, although weak thionin helped to distinguish the amœbæ from the cellular contents of the mucus. In September, a similar method of investigation was carried out at Suvla, but cases of diarrhœa were especially selected, while true dysenteric cases which were met with were excluded. A similar plan of clinical examination was adopted as at Helles, but in addition the condition of the teeth and gums was recorded, and also whether there was any evidence of jaundice. Here also fifty cases were recorded with the necessary details:—

Number of cases examined	...	...	...	50
Number of cases in which amœbæ were found	...	...	...	19
Number of cases in which mucus, blood, or blood and mucus were present in the fæces, but usually in very small quantity	...	...	...	25
Number of cases in which amœbæ were present in the fæces, together with mucus or blood	...	...	...	19
Number of cases in which amœbæ were present in large numbers in the particles of mucus	...	...	...	13
Number of cases with a previous history of diarrhœa	...	...	...	27
(And of these amœbæ were found in	...	...	...	8)

The investigation of the cases of diarrhoea was much more difficult owing to the abundance of bacteria, and it was found that such samples of fæces which contained amœbæ, if left for an hour or so at the then temperature of the Peninsula, were quite useless for examination as the amœbæ could not be recognized from the degenerated epithelial cells. This fact was important, as obviously the longer the time the examination of such samples of fæces was postponed the less possibility of finding amœbæ. The characters of these amœbæ were much more difficult to observe apart from active motility, while the epithelial cells and leucocytes were always more or less degenerated, owing to the heavy bacterial infection. The nature of the bacteria associated with these amœbæ was impossible to determine owing to the lack of apparatus and to the absence of a laboratory, but in a few cases coarse atypical vibrios were found similar to those which Captain Archibald had met with in several samples of diarrhœic stools at Mudros.

These investigations show how common were amœbæ in cases of dysentery and diarrhoea on the Peninsula. Numerous examinations, conducted at odd intervals and not included in these series, confirmed the evidence of the frequency with which amœbæ occurred. At Mudros, I saw a Captain of the Royal Army Medical Corps who had never visited the Peninsula, who was suffering from abdominal pain accompanied by the passage of small quantities of blood, although he was carrying on his full day's work. A sample of blood passed *per rectum* was examined and was found to contain masses of actively motile amœbæ full of red blood corpuscles. The advantage of the results which are recorded here is that they refer to cases examined on the Peninsula, patients who were in the first few days of the acute stages of the disease. Captain Archibald, at Mudros, and the various workers in Alexandria all agreed as to the prevalence of amœbic dysentery and the comparatively few cases of bacillary dysentery. Sir Ronald Ross, in his opening address at Alexandria on the treatment of acute dysentery, makes the following statement in reference to amœbic dysentery: "We know that a very large proportion of all cases among these British soldiers at present in hospital are due to this disease." There was evidence, however, towards the end of September or October that cases of bacillary dysentery, which had been insignificant in number, were becoming more common, although some of these were bacillary infections of the dysenteric group on amœbic cases, as shown by various investigations, including the result of post-mortem examination. With regard to treatment by emetine no one who saw many cases of amœbic

dysentery in the Eastern Mediterranean could question the remarkable efficiency of this drug, of which full details are given in the report of the discussion opened by Sir Ronald Ross at Alexandria, while I had personal experience of the efficiency of emetine in cases of amœbic dysentery among men and officers while on the Peninsula. I think it is important to remember that on several occasions at post-mortem examinations in Egypt on surgical cases amœbic ulceration of the colon in various stages of healing was observed, although there was no previous history of dysentery. It is from cases of this kind that a spread of infection is most likely to occur, and since my return to this country I have seen such a case in a wounded officer, whose fæces contained large numbers of pathogenic amœbæ, but no question of amœbic dysentery had ever been considered in the nursing home.

There is a form of dysentery apparently very rare, which I met with in Alexandria in an officer from Gallipoli. He passed several times a day some clear mucus in small quantity and suffered from tenesmus, but otherwise was in perfect health, while examination of this mucus showed masses of spirochætæ, but no other parasites. After four days' energetic treatment with sulphate of magnesium the mucous discharge completely stopped and did not return.

#### *Other Diseases met with.*

The important diseases which were of common occurrence on the Peninsula have been referred to; certain others that may be of equal importance, but less common, were met with. Scurvy occurred chiefly among the Indian troops, some of whom suffered from a severe form, associated with subperiosteal hæmorrhages, of which I saw several examples in the Indian Hospital at Alexandria. Malaria was by no means uncommon—in fact, the French had a large number of cases during the summer months, at the time when “phlebotomus fever” was especially common. (Edema of the legs, with and without albuminuria, was met with among those who had been some time in the trenches, and true beriberi<sup>1</sup> was recorded in Mudros. Although lice were not unknown on the Peninsula, yet the important diseases conveyed by them were luckily only met with on some two occasions.

<sup>1</sup> See paper by Lieutenant-Colonel W. H. Willcox, R.A.M.C.: “Beriberi, with special reference to Prophylaxis and Treatment,” *Lancet*, March 11, 1916.



## SALONICA—SERBIA.

In November we arrived at Salonica, where our work consisted chiefly in advising on matters for the future as regards water supplies, sanitation, and hospital arrangements. From here we journeyed into Serbia and towards the Bulgarian frontier. The snow blizzards and cold were very severe, more especially as we camped out in tents, generally placed on high, open ground; but no details can be given of the work here for obvious reasons.

In conclusion, I cannot sufficiently express the pleasure it has been to me to be associated in my work with the members of this Committee on Epidemic Diseases and Sanitation attached to the staff of Surgeon-General Babbie, V.C., a man for whom I have the most profound respect.

# The Royal Society of Medicine.

President—Dr. FREDERICK TAYLOR.

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(March 10, 1916.)

OCCASIONAL LECTURE.

## Bacteriological and Experimental Researches on Gas Gangrene.

By Dr. M. WEINBERG.<sup>1</sup>

AMONG the graver complications of wounds are tetanus and gas gangrene. Tetanus was extremely frequent in the beginning of the War, but since the practice of injecting antitetanic serum immediately after wounds have been received was established, fewer and fewer victims have succumbed to this infection. In the case of gas gangrene, results unfortunately have not been so satisfactory.

The number of cases of gas gangrene observed since the beginning of the War, especially after the Battle of the Marne, was considerable. The official statistics are not published yet, and it is not possible to give exact figures. One can, however, judge of the frequency of this terrible complication by the number of observations published; thus Ombredanne has treated during three months, in his hospital at Verdun, 112 grave cases of gas gangrene. It would be no exaggeration to state that after the Battle of the Marne there could hardly have been a military hospital where one or more cases of gas gangrene were not observed. The considerable progress that has been made since that time in the immediate treatment and prompt evacuation of the wounded from the front to the base hospitals appears to have diminished the number of cases of gas gangrene. Nevertheless one cannot think that these

<sup>1</sup> Of the Pasteur Institute, Paris.

measures will be sufficient entirely to prevent this complication, and this because the special conditions of modern warfare make it impossible. That is why we continue to observe cases of gas gangrene after each offensive.

It is very difficult, in spite of the considerable number of observations already published, to make a general survey of the subject of gas gangrene. This question presents still many obscure points, upon which the clinician and bacteriologist are not yet agreed. However, assisted by the results already published and by my own personal observations, I shall try to give as complete a study as possible of the ætiology of gas gangrene and also of its pathology.

Although gas gangrene has been known for a very long time, and although we find the description of it in all classical treatises, the majority of surgeons had a very confused notion at the beginning of the War of this terrible complication of wounds. It is easy, therefore, to understand the reason why this disease has provoked so many discussions in the surgical societies both in England and in France. Surgeons did not at all agree upon the clinical picture of gas gangrene. They were apt to make this diagnosis every time they observed in a bad wound gas infiltration that had a tendency to extend rapidly.

I do not attempt to describe gas gangrene from the clinical point of view. My object is simply to bring before you the results of the bacteriological researches. However, to make myself clear it is necessary first of all to give you a rough picture of that disease.

We can distinguish two principal forms of gas gangrene: the first I would call the classical form, because its description corresponds with that which is found in surgical literature; and the second the toxic form, hitherto not sufficiently differentiated.

Let me first describe a case of classic gas gangrene of the leg. Private B., wounded at the front, was brought to the British Military Hospital at Versailles twenty-four hours after he was wounded. His foot and the lower two-thirds of the leg were dark in colour, and cold. The wound was partly in the gangrenous, and partly in the healthy portion of the leg. The interior of the wound was dark red in colour and the discharge had a putrefactive odour. The leg and the thigh were swollen as far as the junction of the middle and upper two-thirds. The superficial veins were distended, and the skin was discoloured and bronzed. On palpation one could feel crepitation around the wound. The general condition was fair, but the temperature reached 102.5° F. The patient was fully conscious. In a very few hours the crepitation

extended all over the leg and thigh. At the same time four or five large blebs appeared on the thigh containing dark red fluid, and the temperature rose to 104° F. It was decided to amputate immediately at the middle of the thigh. Unfortunately, in spite of this radical treatment, gas extended over the whole trunk up to the neck, and the soldier died the same night. In the two hours before his death this patient suffered from dyspnœa, but he was fully conscious up to the very end. This is a feature which is always present in gas gangrene, and which is extremely distressing to those treating the case. The case shows that the principal symptom in this form of gas gangrene is that of gas development occurring with extraordinary rapidity. Rapid gas development is usually, but not necessarily, an extremely bad symptom; sometimes the very extensive and rapid gas infiltration is produced by microbes of slight pathogenicity which remain in the wound and show no tendency to become generalized. In those cases surgical treatment is successful.

Gangrene of the limb is not a necessary feature of gas gangrene. Very often the starting point of grave gas infection is a small local gangrenous focus set up in the musculature by the projectile. In such a case the evolution of gas gangrene may be as great as in the case just described, but the surgeon by timely incisions and properly applied treatment can often save the limb. There is another form of gas gangrene in which the predominating symptom is œdema, which may be so considerable as almost to mask the gas infiltration.

The following is an instance of the second or toxic form: Sergeant B. was admitted into a French military hospital forty hours after he was wounded; he had been exposed for twenty-four hours between the French and the German lines, after an attack and counter-attack. His arm was not dressed until twenty-six hours after he was wounded. On admission it was found that the wound was on the anterior surface of the middle third of the forearm. The gas crepitation was slight round the wound, but we were struck by the œdema, which was very marked and extended to the middle of the arm. There was no bronzing, but the skin was pale with prominent veins. It was a solid œdema, and crepitation was felt only close to the wound. Incisions were made on both sides of the arm and the wound washed with hydrogen peroxide, but, in spite of that treatment, the œdema extended to the shoulder and the chest, and the patient died twenty-four hours later, without showing any extensive crepitation. Here again the patient was fully conscious to the end.



Sometimes all one can feel is slight gas infiltration immediately around the wound, even up to the termination of a fatal case. Sometimes the gas infiltration is so deep in the muscles that it is difficult to perceive it on the surface, and it is only discovered when the muscle is incised. In other cases the part of the limb above the wound is increased in volume. No œdema nor gas crepitation can be felt because of the increased tension. The superficial veins are dilated. Pyrexia, apathy, and the signs of general intoxication are, however, marked. This is sometimes the beginning of a fatal case of the toxic form.

Now I should mention that the putrid odour so frequently found in both these forms of gas gangrene is not necessarily a symptom of the case and bears no relation to its gravity. This is explained, as will be shown afterwards, by the fact that the putrid odour is not generally set up by the most virulent gas gangrene microbes, but by other organisms of lower pathogenicity.

I might mention that surgeons have described a few cases of very severe gas infection caused by a bullet passing through the limb, leaving an almost imperceptible wound.

#### BACTERIOLOGY.

Since the beginning of the War there has been a great deal of bacteriological research in order to determine the agent or agents producing gas gangrene. Sometimes when the discharge from a bad case of gas gangrene is examined, it is found that only one organism is present. This microbe is the *Bacillus aerogenes capsulatus* described by Welch. The usual name now for it is *Bacillus perfringens*. These cases, in which *Bacillus perfringens* only is found, are exceptional. In nearly all cases it is associated with other organisms. The second slide (fig. 1) shows a case in which *Bacillus perfringens* is found in the discharge with diplococcus and an anaërobic bacillus of no great pathogenicity, yet producing much gas. You will see that in the same case the discharge from the muscles taken far from the wound after the incision again shows the *Bacillus perfringens*. The diplococcus and the slender bacillus we described as "cœur jaune"<sup>1</sup> (fig. 2, A). But it is more usual to find in the discharge from the muscle only the *Bacillus perfringens* with the diplococcus. This patient died, and

<sup>1</sup> We use this name for the present to include those organisms frequently present in the wounds which produce gas abundantly and are of low pathogenicity. They form in deep agar yellow heart-shaped colonies, and do not belong to the other species here described.

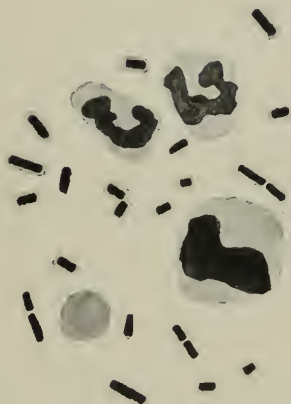


FIG. 1.

Classic form of gas gangrene. The discharge from the wound shows only the *Bacillus perfringens* and leucocytes.

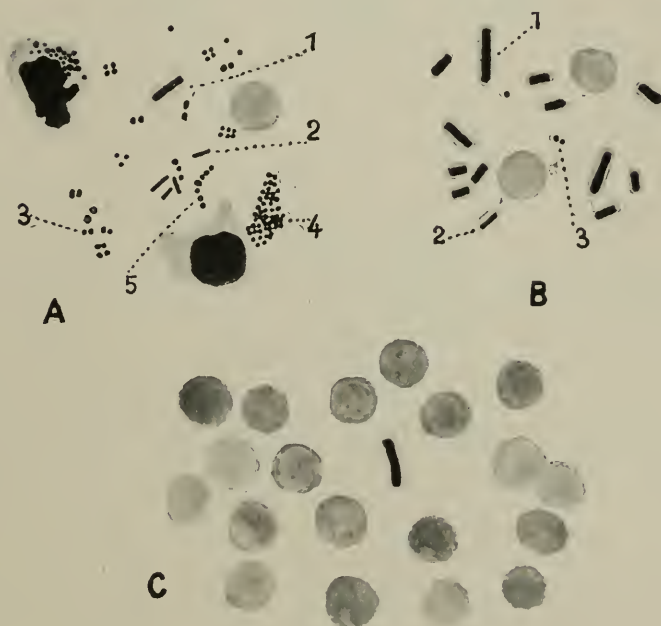


FIG. 2.

Classic form of gas gangrene. A, discharge from wound: 1, *Bacillus perfringens*; 2, "cœur jaune"; 3, diplococci; 4, staphylococci; 5, streptococci. B, discharge from muscle taken far from the wound after incision made by surgeon: 1, *Bacillus perfringens*; 2, "cœur jaune"; 3, diplococcus. C, the blood has shown only the *Bacillus perfringens*.

you see here that in the blood I found only *Bacillus perfringens* (fig. 2, C). There are many cases of gas gangrene in which we find *Bacillus perfringens* associated with many more organisms in the wound. So in the slide before you (fig. 3) you see one of the cases in which *Bacillus perfringens* is found associated with the ordinary agents of suppuration, such as diplococcus, streptococcus, and *Bacillus proteus*, and you will see that *Bacillus sporogenes* is also present. We specially mention *Bacillus sporogenes* because it is often found in wounds, and it is this organism which generally sets up putrefaction in them.

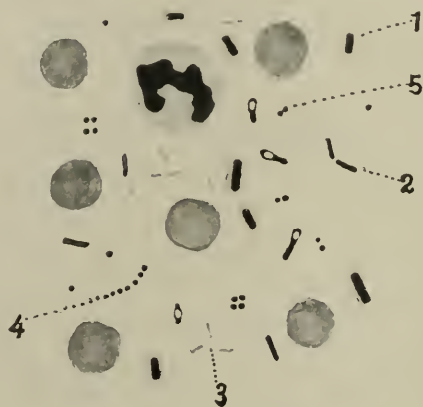


FIG. 3.

Classic form of gas gangrene (putrid variety). Discharge from wound: 1, *Bacillus perfringens*; 2, *Bacillus sporogenes*; 3, *Bacillus proteus* (Gram negative); 4, streptococcus; 5, diplococci.

The first investigations of gas gangrene I made in the British Military Hospital at Versailles showed me that the majority of gas gangrene cases were due to *Bacillus perfringens*. I was very glad to be able to make this statement to Sir Almroth Wright, which he, as well as many other British and French authors, afterwards confirmed. According to my experience up to the present, the *Bacillus perfringens* is found in nearly two-thirds of cases of gas gangrene or gas phlegmon. These results agree with researches made previous to the War and recently summarized by Simonds.

There is another combination of microbes which is found in the classic form of gas gangrene, in which the bacillus of malignant œdema (*Vibrio septique*) is the chief pathogenic organism. Fig. 4 shows

a discharge, the bacterial flora of which demonstrates this point. Here we had a fatal case of gas gangrene terminated by septicæmia caused by the *Vibrion septique*. Fig. 4, A, shows among the diplococci streptococci and a few *Bacillus perfringens*, many slender bacilli, some of which exhibit the form of clostridia. This is the *Vibrion septique*. You see in the same figure a bacillus which is more slender than the *Vibrion septique*—one of the “*cœur jaune*.” In the discharge from the muscle in the same case we found only anaërobic microbes, chiefly the

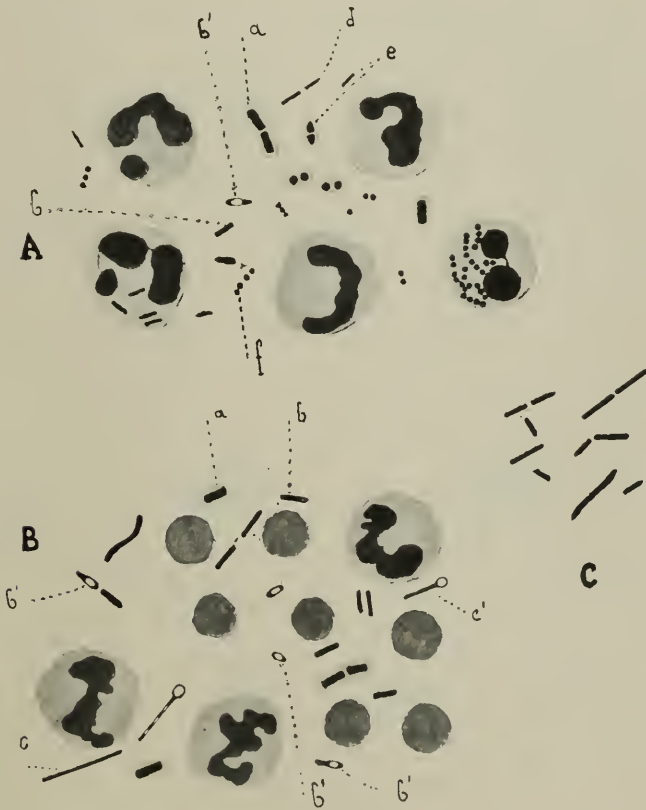


FIG. 4.

Classic form of gas gangrene. Patient died from septicæmia, caused by *Vibrion septique*. A, discharge from the wound: a, *Bacillus perfringens*; b, *Vibrion septique*; b', spore of *Vibrion septique*; d, “*cœur jaune*”; e, diplococci. B, discharge from muscle taken far from the wound: a, *Bacillus perfringens*; b, b', *Vibrion septique* and its spores; c, *Bacillus tetani*. C, culture of *Vibrion septique* obtained by hæmoculture (the blood was inoculated in broth twelve hours before the death of the patient).



*Vibrion septique*, *perfringens*, and a few *Bacillus tetani* (fig. 4, B). The hæmoculture gave a pure culture of the *Vibrion septique* (fig. 4, C).

In 100 cases of gas gangrene and gas phlegmon that I examined I found the *Vibrion septique* only in four cases. So in my experience the *Vibrion septique* is relatively a rare agent in gas gangrene. Many authors, however, mention this organism as being very frequently the

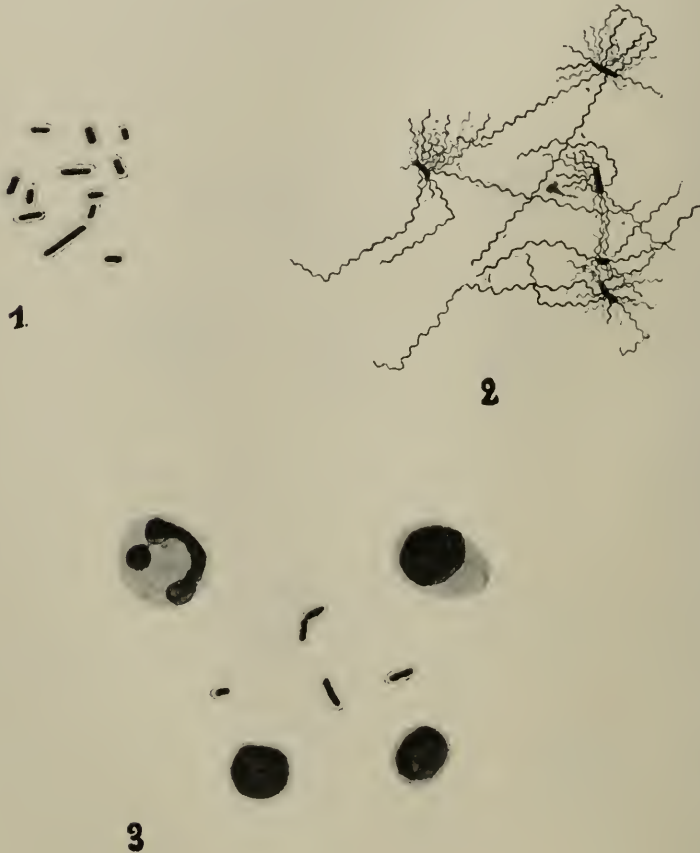


FIG. 5.

*Bacillus fallax*: 1, pure culture on broth; 2, flagella of *Bacillus fallax*;  
3, serosity of muscle of a guinea-pig infected by *Bacillus fallax*.

causal agent of gas gangrene, but as they have not made a complete study of the microbes they call *Vibrion septique*, the evidence for the presence of this organism cannot be considered as conclusive. I believe

that in most cases the organism called *Vibrion septique* was confused with *Bacillus sporogenes*, which has the same morphological characters and, like the *Vibrion septique*, is motile. It is also possible to detect in smears from some cases of gas gangrene a slender Gram-positive

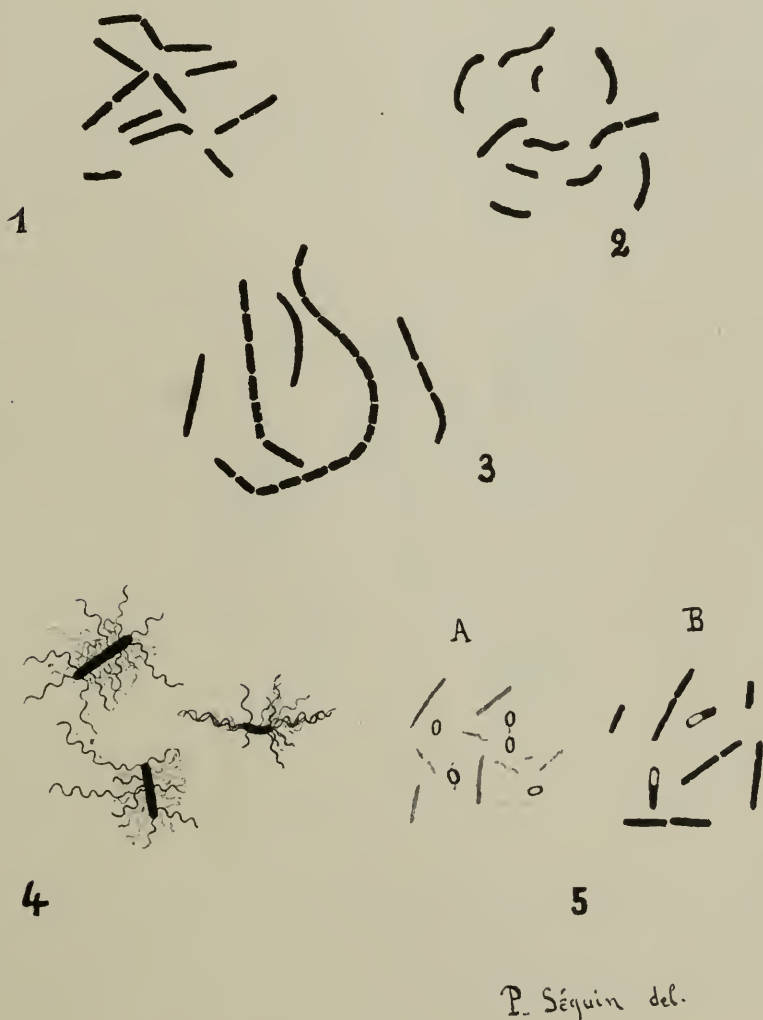


FIG. 6.

Different strains of *Bacillus oedematiens*; 1, strain forming straight elements; 2, strain forming curved elements; 3, strain forming chains; 4, flagella of *Bacillus oedematiens*. A, spores of *Bacillus oedematiens*; B, discharge from muscle of a guinea-pig inoculated with *Bacillus oedematiens*, showing the spore-bearing bacilli.

bacillus (fig. 5), actively motile in hanging-drop preparations, and easily confused with the *Vibrion septique*. We have described this microbe (with M. Séguin) as *Bacillus fallax*, because it has certain characters common to the *Vibrion septique*, *Bacillus perfringens*, and *Bacillus œdematiens*. Its identification involved prolonged and minute investigation (fig. 6). This anaërobic microbe is polymorphic. Its typical characteristic is the formation of a toxin which, if injected into guinea-pigs, produces a special white œdema. But this organism is very rare in the classic form of gas gangrene; some strains found are not very pathogenic. When the wound is infected by a very toxic strain of this microbe the clinical form of gas gangrene is



FIG. 7.

Toxic form of gas gangrene: 1, *Bacillus perfringens*; 2, *Bacillus œdematiens*.

changed, the symptom of œdema being predominant, and we observe the toxic form of the disease instead of the classic form.

In the toxic form the *Bacillus œdematiens* is found, with the same organisms as those generally associated with *Bacillus perfringens* and the *Vibrion septique* in the classic form of gas gangrene. Fig. 7 shows a case in which the *Bacillus œdematiens* is found with *Bacillus perfringens*. In another case (fig. 8) we find the same toxic microbe associated with *Bacillus sporogenes*, and in the third case (fig. 9) the same bacillus is associated not only with *sporogenes* but with diplococcus and a "cœur jaune." This wounded soldier died, and his blood

gave a pure culture of the *Bacillus œdematiens* (fig. 9, B). The *Bacillus œdematiens* causes the toxic form of gas gangrene, but it is not the only agent producing that form. There are other anaërobic microbes which are able to produce the same variety of gas gangrene, for M. Sacquépée has described another bacillus, also toxic and producing œdema, in cases of gas gangrene with clinical symptoms similar to those we have observed in the toxic form of gas gangrene caused by *Bacillus œdematiens*.<sup>1</sup> (In the same toxic form we found the *Bacillus fallax* previously mentioned.) I have also found other œdema-producing microbes which I have not yet had time to identify.

We have only once observed a case of a mixed form of gas gangrene in which the symptoms of œdema and gas production were both very



FIG. 8.

Toxic form of gas gangrene (putrid variety): 1, *Bacillus œdematiens*; 2, *Bacillus sporogenes*; 3, "cœur jaune."

prominent. In that case we found the association of *Bacillus œdematiens* with *Bacillus perfringens*. The patient died from septicæmia caused by *Bacillus œdematiens*. Hæmoculture shows that septicæmia is very frequent in the classic form of gas gangrene but rare in the toxic form, especially if the latter is due to several toxic microbes.

<sup>1</sup> Dr. Sacquépée expressed the opinion that the organism described by him as *Bacille de l'œdème gazeux malin* is the same as *Bacillus œdematiens*. Dr. Roux delegated MM. Veillon and Loiseau to make a comparative study of these two organisms. Dr. Sacquépéc sent them a specimen of the *Bacille de l'œdème gazeux malin* and a culture of *Bacillus œdematiens* was supplied by us. As a result of their investigation these organisms proved to represent two distinct species.



Fig. 10 shows a typical case of a wounded soldier who died of an intoxication due to three toxic microbes, without septicæmia being present.<sup>1</sup> As this figure shows, we found in the discharge from the wound and from the muscles the *Bacillus œdematiens*, the *Vibrion septique* and the *Bacillus fallax*. The soldier died; the blood taken half an hour after death and cultivated under the best conditions was, and remained, sterile.

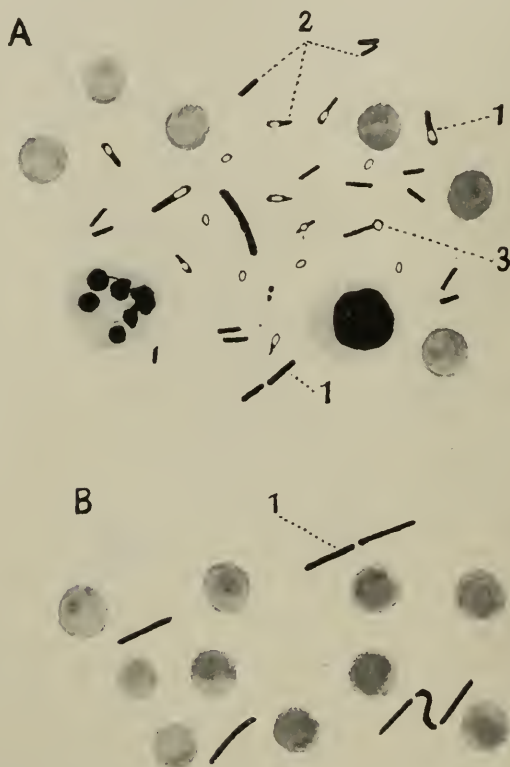


FIG. 9.

Toxic form of gas gangrene. **A**, discharge from wound: 1, *Bacillus œdematiens*; 2, *Bacillus sporogenes*; 3, *Bacillus tetani*; the smears also show diplococci. **B**, *Bacillus œdematiens* alone, from the œdematous fluid.

<sup>1</sup> The word "cénotoxie" has been used by us to describe the condition in which the patient suffers from toxins produced by different organisms, the different toxins in such cases acting simultaneously.

These examples of gas gangrene that I have described clearly show that the bacterial flora of that disease is often extremely complicated; it includes for the most part organisms generally found in intestinal flora. We must add that this flora is often found in wounds which are not complicated by a grave gas infection. I am even convinced that in many cases there are species, perhaps important, which escaped isolation. The difficulty is that certain species do not grow easily in the

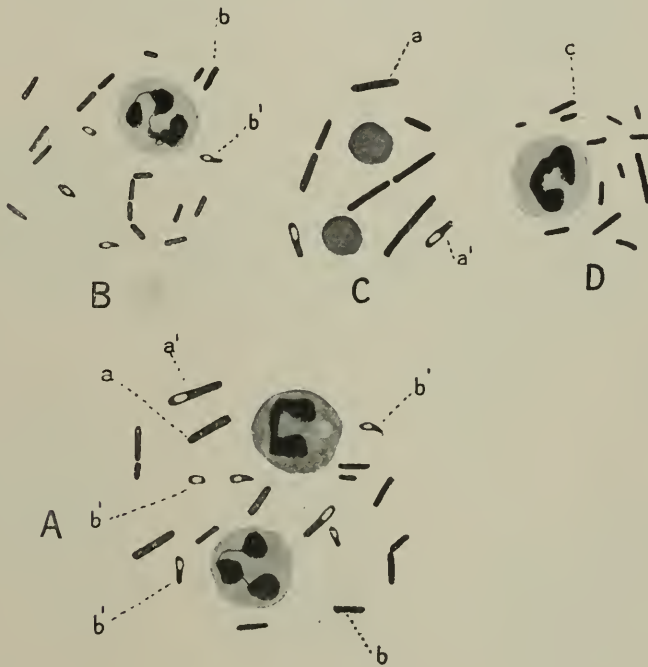


FIG. 10.

Toxic form of gas gangrene. Association of toxic microbes (cénotoxie). **A**, discharge from muscle: *a*, *a'*, *Bacillus œdematiens*; *b*, *b'*, *Vibrio septique*. The figure shows also other slender microbes belonging to *Bacillus fallax*. **B**, discharge from muscle of a guinea-pig inoculated with a pure culture of *Vibrio septique* isolated in that case. **C**, discharge from muscle of a guinea-pig, inoculated with a pure culture of *Bacillus œdematiens* isolated in that case. **D**, discharge from muscle of a guinea-pig inoculated with *Bacillus fallax*, isolated in that case.

media used and that the colonies of different microbes present identical forms in deep agar. Fig. 11, which represents the different aspects of the colonies of *Bacillus œdematiens* in deep agar, demonstrates these

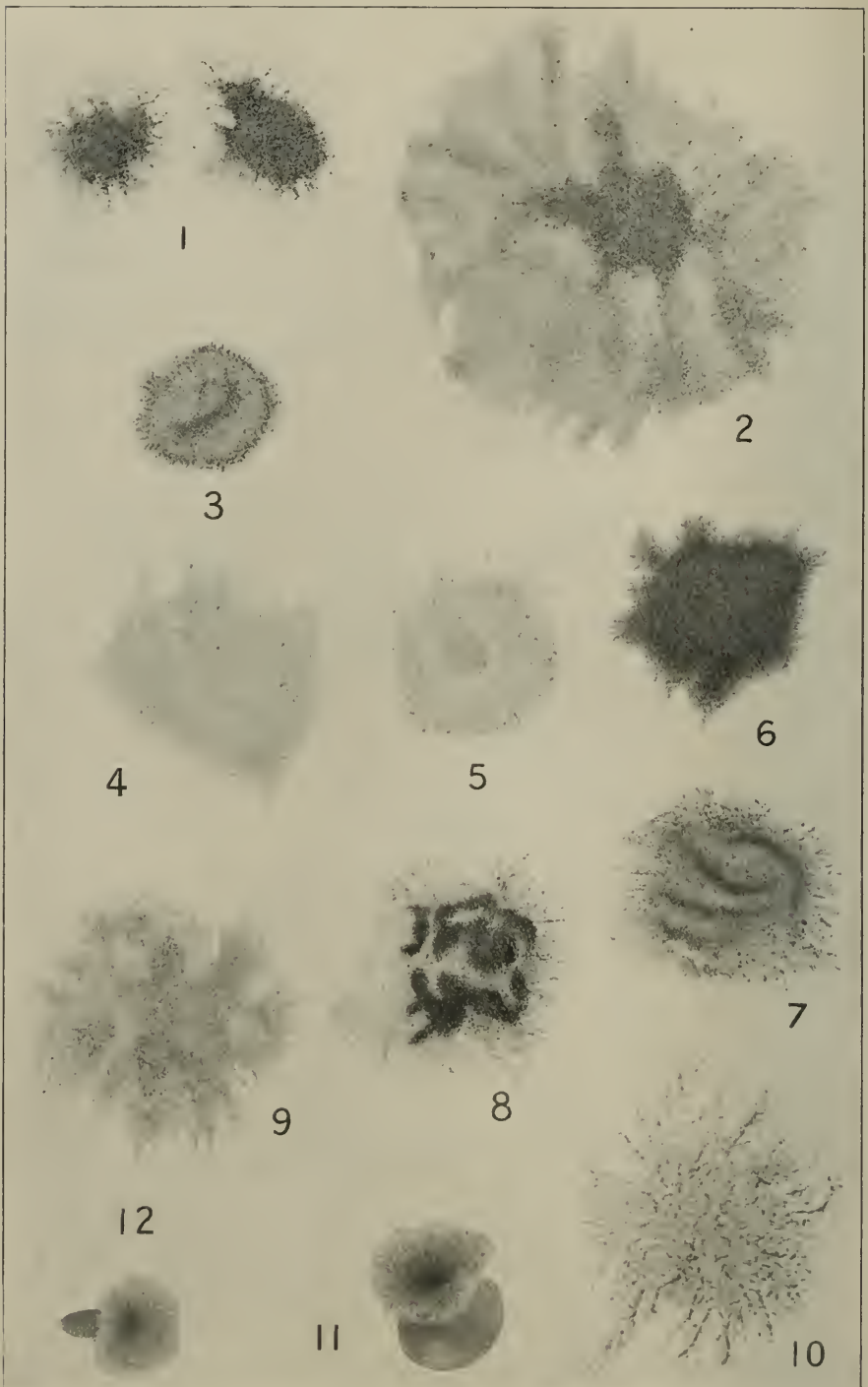


FIG. 11.

Different aspects of the colonies of *Bacillus ademaensis* in deep agar. Colonies 2, 4, and 5 resemble those of *Vibrion septique*; colony 6 resembles that of *Bacillus sporogenes*; colonies 9 and 10 resemble those of *Bacillus tetani*; colonies 11 and 12 are varieties of "cœurs jaunes," which can be formed by many different species (*Vibrion septique*, *Bacillus sporogenes*, &c.).

difficulties. *Bacillus œdematiens* yields many different forms, some of which resemble those of the *Vibrion septique*, others those of *Bacillus sporogenes* and even those of *Bacillus tetani*. It is the same in the case of organisms which form lenticular colonies in solid media. Fig. 12

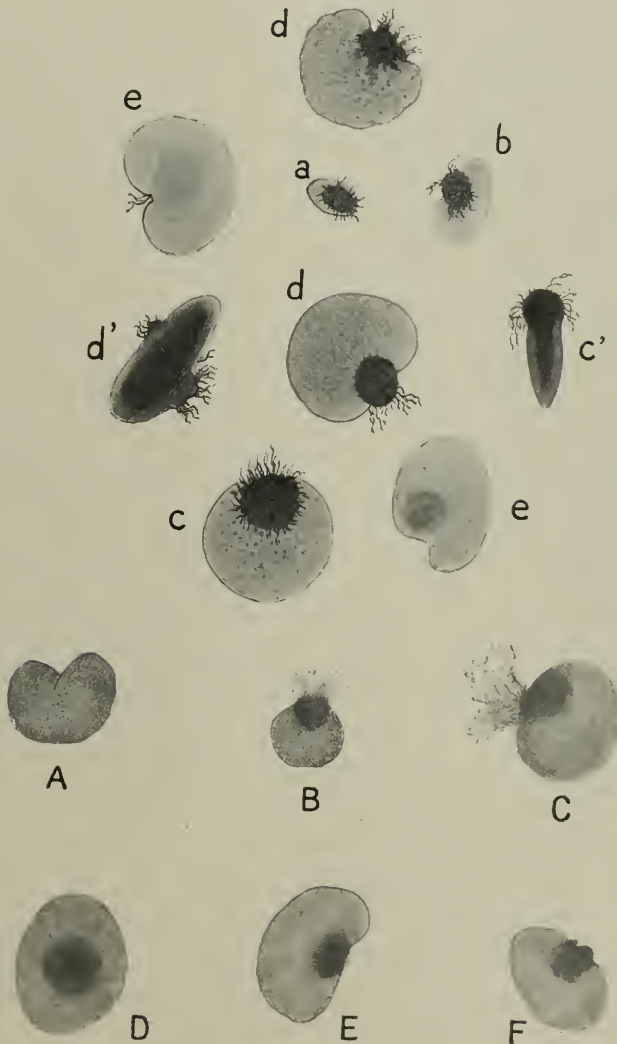


FIG. 12.

Different aspects of "cœurs jaunes." The colonies of the upper part of this figure (a to e) are those of *Vibrion septique* (deep agar). The colonies of the lower part (A to F) are those of *Bacillus fallax*. Other species, such as *Bacillus perfringens*, *Bacille D*, &c., may present the same aspect.



shows the different aspects of colonies formed by the group we call "cœur jaune." They are characteristic; oval or heart-shaped, amber-coloured, finely granulated; in the cleft of the heart can be seen a small excrescence from which grows a tuft of fine entangled filaments. These heart-shaped colonies, like the arborescent colonies, are common to many different organisms: in one case recently investigated in association with Dr. Séguin deep agar tubes contained only heart-shaped colonies identical in appearance; but examination showed that certain colonies were *Bacillus perfringens*, others *Vibrion septique*, and others *Bacillus fallax*. So, if we make a complete study of one case of gas gangrene it is necessary to examine a large number of colonies presenting identical appearances in deep agar. This is the principal reason that the study of nearly all cases of gas gangrene demands prolonged and delayed observation.

#### EXPERIMENTAL PRODUCTION OF GAS GANGRENE.

Now that we have become acquainted to some extent with the flora of gas gangrene, the next step is to determine what organisms play the principal part in the evolution of that disease. It is evident that we can only assign the pathogenic rôle to organisms which produce in animals the symptoms observed in man.

Let us first of all consider the classic form of gas gangrene. It is very easy to establish experimentally that the *Bacillus perfringens* reproduces in the guinea-pig most of the symptoms observed in the wounded. When we inject into the thigh muscle of a guinea-pig one fatal dose of *Bacillus perfringens*, we produce a gas phlegmon which ends in fatal septicæmia. A few hours after injection one can feel crepitation which extends progressively to the abdomen. Sometimes the skin is raised in blebs by a reddish serous discharge; death quickly follows in from eight to twelve hours. At the autopsy, when the gas phlegmon is incised, a great quantity of gas escapes, often with a putrid smell; the muscles are of a dirty grey colour, and we find, sometimes far removed from the gas—for instance in the axilla—œdematous foci of toxic origin without any organisms. The extremely rapid course of the gas gangrene sometimes observed in man and in animals is easily explained by the susceptibility of certain individuals to the toxins of *Bacillus perfringens*. Indeed, when we standardize that toxin, we often kill certain guinea-pigs in a few minutes by intravenous injection of 1 c.c. or 2 c.c. of toxin, although many other of these

animals injected with the same dose show no serious symptoms. We can also produce in the guinea-pig with the *Vibrion septique* the chief symptoms of the classic form of gas gangrene.

There is one point to which I must draw your attention. The *Vibrion septique*, although a gas-forming microbe, develops much less gas in the tissues than does *Bacillus perfringens*. This accords with my observations, because in three cases of gas gangrene with much development of gas the *Vibrion septique* was associated with *Bacillus perfringens*, or with "cœur jaune"—that is to say, organisms which produce a great deal of gas. In the fourth case, in which I found the *Vibrion septique* associated with *Bacillus œdematiens*, clinically it was a toxic form of gas gangrene in which the gas symptom was very slightly marked. It is still easier to produce experimentally in animals the toxic form of gas gangrene with *Bacillus œdematiens*. When we inject into the thigh muscle of guinea-pigs cultures of various strains of that organism, we produce in these animals a toxic œdema which develops progressively up to the abdomen, and masks the gas infiltration more or less completely. The muscles, which are red and hyperæmic, show less gas infiltration the more toxic the strain employed for injections. It is also easy to induce the mixed form of gas gangrene by injecting into the animals a mixture of *Bacillus perfringens* or a strain of "cœur jaune" with the *Bacillus œdematiens*. We may here observe the development of a very extensive œdema with production of much gas. Neither is it difficult to produce a putrid variety of every clinical form of gas gangrene. Injection with the pathogenic microbe, the *Bacillus sporogenes*—that is to say, the organism chiefly found in the putrid form of gas gangrene in man—is enough to set this up. We then observe at the point of injection a gangrenous focus with a most putrid smell and the formation of white blebs. The same bullous form has been observed in man infected by *Bacillus sporogenes*.

The bacteriological study of a case of gas gangrene can only be considered complete when we have succeeded in reproducing in the guinea-pig the same form of the condition. Sometimes this is only possible when the animal is injected, not merely with the most pathogenic organisms found in the lesion but with the associated organisms present in the original condition. This is the best indication of the important effects that may result from the association of different varieties of bacteria in gas gangrene infections.

## ÆTIOLOGY.

The conditions under which we produce gas gangrene in animals experimentally differ from those we find in the wounded. To produce this disease in the guinea-pig we must inject for instance  $\frac{1}{4}$  c.c. of the culture of *Bacillus perfringens* or  $\frac{1}{10}$  c.c. or  $\frac{1}{20}$  c.c. of the *Vibrio septique*. The amount of microbes thus injected is far greater in the majority of cases than the quantity which enters a wound from contamination of clothes or from earth driven in with projectiles. On the other hand, if we examine a wound a very short time after it has been inflicted, we sometimes find only a very few microbes of a pathogenic species. Nevertheless, in spite of early surgical treatment, gas gangrene may occur. The micro-organisms must therefore have found especially favourable conditions for their development.

The dissection of limbs amputated for gas gangrene, together with certain clinical observations and experiments on animals, have enabled us to determine what these conditions are. First, gangrene of a limb is not caused by the microbes of gas gangrene. Here you see a picture of a leg affected by gas gangrene. The gangrene has extended to the greater part of the leg. The dissection of this leg showed that the gangrene is caused by complete obliteration of the popliteal artery. The projectile had severed the artery immediately before its division. Here it is a mechanical agent which has stopped the circulation in the leg. In the other case of gas gangrene which I saw the same day at the British Military Hospital at Versailles the gangrene had extended only to the lower third of the leg and foot, and in this leg the anterior and posterior tibial and peroneal arteries were obliterated by an inflammatory clot caused by non-gas-producing organisms. This gangrene produced by vascular obliteration forms an excellent soil for the development of gas infection. Certain clinical observations made on the wounded in this direction are as conclusive as experiments on animals.

There is a harrowing story of a French major who was wounded at the Battle of Champagne. This officer was wounded in the forearm and in the popliteal space. His wound of the forearm was serious and complicated by a fracture of the ulna. It healed, however, in six weeks. The small wound in the popliteal space was not considered serious. The discharge from the wound contained only diplococci, staphylococci, and only very few *Bacillus perfringens*. To find one *Bacillus perfringens* it was necessary to examine several fields. About two months after he was wounded, an aneurysm of the popliteal artery was noticed, and the

surgeon decided to tie the femoral artery. Two days after the ligature typical gas gangrene developed, and when I was called in a few hours before his death I found the patient in a septicæmic condition. A pure culture of *Bacillus perfringens* was obtained from his blood.

There is another case which developed conditions almost similar to the above, but the gas-producing organisms were fortunately not very pathogenic. A soldier had been treated for three months, for a compound fracture of the tibia by a piece of shell, in the Military Hospital at the Grand Palais in Paris. An arterial aneurysm was noticed in the calf. In the morning the surgeon tied the popliteal artery, and as the result was not satisfactory he ligatured the femoral at midnight. The patient developed an enormous gas infection involving both legs and the trunk as far as the neck. Fortunately for the soldier the gas-producing organism was not very pathogenic, and no organisms were found in the affected tissues away from the wounds. The affected leg was amputated and the soldier recovered, although the gas infiltration persisted for three weeks.

Thus we see that the alteration of the tissues, and especially of the muscle, following vascular obliteration is most favourable for the development of gas-producing organisms. The local destruction of the tissues by the projectile is sufficient to produce a starting-point for gas infection without the presence of gangrene of the limb.

Experimentally it is very easy to show how artificial injury of muscle and hæmorrhage favour the infection in question. Thus, if you lacerate the muscle of a guinea-pig with a needle, or produce a slight hæmorrhage into it, the infection even of a weak strain of *Bacillus perfringens* rapidly causes gas phlegmon. This disintegration of muscles which is so favourable to the development of gas gangrene is not only produced mechanically: we have found it is brought about by certain organisms which are introduced by the projectile into the tissues.

In the discharge from certain wounds of soldiers affected by gas gangrene we have two different species of anaërobic microbes. The one species disintegrates the muscle without producing any smell. The second produces a dissolution of muscle accompanied by a distinctly putrid odour. So, for instance, if we inject 2 c.c. or 3 c.c. of a culture of this second myolytic but non-gas-producing microbe, the next day the muscle is dissolved, a reddish fluid being produced and the bone is bared. If we associate one of these two organisms with a gas-producing microbe which has lost its pathogenicity, the gas infection develops rapidly.



The few facts which I have just set forth show conclusively that the most favourable conditions for the development of a gas infection are the obstruction of large blood-vessels or the local destruction of muscular tissue by mechanical or biological agents.

#### TREATMENT.

Now, how should we treat a case of gas gangrene? We have previously established the fact that the flora of gas gangrene is the same as that of almost every wound, and that a wound may become, sooner or later, sometimes even a very long time afterwards, the starting-point of a grave gaseous infection. It is therefore evident that early energetic treatment of wounds is a preventive of gas gangrene. It is only possible to apply this treatment when the wounded are very quickly removed behind the lines. After the Battle of the Marne the rapid evacuation of the wounded could not be effected; the result was that the number of cases of gas gangrene was so large that there was hardly a military hospital in which there was not a case of this disease. As already stated (p. 119), Dr. Ombredanne treated over one hundred cases in his hospital at Verdun. Now that the evacuation and the first aid treatment of the wounded are considerably improved, we find relatively fewer cases of gas gangrene, even when the number of the wounded is considerable. It is obvious that this marked decrease in the number of cases of gas gangrene is also due to the experience in the treatment of such wounds acquired by surgeons since the beginning of the War.

I have not time to review all the different methods of treatment of wounds practised by surgeons. I will only state the principle upon which all are agreed—namely, that the most essential condition of efficacious treatment is to open the wound immediately as widely as possible, to extract the projectiles and particles of clothing, to cut away all gangrenous tissue, and when a fracture is present to remove all the fragments of bone. This method of treatment is combined with continuous or frequent irrigation with normal saline solution, hypertonic saline solution alone or with carbolic acid, or various antiseptics (zinc chloride, very weak solution of silver nitrate, magnesium chloride, quinine hydrochlorate, &c.). Good results have also been obtained by the application of superheated air and by intravenous injection of salvarsan. Special mention must be made of the use of the Leclainche et Vallée serum and that of different vaccines, which certainly assist the organism in overcoming the infection. The Leclainche et Vallée

polyvalent serum seems to have given particularly good results in wounds, especially those infected by streptococci.

At the beginning of the War we prepared an anti-*Bacillus perfringens* vaccine which yielded us good results in certain cases of subacute gas gangrene and in certain wounds, in the flora of which the *Bacillus perfringens* was the most pathogenic microbe. The same treatment has been tried by Wright and his colleagues, who associated the *Bacillus perfringens* with streptococci. An anaërobic autovaccine has been tried with the aërobic organism of the wound.

In my opinion the best autovaccine is one prepared with all the organisms that are to be found in the wound to be treated, both aërobic and anaërobic. Such a vaccine cannot be prepared by the classic methods—that is to say, by heating the microbe at 55° C. to 60° C.—because the spores resist that temperature. To avoid this difficulty, we treated the mixture of organisms with iodine, a method already employed by Ranque and Senez in the preparation of antityphoid vaccine.

It is extremely important that the vaccine should be administered as quickly as possible. To carry this out we prepare the *autovaccin iodé total* (omnivalent iodized autovaccine) from the wound discharge. This vaccine can be prepared in one to two hours—that is to say, the wounded man can be treated immediately after his admission to the hospital. Several injections of this vaccine are made daily or every two days, and during that time a complete bacteriological investigation of the case can be made, and, if necessary, a new autovaccine may be prepared with pure cultures of the isolated microbes.

It is always necessary to accept with a certain reserve the results obtained by vaccine therapy. Nevertheless we have seen certain cases in which the effect of the *autovaccin iodé total* was indisputable. In these cases the vaccine was only administered when the local and general condition of the patient became worse in spite of large incisions and copious antiseptic treatment.<sup>1</sup>

<sup>1</sup> In many cases we have successfully completed this vaccine therapy by iodizing the wound. After having dried out the wound in a special manner, we fill it with a solution of iodine:—

Iodine ...	...	...	1 gm.
Potassium iodide ...	...	...	2 gm.
Water ...	...	...	1 litre.

Where the wound is perforating, the lower opening is temporarily plugged. This solution is left in contact for fifteen minutes or longer (thirty minutes) if a rapid bacteriological examination has shown the presence of spores. The wound is then gently dried out and filled with dry gauze. By this rational method of treatment the iodine will not only act as an antiseptic, but will transform the microbes with which it comes in contact into a vaccine. So in this way we are treating the patients with iodized vaccine prepared *in vitro* and *in vivo*.

The surgical treatment of gas gangrene at the earliest manifestations is much the same as that of wounds not complicated by gas infection. In the case of gangrene of a limb, amputation is always necessary. Many surgeons, when they consider amputation unnecessary, apply the actual cautery over the whole extent of the invaded area. Unfortunately many cases of gas infection are fatal in spite of all treatment. In order to attempt to save such cases we are trying to prepare an active serum with the three most pathogenic organisms of gas gangrene: *Bacillus perfringens*, the *Vibrio septique*, and *Bacillus œdematiens*.

It would have been very interesting to have prepared antitoxic serum for all these organisms. It is impossible in the case of *Bacillus perfringens*, as its toxin is very slightly active. The serum of the sheep injected with a toxin of *Bacillus perfringens* for three months is able to neutralize a few fatal doses of that microbe. That is why the anti-*Bacillus perfringens* serum that we prepared is antimicrobial. The horse received increasing doses of living *Bacillus perfringens*. Now it is able to withstand a weekly injection of microbes obtained by centrifugalizing one litre of broth.

Although the horses have absorbed such a great quantity of microbes during the year, their serum is not so active as I should have expected it to be; 1 c.c. of this serum neutralizes only 200 fatal doses of *Bacillus perfringens*. Although the activity of that serum is relatively small, yet its use in certain cases of gas gangrene has certainly been beneficial.

I shall only mention fifteen cases of gas gangrene in which the serum was tried, because in these cases the complete bacteriological study was possible. This serum was absolutely inefficacious in five cases of septicæmia due to *Bacillus perfringens*; but in five other grave cases, not complicated with septicæmia, the intravenous injection of this serum was rapidly followed by amelioration of the symptoms both local and general. I was called to the Hôpital St. Michel in Paris to examine a soldier with a compound fracture of the thigh with gas gangrene. The wound had been freely incised the day before, but in spite of that the general condition was worse and crepitation was noticed above the patella, at some distance from the wound. I injected intravenously 22 c.c. of the anti-*Bacillus perfringens* serum. The patient had a rigor for an hour, probably an anaphylactic reaction (he had been injected some time previously with an antitetanic serum). The next day there was an improvement and the crepitation disappeared.

I may mention one other case that I saw at the British Military Hospital at Versailles. It was a patient of Captain Raleigh. He called me in as the case was desperate. The general condition was very bad, but there was no septicæmia. I injected the first time 20 c.c. of serum; the next day the general condition was no worse, and I injected 37 c.c. of serum. After the second injection there was marked improvement, and after four days he was considered out of danger.

There is no time to go into details of the other cases. Suffice it to say that bacteriological study has shown me that the anti-*Bacillus perfringens* serum has had a favourable influence in those cases of gas gangrene in which the *Bacillus perfringens* was the chief pathogenic organism of the flora.

The antitoxic serum of the *Vibrion septique* was prepared by Mlle. Raphaël and M. Frasey by injection into a horse of a toxin prepared with the *Vibrion septique* from the rabbit. We prepared at the same time the same serum with a toxin of the *Vibrion septique*, which caused gas gangrene in a man; 1 c.c. of this serum neutralizes 500 to 1,000 fatal doses of toxin.

The anti-*œdematiens* serum which we first prepared in conjunction with Dr. Séguin in work on sheep is also active. We obtained with *Bacillus œdematiens* a very active toxin which kills a guinea-pig with a dose of  $\frac{1}{200}$  c.c. to  $\frac{1}{400}$  c.c. We hope that the horse we are now immunizing with this active toxin will yield us very good serum, because the first titrations are already satisfactory.<sup>1</sup> We have reason to believe that the antitoxic serum obtained with the toxin of the *Vibrion septique* or *Bacillus œdematiens* will have a greater preventive than curative action. From experiments on animals we find that these toxins become so rapidly attached to the nervous system that it is very difficult to cure an animal even as little as half an hour after one fatal dose of toxin has been injected intravenously.

These results, however disappointing, have not discouraged us from further investigations. The curative value of the antitetanic serum is very unreliable; it is, however, an established fact that its preventive use has saved the lives of many of our soldiers in this War. It is, indeed, the results obtained by the preventive treatment of tetanus

<sup>1</sup> Since delivering this lecture the immunization of this horse with the toxin of *Bacillus œdematiens* has been completed. The serum produced is very satisfactory, inasmuch as  $\frac{1}{40000}$  c.c. of serum neutralizes one lethal dose of the toxin of *Bacillus œdematiens*. This serum was tested with good results in three cases of gas gangrene with toxic symptoms, in which *Bacillus œdematiens* was isolated from the wound or muscle discharge of the patient.



which are an indication of the line we have to take in similar researches.

It is true that the number of the pathogenic organisms of gas gangrene increases in proportion as technique improves and research continues. At the same time the problem grows more complex. This makes it the more interesting, and the solution does not appear to be impossible. First, it is not necessary to prepare sera against all the pathogenic microbes found in gas gangrene. It is sufficient to retain only the species most universal and at the same time most pathogenic. At the present moment these number but three or four: *Bacillus œdematiens*, *Bacillus perfringens*, *Vibron septique*, and perhaps the organism described by M. Sacquépée. The ideal plan would have been to prepare in the same horse an antipolytoxic serum active at the same time against all these organisms. Unfortunately the few experiments made in this direction have shown that it is excessively difficult, if not impossible, to carry out this plan. We can avoid that difficulty by preparing separately sera active against each of these pathogenic species, then mixing the sera so obtained. This will be as easy to manipulate as simple antitetanic serum.

In my opinion the best practical method of dealing with such a case of gas gangrene is to inject immediately in the wound and the neighbouring tissues a mixture of the sera of the three most pathogenic microbes (*Bacillus perfringens*, *Vibron septique*, and *Bacillus œdematiens*). A rapid bacteriological investigation then made by an expert will show which sera must be continued.

I consider that the attention of the bacteriologists should be directed to the subject of these mixed sera if they wish to co-operate actively with surgeons and hygienists in the campaign against gas gangrene.

I have now finished laying before you the results of recent researches on gas gangrene. I hope that the efforts made in this direction will not be in vain and that the interest they have aroused is more than purely theoretical. If they confirm the great importance of intestinal flora, they also show that there exists in the ground, in mud, and in dung a large number of new microbes most pathogenic to man and sometimes as toxic even as *Bacillus tetani*. They enable us to understand the ætiology and the evolution of the different forms of gas gangrene, and let us hope that we are on the way to the discovery of a rational treatment of this terrible complication of wounds.

## BIBLIOGRAPHY.

[The following list comprises articles dealing with the Bacteriology of Gas Gangrene that have appeared during the War.]

- BARGER, C., and DALE, H. H. "Note on a Supposed Soluble Toxin, produced in Artificial Culture by the Bacillus of Malignant Edema," *Brit. Med. Journ.*, 1915, ii, p. 808.
- BINGOLD. "Gazbazillensepsis," *Deutsch. med. Wochenschr.*, 1915, p. 191.
- BOWLBY, Sir ANTHONY A., and ROWLAND, SIDNEY. "A Report on Gas Gangrene," *Brit. Med. Journ.*, 1914, ii, p. 913.
- COSTA, S., et TROISIER, J. "Sur l'association fréquente du pneumocoque et du *Bacillus perfringens* dans les blessures de guerre, notamment dans le syndrome 'gangrène gazeuse,'" *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 283.
- Idem.* "Syndrome mortel d'œdème gazeux dans une blessure de guerre, provoqué par le *Bacille neigeux*," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, pp. 352-354.
- Idem.* "Action hémolytique de certaines bactéries anaérobies des blessures de guerre," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, pp. 354, 355.
- Idem.* "Sur un groupe de bactéries anaérobies des blessures de guerre intermédiaires entre le *Bacillus perfringens* et le *Vibrion septique*," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, pp. 430-433.
- DEAN, H. R., and MOUAT, T. B. "The Bacteria of Gangrenous Wounds," *Brit. Med. Journ.*, 1916, i, p. 77.
- DISTASO, A. "Flora of Wounds and Flora of Putrefaction," *Lancet*, 1916, i, p. 74.
- DOYEN et YAMANOUCHE. "Flore bactérienne des plaies de guerre," *Compt. rend. Soc. de Biol.*, 1914, lxxvii, pp. 503, 512.
- DUDGEON, L. S., GARDNER, A. D., and BAWTREE, E. "On the Bacterial Flora of Wounds produced during the Present War," *Lancet*, 1915, i, pp. 1222-1225.
- EMERY, W. D'ESTE. "Some Factors in the Pathology of Gas Gangrene," *Lancet*, 1916, i, p. 948.
- FLEMING, A. "Some Notes on the Bacteriology of Gas Gangrene," *Lancet*, 1915, ii, pp. 376-378.
- FRASER, JOHN. "The Value of Hypochlorous Acid in the Treatment of Cases of Gas Gangrene," *Brit. Med. Journ.*, 1915, ii, p. 525.
- HATCH, W. K. "Gas Gangrene and Tetanus," *Brit. Med. Journ.*, 1915, i, p. 545.
- LÉVY, FOURCADE, et BOLLACK. "Sur la présence du *Bacillus perfringens* dans les plaies de guerre," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 284.
- ORTICONI, A. "Sur la présence du *Bacillus perfringens* et d'un bacille pyogène dans les plaies gazeuses de chirurgie de guerre," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 126; *Bull. et Mém. Soc. de Chir. de Par.*, February 16, 1915.
- PAGE, C. MAX. "Gangrene in War," *Lancet*, 1914, ii, p. 390.
- PENHALLOW, P. "Latent Gas-bacillus Infection in a Healed Bullet Wound," *Lancet*, 1916, i, p. 866.
- POLICARD et PHELIP. "Les premiers stades de l'évolution des lésions dans les blessures par projectiles de guerre," *Compt. rend. Acad. des Sci.*, 1915, clxi, p. 15.
- RAPHAËL, Mlle. A., et FRASEY, V. "Toxine du *Vibrion septique* et antitoxine correspondante," *Compt. rend. Acad. des Sci.*, 1915, clxi, p. 361.
- REVERCHON et VAUCHER. "Constance et précocité de la présence de *Bacillus perfringens* dans les lésions du gangrène et d'infection gazeuse consécutives aux plaies par projectiles d'artillerie," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 146.
- ROBERTSON, M. "Notes upon Certain Anaërobes isolated from Wounds," *Journ. of Path. and Bact.*, 1916, xx, p. 327.
- ROSENTHAL, G. "A propos du Wright vaccin antiperfringens," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 365.

- SACQUÉPÉE, E. "Le bacille de l'œdème gazeux malin," *Compt. rend. Soc. de Biol.*, 1915, lxxvii, pp. 316, 540, 588.
- Idem.* Études sur la gangrène gazeuse; le bacille de l'œdème gazeux malin," *Ann. de l'Inst. Pasteur*, 1916, xxx, p. 76.
- SARTORY, A., et LASSEUR, PH. "Étude d'un bacille nouveau provenant d'un pus de blessure par obus," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 68.
- SARTORY, A., et SPILLMANN. "Sur la bactériologie de la gangrène gazeuse," *Compt. rend. Acad. des Sci.*, 1915, clx, p. 210.
- SEEFISCH, E. "Die Gasphegmone im Felde," *Deutsch. med. Wochenschr.*, 1915, p. 256.
- SIMONDS, J. P. "Studies in *Bacillus welchii*, with special reference to Classification and to its Relation to Diarrhoea," *Monographs of the Rockefeller Institute for Medical Research*, New York, September 27, 1915, No. 5, 130 pp.
- STEINHARDT-HARDE, E. "Communication préliminaire sur la gangrène gazeuse," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 18.
- Idem.* "Gangrène gazeuse à *Bacillus perfringens*," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 134.
- SWAN, R. H. JOCELYN, JONES, ISAAC, and MCNEE, J. W. "The Occurrence of Acute Emphysematous Gangrene (Malignant Oedema) in Wounds received in the War," *Lancet*, 1914, ii, p. 1161.
- TAYLOR, K. "Factors responsible for Gaseous Gangrene," *Lancet*, 1916, i, p. 123.
- TIDY, H. L. "The Treatment of Wounded by Vaccines," *Lancet*, 1915, ii, p. 326.
- TIETZE und KORBSCHE. "Zum Kapitel der Gasphegmone der Pia Mater," *Deutsch. med. Wochenschr.*, 1915, p. 340.
- WEINBERG, M. "Un cas de gangrène gazeuse à *Vibrio septique*," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 141.
- Idem.* "Premiers essais de vaccinothérapie des infections gazeuses," *Compt. rend. Soc. de Biol.*, 1914, lxxvii, p. 543.
- Idem.* "Recherches sur la gangrène gazeuse," *Compt. rend. Acad. des Sci.*, 1915, clx, p. 325.
- Idem.* "Sur la bactériologie de la gangrène gazeuse (à propos de communications précédentes)," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 286.
- Idem.* "Le *Bacillus œdematians* et le Bacille de l'œdème gazeux malin (Sacquépée) sont deux espèces différentes," *Compt. rend. Soc. de Biol.*, 1916, lxxix, p. 174.
- WEINBERG, M., et SÉGUIN, P. "Notes bactériologiques sur les infections gazeuses," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, pp. 274, 279.
- Idem.* "Le *Bacillus œdematians* et la gangrène gazeuse," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, pp. 507-512.
- Idem.* "Flore microbienne de la gangrène gazeuse; le *Bacillus fallax*," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 686.
- Idem.* "Recherches sur la gangrène gazeuse," *Compt. rend. Acad. des Sci.*, 1915, clxi, p. 744.
- Idem.* "Deux cas de gangrène gazeuse consécutifs à la ligature des gros vaisseaux," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 736.
- Idem.* "Un vibrio septique à aspect atypique en gélose profonde. Fréquence de l'aspect 'cœur jaune' chez divers anaérobies de la flore de la gangrène gazeuse," *Compt. rend. Soc. de Biol.*, 1915, lxxviii, p. 736.
- Idem.* "Formes pseudo-graves d'infections gazeuses," *Compt. rend. Soc. de Biol.*, 1916, lxxix, p. 116.
- Idem.* "Le *Bacillus fallax* et la gangrène gazeuse," *Compt. rend. Soc. de Biol.*, 1916, lxxix, p. 581.
- WRIGHT, Sir A. E. "Memorandum on the Treatment of Infected Wounds by Physiological Methods," *Lancet*, 1916, i, p. 1203.
- Idem.* "Wound Infections: some New Methods for the Study of Various Factors which come into Consideration in their Treatment," *Lancet*, 1915, ii, pp. 821, 957, 1009, 1063.

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(July 30, 1915.)

## OCCASIONAL LECTURE.

### Observations on the Mode of Spread and Prevention of Vesical and Intestinal Bilharziosis in Egypt, with additions to August, 1916.

By ROBERT T. LEIPER, D.Sc., M.B., F.Z.S.<sup>1</sup>

HÆMATURIA has been a common affection of inhabitants of the Nile Delta for centuries, and at the present time occurs in not less than half of the total population of Lower Egypt.

The bleeding is due to erosion of the mucous membrane by innumerable minute hard-shelled eggs. These eggs are laid by filiform trematode worms which inhabit the finer branches of the portal veins. Owing to their peculiar shape and hardness the eggs move through the tissues with every contraction of the organ and eventually reach the lumen. In some cases the bladder wall alone is affected, in others the large intestine is similarly attacked, giving rise to dysenteric symptoms. Treatment can only be local and palliative, for the eggs, which give rise to the symptoms, act simply as foreign bodies and are actually in course of elimination when they produce their evil effects.

The parasites were first found by Bilharz in 1851 and were recognized by him to be digenetic trematodes, which he named *Distoma hæmatobium*. On account of morphological peculiarities, and especially as the worms were bisexual, Cobbold, in 1859, transferred the species to a special genus *Bilharzia*.

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As some mollusc is the intermediate host of every digenetic trematode of which the development is known, it was naturally inferred that a common freshwater mollusc, and probably a gastropod, was responsible for the spread of bilharziosis from man to man. Cobbold made numerous attempts as early as 1870 to elucidate the life-cycle along these lines. Later Sonsino, Lortet and Vailleton, and Looss made similar investigations. All failed to find among the freshwater snails any species which could be shown to convey the bilharzia parasite either under natural conditions or by experimental infection.

These uniformly negative results led Looss to formulate the view that an intermediary host was unessential and that infection in Egypt was direct from man to man, the larval metamorphosis taking place in the human liver instead of in the liver of a mollusc.

Attempts to infect animals direct with freshly hatched embryos proved abortive, but these negative results were explained away on the ground that man was the only known host of the *Bilharzia hæmatobium* and that animals consequently were insusceptible. The latter objection did not apply, however, to the form *Bilharzia japonicum*, which in the far East gives rise to dysenteric symptoms in several of the lower animals in nature as well as man. In this case it was experimentally shown that animals could be infected by exposure to water in the rice fields but not to direct infection with embryos from newly hatched eggs. Later Miyairi announced that he had succeeded in infecting animals from a molluscan intermediary common in the rice fields. These results the author confirmed, while differing as regards the larval development. The upholders of the direct infection theory were, however, unwilling to admit the applicability of such a cycle to the Egyptian species, in view of the failure of successive observers to implicate a freshwater snail in Egypt. Writing in 1914, with reference to the probability that a mollusc acted as intermediary for Asiatic schistosomiasis, Looss says: "If this statement is correct, *Bilharzia japonica* must differ essentially in its development from *Bilharzia hæmatobia*, for it seems *a priori* difficult to understand how an intermediate host that lives in water can participate in the spread of *Bilharzia hæmatobia* in the towns of Egypt."

With the concentration of large numbers of troops in Egypt, in Mesopotamia, and in German East Africa, where endemic hæmaturia is rife, it became of immediate importance to ascertain the exact mode of infection and the steps necessary to safeguard against infection. If Looss's hypothesis were correct then all water in a given area could

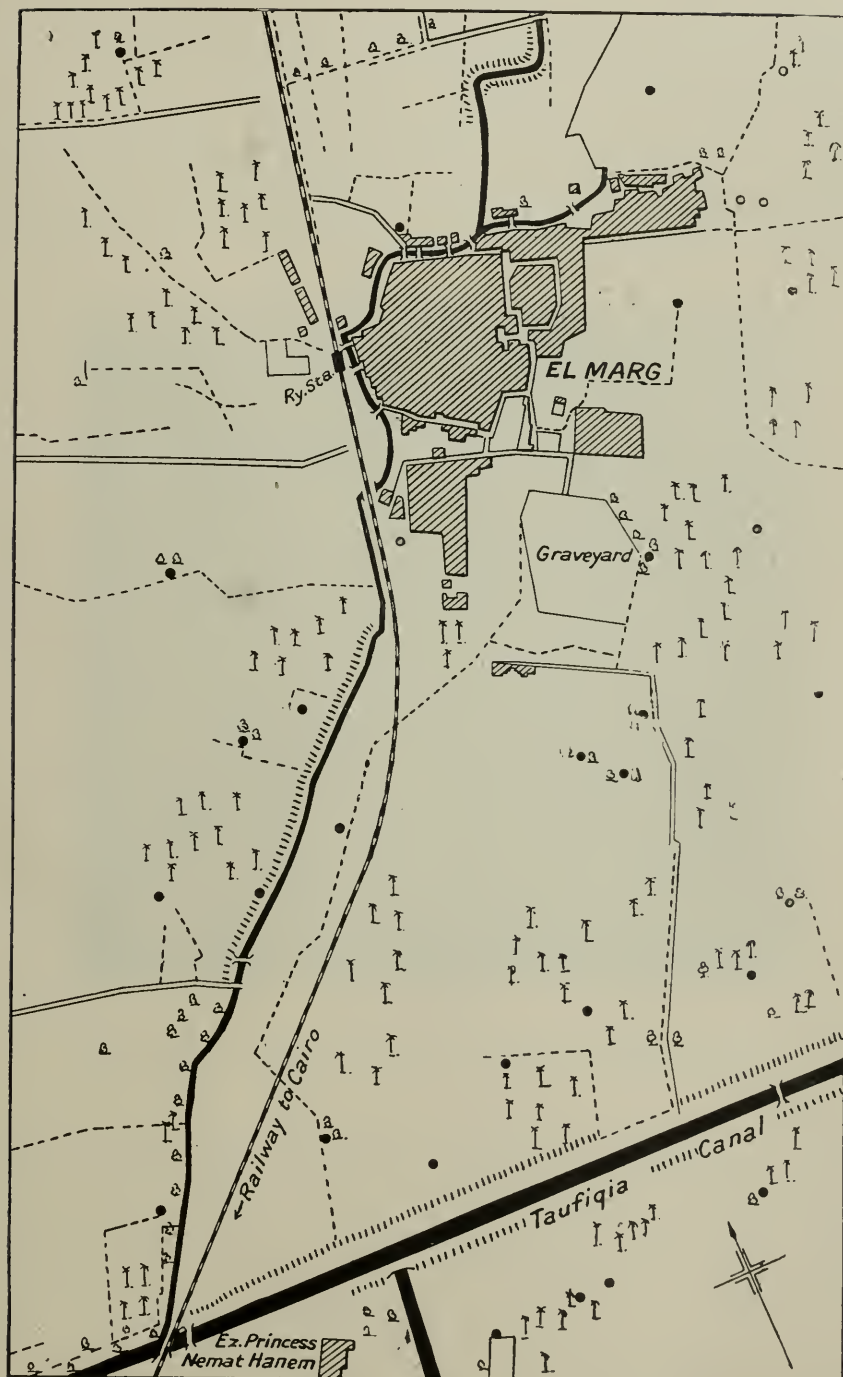


FIG. 1.  
El Marg and the Marg canal.

be rendered quite safe simply by the removal of the native population, for the newly hatched embryo is known to live only twenty-four hours. If on the other hand a molluscan intermediary intervenes then a cleared area might remain highly infective for months.

In view of these facts the author was sent to Egypt by the War Office, in February of 1915, "to investigate bilharzia disease in that country and advise as to the preventive measures to be adopted in connection with the troops." Dr. Cockin and Dr. J. G. Thomson were



FIG. 2.

Marg canal above the village ; children collecting water for domestic use.

appointed to assist in the laborious work of collecting and prosecuting. With the more abundant material thus available it was hoped that attention could be directed earlier to the problems of field prophylaxis. Dr. Cockin was unfortunately invalided home shortly after the work commenced. Dr. Thomson remained until July when he transferred for general service in the Royal Army Medical Corps. From July until October observations were made on experimentally infected

animals and, in conjunction with Dr. H. H. Dale, on the therapeutic effect of certain drugs on the bilharzia worms within the portal system. From November until February the author was again occupied with field investigations, and during March and April with the practical application of the preventive measures recommended.

During the whole period of the inquiry the authorities of the School of Medicine in Cairo placed the most generous laboratory facilities at our disposal.



FIG. 3.

Marg canal entering the village ; women washing garments in Marg canal.

#### MODE OF SPREAD.

As a preliminary to the systematic dissection for trematode larvæ a typical set of all the freshwater shells of Egypt had to be assembled, so that infected shells could be diagnosed by the various collectors before they were broken up in the course of the dissection. A set of these shells is to be seen in the Museum of the London School of



Tropical Medicine and is figured and described in Part IV of the official report in the *Journal of the Royal Army Medical Corps*.

The next step was to fix upon a small endemic area and to collect for examination large numbers of all the shells found therein without predilection. After a study of the country surrounding Cairo, the small agricultural village of El Marg was decided upon as suitable for the purpose. Here no less than 90 per cent. of the children in the native schools were found to have bilharzia eggs in their urine. There was no



FIG 4.

Marg canal running northwards through the village.

village pond with inaccessible bottom to complicate the problem of the origin of the infection. The fields around the village were cultivated by the villagers and were irrigated by a single tertiary canal which traversed the village from south to north and was open to gross contamination during its whole course. This canal formed, too, the source of supply for drinking and ablution for the entire village. In the spring and summer the water was periodically shut off for

several days by the Irrigation Department and the bed of the canal became dry, so that it was possible to examine this minutely in its whole length and ascertain that no species had escaped our notice in the mud at the bottom. Finally El Marg village and canal lay alongside the railway and was served by a half-hourly service of trains from Cairo (fig. 1). It was possible to reach the collecting ground and to return to the laboratory with a large amount of material in two hours.



FIG. 5.

Marg canal entering the cotton fields north of the village where the *Bilharzia hæmatobium* cercaria in *Bulinus* was first found.

Molluscs were examined for infection with trematode larvæ in two ways: (1) By inspection: living snails were sorted out into separate species and each batch was then placed in a tall cylindrical glass filled with water and allowed to stand overnight. In the morning numbers of free-swimming larvæ might be seen, with a hand lens, in the supernatant water. (2) By dissection: larvæ obtained by this method were found to be useless for experimental purposes as they were often

immature. The method followed was simple but tedious. The shell was crushed and then placed on a slide. The whole animal was then teased out by means of dissecting needles improvised by mounting gramophone record pins in universal holders. When trematode larvæ were present they occurred in large numbers and could be detected with the naked eye. These were mounted in lactophenol and set aside for more detailed examination. Where it was important to retain the actual shell of an infected snail for purposes of differential diagnosis a "window" was cut in the shell just above the orifice of the operculum and the contents removed, as shown in fig. 6. By such means several hundreds of all the molluscs in and around the village of El Marg, and elsewhere, were systematically examined. Within a few weeks from the commencement of the inquiry all the trematode larvæ



FIG. 6.

Method of removing the liver from suspected shells.

hitherto recorded for Egypt, save one, were obtained. Moreover others not previously described were found, and amongst these were three cercariæ which presented the peculiar morphological features in the alimentary canal which characterize the bilharzid worms.

In the subjoined tables the various genera of freshwater gastropods found in Egypt are arranged to show their systematic relationships; the main outlines of Lühe's classification of the cercariæ are given; and the various cercariæ found in the course of these investigations are listed in accordance with these groupings. Lühe's classification is based primarily on the position of the suckers and secondly on the character of the tail in the cercaria: the former character being one which persists in the adult while the latter is of purely larval and evanescent value. The bilharzid cercariæ appear under the subdivision "furcocercous," and are there grouped unnaturally with forms like *Cercaria vivax* and *Cercaria fissicauda*, which possess a muscular

GENERA OF EGYPTIAN MOLLUSCA (GASTROPODA).

CLASS GASTROPODA.

EUTHYNEURA	Opisthobranchia	{ Tectibranchia Nudibranchia }	{ (marine forms)			
	Pulmonata	Stylommatophora		Helicidæ	{ HELIX. EREMINA.	
		Basommatophora		Pupidæ	{ LEUCOCHILOIDES. CALAXIS.	
				Succineidæ	—SUCCINEA.	
				Planorbidæ	{ PLANORBIS. BULINUS.	
				Physidæ	—PHYSA.	
				Limnæidæ	—LIMNÆA.	
				Ancylidæ	—ANCYLUS.	
STREPTONEURA	Aspidobranchia	Docoglossa				
		Rhipidoglossa		—Neritidæ	—NERITINA.	
	Pectinibranchia	Tenioglossa—Platypoda—		Paludinidæ	{ VIVIPARA. CLEOPATRA.	
				Ampullaridæ	{ AMPULLARIA. LANISTES.	
				Valvatidæ	—VALVATA.	
				Hydrobiidæ	{ BYTHINIA. HYDROBIA.	
		Stenoglossa	{ Toxiglossa Rhachiglossa	Melaniidæ	—MELANIA.	

LÜHE'S CLASSIFICATION OF CERCARIÆ.

- |                              |   |
|------------------------------|---|
| (A) GASTEROSTOME cercariæ.   | Mouth opening in the middle of the ventral surface. Intestine simple sac-shaped. Two long projections from the end of the body. |
| (B) MONOSTOME cercariæ.      | Ventral sucker lacking.   |
| (C) AMPHISTOME cercariæ.     | Ventral sucker at the posterior end of the body.  |
| (D) LOPHOCERCARIÆ.           | Cercariæ with longitudinal cuticular projections along the sides of the body. Tail forked.                                      |
| (E) DISTOME cercariæ.        | Ventral sucker towards middle of body.  |
| (1) Cystocercous cercariæ.   | Base of the tail forms a space into which the body can be drawn.  |
| (2) Rhopalocercous cercariæ. | Tail having as great or greater width than the body.  |
| (3) Leptocercous cercariæ.   | Tail straight, slender, and narrower than the body.   |
| (a) Gymnocephalous cercariæ. | Anterior end rounded, without stylet or boring spine.   |
| (b) Echinostome cercariæ.    | Anterior end with a collar and crown of thorns.   |
| (c) Xiphidiocercariæ.        | Anterior end with stylet.   |
| (4) Trichocercous cercariæ.  | Tail set with spines.   |
| (5) Cercariæ.                | Tail entirely undeveloped.  |
| (6) Rattenkönigcercariæ.     | Cercariæ with tails joined, forming a sort of colony.   |
| (7) Microcercous cercariæ.   | Tail stumpy.  |
| (8) Furcocercous cercariæ.   | Tail forked at its end.   |



[illegible]

pharynx. The lophocerca are also furcocercous, so there is no doubt that the presence of a "bifid" tail in cercariæ is an indication of convergence merely and not of phyletic affinity.

The table of molluscan genera brings out the further interesting fact that whereas the Asiatic bilharziosis is transmitted by a species of snail belonging to the hydrobid family of the streptoneura, the



FIG. 7.

*Bilharzia cercaria*  
in *Bulinus* spp.



FIG. 8.

*Bulinus dybowski.*



FIG. 9.

*Bulinus innesi.*



FIG. 10.

*Bulinus contortus.*

Egyptian bilharziosis is spread by the planorbiidæ, a family of the other main subdivision of the gastropoda—viz., the euthyneura. This somewhat surprising result shows the wisdom of the policy followed of collecting *all* the species of shells in the selected area without any preconceived notion as to the probable or "likely" host.

## DIFFERENTIATION OF BILHARZID CERCARIÆ.

The bilharzid cercariæ were readily subdivided into two groups. One characterized by the presence of two eye or pigment spots, and of cuticular keels along the bifid portions of the tail, the other without either of these features.

The former type occurred in *Planorbis mareoticus* (fig. 14) and *Melania tuberculata* (fig. 13) in the ponds of the Zoological Gardens at Giza; in *Planorbis boissyi* (fig. 12) in a marsh to the south west of the town of Ismailia and in *Melania tuberculata* in a small stream on the outskirts of the town of Suez. None was ever found in these species of snails from El Marg. During the winter months large numbers of aquatic birds hibernate in Egypt and frequent the waters from which these eyed cercariæ were obtained, moreover, similar eyed cercariæ have been recorded in Europe and North America. On these grounds it was deduced that these forms probably represented the larval stage of an avian bilharzial worm, such as *Bilharziella polonica*, which is common in ducks.

The two other bilharzid cercariæ were very similar in structure. One occurred in various species of *Bulinus*—viz., *Bulinus dybowskii* (fig. 8), *Bulinus innesi* (fig. 9) and *Bulinus contortus* (fig. 10), the other (fig. 11) in *Planorbis boissyi* (fig. 12). Both were found at Marg, the latter more abundantly than the former.

The canal water at Marg was obviously and grossly contaminated daily with excrement of the inhabitants and of their domestic cattle. Man and cattle are the two animals in which bilharzial infection is known to occur in Egypt. It was apparent therefore that the two larvæ probably represented the infective stages of the *Bilharzia hæmatobium* of man, the *Bilharzia bovis* of cattle, or possibly some other common but undetermined species. The exceeding heavy infection of *Planorbis boissyi* at Marg was somewhat puzzling, for this species has a peculiarly restricted distribution in the Nile Delta. The relation of these larvæ to specific adults could only be unequivocally established by experimental infection of susceptible animals.

## EXPERIMENTAL TRANSMISSION.

The next stage in the inquiry—viz., the experimental production of adults from the larval forms presented an initial difficulty. It had been maintained already in explanation of the negative experiments on direct infection, that the human bilharzia in Egypt was peculiar to

man, and that other animals were not susceptible. In view of this objection the author examined the original specimen of *Bilharzia magnum* (now in the Hunterian Museum) found by Cobbold in a monkey



FIG. 11.

*Bilharzia cercaria* in  
*Planorbis boissyi*.



FIG. 12.

*Planorbis boissyi*.



FIG. 13.

*Melania tuberculata*.



FIG. 14.

*Planorbis mareoticus*.

*Cercocæbus fuliginosus* in 1857, and concluded that this parasite was probably identical with the bilharzia of man, as Cobbold had afterwards hinted. There seemed every probability therefore that even if other



animals proved resistant to experimental infection this species of monkey might give a positive result. A number of specimens of *Cercocebus fuliginosus* were taken out from England for experimental purposes. Cattle, sheep, guinea-pigs, rats, mice, and desert rats were also submitted to infection.

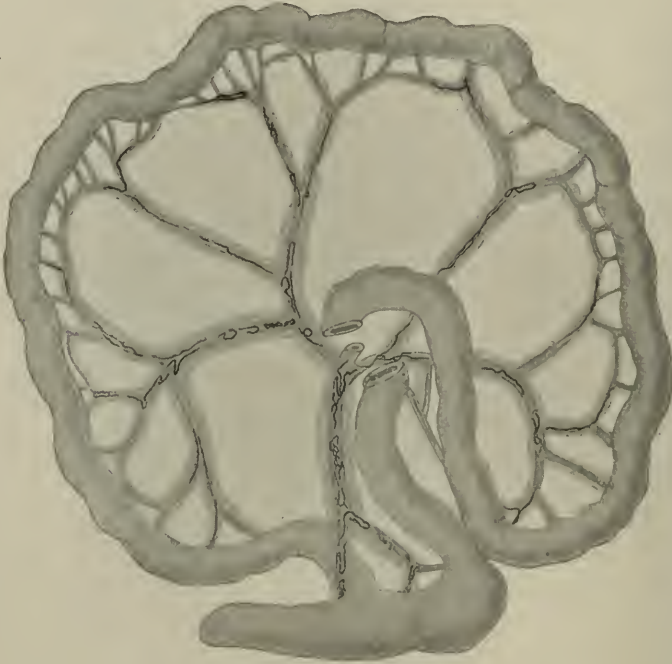


FIG. 15.

Mesentery of mouse experimentally infected with bilharzia



FIG. 16.

Edge of liver of mouse experimentally infected with bilharzia.

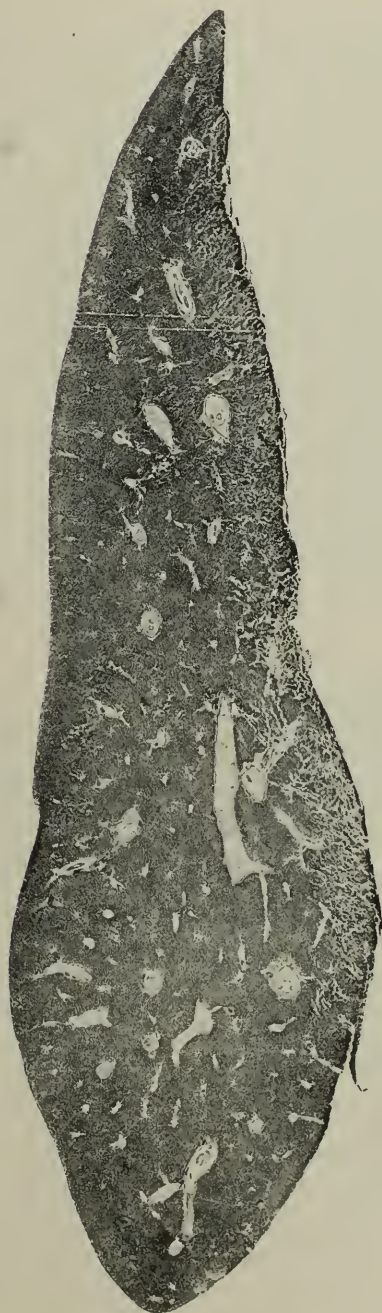


FIG. 17.

Section of liver of white rat experimentally infected with bilharzia,  
showing veins blocked by worms.

A large series of experiments were made both with the cercaria from *Planorbis boissyi* and from *Bulinus dybowskii*. These made on cattle and sheep were negative. Monkeys, guinea-pigs, rats, and mice were positive. Monkeys and mice were successfully infected from *Bulinus* spp., while monkeys, guinea-pigs, white rats, desert rats, and mice succumbed to infection from *Planorbis boissyi*. The eggs produced by the experimentally reared female worms showed conclusively that the parasites were those known to be the cause of bilharziosis in man in Egypt, and as will be seen later incidentally solved the mystery of the relation of the lateral and terminal spined types of eggs. Most of the animals died from hyperinfection, resulting in blockage of the portal veins with parasites (fig. 15), and in some cases among the guinea-pigs in acute necrosis of the liver. The monkeys and some of the rats survived sufficiently long to enable one to diagnose successful infection by the typical clinical symptoms of bilharziosis—viz., the passage of blood and mucus containing typical terminal and lateral spined eggs, the presence of eosinophilia and progressive wasting.

#### MODE OF ENTRY.

The initial experiments were made by placing the animals for about an hour in a shallow dish of water into which large numbers of cercariæ had been naturally discharged by infected shells. The animals were noticed to lick themselves frequently, and often it appeared that the skin was temporarily irritated and congested by the water. No reaction in any sense comparable with that following infection with ankylostome larvæ was observed. Two further series of experiments were devised to ensure in the one case that the possibility of skin infection was eliminated, and in the other that the possibility of oral infection was excluded. In both series heavy, in fact fatal, infection resulted. From this it is concluded that infection can take place both by the mouth and through the skin.

The fact of skin infection was established by immersing newly-born mice partly in heavily infected water. It was observable that the cercariæ swarmed on to the surface of the body, and that within half an hour the bulk had actually disappeared from the water, leaving, however, their tails behind them. These mice were then killed and sectioned in paraffin. The bilharzia cercariæ were found at all stages of entry through the unbroken skin, some of the more striking sections are illustrated by photograph in figs 18, 19, 20.

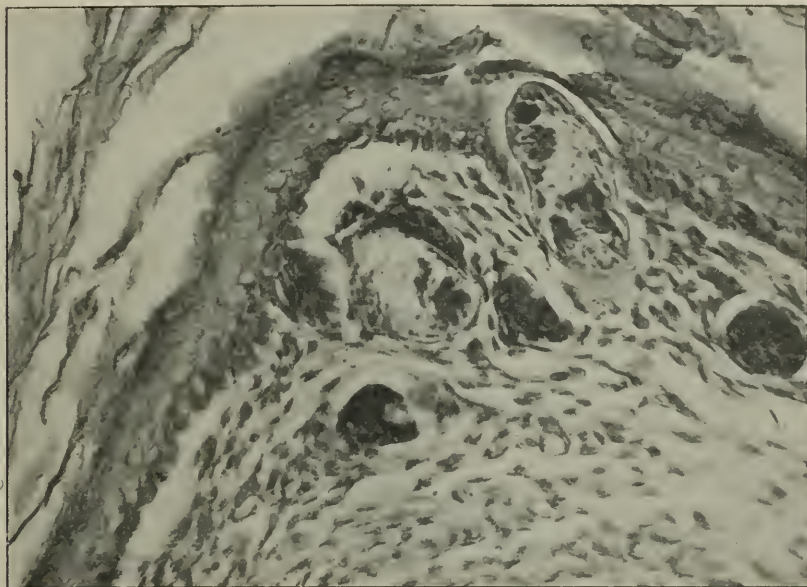


FIG. 18.

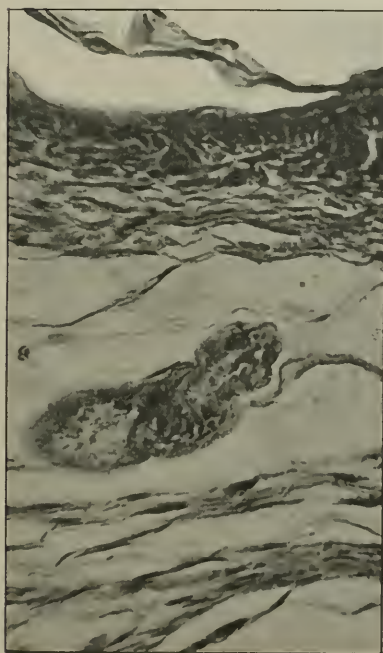


FIG. 19.



FIG. 20.

Figs. 18-20.—Sections of skin of a newly-born mouse which had been immersed for half an hour in water containing large numbers of bilharzia cercariæ.



Oral infection was obtained by pipetting about a tablespoonful of infected water, on one occasion only, on to the tongue of the monkey. In other experiments the monkey was allowed to drink the infected water naturally from a cup. In no case did any of the fluid come in contact with the skin.

Now, by experiment it was ascertained that the cercariæ could not survive momentary exposure to hydrochloric acid in the dilution even of 1 in 500; moreover, the monkey, within two or three minutes, showed by grimaces and reluctance to repeat the tasting of the water that the cercariæ produced a stinging sensation in the mouth. It is inferred, therefore, that when infection results from drinking water the parasites enter by piercing the mucosa of the mouth and œsophagus; any carried into the stomach being killed by the gastric juice.

#### DEVELOPMENT IN INTERMEDIATE HOST.

The development of all digenetic trematodes follows one or other of the four lines of metamorphosis tabulated in fig. 21. The bilharzia worms do not differ in any respect from certain other trematodes.

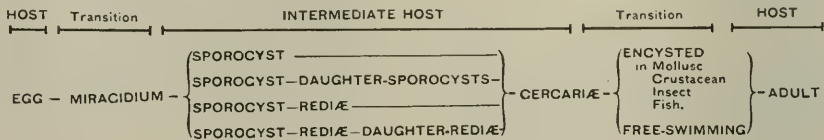


FIG. 21.

Life-cycle of a digenetic trematode.

The newly hatched embryo enters the particular snail and becomes a sporocyst. From this secondary sporocysts develop and increase to such an enormous extent that the whole of the liver of the snail becomes infiltrated with thin-walled tubes (fig. 22). An infected liver can be recognized at once by the naked eye, for the normal greenish or dark brown colour is changed to a light yellow. This change frequently shows through the shell, so that with some experience it is possible to select infected shells at the time of collecting.

Within the sporocysts large numbers of "bifid tailed" cercariæ develop. These are not infective until they are fully grown and naturally discharged from the snail. The injection of large quantities of teased infected liver under the skin results in very slight infection and often in none.

## DEVELOPMENT IN DEFINITIVE HOST.

The cercaria is a young adult provided with a larval appendage. At the moment of infection the tail is discarded and the body burrows into the lymph and blood systems of vessels whence the liver is reached. In the liver the cercarial body undergoes gradual growth and differentiation. The alimentary canal soon begins to extend backwards and in the cercariæ derived from *Planorbis boissyi* the two lateral branches unite very early, in those from *bulinus* the gut



FIG. 22.

*Bilharzia* daughter-sporocysts teased from liver.

branches remain apart until the parasite is approaching adult size, uniting them to form a short cæcum. The sexes are undifferentiated in the cercariæ unless a marked difference in size between the cercariæ discharged from the same snail may indicate that there are male and female producing sporocysts. The sexes are first distinguished by the broadening out of the males in the early stages of their growth in the liver. The females commence to produce eggs in from six to ten weeks after infection.

## OVA AND ADULTS.

As stated above both the terminal spined and lateral spined varieties of bilharzia eggs were obtained as a result of the experimental infections. It had been maintained by Looss that the terminal spined eggs were the normal sexual product of the *Bilharzia hæmatobium*, while the lateral spined eggs were abnormal eggs due to females which had reached puberty but had not been fertilized. He supposed that even after fertilization the "lateral-spine habit" might persist for some time. Manson, on the other hand, had always maintained that the lateral spined egg, having a different geographical distribution and producing normally dysenteric symptoms only, was the product of a different species. This view was strongly supported by Sambon, who maintained with great skill a vigorous polemic against Looss's position. Sambon was forced to regard the adult parasites as practically indistinguishable in the absence of reliable details. Those interested in the subject should read his articles in the *Journal of Tropical Medicine* for 1907 and 1909, and Looss's criticisms in the *Annals of Tropical Medicine* for 1908, and the *Journal of Tropical Medicine* for 1911.

In brief, the experimental results are that those animals infected with cercariæ from *Bulinus dybowski*, &c., always produce adult worms which give rise to terminal spined eggs only, while those infected with cercariæ from *Planorbis boissyi* give rise equally constantly to lateral spined eggs. In no case do both varieties arise from the same intermediate host.

Moreover the adult worms reared from these two sources show constant and readily ascertainable morphological differences. One of the most reliable features is the number of testes, another is the relative length of the cæcum. In *Bilharzia hæmatobium* (*sensu strictiore*)—i.e., reared from *Bulinus* spp., there are four or five large testes and the gut branches do not unite until late in the growth of the worm, resulting in a short cæcum. In *Bilharzia mansoni* (Sambon)—i.e., reared from *Planorbis boissyi*, there are eight or nine testes, and the gut branches unite very early in development of the worm, forming a long cæcum. This difference in the point of union of the lateral guts results in the females in a difference in the position of the ovary and in the length of the uterus. In *Bilharzia hæmatobium* the uterus is long and contains, consequently, many eggs at a time, while in *Bilharzia mansoni* the uterus is short and, consequently, only one or two eggs are seen at one time. The worms also seem to have a different effect on

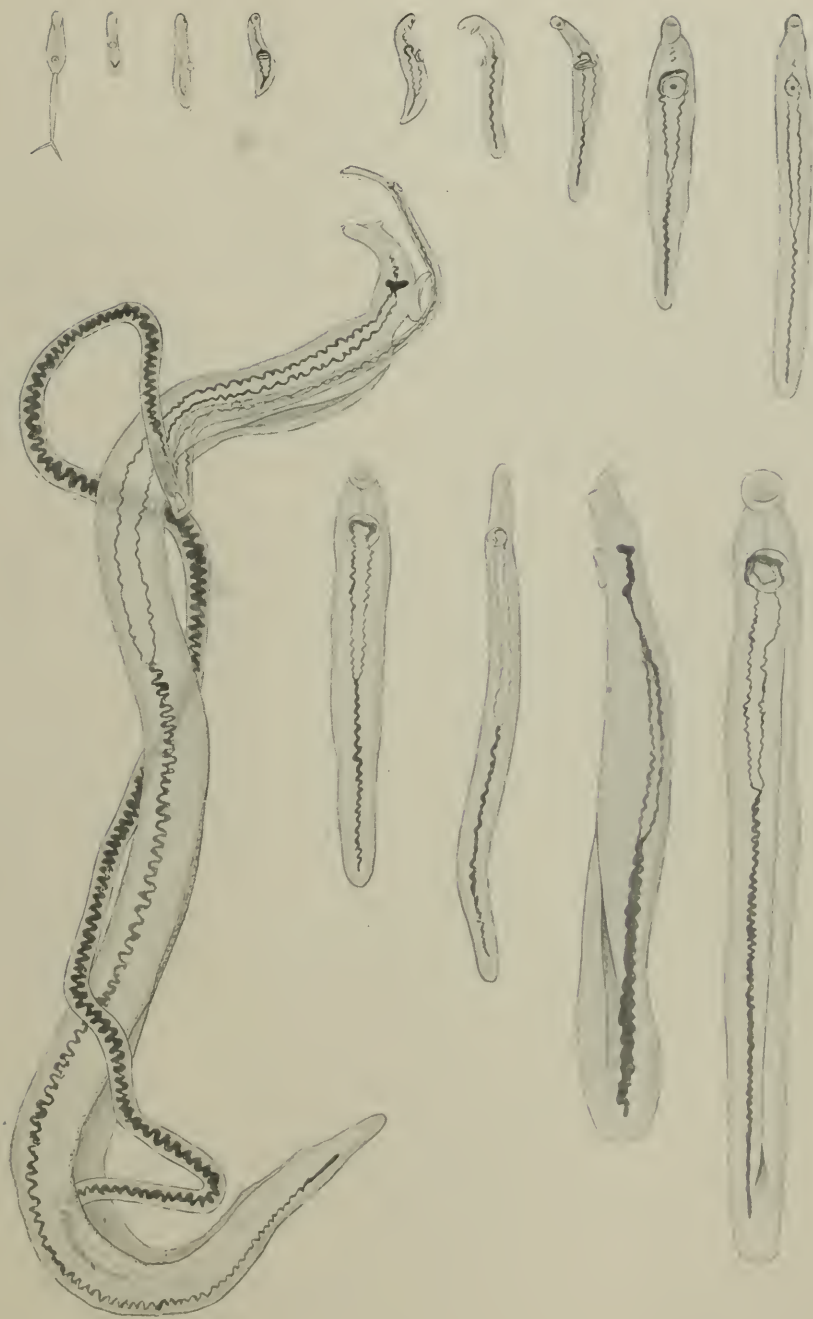


FIG. 23.  
Development of bilharzia within the mammalian body.



the blood of the host, for in *Bilharzia mansoni* infections there is always a heavy deposition of granules of black pigment in the liver. Lastly, as regards habitat. The males of *Bilharzia hæmatobium* appear to leave the liver early and to pass down into the finer branches of the mesenteric vessels before they attain maturity. The females found in the gynæcophoric canal are diminutive. The males of *Bilharzia mansoni* remain in the liver until the females in copulâ begin to lay eggs and large numbers of lateral spined eggs are frequently laid in series by coupled worms in the veins even on the edge of the liver. The eggs found in bilharzial cirrhosis of the liver may not be adventitiously carried thereto by the blood stream from the mesenteric vessels as has hitherto been supposed, but may have been actually laid in the liver by adult worms.

#### PREVENTION.

From the foregoing account it is evident that the contamination of water does not result in rendering it infective unless the special snails are present and a sufficient number of weeks have lapsed to permit of the development cycle in the snails having become complete. Once a snail is infected it appears to be able to discharge cercariæ in large numbers into the water daily for several weeks.

Infected and discharging snails were found throughout the whole year, but only with great difficulty during the winter. In Egypt the rise of temperature at the commencement of April was at once followed by an enormous increase in the number of infected shells obtainable in a short visit to the collecting ground.

Isolated wells in the fields deriving their water from the subsoil are little liable to be infective, as molluscs have not been found in them. All canal water is, however, open to suspicion, for during the life of a free-swimming cercaria the water, into which it was discharged by a sedentary snail, may have travelled many miles.

There are certain points about the bionomics of the bilharzial cercariæ which indicate the lines upon which a rational prophylaxis can be based. The cercaria, although provided with mouth, is unable to nourish and maintain itself indefinitely in water. Within twenty-four hours of being discharged a large proportion die. Practically none survives for more than forty-eight hours.

Storage for forty-eight hours renders suspected water free from danger without further treatment, provided no infected shell has been

admitted to the storage tank. Tanks may be improvised from tarpaulins, sail-cloth, &c.

The cercaria is highly susceptible to certain drugs in weak solution; but most of these cannot be taken in drinking water in the necessary strength. Happily the tablets of acid sulphate of soda used for sterilizing water are efficient cercariacides. This water is useful only for drinking purposes. Chlorine in the strength 1 in 1,000,000, as used for the bacterial sterilization of water, is useless against the bilharzia cercariæ. A strength of 1 per 500,000 of available chlorine is essential and the chlorine must then be removed to render the water palatable for drinking. Boiling of course immediately kills the cercariæ. Where it is practicable to deal with the drinking and ablution water separately the former may be sterilized by the use of the sodium bisulphate tablets and the latter by the addition of lysol, creolin, army cresol, in the strength of 1 in 10,000 for immediate use and 1 in 50,000 after some hours' standing.

The filtration of water through sand filters depends for its efficiency solely on the length of time interposed between the infected snails and the consumer. In the spring of 1916 the author made a series of experiments, at the request of Surgeon-General Sir W. Babbie, to ascertain whether filters on the Jewell system offered a dependable block to the passage of bilharzia cercariæ. This system consists in the addition of alum in a settling tank prior to filtration through sand about a metre in depth, and provides for six possible traps for the bilharzia cercariæ during the passage from the source to the consumer, viz.: (a) in the settling tank—(1) time factor; (2) exposure to oxygen; (3) chemical action of alum; and (4) arrest from entanglement in the flocculent precipitate; (b) in the filter—(5) arrest on the surface by the "vital layer"; and (6) arrest due to depth of sand. A working model was kindly supplied by Mr. McCroquidale, manager of the Cairo Water Works.

The conclusions were:—

(1) The bilharzia cercariæ survive and remain actively swimming for a much longer period than the time (five to eight hours) that the water takes to pass through the settling tank.

(2) Oxygen has a stimulating effect on the cercariæ and is a necessity for their continued activity.

(3) Alum, in the dilutions used for sedimentation of canal water, has no effect on the bilharzia cercariæ.

(4) The bilharzia cercariæ are not entangled in the flocculent alum

precipitate. They are seen swimming freely in the supernatant fluid twelve hours after the addition of the alum.

(5) The "vital" layer formed by the deposition of alum on the surface of the sand and the arrest of bacteria and fungi thereon, does not arrest the bilharzia cercariæ. These were found to pass easily through the layer formed by the passage for half an hour of aluminized water taken from the settling tanks of the Cairo Water Works. The same result followed in another test made by passing freshly forming alum precipitate on to a small area of sand for an hour, thus producing an abnormally thick layer. This, too, offered no obstacle to the leech-like progression of the cercariæ, for they were found actively swimming in the filtrate twenty-four hours later.

(6) Finally, depth of sand presented no insuperable barrier, for very active cercariæ were found in the filtrate of our working model within one hour after their addition to the inflow of aluminized water: a depth of thirty inches of sand having been traversed in this interval. The sand was a sample of that ordinarily used by the Cairo Water Works. Sand of the finest grain used in filtration was similarly tested and proved inefficient.

At Cairo infected snails are probably some miles away from the works, and the *additional* delay after the intake of water from the main stream of the Nile is about twelve hours. At Ismailia under a different system the delay is about twenty-four hours. The more uniform dispersal of the cercariæ in the filtered water has also to be borne in mind. The "time factor" in the life of the bilharzia cercariæ apparently affords a satisfactory explanation of the relative immunity of Europeans in those Egyptian towns where there is both a filtered and raw water supply. It may here be remarked that, in raising the objection that an intermediate host that lives in water could scarcely participate in the spread of bilharziosis in the towns of Egypt, Looss apparently overlooked the fact that even in Cairo raw Nile water is distributed by a separate series of pipes to all parts of the town and that Europeans do not themselves use this supply for domestic purposes.

#### ERADICATION.

It is with some hesitation that one puts forward definite proposals for the eradication of a disease that is being disseminated by almost every other person in the country. There are, however, certain peculiarities in the physical conditions of Egypt which are found nowhere

else, which are inimical to the bilharzia cercariæ and its carrier, and which, if properly exploited, might result within a few years in a great diminution in the severity of the disease.

In Egypt there is practically no rainfall throughout the year ; there is assuredly none during the summer months. Water is absolutely essential for the life of the bilharzia outside the body and for that of its intermediate host. The whole of the water of Egypt is derived from



FIG. 24.

A cul-de-sac in Marg canal, dry during the summer rotations.

the Nile, and its distribution is rigorously controlled by the Irrigation Department of the Government. During the summer months those lands not provided with perennial irrigation fall out of cultivation, and it is a remarkable fact that, in those districts in which this occurs, the incidence of bilharziosis is very low. It is high in those districts in which the land is perennially irrigated. It would appear, therefore, that the introduction and extension of perennial irrigation since the



beginning of the nineteenth century has resulted in a great increase in the amount of bilharziosis in the country. The perennial irrigation channels provide suitable breeding places for the snails necessary for the development of the disease outside the body and enable these to survive from year to year, where formerly they must have been killed off in enormous numbers by the annual drought succeeding the fall of the Nile flood.



FIG. 25.

Marg canal as it enters the village, a few days after the water has been cut off in the "rotations."

In a perennially irrigated district like that in which Marg is situated the water, during the months between April and July, is cut off for a period of three weeks in every month. During this period of stoppage the water gradually disappears from the canal (fig. 24) and large numbers of the *bulinus* and *planorbis* perish, as they are without opercula and cannot withstand prolonged desiccation like operculated forms, such as *vivipara* and *melania*. Owing to the irregularities in the bed of the canals, however, pools of water are left and enormous numbers of the

bilharzia carriers congregate therein and survive until the next period of flow (fig. 25).

When it is recollected that the molluscs are relatively slow breeders it should appear as feasible to eliminate them by some such temporary adjustment of their environment as by other means it has been proved possible to eliminate the carriers of malaria and yellow fever.

#### EXPERIMENTAL TREATMENT.

The symptoms of the disease commence only after the worms have reached maturity and have laid enormous numbers of eggs in the tissues. It is obviously impossible to deal with these eggs, as they act as foreign bodies, and are in course of elimination when they are producing the clinical results. The adult worms live for a considerable period, and it seemed that if they could be killed off shortly after the onset of symptoms, the cessation in egg production might greatly lessen the duration of the disease. Dr. H. H. Dale kindly undertook a series of experiments upon a number of rats which had been experimentally infected with large numbers of bilharzia worms. The drug in each case was pushed until toxic symptoms were evident, and then until the animal died from over-dosage. Dissection shortly after death in every case showed that the bilharzia worms were still alive, although the doses used were relatively much higher than could have been administered to a human being. Among the drugs used were salvarsan, quinine, betanaphthol, and thymol.

#### ACKNOWLEDGMENTS.

During the year occupied by the investigations outlined above, the author was indebted to many quarters for assistance and information. To the authorities of the School of Medicine in Cairo, and particularly to the director, H. P. Keatinge, Esq., C.M.G., who took a keen personal interest in the work, special acknowledgment is due for the generous facilities provided for the laboratory work. The heads of several other Government departments in Egypt afforded invaluable official information and expert opinion on particular aspects of the bilharzia problem: notably Dr. C. Todd, of the Bacteriological Institute; Mr. E. Hurst, of the Physical Science Department; Mr. Lucas, of the Chemical Laboratory; Major Flower and Messrs. Nicoll and Bonhote, of the Zoological Service; Mr. J. I. Craig, of the Statistical Department; Mr. Adamson,

of the First Irrigation Circle; Mr. Branch, secretary of the Sultanieh Society of Agriculture, and Dr. Innes, Librarian at Kasr-el-Aini.

The field investigations would not have been possible without the financial assistance opportunely provided by the Medical Research Committee (Insurance Act). The Wandsworth Trust enabled the observations to be made more complete by a second visit to Egypt in the autumn of 1915.

Throughout the whole period of the inquiry much encouragement was derived from the warm interest taken in the progress of the investigations by Colonel W. H. Horrocks, K.H.S., and Dr. William Fletcher, F.R.S.

Acknowledgment is also gratefully made for the very helpful assistance given in the field work by Dr. J. G. Thomson. A considerable number of the molluscs were collected and prosected by him, and he found a cercaria in one of his dissections which has been shown to be the infective stage of the *Bilharzia mansoni*.

For over fifteen months Pte. W. McDonald acted as laboratory assistant and, when Dr. Cockin was invalided home, he took over part of the prosecting in addition to his other duties. It was mainly due to his persistent work and scientific acumen that the cercaria of the *Bilharzia hæmatobium* was discovered. Recognition is due to him for the sustained application and willing service with which he contributed to the success of the research at every step.

I

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SECTION OF ANÆSTHETICS



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(April 14, 1916.)

### Observations on the Influence of Anæsthetics on the Temperature of the Body.

By M. S. PEMBREY, M.D., and F. E. SHIPWAY, M.D.

IN this preliminary paper we shall not refer in detail to the work of previous observers upon this subject; it is only necessary to remark that, upon the chief points, the results of physiologists and clinicians agree. The influence of anæsthetics on the temperature may be exerted upon the loss of heat, the production of heat, or upon both of these processes. In man the practical methods of observation are the determination of the deep and surface temperatures, and, as a measure of the production of heat, the determination of the respiratory exchange. The former method we have used in man, and both methods have been employed by one of us in the case of animals.

Deep anæsthesia abolishes the regulation of both the loss and production of heat, so that the response of the warm-blooded animal to external heat and cold resembles that seen in cold-blooded animals; a fall of external temperature diminishes, a rise increases the production of heat. It is this fact which complicates the problem. The internal temperature of a patient may show a fall, a rise, or no change according to the conditions involved during the period of anæsthesia. A fall in the rectal temperature from 100° F. to 97° F. is within



## 2 Pembrey and Shipway: *Anæsthetics and Body Temperature*

physiological range, when the whole extent of the daily variation in temperature is considered, but in the case of an anæsthetized patient subjected to an operation in a warm theatre (72° F. to 74° F.) such a fall may occur within one hour.

During short operations of slight severity the necessity for precautions against the loss of heat is not urgent, for in an adult there is, owing to the mass of the body, a reserve of heat which is not rapidly dissipated in a hot theatre. In such cases the advantages of warm ether, as compared with cold ether, may not be so apparent. On the other hand, in the case of long operations or operation upon a patient possessing a low resistance, the difference may be of great practical importance.

The "open method" used is that in which ether is dropped continuously on to some fabric spread over a Schimmelbusch mask, which is so closely applied to the face that the whole of the respiratory current passes through the fabric; when warm ether is given the end of the tube from the apparatus is placed under the mask and ether vapour pumped in at each inspiration. In most cases two layers of washed domette, in the remainder about twelve layers of gauze, have been used, and the mask has rested upon a shaped pad of flannel. With this method the temperature under the mask through which the patient is breathing varies from 48·2° F. to 78·8° F. with cold ether, whereas with warm ether it is about 89·6° F. A patient breathes about 5 litres of air per minute, and the expired air is raised to 96° F. or thereabouts. It is obvious, therefore, that more heat must be lost from the respiratory tract, although the expired air with cold ether may not be warmed to the same extent.

With such an "open method" as the one just described we have never observed a lower temperature in the inspired air under the mask than 48·2° F., the temperature of the air of the theatre being 77° F., and we think that some explanation is necessary for the exceedingly low temperatures recorded by some observers. It is necessary to remember that the air in the mask is being warmed constantly by the skin of the face and every few seconds by the expired air. For example, with a Schimmelbusch mask of two layers of domette and a pad placed over the face, the temperature of the air was 91·4° F. to 93·2° F. five minutes after the mask had been placed over the face, the temperature of the room being 71·6° F. to 73·4° F. The mask and pad were removed from the face and ether dropped upon the domette as

during an administration: the temperature of the air fell to 32° F.; they were immediately placed over the face, and within one minute the temperature of the air rose to 61·7° F.

As regards the temperature of the body, observations of the rectal temperature are necessary, for the determinations in the mouth are unreliable both before and especially after an administration of an anæsthetic. The surface temperature of the skin may rise under the influence of ether owing to dilatation of the cutaneous vessels, and thus increase the loss of heat so much that the internal temperature falls. With warm ether the loss of heat from the skin can be more readily compensated.

The following points are of practical as well as physiological interest in connexion with the influence of anæsthetics upon the temperature. The excitability of the medulla is increased by a rise, diminished by a fall in the internal temperature; this effect upon the respiratory centre is well known, and in a pronounced form is seen in heat polypnœa or dyspnœa. The heart is stimulated by warmth, and the exchange of gases between the blood and the tissues is facilitated by a rise, delayed by a fall in temperature.

A normal man reacts to external cold by diminishing his loss and increasing his production of heat; the anæsthetized man, paralysed for sensation and movement, has lost this control, and the level of the chemical changes, which are a measure of vital activity, can be maintained only by external warmth. This loss of control involves also the possibility of an abnormal rise in the temperature of the patient if he be exposed to excessive heat.

A great practical advantage of the warm ether is that it enables the surgeon to operate in a cooler theatre; the ideal must be to keep the patient warm without exposing the staff to the depressing effect of high temperatures. Apart from diminished efficiency and endurance, a warm and moist atmosphere introduces the danger of the sweat of the surgeon undoing the elaborate precautions taken to preserve aseptic conditions. With an operation table warmed by an electric heater or some other safe method, it should be possible to operate in a theatre of moderate warmth.

The "open method" is not strictly an open one. The mask placed upon a pad over the face confines the air to a considerable extent; this is shown by the determinations of the moisture and the carbon dioxide in the air space. Thus the dry and wet bulb thermometers recorded

#### 4 Pembrey and Shipway : *Anæsthetics and Body Temperature*

88·7° F. and 86° F. under the mask, and waved in the air of the room 72·5° F. and 65·3° F. The breath moistens the air and thus obviates the necessity of adding moisture to the air laden with ether vapour. The amount of carbon dioxide may be 3 or 4 per cent. of the total 250 c.c. of air under the mask ; there is, according to the closeness with which the mask is applied to the face and the thickness of the material, a certain amount of rebreathing which may be an advantage when the stimulating effect of carbon dioxide is required.

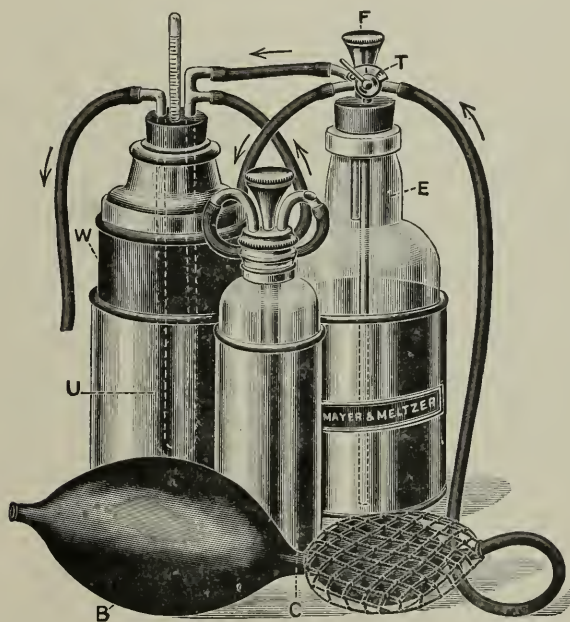
#### ADDITIONAL REMARKS BY DR. SHIPWAY.

One part of the evidence for the use of warm anæsthetic vapours has been given in our joint paper ; the other part is due to some American anæsthetists, who showed by experiments on animals and by clinical observation that anæsthetics heated to blood temperature are increased in value as regards life without their anæsthetic effects being decreased. The cause appears to be a physiological one, in that warmth increases the respiratory activity and stimulates the heart. My own experience has been that with warm vapours anæsthesia can be pushed to the deep degree sometimes required in abdominal surgery with greater safety and with lessened liability for respiration to become dangerously weak, and the centre recovers its activity more quickly, than in the case of anæsthesia with a cold vapour. I have wondered whether the greater safety of chloroform in hot climates can be due entirely to the increased rate of elimination, and is not due, for the most part, to the high temperature at which it is exhibited.

Warm vapours are less irritating and are absorbed more readily than cold, so that induction is quicker and accompanied with less excitement, whilst the anæsthetic effect is increased. Breathing is calmer, and secretion of mucus is considerably less ; this latter point has proved to be of some practical importance when using ether in casualty clearing stations, where there is frequently no time to give atropine. The chief advantage of warm vapours is that there is less shock, due partly to the greater ease and calm of breathing, but mostly to the diminished loss of heat. I have quoted figures in an article published in the *Lancet*,<sup>1</sup> which show that the average loss of heat is at least three times greater with "open ether" than with warm ether, and

<sup>1</sup> *Lancet*, January 8, 1916, p. 70.

eighteen months' experience of this method has taught me that patients take warm vapours better and suffer from less depression and shock, both during and after operation, than when the anæsthetic is delivered at the ordinary temperature. Captain G. Marshall, R.A.M.C., and others find that this method of anæsthesia is advantageous in lessening shock in badly wounded and exsanguinated soldiers. A small loss of heat is of vital importance in these unfavourable cases or in patients with small resistance.



A description of the apparatus is given in the article already mentioned. Briefly, it may be said that it is a regulating apparatus, by means of which the warmed vapours of ether, chloroform, or of mixtures of C.E. in any desired proportion can be given, and in concentration suitable for any operation; the dosage is practically constant, the anæsthetist is more out of the operator's way than when he is using a drop bottle, and administration made more easy in the case of patients lying in awkward positions. The best points of "open ether" are preserved and there is economy of ether, 4 oz. or under being used per



## 6 Pembrey and Shipway: *Anæsthetics and Body Temperature*

hour. I find that the same ether can be employed several times if what remains at the end of the administration is returned to the stock bottle, which is then filled up with fresh ether to its original amount. Tests for impurities,  $N_2$ , peroxides and aldehydes must be made from time to time. Owing to difficulties in manufacture caused by the War, slight alterations have been made in the apparatus which have affected the temperature readings quoted in the *Lancet*.

### DISCUSSION.

Mr. CARTER BRAINE: I do not advocate "open ether"; I still employ the Clover or the Ormsby inhaler, which possess certain advantages in anæsthetizing the powerful adult. I think the pioneers of ether administration in this country endeavoured to employ warm ether by the method of re-breathing, by warming the inhaler or the sponge, and wrapping up the inhaler in a hot towel. I have returned to the Clover inhaler during the last two years, and now that it has become the routine practice to give atropine and morphine, I have found it an excellent machine with which to work. In order to render the administration of a slightly open ether type I have fixed a tap at the lowest part of the bag, by means of which air enters during inspiration, and some of the contents of the bag are expelled during expiration. A certain amount of re-breathing is beneficial in preventing acapnia, and I attribute the very satisfactory results obtained to the patient breathing a warm atmosphere with conservation of his  $CO_2$ .

Mr. G. ROWELL: I have used Dr. Shipway's apparatus with varying temperatures, and have had two pure ether cases in which with a temperature in the flask of  $130^{\circ}$  F. to  $135^{\circ}$  F. slight but definite symptoms of faintness gradually arose—namely, feeble breathing, some pallor and weakened pulse. These appeared to be directly associated with the very warm vapour administered. I prefer many layers of gauze to two layers of domette, which offer much more obstruction to the breathing. I employ the Clover inhaler on occasion; after induction, most of the advantages of open ether can be gained by using this instrument nearly all the time without the bag.

Dr. G. A. H. BARTON: No very convincing evidence, either physiological or clinical, has been produced in favour of the warmed ether method.

Mr. H. BELLAMY GARDNER: I am glad to hear that all that are necessary to maintain the patient's bodily warmth during operations are a warm operating table and dry cloths and blankets, and perhaps the prevention of the fall of

temperature in the vapours inhaled by artificial means. Raising the temperature of the operating theatre I have always found to be a mistake, as not only does it set up early exhaustion in the operators and anæsthetist, but in the patient as well.

The PRESIDENT: The question of loss of heat during anæsthesia is a very vital one, especially since we are told that in the narcotic state man becomes, as it were, a cold-blooded animal, and, for the time being, loses much of his normal power of readjustment of his temperature. The warm ether method is therefore to be preferred to that of the ordinary so-called open ether method where the ether is allowed to evaporate on the mask, and in its evaporation to abstract a certain amount of heat from the patient. Perhaps the discussion has become narrowed down too much into a controversy between the Clover and the so-called open method.

Dr. SHIPWAY (in reply): It is clear from a study of the writings of the pioneer anæsthetists that they warmed the ether vapour in order to aid the administrator and not in order to help the patient; the work on this subject is quite recent. I cannot understand the cases mentioned by Mr. Rowell, and I doubt whether the vapour was too warm. I have never seen faintness occur during warm vapour anæsthesia apart from that caused by the operation itself; in fact, my results have been undoubtedly better since I have used this method. Too high a temperature of the vapour supplies a warning by the production of sweating, flushing, and oozing in the wound, due to an unusual amount of dilatation of cutaneous vessels; if used with judgment the method will improve the anæsthetist's results.

Dr. HAROLD LOW (President) showed an apparatus for the Intra-tracheal Insufflation of Ether, designed by Mr. K. H. McMillan, of St. Thomas's Hospital, and Dr. Shipway also showed an apparatus of new design.

Captain C. T. W. HIRSCH, R.A.M.C.(T.), described a Modification of Waller's Chloroform Inhaler (see *Lancet*, 1916, i, p. 730).

#### CORRIGENDUM.

Fellows, and Members of Sections are particularly requested to make the following correction in their copies of the *Proceedings* :—

Section of Anaesthetics, *Proceedings*, 1915, viii, p. 7. Line 7 from bottom: Between "bottle in the dark" and "and frequently shaken" insert the words "with one-tenth its volume of a freshly prepared 10 per cent. solution of potassium iodide."

*The insertion has been made as directed above.*

I

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## Balneological and Climatological Section.

President—Dr. WILLIAM GORDON.

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(November 25, 1915.)

### PRESIDENTIAL ADDRESS.

#### The Influence of Soil on the Prevalence of Phthisis.

GENTLEMEN,—I thank you for the honour you have done me in electing me your President. It is a distinction which I greatly appreciate and shall do my best to deserve.

The subject suggested to me for this address, "The Influence of Soil on the Prevalence of Phthisis," is one at which I have worked occasionally for several years past; so that such conclusions as I shall venture to submit to you are not merely the outcome of recent and hurried reflection. It is necessary for me to say this, because work for the War—the first privilege and duty of us all—has naturally imposed limitations on every other kind of undertaking. I am very conscious of the effect of these limitations on the attempt I have made to deal with so large a subject, and I can only trust that, where my paper seems to fall short of what the occasion claims, excuse may be found for me in the exigencies of the time.

*Acknowledgment.*—It is customary in writings which owe something of their value to the help of others to include in a final paragraph, when the interest is over and the attention tired, a summary of acknowledgments, and under ordinary circumstances this is both convenient and correct; but in the present instance, where so much is owed, and so much has been given under circumstances of peculiar difficulty, it seems incumbent on me to acknowledge at the outset how greatly I am indebted to the many medical officers of health who have furnished



me with information. Some have written from distant military stations, others have taken the necessary time from a scanty leave from abroad. Numbers have told me more than unfortunately I now find it possible to use. For such help, so bestowed, as well as for not a few generous offers of further aid in better days to come, I offer my most cordial thanks. The list of names would be far too long to read in my allotted time, so in the table which deals with the subject of their communication I have given the name of each correspondent opposite the name of the district with which he has dealt. My best thanks are also due to Dr. T. H. C. Stevenson, of Somerset House, who has kindly enabled me to obtain the necessary figures; to Dr. J. M. Martin, for many valuable facts respecting the County of Gloucester; and to the medical officers of health for a number of counties noted in the table, to whom I had occasion to address inquiries, for the important assistance they have been so good as to give me.

#### HISTORICAL.

Soil may be supposed to affect those who live on it in a number of ways: by its dampness or dryness, by its warmth or coldness, by the dust blown from its surface, by the substances dissolved in its springs, by the special configuration of its surface, the type of vegetation covering it, the building material it provides; and indirectly by the occupations determined by it, such as farming or fruit-growing on the one hand, and mining or quarrying on the other. Only the first of these has been hitherto investigated, and in dealing with the subject of soil and phthisis in general it is necessary to begin with a brief résumé of that investigation and its results.

#### *Bowditch's Paper.*

More than half a century ago, almost simultaneously on both sides of the Atlantic, the idea was mooted that dampness of soil promotes prevalence of consumption. Bowditch, in America, gathered from the practitioners of Massachusetts their impressions as to the prevalence of the disease in their several townships, and as to the dampness or dryness of their local soils, and although he was unable to carry out a statistical inquiry, the consensus of opinion which he thus elicited, whilst it supplies no absolute proof of his proposition, remains as a body of suggestive evidence which it would be probably imprudent to ignore.

*Buchanan's First Report.*

About the same time, in England, the remarkable decline in the phthisis death-rate of Salisbury, following the installation of efficient subsoil drainage, served to initiate Buchanan's elaborate statistical compilation, which has formed the basis of modern scientific opinion on this question. The case of Salisbury deserves more attention than it has individually received.

*Salisbury.*—In the first place, there is no doubt that the subsoil drainage works changed the city from a very wet place to a reasonably dry one. In 1851 Dr. Rammell had reported that numerous streams of water ran through most of the streets; that the soil, a porous gravel, contained a great deal of water, which everywhere rose to within a short distance of the surface; that the foundations of the houses were without exception damp; and that the cathedral had been repeatedly flooded. On the other hand, in 1865, after the drainage, Dr. Buchanan found the subsoil dry, the subsoil water lowered 4 or 5 ft. all over the city, cellars of considerable depth no longer subject to floods, whilst the flooding of the cathedral had not again occurred. In the second place—and equally important to observe—no such changes in social and housing conditions had been needed or made in Salisbury as were made in the other towns subsequently examined: changes so great as to be capable themselves of accounting for a very great lessening of the death-rate from phthisis. The annual death-rate from phthisis in Salisbury, with no other obvious circumstance but the subsoil drainage to account for it, fell from the high figure of  $44\frac{1}{3}$  per 10,000 for the years 1844 to 1852 to  $22\frac{2}{3}$  per 10,000 for the years 1857 to 1864—fell, in fact, extremely rapidly to about half. Such a fall far exceeded in rate the general decline in the mortality from the disease already in progress throughout England generally.

*The other Towns.*—Of the twenty-three other towns where drainage works had been established, Buchanan found that eighteen had undergone more or less drying, and that of these eighteen, fourteen showed a coincident lessening of their phthisis death-rates. But in few was the fall so striking as in Salisbury; in several a decline had occurred in absence of any drying, and in two, in spite of some drying, a considerable increase of the mortality from consumption had been observed. What constitutes, however, the prime objection to accepting the evidence of these towns, in favour of the fall in phthisis prevalence being the effect of the subsoil drainage, is the fact that coincidentally

with that drainage such changes in housing and other social conditions were carried out as might not unreasonably be held to account for the diminution of the disease. It seems to me, after a careful study of this first report of Buchanan, not unfair to say that except for the exceedingly suggestive instance of Salisbury, it left matters very much in the position in which it had found them.

*Buchanan's Second Report.*

Buchanan's second report dealt with the registration districts of Kent, Surrey and Sussex, then the only counties of which the geological survey had been completed. Suitable, however, as this area might be from the point of view of variety of soils, its westerly wind exposure, in places considerable and in some uncertain, makes it a particularly difficult field of inquiry. When Buchanan wrote, nothing was known of the effects of rain-bearing winds, although he recognized and referred to some unknown conflicting influence; and this want of knowledge made the difficulty even greater. The effects which Buchanan ascribed to dampness of soil were, in fact, in certain instances, the effects of rain-bearing winds, especially of the south-west wind, the chief rainy wind of the area. It is, however, noteworthy that when the influence of this wind is, as far as possible, eliminated, by only considering districts which do not seem exposed to it, although the results which Buchanan ascribed to soil become rather inconsiderable, they by no means disappear. Thus in the first of his two tables of "fairly comparable districts" an increase of 153 per cent. in the proportion of the population residing on the wetter soils entailed an increase of 15 per cent. in the phthisis death-rate, and, in the second of these tables, an increase of 100 per cent. in the population living on wetter soil caused a 20 per cent. increase in the death-rate. Differences, it may be said, of no great magnitude, and possibly due to other causes, but still differences which it would probably be unwise to quite discredit. Moreover, Buchanan found there was a difference between high-lying and low-lying pervious soils, and between flat and sloping surfaces of impervious soils, in respect of the phthisis prevalence upon them.

A certain number of objections have from time to time been brought against Buchanan's conclusions, but as these objections are themselves highly inconclusive, and I have already dealt with them elsewhere, I will not now trouble you with them. It must be admitted that both Bowditch and Buchanan, the pioneers of thought in this direction,

failed to establish their thesis—failed not from lack of power in themselves, but because the requisite material was not then available. But I think it must also be admitted that they made their views exceedingly probable, and that we shall be mistaken in following the lead of those who have thrown them aside as negligible.

#### PRESENT INQUIRY.

In resuming the inquiry where Bowditch and Buchanan left it, we are fortunate in the possession of immense advantages recently acquired. The extension of statistics of increased reliability, the advance of geological knowledge, of knowledge of other laws governing the disease, and the lavish production of elaborate maps, seem to bring within our reach the final solution of the problem. Given certain lines of research the decision is, I think, merely a matter of time, labour, and money. I by no means presume to offer you to-day a completed proof. All that I hope to do is to make the probability of a relation of phthisis prevalence to soil considerably greater, and by indicating a method of procedure perhaps to simplify the further working.

*Mode of Proceeding.*—In the first place I would suggest that, instead of seeking to establish this or that property of a soil as conducing to a greater or less mortality from phthisis, it will be safer to try to discover simply whether any difference exists between different kinds of soil in respect to the phthisis mortality upon them, and *afterwards* to ask what property of the soils in question is responsible for such differences as we find.

*Approximate Elimination of other Influences.*—Secondly, at the risk of appearing tedious, I must insist on the necessity of eliminating other influences, and must, for that purpose, bring up to date what is known respecting these influences. The co-existing influences affecting phthisis prevalence which might be held to count in this country are the following: Westerly winds, altitude, rainfall, density of population, occupation, race, and the date of observation.

*Westerly winds* form the most difficult interfering influence to eliminate. No one who has tried to work out the problem we are dealing with, and aware of the effects which these winds produce, will question their vexatious interference. And so powerful are their effects—altogether overshadowing, as I have repeatedly shown, the slighter effects of soil—that, if the problem is to be solved with reasonable certainty, it must be dealt with extensively in practically complete



shelter from these winds. In England, however, this means the use of small units of area. Parishes only will, for the most part, be entirely suitable, and the task of choosing them and dealing with their separate death-rates will not be a trifling one. I am going to place before you a limited number of such observations. With larger areas, such as registration districts or sanitary districts, it is only possible in a small minority of cases to label them either as markedly exposed or markedly sheltered, and in the survey to be presently submitted of the rural sanitary districts of England, all that can be done is to exclude wholly such as are conspicuously exposed, and to consider separately from each other those which are markedly sheltered and those neither markedly sheltered nor markedly exposed. The exposed districts cannot safely be compared even with each other, since their exposures differ so considerably.

*Altitude*, I have lately shown, has no proved influence *per se* on phthisis prevalence, even in regions where its differences are extreme. In this country where such differences of height, as were once supposed to produce differences in the incidence of the disease, do not exist, altitude has no effect, so far as I can discover, beyond that, in exposure, it increases the effect of the exposure. This last is, in fact, what one would expect, since higher ground is swept by stronger winds and heavier rain. Buchanan's comparison between higher and lower levels of pervious soils, in respect of difference in drainage, makes the altitude of dwellings a point worth noting, and so, in the succeeding tables, dealing with rural sanitary districts, their altitudes are in most cases stated.

*Rainfall* in shelter is practically without effect on phthisis mortality. In exposure I have found (the work is still unpublished) that greater rainfall apparently increases the mortality, but whether this relation is real, in the sense that with an equal degree of wind a heavier rainfall determines a higher death-rate, or that the effect is due to a greater stress of wind accompanying the greater fall of rain, remains unsettled. Whatever the reality of this may prove to be, it will be prudent, in dealing with districts not completely sheltered, to add the approximate amount of rainfall throughout their areas. I do not think you will decide from the tables submitted that the amount of rainfall has had any notable influence upon the figures.

*Density of population* exerts so powerful an influence on phthisis prevalence that it is imperative to get rid of it by dealing with areas from which all large towns are excluded.



*Occupations*, similarly, which specially tend to increase proclivity to the disease must be excluded from the districts considered, and this I have taken special means to do.

*Race* is so powerful a factor that I have ruled Wales and Monmouth out of the inquiry, because there seems no doubt that, for some reason or another, the Welsh are more prone to the disease than the English.

*Dates of observation* in any such inquiry must in this country be contemporary, since the death-rate from phthisis is more or less steadily declining in all parts of it.

Whether any error will be introduced by differences in the efficiency of local measures adopted against the disease is difficult to say. Probably none of consequence to this inquiry. All I could attempt towards avoiding this source of fallacy was to exclude well-known health resorts.

#### SMALL SHELTERED UNITS OF AREA.

In a paper published some years ago on the distribution of phthisis amongst females in the *streets of Exeter*, whose roadways were sheltered from westerly winds, I was able, after taking every precaution against fallacy which I could think of, to arrive at a small table, whose only obvious drawback is its inconsiderable populations (Table I). In a later paper I added the following table of *Devonshire parishes* sheltered from westerly winds during the ten years 1890-99 (Table II). To these I now add for the same ten years (1890-99) two other groups of similarly sheltered parishes, one, on chalk, in two rural sanitary districts of Dorset, the second, on igneous rocks, in the English Lake country (Table III).

TABLE I.—FEMALE PHTHISIS DEATH-RATES IN THE SHELTERED STREETS OF EXETER, FROM 1892 TO 1901.

Soil	Total population	Female phthisis death-rate in terms of the total population
Gravel ... ..	407	0.24
Sandstone ... ..	905	0.33
Clay ... ..	3,423	0.55

TABLE II.—PHTHISIS DEATH-RATES (MALE AND FEMALE) PER 1,000 PER ANNUM IN DEVONSHIRE PARISHES SHELTERED FROM SOUTH-WEST, WEST, AND NORTH-WEST WINDS, FROM 1890 TO 1899.

Soil	Population, 1891	Phthisis death-rate per 1,000	Average approximate rainfall in inches
Permian Sandstone or Conglomerate (3 villages)	973	0.10	38
Devonian (8 villages)	2,124	0.32	50
Millstone Grit (7 villages)	4,117	0.53	41
Granite (5 villages)	1,986	0.70	52

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TABLE III.—FEMALE PHTHISIS DEATH-RATES IN TWO GROUPS OF PARISHES SHELTERED FROM ALL WESTERLY WINDS, FROM 1890 TO 1899.

Name of parish	Mean female population	Total female deaths	Female phthisis death-rate	Approximate rainfall in inches
<i>Chalk (17 Dorsetshire Parishes).</i>				
Sydling St. Nicholas ...	239	3	—	—
Nether Cerne ...	36	—	—	—
Godmanstone ...	72	—	—	—
Alton Pancras ...	99	1	—	—
Puddletrenthide ...	310	3	—	—
Puddlehinton ...	186	2	—	—
Chesilbourne ...	112	1	—	—
Dewlish ...	182	—	—	—
Milton Abbas ...	352	1	—	—
Turnworth ...	58	—	—	—
Winterbourne Houghton ...	99	—	—	—
Winterbourne Stickland ...	196	—	—	—
Winterbourne Glenston ...	50	1	—	—
Winterbourne Whitechurch ...	190	—	—	—
Winterbourne Kingston ...	216	5	—	—
Tarrant Gunville ...	179	—	—	—
Tarrant Hinton ...	96	—	—	—
Totals and averages ...	2,672	17	0·63	30
<i>Igneous Rocks (5 Cumberland and Westmorland Parishes).</i>				
Ambleside ...	1,320	10	0·7	79
Rydal ...	265	2	0·7	77-87
Grasmere (some Alluvium) ...	464	2	0·4	89-97
Langdale ...	379	4	1·0	92-119
Borrowdale ...	253	2	0·7	92-135
Totals and averages ...	2,681	20	0·70	—

## THE RURAL SANITARY DISTRICTS OF ENGLAND.

But it seems to me desirable, before proceeding further with this piecemeal attack upon the problem, to see whether a general survey of the country as a whole (now entirely mapped by the Geological Survey, and possessing a death registration of great accuracy) does or does not bear out the conclusions suggested by the far smaller figures just quoted. At one time I attempted to ascertain this by arranging in ascending order of female phthisis death-rate the registration districts of England and Wales from 1881 to 1890, and from 1891 to 1900, and arrived at the following results:—

(1) That Trias was commonly associated with a low phthisis death-rate.

(2) That Millstone Grit and the Carboniferous Limestone Series were generally associated with a higher death-rate.

(3) That Chalk and London Clay occurred in almost all parts of the scale.

(4) That the Welsh soils were associated with a remarkably high death-rate, but that, as those soils common to Wales and England were associated with a higher death-rate in Wales than in England, the higher death-rates in Wales were probably, in part at least, due to race.

(5) That the above contrasts in soils came out more clearly in 1881-90, the wetter decade, than in the succeeding drier decade, 1891-1900.

(6) That density of population, industrial centres and health resorts seriously interfered with the comparison of the soils.

At that time also the county maps showing the sanitary districts were not very easily compared with geological or topographical maps, so that precise judgments on slope, soils, and exposures were hardly attainable. The work was therefore discontinued. Now, however, the case is widely different. For four years (1911-14) statistics are available of the phthisis death-rates in the rural sanitary districts which contain no large towns, industrial centres, or considerable health resorts, and where the conditions of life are presumably more uniform than in urban districts. The period is short, but, by confining attention to considerable populations, I think this disadvantage need not cause us serious concern. Dr. T. H. C. Stevenson has most kindly enabled me to obtain from Somerset House the female death-rates of these districts. Moreover, the new excellent county maps showing the sanitary districts and the parish boundaries are so detailed, so clear, and on so large a scale ( $\frac{1}{2}$  in. to the mile) as to make exact comparison with geological and topographical maps extremely easy. For the geological formations I have had recourse to the  $\frac{1}{4}$  in. to the mile Survey maps issued in 1907, which are complete for England and Wales. The admirable topographical maps ( $\frac{1}{2}$  in. to the mile) published by Messrs. Bartholomew and Co. enable one readily to determine slope, exposure and altitude.

I have proceeded to eliminate the influences requiring to be got rid of as follows:—

*Race.*—In view of my experience with the registration districts I have excluded all Wales and Monmouth.

*Density of Population.*—The rural sanitary districts can be scarcely said to experience any effects of this.

*Occupation.*—In order to be sure that female occupations specially predisposing to phthisis, such as factories, were excluded, I wrote to all the medical officers of health of these districts asking if such occupations were or were not common in their areas. More than three-quarters

were good enough to reply, and in the overwhelming majority the answers were in the negative. Moreover, in a table I shall presently show you of soils on single formations, I have a reply to this effect in respect of every district included. By restricting the investigation to female deaths the difficulty of occupation is minimized.

*Rainfall.*—The approximate rainfall of each district is added in the chief tables from which deductions are drawn.

*Westerly Winds.*—All districts markedly exposed or sheltered are specially distinguished. The further details of procedure will be mentioned as I go on.

Taking, then, the whole of the rural districts of England, and comparing carefully the county maps with the geological and topographical maps, I have made an analysis of the geological formations lived on, of the altitudes of the dwellings, the general degree of slope, westerly exposure and shelter of each. There were altogether 590 areas to be so dealt with. I have arranged the results in Table IV,<sup>1</sup> which shows for every separate area the county (in order to simplify its location), its female population according to the Census of 1911, its female phthisis death-rate for the four years 1911 to 1914, a statement whether female occupations predisposing to phthisis are common in it or no, whether it is markedly exposed to or markedly sheltered from westerly winds, the general character of its slopes, the approximate heights of its dwellings, the name of the medical officer of health who kindly replied to my inquiries, whether he considers his district generally damp or dry, what are the geological formations underlying it, and what are the surface soils where it has seemed necessary to state them. The districts are arranged in ascending order of female phthisis death-rates, and the geological formations in each are placed as far as possible in the order of the population upon them, the most populous coming first.

Now, in most cases it will be seen that the geological structure is complex, and sometimes remarkably so; and therefore before attempting to draw conclusions it is necessary to simplify the problem by dealing only with such areas as are practically on a single geological formation. Of these there are, fortunately, a large number, but some have to be excluded, viz.:—

(1) Districts either markedly exposed to or markedly sheltered from westerly winds.

(2) Districts in which female occupations are common which may be reasonably thought to affect the female phthisis death-rate.

<sup>1</sup> Table IV is not printed, but has been deposited for reference in the Library of the Society.



(3) Districts which have a female population of less than 1,000. This is because the period is so short and because the districts are to be compared in this table individually with each other. In a subsequent table, where only the aggregate death-rates on the different soils are compared, these less populous districts will be included in making up the aggregates (Table VIII).

Table V (*see pp. 12-15*) shows the remaining districts (137) arranged in ascending order of female death-rates. At a glance we see :—

(1) Trias, chiefly associated with low death-rates.

(2) Chalk, most often associated with low death-rates, but less closely than Trias.

(3) Alluvium and Oxford Clay, chiefly associated with the higher death-rates.

(4) Magnesian Limestone and the Carboniferous Limestone Series occupying an intermediate position.

It will be observed also that London Clay has much the same relation to the death-rates that Chalk has, and that Millstone Grit tends to be associated with the higher mortality. Thus, so far, my former observations on soils and phthisis, made with the registration districts of England, are confirmed. It is next necessary to arrange similarly those districts which were only excluded on account of their marked shelter. Table VI (*see p. 17*) gives the result, and so far as it goes—for the list is small (of only eighteen districts)—confirms Table V. Finally, in Table VII<sup>1</sup> I have arranged the districts of Table V according to their soils, adding, however, in forming each aggregate, such districts as were only excluded from Table V on account of their population being under 1,000, and the summary at the end shows a very interesting disparity of death-rates. Aggregate populations under 10,000 have not been considered. Now, in considering these figures we have always to bear in mind that westerly exposure and shelter have only been avoided where it was markedly present, so that a certain element of uncertainty must remain as to the precise values of the soil relations, certainty being only attainable in complete shelter. But a good deal can be said to lessen this uncertainty. The table of markedly sheltered districts bears out the indications of Table V. Trias is most certainly not a well-sheltered soil, since it is impossible to deal with it as I have already dealt with Chalk, Devonian and volcanic rocks, in groups of sheltered parishes. Nor are Alluvium and Oxford Clay especially exposed; they are probably not more exposed than Trias or Chalk, and they certainly

<sup>1</sup> Table VII is not printed, but has been deposited for reference in the Library of the Society.





Southwell ...	9,946	19	0.48	Slight	24-300	Dry	Trias; some Alluvium...	...	24
Congleton ...	6,259	12	0.48	Moderate	140-400	Damp	Trias	...	34
Wheatonhurst	3,147	6	0.48	Flat	25-260	—	Lower Lias	...	30
Chesterfield ...	36,064	70	0.49	Good	150-850	Damp	Coal; a very little Millstone Grit and Carboniferous Limestone Series	...	35
Upton-on-Severn	7,877	15	0.49	Slight	40-300	—	Trias	...	—
Ellesmere ...	4,144	8	0.49	Flat	250-400	—	Trias; a little Permian	...	36
Mildenhall ...	4,088	8	0.49	Mostly flat	5-140	Rather damp	Chalk; a little Alluvium	...	24
Tamworth (part)	2,574	5	0.49	Moderate	170-280	—	Trias; a little Alluvium	...	27
Pershore ...	6,509	10	0.50	Rather flat	70-300	—	Lower Lias; a little Trias	...	29
Stapleford ...	4,983	10	0.50	Fair	90-275	—	Trias; some Alluvium	...	29
Bucklow ...	11,862	24	0.51	Mostly flat	50-240	—	Trias...	...	30
Gnosall ...	2,369	5	0.53	Good	240-470	—	Trias; a little Permian	...	30
Clowne ...	8,420	19	0.56	Good	200-450	Dry	Magnesian Limestone; a little Coal	...	35
Hungerford ...	4,491	10	0.56	Good	300-500	Dry	Chalk; a little Reading Beds	...	27
Marshall ...	6,166	14	0.57	Flat	2-20	—	Alluvium	...	23
Blandford ...	4,401	10	0.57	Good	94-450	—	Chalk; a little Upper Greensand and Alluvium	...	36
Barnet ...	2,627	6	0.57	Good	250-430	—	London Clay; a little Lower London Tertiaries	...	29
Lichfield ...	13,273	31	0.58	Fair	160-500	—	Trias	...	25
Hartismere ...	5,575	13	0.58	Slight	84-200	Dry	Chalk	...	27
Bumpstead ...	1,302	3	0.58	Slight	200-370	Fairly dry	Chalk; Boulder Clay, higher ground	...	21
Ongar ...	5,093	12	0.59	Fair	100-320	Damp	London Clay; a little Bagshot and Bracklesham Beds	...	25
Andover ...	5,451	13	0.60	Good	150-700	—	Chalk; a very little Alluvium	...	31
Castle Donington	3,335	8	0.60	Slight	100-300	Damp	Trias; some Alluvium	...	25
Staines ...	11,843	28	0.62	Mostly flat	40-100	—	London Clay; a little Alluvium	...	24
Newmarket ...	9,722	24	0.62	Mostly flat	10-390	Fairly dry	Chalk; a little Gault and Alluvium	...	22
Flaxton ...	4,004	10	0.62	Flat	50-120	Dry	Trias; a little Alluvium and Lower Lias	...	26
Buntingford	2,424	6	0.62	Fair	200-485	—	Chalk	...	25
Tetbury (part)	2,004	5	0.63	Good	350-700	—	Great Oolite; a little Inferior Oolite	...	29
Hitchin ...	9,484	24	0.63	Fair	190-526	Fairly dry	Chalk; Boulder Claycaps in highest parts	...	24
Saffron ...	3,150	8	0.63	Flat	11-135	Fairly dry	Trias...	...	30
Wangford	2,376	6	0.63	Slight	5-170	Damp	Norwich Crag...	...	26
Lexden, &c...	9,772	25	0.64	Flat	12-225	—	London Clay; a very little Red Crag	...	24
Misterton ...	1,991	5	0.64	Flat	20-50	Damp	Trias; some Alluvium	...	30
Tubbury ...	4,595	12	0.65	Varies	150-470	Damp	Trias; a little Alluvium	...	24
South Mimms	1,534	4	0.65	Good	230-420	—	London Clay; a little Reading Beds and Chalk	...	25
Romford ...	12,877	34	0.66	Much flat	5-325	Dry	London Clay	...	22
Wirral ...	10,214	27	0.66	Gradual	15-340	Dry	Trias...	...	28
Blything ...	9,099	24	0.66	Slight	8-180	—	Norwich Crag; a very little Alluvium	...	26
Sculcoates ...	4,145	11	0.66	Much flat	25-350	Damp	Chalk	...	22
East Stow ...	3,094	8	0.66	Gradual	80-250	Damp	Chalk	...	26

TABLE V (continued).

Name of district	Female population, Census of 1911	Total female phthisis deaths 1911-14	Average annual female phthisis death-rate 1911-14	Slope	Approximate height of dwellings above sea-level (feet)	Generally damp or dry	Geological formations lived on	Approximate annual rainfall in inches
Runcorn	14,080	38	0·67	Slight	250-300	—	Trias...	26
Dover	3,726	10	0·67	Good	200-400	—	Chalk	28
Brandon	2,964	8	0·67	Slight	11-180	Dry	Chalk	27
Wharfedale	4,813	13	0·68	Good	140-700	Dry	Millstone Grit	80
St. Ives	4,791	13	0·68	Flat	15-127	—	Oxford Clay	23
Swaffham	3,693	10	0·68	Slight	25-200	Dry	Chalk; a little Gault	28
Torrington	4,366	12	0·69	Fair	170-600	—	Millstone Grit	38
Rye	3,638	10	0·69	Moderate	10-250	—	Hastings Beds; a little Alluvium	28
Aysgarth	2,180	6	0·69	Good	500-1,400	—	Carboniferous Limestone Series	60
Northwich	11,385	32	0·70	Mostly flat	100-150	Damp	Trias, a little Alluvium	26
Titchhurst	6,817	19	0·70	Good	20-580	Fairly dry	Hastings Beds; a very little Alluvium	23
Chertsey	4,622	13	0·70	Mostly flat	80-300	Fairly dry	Bagshot and Bracklesham Beds	23
Newent	3,549	10	0·70	Slight	150-300	—	Trias; a very little Silurian	30
Sedgefield	16,135	46	0·71	Moderate	140-500	—	Magnesian Limestone; a little Trias	28
Chester-le-Street	30,902	89	0·72	Mostly good; varies	100-700	Dry	Coal ...	28
Wayland	7,258	21	0·72	Slight	80-200	Dry	Chalk	27
Thedwastre	4,455	13	0·73	Gradual	125-350	Damp	Chalk	26
Alcester	6,079	18	0·74	Mostly slight	100-400	—	Trias; a very little Lower Lias and Alluvium	27
Walsingham	8,669	26	0·75	Moderate	10-320	—	Chalk; a very little Alluvium	27
Thetford	4,973	15	0·75	Moderate	25-170	—	Chalk	26
Chischurch	3,006	9	0·75	Flat	25-200	Dry	Bagshot and Bracklesham Beds; a little Oligocene	27
Lanchester	15,103	46	0·76	Good	300-800	Rather damp	Coal; a little Millstone Grit	28
Bedford	9,518	29	0·76	Mostly flat	80-300	Damp	Oxford Clay; a little Great Oolite and Cornbrash	23
Warrington	6,871	21	0·76	Flat	45-128	Damp	Trias...	33
Ely...	6,290	19	0·76	Mostly flat	5-120	Damp	Kimmeridge Clay; some Lower Greensand and Alluvium	22
Leadbury	6,413	14	0·76	Fair	150-600	—	Devonian; a very little Silurian	29
Spalding	6,488	20	0·77	Flat	8-18	Varies	Alluvium	23
Bosmere, &c.	6,806	21	0·77	Slight	40-300	—	Chalk; a very little Alluvium, &c.	26
Crowland	1,307	4	0·77	Flat	8-10	—	Alluvium	23
Swavesey	1,296	4	0·77	Flat	15-150	—	Oxford Clay; a little Kimmeridge Clay, &c.	23
Dunmow	7,662	24	0·78	Good	60-360	Damp	London Clay; much Boulder Clay over	24
East Grinstead	6,695	21	0·78	Good	100-500	Fairly dry	Hastings Beds; a very little Weald Clay	31
East and West Flegg...	5,104	16	0·78	Flat	10-60	—	Norwich Crag; a very little Blown Sand	25

[illegible]



have a lower rainfall. Millstone Grit is apt to be more exposed than some other soils, because the villages upon it are so commonly perched on its hills—as I believe, to avoid the cold and damp of its valleys. The Carboniferous Limestone districts, I think, are perhaps amongst those with most pronounced shelters and exposures.

#### CONCLUSIONS.

From all these tables, from my previous examination of the registration districts, from the study of the groups of sheltered parishes and sheltered streets, as well as perhaps from Sir George Buchanan's second report, I think it may be reasonably concluded—

(a) That certain geological formations tend to influence the phthisis mortality of those who live on them.

(b) Secondly, that this influence is moderate in degree.

I have already shown how small were the differences arrived at by Buchanan. The differences I am now showing are not very great. Only where conspicuously different soils are compared in shelter do marked contrasts emerge. Fuller work in sheltered parishes may, however, bring out greater differences.

But why do these differences exist? Bowditch and Buchanan said it was because of differences as regards dampness and dryness. I will confine myself to their theory. Bowditch, as I have said, left a very probable case for his proposition, and this probability was increased by Buchanan. In the sheltered streets of Exeter the differences in phthisis mortality obviously correspond to differences in perviousness of soil. In the groups of country villages which I have quoted the most pervious came out best, the least pervious worst, although Chalk was less fortunate than might have been expected. In Table VII the same relation can be shown to hold.

*Trias* appears to be generally a very pervious formation, and is especially associated with low mortality. It is divided into Upper and Lower *Trias*. Upper *Trias* consists of light marls with a less amount of sandstone, and is often associated with Rhætic beds, which form a sort of transition towards *Lias* which is also associated with low death-rate. Lower *Trias* seems to be wholly composed of sandstone and pebble beds. Lower *Trias* should be probably the more pervious of the two, and we find from Table VII it is more closely associated with lower mortalities than the Upper *Trias*; for this no obvious cause is discernible except its own characters. In respect of *Upper Trias* we may adopt Buchanan's tests of slope and altitude. Table VIII shows that

TABLE VI.—THE SAME AS TABLE V, EXCEPT THAT THIS DEALS EXCLUSIVELY WITH DISTRICTS MARKEDLY SHELTERED FROM WESTERLY WINDS, WHICH DISTRICTS WERE EXCLUDED FROM TABLE II.

Name of district	Female population, 1911	Total female phthisis deaths 1911-14	Average annual female phthisis death-rate 1911-14	Slope	Approximate height of dwellings above sea-level (feet)	Generally damp or dry	Geological formations lived on
Wath ...	1,080	1	0.23	Slight	92-130	Dry	Trias
Bredwardine ...	1,024	1	0.24	Good	220-800	—	Devonian ; some Alluvium
Berkhamsted ...	2,473	3	0.30	Good	300-600	—	Chalk ; a very little Gault
Amesbury ...	4,395	6	0.34	Good	180-350	—	Chalk ; some Alluvium
Marlborough ...	2,164	3	0.35	Good	400-600	Dry	Chalk
Chapel-en-le-Frith ...	8,125	12	0.37	Good	500-1,100	Dry	Carboniferous Limestone Series ; a little Coal and Millstone Grit
Tarvin ...	6,761	10	0.37	Rather flat	35-350	Dry	Trias
Bowland ...	2,662	5	0.47	Good	200-1,000	Rather damp	Carboniferous Limestone Series ; a little Alluvium
Pateley Bridge ...	3,740	8	0.53	Good	20-1,000	Dry	Millstone Grit ; a little Alluvium
Dulverton ...	2,360	5	0.53	Good	300-1,000	Damp	Devonian
Stockbridge ...	3,240	7	0.54	Fair	130-230	Dry	Chalk ; a little Alluvium
Tenbury ...	2,319	5	0.54	Good	145-800	—	Devonian ; a little Alluvium
Glendale ...	4,474	13	0.73	Good	66-600	Dry	Carboniferous Limestone Series ; a little Alluvium, &c.
Penistone ...	2,653	8	0.75	Good	270-800	Damp	Coal ; a very little Millstone Grit
Rothbury ...	2,294	7	0.76	Good	200-600	Dry	Carboniferous Limestone Series ; a little Alluvium, &c.
Norton ...	2,129	7	0.82	Good	400-700	—	Coal
Dore ...	3,212	14	1.09	Good	250-1,000	—	Devonian ; a very little Silurian
Wigmore ...	1,886	13	1.72	Good	400-600	—	Silurian ; a little Devonian

## Summary.

	Female population	Female phthisis death-rate
Trias	7,841	0.35
Chalk	12,272	0.38
Carboniferous Limestone Series	17,555	0.52
Devonian	8,915	0.70

SUMMARY OF TABLE VII.

Geological formation	Female population, Census 1911	Total female phthisis deaths (1911-14)	Average annual female phthisis death-rate (1911-14)	General degree of slope	Average approximate heights of dwellings above sea-level (feet)	Average approximate annual rainfall in inches	Remarks
Lower Lias ... ..	20,057	37	0.46	Flat	114-307	29	Limestones, clays, and shales
Trias ... ..	186,425	437	0.59	Slight	125-305	—	Sandstones, light marls, pebble beds (pervious)
London Clay ... ..	93,460	239	0.64	Fair	91-312	—	Much covered with gravel; good slopes often
Bagshot and Bracklesham Beds ... ..	11,364	29	0.64	Flat	40-250	26	Mostly dry
Chalk ... ..	151,058	405	0.67	Moderate	100-345	—	Dry on higher ground
Hastings Beds... ..	28,361	76	0.69	Good	58-445	29	Generally dry, sands, sandstone, less clay
Coal ... ..	98,872	272	0.69	Good	175-496	—	—
Carboniferous Limestone Series ... ..	10,828	32	0.74	Good	166-675	38	—
Magnesian Limestone ... ..	52,950	157	0.74	Fair	153-503	30	—
Devonian ... ..	40,318	127	0.78	Good	117-647	33	Varies much in perviousness; steep slopes often
Alluvium ... ..	45,718	148	0.81	Flat	7-24	23	Often damp
Oxford Clay ... ..	27,181	91	0.83	Flat	50-228	23	Stiff clay, with shales; generally damp
Millstone Grit ... ..	16,010	53	0.83	Good	177-738	44	A rather damp soil in valleys, I think
Lower Greensand ... ..	11,919	42	0.88	Good	50-600	26	—
Norwich Crag ... ..	49,391	180	0.91	Flat	8-124	25	Probably damp at lower levels

the lower death-rates of Upper Trias are associated both with greater slope and greater altitude—altitude acting simply by affording more thorough drainage.

TABLE VIII.—DISTRICTS ON UPPER TRIAS (13) WITH FEMALE POPULATIONS OF OVER 1,000.

Name of district	Female phthisis death-rate	Slope	Heights	Averages of slopes and heights
Solihull ...	0.39	Moderate	300-500	150-370 ft., moderate slope
Uttoxeter ...	0.43	Fair...	200-500	
Congleton...	0.48	Moderate	140-400	
Upton-on-Severn ...	0.49	Slight	40-300	
Tamworth (part) ...	0.49	Moderate	170-280	
Bucklow ...	0.51	Rather flat	50-240	88-251 ft., almost flat
Castle Donington ...	0.60	Slight	100-300	
Sefton ...	0.63	Flat	11-135	
*Misterton...	0.64	Flat	20-50	
Tutbury ...	0.65	Varies	150-470	
Northwich ...	0.70	Mostly flat	100-150	88-251 ft., almost flat
Newent ...	0.70	Slight	150-300	
Alcester ...	0.74	Slight	100-400	

The low death-rate on *London Clay* as compared with the high death-rate on *Oxford Clay* may be explained much as Buchanan explained the difference between *London Clay* and *Weald Clay*—i.e., by the fact that *London Clay* has generally good slopes and much overlying gravel. The *Oxford Clay* districts are for the most part flat, and if equally impervious should be more damp.

*Bagshot and Bracklesham Beds* in the districts included in Table VII are mostly dry. *Chalk* is very dry on high ground but varies a good deal at lower levels. The disturbing effect of glacial deposits probably comes in here. The ice sheet which once covered England stopped at the line of the Cotswolds and the Thames. North of this line *Boulder Clay* is common; south of the line it does not exist, and the chalk is close to the surface almost everywhere. Table VII shows that, whereas the death-rate on *Chalk* generally is 0.67 per 1,000, that in the districts south of the Thames is only 0.56. With *Chalk* also, as with *Upper Trias* (Table IX), the lower death-rates are on the higher ground. *Hastings Beds* are generally pervious and dry. *Coal* is a doubtful soil to consider, since coal-mining is an occupation which is curiously antagonistic to phthisis.

TABLE IX.—DISTRICTS ON CHALK WITH FEMALE POPULATIONS OF OVER 1,000.

*North of Thames (24).*

First 12 districts have average height 113 to 336 ft., and moderate slope.

Second 12 districts have average height 62 to 300 ft., and rather slighter slope.

*South of Thames (7).*

Whole 7 districts have average height 142 to 414 ft., and good slope.



*Limestones* when high and uncovered are dry, but glacial deposits have to be taken into account, and many of the districts are on clay as surface soil.

On *Devonian* I lay little stress, since the differences in death-rate seem to me rather due to differing westerly exposures.

*Alluvium, Oxford Clay and Norwich Crag* are all flat, and Alluvium and Norwich Crag are very low-lying as well, so that a certain amount of dampness seems unavoidable, at least in winter; and this expectation is borne out by the notes from medical officers.

On *Millstone Grit* I lay no stress except in shelter as in Table VI, because of the position of its villages already alluded to. Of Lower Lias and Lower Greensand I know too little to speak.

(c) Taken altogether there seems here enough to justify us in concluding that dampness and dryness play a considerable part in determining the differences.

Lastly, how do dampness and dryness act? Many of my correspondents have insisted on the effect of damp houses, and in the Exeter streets it is more likely that the soils acted by causing dampness of houses than in any other way. Indeed, it is difficult to see how else they could have acted. In the case of Salisbury the house-drying which occurred similarly seems to have been the chief change.

(d) It is therefore likely that dampness of soil acts at least partly through dampness of houses.

I am of course aware that much more might have been made of this mass of material, which, thanks to the kindness of so many of my professional brethren, it has been possible to collect, than I have made of it. But at such a time as this, work of this kind can only be the labour of leisure moments. All that I venture to claim for my figures is, first, that they have made much more probable a relationship of soil to phthisis prevalence; and, secondly, that they perhaps have indicated a method by which certainty can ultimately be reached.

On the motion of Dr. LEONARD WILLIAMS, seconded by Dr. CLIPPINGDALE, a vote of thanks to the President was unanimously passed.

## Balneological and Climatological Section.

President—Dr. WILLIAM GORDON.

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(January 20, 1916.)

### The Hydrological Treatment of Gastro-intestinal Stasis.

By J. CAMPBELL MCCLURE, M.D.

I FEEL that some apology is due to you for the fact that, although this paper bears the title of the hydrological treatment of gastro-intestinal stasis, I intend to dwell at some length on the causation of intestinal stasis, and to limit my remarks on hydrological treatment to a somewhat small compass, having regard, in this connexion, to principles rather than to detail. I feel that it would be presumption on my part to do otherwise, as I am addressing an audience of men who, for the most part, have devoted many years to the practice and study of hydrology. At the same time, an onlooker like myself has certain advantages which distance lends in the estimation of perspective. I make no excuse for reopening the subject of alimentary stasis, since the recent work of Keith and Alvarez has excited general interest in this much-argued question.

Before going further I should like to state definitely what I mean by gastro-intestinal stasis. One naturally excludes cases in which delay in the passage of the gastro-intestinal contents is due to an actual obstruction in the stomach or bowel caused by a tumour or a cicatricial contraction. The type of stasis that we are considering is associated with an atonic condition of the alimentary tract, and delay in the passage of the contents occurs at certain points—namely, the stomach,

the duodenum, the ileocaecal region, and the colon. I include the stomach because the consideration of stasis of a functional kind in this organ has been neglected, while too much stress has been laid on the intestinal condition. It is a well-known fact that ileal stasis may be either the cause or the effect of stasis higher up in the gastro-intestinal tract, and it is my experience that in a large number of cases of alimentary stasis a dilated and atonic stomach has to be dealt with before the intestinal condition can be ameliorated. It is well also to establish some kind of standard of alimentary stasis which is pathological, and I think it is right to consider that if one finds splashing on succussion with some evidence of gastric enlargement three and a half hours after food, there is definite evidence of gastric stasis. Similarly, if an opaque meal be given and has not left the stomach entirely at the end of four hours, gastric stasis is certainly present. It is my habit also to consider that if the ileum is not empty eight or nine hours after the ingestion of an opaque meal, or if most of the meal is not in the descending colon twenty-four hours after its ingestion, a state of true intestinal stasis exists.

The two most important theories regarding the causation of alimentary stasis which have been advanced are those of Arbuthnot Lane and Keith. One can, I think, without offence describe Arbuthnot Lane's view as the extreme surgical view: he considers that stasis is produced by the dragging of an atonic bowel on firm peritoneal bands, with the subsequent formation of kinks, which cause obstruction to the passage of the gastro-intestinal contents. The great objection to this theory is that neither on post-mortem examination nor on the operating table nor on the X-ray plate has there ever been found an occlusion at the point of kinking, and no evidence has been produced of a dilatation or hypertrophy of the bowel behind the kink such as one would reasonably expect to find were the kinking sufficiently obstructive to cause serious delay in the passage of the intestinal contents for a long time.

Keith's theory is quite different. In the Cavendish Lecture for 1915, and in a paper read before the Electro-Therapeutical Section of this Society on October 15, 1915, he put forward the theory that alimentary stasis was due primarily to a defect of innervation, and said that he found that in the course of the gastro-intestinal tract there were definite aggregations of myenteric plexus situated at certain points in the stomach and intestine. The aggregations of myenteric plexus are found at the œsophago-cardiac junction, at the pylorus, in the third part of the duodenum, at the jejuno-iliac junction, at the lower end of the

ileum, and in the distal colon. These areas he called nodal areas, and he explained that their chief function was to regulate the rhythm of contractions in the segment of the gastro-intestinal tract immediately beyond them. He also drew attention to the fact that disturbance of the rhythm of contraction in one nodal zone affects the rhythm in other zones. For instance, if the rhythm were upset in the ileocaecal region a corresponding upsetting of the rhythm was common in the duodenum. Keith made a very careful study of numerous specimens which he had obtained from operation and post mortem, and showed that in these nodal areas there were to be found gross pathological changes in the structures composing Auerbach's plexus. He considers that the structural changes found in the plexus are sufficient to derange the innervation of the intestine and cause stasis, and he also points out that stasis occurs in areas near the nodal zones. One cannot help feeling attracted by this theory of Keith's that alimentary stasis is caused by some disturbance of innervation in the gastro-intestinal tract; but I should like to venture further back in the history of this condition, and suggest that there is a disturbance of innervation before any gross structural change appears in Auerbach's plexus.

I believe that the firm peritoneal bands described by Lane and the gross structural changes in Auerbach's plexus described by Keith are both late events in the course of intestinal stasis, and are probably the result of a low inflammatory process produced by the action of toxins or bacteria which have passed through an atonic intestinal mucosa. It seems likely that intestinal stasis, apart from an infection of the gastro-intestinal tract, is a condition which may exist for a long time without giving rise to symptoms. We all know people who have lived long and comfortably even though they have been the subjects of chronic constipation all their days. If, however, such a person were to acquire an infection of the gastro-intestinal tract, it is easy to see how the antecedent tendency to intestinal stasis would aggravate the effects of the infection. I do not think, however, that stasis always precedes infection. Infection may actually produce stasis in a susceptible person, that is to say, in a person whose nervous system renders him easily liable to disturbances of innervation in the gastro-intestinal region. The main point is that whether stasis precedes or succeeds an infection, there is in all probability an inherent tendency to disturbance of the gastro-intestinal innervation in all patients in whom alimentary stasis exists.

Following out Keith's theory that there is some disturbance of



innervation at the back of the condition of alimentary stasis, one may obtain a good deal of information by a study of what occurs in functional atony of the stomach, where a considerable degree of distension and dilatation of the organ may occur without there being any organic obstruction at the pylorus. In this condition one finds clinically an atony of the main body of the organ with a tendency to hypertonus, or lack of proper relaxation of the sphincteric areas at the cardia and pylorus. To this state of sphincteric spasm Hertz has given the name of "achalasia," which is a convenient and descriptive term. It is interesting to remember, in this connexion, that it is in these two sphincteric areas, at the cardia and the pylorus, that the two considerable aggregations of myenteric plexus exist in the stomach. Similarly, it is most probable that in the intestine there exist areas, more or less coincident with the nodal areas described by Keith and Alvarez, in which a sphincteric action may occur, although not so completely as in the cardiac and pyloric regions of the stomach. This is particularly noticeable at the lower end of the ileum, where the lumen of the gut is sometimes seen during an X-ray examination to be greatly narrowed, and the opaque contents are visible as a thread-like shadow.

It seems likely that in the stomach a condition of achalasia at the cardia and at the pylorus, accompanied by an atony of the body of the organ, is produced by some disturbance of the balance normally preserved between the action of the vagus and the splanchnics, and it has been pointed out by Percy Mitchell, Wethered and myself that this achalasia and atony may be corrected by stimulation of the skin over the left side of the abdomen in certain definite areas—namely, along the rib margins between the seventh and tenth costal cartilages, and over the lower borders of the ribs about the level of the normal upper border of the stomach. Similarly, it seems likely that a condition of achalasia of the sphincteric areas in the intestine accompanied by atony of neighbouring internodal areas may be produced by just such a loss of balance between vagus and splanchnics. This condition would produce, as in the stomach, a stasis of the intestinal contents near these areas. Some little time ago I endeavoured to find out if there were other areas on the skin surface of the abdomen through which stimulation could be applied which would produce a more vigorous peristaltic action in the intestine, with increase of its tone. It occurred to me that the duodenum could be stimulated through a skin area a little above the point to which pain produced by the gall-bladder is referred, and also, recollecting how in tuberculous peritonitis pain is so often referred to

the umbilicus, that one could possibly find skin areas in the umbilical region through which the small intestine could be stimulated. It is my belief that by stimulation of the skin just within the right costal margins, above the point to which gall-bladder pain is referred, it is possible so to stimulate the duodenum; and experiment suggests that a similar stimulation of the lower part of the ileum and of the cæcum can be effected through an area of skin in the middle line just below the umbilicus.

Following up these observations, I endeavoured to find out whether similar areas existed through which various parts of the colon might be stimulated, and I conducted a series of experiments by means of which I tried to localize those points on the abdominal wall to which pain would be referred when violent overaction and cramp of the colon were provoked. I introduced a rubber bag into the rectum, inflated it very rapidly to over-distension, and I found that in the fifteen patients on whom I made the experiment the result was violent pain in the suprapubic region, deep-seated discomfort over the sacrum, and also discomfort of a much more superficial description in both iliac regions. I chose the two iliac regions as the probable stimulation areas, and I am becoming convinced that through these areas it is possible to induce increased peristalsis in the colon. Keith's observations on the aggregations of Auerbach's plexus seem to me to suggest an explanation of the occurrence of such skin areas, in so far as they hint at a possible spinal connexion of a segmental nature with various parts of the intestine, and, at the same time, suggest strongly the direction of the paths by which pain may be referred from the intestine to various portions of the abdominal wall, and the paths by which an impulse from the skin may travel to produce a contraction of the stomach or intestine.

So far the hydrological element in this paper has been slight, but it seems to me that a study of those stimulation areas may be of great use to the hydrologist in the application of douches, and in the explanation of the effects of baths of all kinds and of electrical treatment on gastro-intestinal stasis. One has to remember that in the gastro-intestinal stasis which produces symptoms we are dealing with two conditions: an atonic condition of the stomach or intestine, with delay in the passage of the gastro-intestinal contents, and also with a microbic invasion of the alimentary tract, which may either precede or follow the existence of such stasis. Lavage of the stomach is a useful and effective way of removing infection, and the drinking of large quantities of water, especially water highly charged with certain salts, may be very useful

in washing out infection from the small intestine, or even from the large intestine. The Plombières douche is certainly in many cases an extremely efficacious way of removing infective material from the colon. If, however, lavage of these various kinds is used alone, there remains, after the infection has been removed, an atonic condition of the gastro-intestinal tract which will encourage reinfection. It should be the object of the physician not only to remove infection, but to endeavour to prevent its recurrence, and it is here, I think, that the use of baths and douches of various kinds forms an important part of the treatment of alimentary stasis. Baths and douches, however, must be employed in a careful and intelligent way, and I do not believe that it is sufficient to direct a powerful stream of water against the body to produce the maximum benefit. It is true that such a practice will undoubtedly increase the power of the skeletal muscles, which is an extremely important thing in intestinal stasis; but I believe that douches, directed particularly to the stimulation of the skin in the areas which I have already described, will produce a very much more powerful effect in restoring tone to the intestine than any more haphazard method.

One has to remember that in inducing contractions of the stomach and intestine by stimulating various areas of skin one probably acts by means of a true reflex. In the case of the stomach I have found that to excite this reflex repeatedly in a short time one has to employ stimulation of a very gentle kind, which must be intermittent. Ordinary massage of the abdomen is incapable of inducing the desired reflex action, and really heavy massage of the abdomen may, besides, be an extremely painful process. In a lesser degree the same remarks apply to the intestine; the stimulation must be delicate and intermittent. Personally, I use my fingers as the means of stimulating the reflex, and in the case of the stomach, at least at the beginning of treatment, this seems to be the best way, because one can observe by auscultation when a powerful contraction takes place, and cease stimulation for a time. It seems to me, however, that stimulation ought to be not only intermittent and delicate, but varied, even in the case of the stomach, so that after the patient has been treated for a time by digital stimulation, douches and baths might be employed with great advantage, not only to continue the stimulation, but to increase the power of the abdominal muscles. The douches and baths, however, ought to be of a somewhat different character from those at present in common use. I cannot imagine any sensation more horrible for a person with a neurotic temperament and hypersensitive body wall than to be suddenly

smitten under the fifth rib by a stream of water violently projected by an unfeeling bath attendant. Such a process may result in one powerful contraction of the stomach, but carried on even for a few minutes, may well induce cramp or sluggishness of the organ for some time afterwards. It is true that cutaneo-gastric reflexes can be produced by pain, but, while other methods remain, it is not wise to employ this as a therapeutic measure. In the treatment of gastric stasis, I would suggest the use of sprays of varying degrees of hot and cold water, or of that excellent institution, the under-water douche, in which the effects of bath and douche are combined. Similarly, with regard to intestinal stasis, the mere haphazard douching of the patient's abdomen is not enough. Douching, combined with ordinary massage, may do good in a certain number of cases, but in many it does positive harm. The tender abdomen must be treated tenderly: it is a known fact that rough handling of the right iliac region may induce serious discomfort in a person who has had appendicitis. I would suggest that in the treatment of intestinal stasis the various skin areas suitable for the purpose—in the right hypochondrium, below the umbilicus, in the right iliac region, and in the left iliac region—be stimulated *seriatim* by douches of a suitable kind, and that such treatment be followed up with advantage by the employment of various electrical currents. With regard to these technical details I speak with the deepest humility, and merely present to you the roughest possible sketch in the hope that someone may be able to fill in the details.

I should like to refer once more to the atonic and easily distensible stomach, which is so often found in conjunction with intestinal stasis. I have met many people to whom the drinking of vast quantities of saline water has brought severe discomfort, which has lasted long after the patient has left the spa at which he sought relief. I do not think that any patient should be subjected to treatment by the drinking of large quantities of water until an accurate estimate of the muscular power of the stomach has been made and until the atonic stomach has been restored to something like normal action. The Plombières douche, too, which is a very useful weapon in many cases, despite the humiliation which its employment brings to a proud man, sometimes fails in the case of patients of delicate stomach, both on account of the mephitic vapours produced during its use, and of the nausea which many patients feel during and after lavage of their rectum and colon. I have seen many patients who have been unable to tolerate



the use of the Plombières douche, and in all of them I have found a tendency to atony and distension of the stomach. Such atony and distension may be a primary fault or may be the result of the colitis. One must remember the fact that disturbance of the normal rhythm of contraction in one gastro-intestinal segment may be reflected to another. I would suggest, therefore, that any condition of atony or stasis in the stomach should be corrected before the persistent use of the Plombières douche is adopted.

I feel that I may have dwelt perhaps too long on the theories of the causation of gastro-intestinal stasis, but my excuse is that one cannot at the present day advocate any form of treatment without having the support of physiological and pathological observation.

## DISCUSSION.

Dr. NEVILLE WOOD: I have investigated the response of the hollow abdominal viscera to stimulation of the skin lying over them. By means of the phonendoscope I have verified the existence of such response in four regions—the epigastric, umbilical, and the right and left iliac—influencing severally the stomach, the small intestines, the cæcum with the ascending colon, and the sigmoid with the descending colon. I have found, however, that the reflex cannot be elicited, or at any rate demonstrated, in every case, especially at the first or second attempt. Difficulty arises with strong, healthy subjects, or when there is great tenderness on the one hand, or, on the other, much dilatation of a viscus with, presumably, atrophy of its tunics. Again, different kinds of stimulation are required in different cases, while resort to various devices may be necessary—for example, in gastropstosis the organ must be raised and supported by the hand. The correct technique for each case requires careful study, and sometimes the exercise of much patience, for what may be described as the education of the reflexes.

Dr. BUCKLEY (Buxton): Dr. McClure's work throws a flood of light on the important question of the treatment of gastro-intestinal stasis. I may lay claim to some experience of the effects of douching, and it now seems clear that the beneficial effects of abdominal douches applied under water along the course of the colon, as has hitherto been the custom, are not due to a mechanical effect on the colon itself but to the fact that the areas identified by Dr. McClure are necessarily stimulated in the operation. I hope to test this by localized douching when an opportunity occurs. Massage also, which has yielded such variable results when employed for constipation, may prove of much greater value if directed to the stimulation of these points. I have noticed that, when combined with faradism, massage often gives better results in the treatment of atonic conditions of the stomach and bowel than when used alone. Probably this may be explained by the faradic current acting as a more suitable stimulus than the deep kneading movements which are generally regarded as most desirable in massage of the abdomen. It has often occurred to me that such massage may readily do more harm than good.

Dr. FORTESCUE FOX: Dr. McClure's paper illustrates the service that the physician may render to the hydrologist, for the physician observes the later results of hydrological treatment at *many* health resorts. From the point of view of ætiology, which is always a fascinating and fruitful study, chronic ailments may generally be attributed to infection or to nervous disturbance, or to a combination of both. The surface treatments of hydrology are founded upon reflex actions, and therefore an exact knowledge of reflex actions is of much value to the hydrologist. The same may be said of the balance of nervous

actions to which Dr. McClure has alluded. It is a fact, too often overlooked, but in need of emphasis, that there is in health and disease a delicate functional balance between the skin and the internal organs. The skin has a close functional relation with the gastro-intestinal mucous membrane and with the kidneys (according to Dastre and Morat's law), and finally with the highest nervous centres. All these parts are derived from the same embryonic layer, and those who practise surface treatments often observe the effects produced upon these related parts. Dr. McClure's reference to the value of *gentle* cutaneous stimulation, which has been rather neglected at the health resorts in this country, is most appropriate and welcome. Many years ago I studied the effects of douches *without pressure* at Aix-les-Bains. With this douche, water is poured very gently over the recumbent body of the patient at a temperature a little below blood heat. It was long ago observed that this form of thermal and very gentle mechanical stimulation produced a special effect upon the circulation and nerve centres, partly stimulant and partly sedative. There is no doubt that many favourable reflex actions can be far better produced by gentle means of this kind than by a more powerful stimulation, and I hope that as a result of this discussion British hydrologists will extend their observations in this fruitful field of inquiry.

Dr. CAMPBELL THOMSON: From Dr. McClure's investigations it appears that in seeking to stimulate the walls of the alimentary canal two distinct points must be borne in mind—viz., (1) the particular areas over which stimulation to be effective must be made, and (2) the nature of the stimulation which it is necessary to employ. The latter would seem to be a very important matter, and while agreeing that a strong or crude stimulus, such as arouses muscular contractions of defence, is not likely to be successful, I think that further inquiry is necessary in order to determine the nature and intensity of the particular stimulation that is likely to produce the maximum effect over a given area.

Dr. LEONARD WILLIAMS: With much of Dr. Campbell McClure's paper I cordially agree; from the view expressed in his closing sentence I differ. As a clinician, I decline to be held in check by the laboratist. The physiologist will tell you that venesection is useless, and propounds paradoxes about bleeding a man into his own blood-vessels. Experience tells me that venesection does good. The laboratist will tell you that the skin has no absorptive power. I know that it has, and I shall continue to cure syphilis by the inunction of mercury. In saying this, however, I do not belittle the work which the physiologist has so often done by pointing a better way to the clinician. Of this there could be no finer example than the masterly researches into the motors of the intestinal tract conducted by Professor Keith and unveiled by him in his Cavendish Lecture. They must necessarily colour our conception of the immediate causes of intestinal stasis and modify our hitherto accepted treatment. They point the way to the discovery and correction of the individual peccant motor. Dr. McClure appreciably advances us along

this path, and his appeal to the practising hydrologist is both pertinent and timely. It is the logical hydrological outcome of the new gospel. That the microbe is a result of stasis is accepted as a matter of course. That does not surprise me any more than a dandelion growing on a dung-heap. What we want to know is why the dung-heap is at the roadside or in the intestines. You might as well believe that the dandelion produces the dung-heap as to suggest that *Bacillus coli communis* causes the stasis. We seem to have forgotten the Parable of the Sower, which tells us that the success of the seed implies suitability of the soil; improve your *milieu* by irrigating it with sound sanguineous fluid, and the microbe dies. And the way to secure sound sanguineous fluid is to take care of the internal secretory glands, those elusive tributaries of the liquor sanguinis upon whose proper balance so much depends. In the case of the abdomen, the glands which are presumably of the greatest moment are the members of the chromaffin system, with the adrenals at their head. If we would study the intestinal motors and their "petrol" supply, we must not forget the association of these glands with the sympathetic system, nor the power of their secretion over unstriped muscle. Nor is it ever wise, in connexion with the ductless glands, to lose sight of their interdependence. A lazy chromaffin system yielding a meagre tribute to the blood-stream and causing stasis may be lazy because it lacks the wonted stimulus from the pituitary, the ovary or the testicle. It may be necessary to look far beyond the obvious offender to find the gland round which the disturbance of function is stealthily gyrating. Until we learn to include in the indictment the ductless gland or glands which are failing to deliver the necessary "petrol," we shall fail of our larger purpose.

Dr. R. A. YOUNG: Dr. Campbell McClure's paper is an example of the opportunities still afforded for careful original clinical observation, and is also an illustration of the fact that the physician's researches sometimes point the way to fruitful fields of investigation for the physiologist. I have long been convinced of the value of the method of cutaneous stimulation of the abdominal walls according to the method introduced by Dr. Percy Mitchell and further investigated by Dr. McClure and Dr. Wethered. I have seen very good results from its use in cases of atonic dilatation of the stomach and concur in the opinion expressed that it is possible for patients to employ the method themselves if it be explained to them and they be gifted with sufficient patience and persistence to employ it. The areas described by Dr. McClure as causing contraction in the duodenum and intestine are new to me, and I shall be very interested if Dr. McClure in his reply will give some further details as to the method of the stimulation he employs and its direction. I quite agree with the suggestions made for the utilization of these areas by the hydrologist in his special forms of treatment, but I should like to know if any X-ray observations of opaque meals have been carried out with a view to confirming the effect of stimulation of the areas described upon the duodenum and intestine; and also whether any cases of intestinal stasis have yet been treated by this method.



I am of opinion that the method is not so well known as it should be for the treatment of atonic dilatation of the stomach, and I regard it as unfortunate that some descriptive name has not been applied to it.

Dr. CAMPBELL MCCLURE (in reply) : I thank the Section for their kind reception of my paper, which is admittedly suggestive rather than final. The work mentioned is raw and incomplete, but from the tone of the discussion I gather that it may be helpful. I am much interested in the fact that Dr. Neville Wood has been able to confirm all the skin stimulation areas but one. It is interesting and encouraging to find that experienced hydrologists like Dr. Buckley and Dr. Fortescue Fox agree with the principles of light and selective stimulation of the abdomen as opposed to the more haphazard methods. In reply to Dr. Campbell Thomson, I believe that the more careful study of the defence reaction of muscles will tend to confirm the opinion that there is a definite connexion between the various parts of the gastro-intestinal tract and the abdominal wall ; and I am sure that the aggregations of myenteric plexus described by Keith and Alvarez will furnish the basis of an explanation of such a connexion. I concur with Dr. Campbell Thomson in his view that a more definite localization of the skin areas is very necessary, and that it is equally necessary to make further experiments on the best methods of stimulating these areas. I quite agree with Dr. Williams that at the back of the whole condition of gastro-intestinal stasis there is probably a disorder of one or another of the ductless glands, and that more knowledge of the interdependence of these glands is of the utmost importance in the study of disorders of innervation of a functional kind. With regard to the remarks made by Dr. Young, the direction of stimulation of the areas in the right hypochondrium, below the umbilicus and in the iliac regions, is from above downwards, and so far I have employed only digital manipulation in the stimulation of these cases. As Dr. Young has suggested, some importance may attach to the direction of such stimulation. I have treated successfully one case of colic stasis in which the results were confirmed by an opaque meal and X-ray examination.

## Balneological and Climatological Section.

President—Dr. WILLIAM GORDON.

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(*March 2, 1916.*)

### The Thames Valley : Certain of its Natural and Medical Attributes.

By S. D. CLIPPINGDALE, M.D.

AFTER acknowledging the valuable help received from the following Medical Officers of Health—Dr. Alfred Greenwood (Kent), Dr. Harry Jones (Kingston-on-Thames), Dr. Frank Laurance (Cricklade), Dr. J. Middleton Martin (Gloucestershire), Dr. James J. Paterson (East Berkshire), Dr. Charles Grant Pugh (Southend-on-Sea), Dr. William Sisam (Berkshire), Dr. John C. Thresh (Essex), Dr. John Tubb Thomas (Wiltshire), Dr. Herbert Williams (Port of London), and the Medical Officer for Oxfordshire—Dr. Clippingdale said :—

The River Thames is, and some of its tributaries are, possessed of certain attributes which seem to have a definite bearing upon the health of the riparian dwellers. The subject, I think, has not been dealt with before in a concrete form, and seems a fitting one to bring before this Section of the Royal Society of Medicine. I regret, however, that from want of a deeper knowledge of the subject I can only do so in a very imperfect manner.

*Definition.*—First it is necessary to define the River Thames, for, strange as it may seem, the identity of our metropolitan stream is by no means beyond dispute. Until recently that part of the river which proceeds westwards from its junction with the Thame at Dorchester was known as the Isis. It was so designated by John Leland in the time of Henry VIII, by Edmund Spenser in the time of Queen

Elizabeth, and by Izaak Walton in the time of the Commonwealth. The Isis after its junction with the Thame was called by the Romans "Thamesis," and upon an old map of which I have a copy, it is spelt as a hyphenated word. The Isis is so named upon all maps published down to thirty years ago and later, and is still so called by the people of Oxford and Oxfordshire. Of late, however, an edict has come forth from those who are supposed to know, and founded upon some words in a Saxon charter, that the name Thames applies to the entire length of the river, and that the river Thame is a tributary.

*Source.*—The Thames has a great many springs which claim that honour. Those who attended the meeting of the British Medical Association at Cheltenham, in 1901, will remember being taken to a place in the parish of Charlton Kings called the "Seven Springs," over which is the inscription:—

"Hic Tuus O Tamesine Pater Septemgeminus Fons."

*Length and Fall.*—From the Seven Springs to the North Sea the Thames runs a course of about 250 miles. The head of the river is 600 ft. above its mouth, so that its fall is about 1 in 2,000—i.e., 21 in. to a mile.

The *width* of the river at Oxford is about 100 yds., at London Bridge 260 yds., at Gravesend 800 yds., and at the Nore 5 miles.

The *area* of the Thames basin is 6,160 square miles, equal to about one-seventh of the whole of England.

The *amount of water* in the river may be imagined when it is remembered that 100,000,000 gal. pass Lechlade and 380,000,000 gal. pass Teddington in one day.

As to the *age* of the river, Professor Huxley [14] estimates that the chalk basin alone has taken 7,200 years to form, and that the entire history of the river is a matter of hundreds of thousands of years. "There is clear evidence," he says, "that the Thames Valley was the haunt of savages armed with flint weapons, and that elephants, rhinoceroses, bears and hyenas roamed through its forests."

*Tributaries.*—The Thames has about thirty tributaries excluding those which, like the Fleet and the Tyburn, have been covered over and converted into drains. The tributaries on the right or southern bank are the Swill, the Cole, the Ock, the Pang, the Kennet, the Loddon, the Chert, the Wey, the Mole, the Beverley, the Wandle, the Ravensbourne and the Darent. The tributaries of the left or northern side are the Churn, the Gloucestershire Colne, the Leach, the

Windrush,<sup>1</sup> the Cherwell, the Thame, the Wye, the Middlesex Colne, the Brent, the Lea, the Roding, the Bourne and the Ingreburn. The streams of the right bank take origin from three ranges of hills from west to east—namely, the Wiltshire Downs, the Surrey Hills and the North Downs. Those on the left bank also form three ranges—the Cotswold Hills, the Chiltern Hills and the Laindon Hills. Besides the surface streams, the volume of water is considerably increased by certain springs in the bed of the river. In addition to the actual rivers, certain dry water-courses are found in both the right and the left watersheds. These represent rivers which formerly drained into the Thames but have been cut off by the rising crest of the watersheds and now drain into other rivers. To this process the term “decapitation” has been applied by geologists.

One river I have not had an opportunity of mentioning before, and should like to refer to now, has a point of medical and historic interest. This is the Lea, which separates Middlesex from Essex. At the time of the Great Plague (1665-66), not only was the City of London infected, but also the roads leading to it, and the plague-stricken inhabitants were threatened with starvation. The good people of Ware, in Hertfordshire, however, using the Lea as a highway, were able to send food to the distressed citizens. In recognition of this benevolent act, all craft trading from Ware are allowed to the present day to enter the Port of London duty and toll free.

*Sanitation.*—All rivers are, of course, liable to abuse. Before the establishment, in 1855, of the Metropolitan Board of Works, the Thames was used as a common cloaca. Its condition then is, perhaps, best described in the report to Parliament in 1836 of Lord Euston's Committee. The Metropolitan Board of Works, however, by constructing arterial drainage, with outfalls far down the river, and the various water companies, by taking their inlet far up the river, have made the Thames water fairly pure.

*Geology* (see diagram, p. 36).—The structure of the Thames basin is interesting geologically and important medically. Details of its geology will be found in the works of Huxley [14], Jordan [15], Phillips [18], Prestwich [19], Whitaker [21] and Woodward [22]. It must suffice here to give a skeleton sketch of the subject and to indicate its points of medical interest. Every river must, of course, be held up by a “basin”

<sup>1</sup> The water of the Windrush contains some ingredient which is said to produce the peculiar whiteness of the blankets manufactured at Witney, a town which stands upon its course.



of some kind, otherwise its water would percolate and disappear. The Thames is held up by two basins which fit one into the other like two table basins of different sizes. The upper of these basins is of clay, the lower of chalk. Between the two is a deposit of what are called "Lower London Tertiaries." Upon the clay basin is a layer of gravel, and above this is a layer of alluvial soil. The lower basin, of chalk, rests upon solid rock.

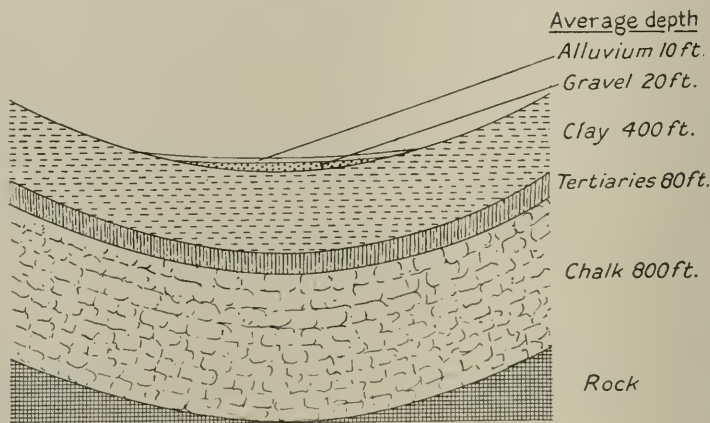


FIG. 1.

Diagram of the Thames basins.

With regard to these different deposits, the uppermost, or *alluvium*, is simply material which has been washed down from the watershed, and naturally varies in different places. In the west it is of a chalky nature; in the east it is a rich loam. The thickness of the alluvial soil is from a few inches to about 10 ft.

The *gravel* which underlies the alluvium is what is known as "valley gravel," but is structurally the same as other gravels. Medicinally it is of interest as containing an oxide of iron and a little arsenic. The depth of this gravel varies from about 5 ft. to about 50 ft. It is into this gravel that the wells were sunk which were so graphically described by our late President, Dr. Septimus Sunderland, in his inaugural address, an address which, happily for those who did not hear it, has now been reprinted in book form [20]. In some places the gravel is replaced by *brick earth*, a soil, says Huxley, very suitable for the growth of vegetables and, as doctors know, very productive of rheumatism.

Beneath the gravel lies the *clay*. This contains fossils, and is really a marine mud. The depth of the clay in the London district is about

400 ft. It gets thinner towards the mouth of the river, and is absent between Windsor and Maidenhead. Otherwise it underlies the Thames everywhere, but bears different names. In London we call it "London clay," in Oxfordshire it is known as "Oxford clay," while up the little River Thame it is called "Kimmeridge clay." To a scientific geologist there is apparently a difference between these different kinds of clay, but to an amateur very little, and, from a sanitary point of view, none.

The ridges of the Thames clay basin form what are called the northern and southern heights of London, and are sometimes 400 ft. above sea-level, so that the highest and consequently the healthiest parts of London are upon clay. The soil lying next or near the water-course is always either gravel or alluvium. These matters I indicated in a paper I had the honour of reading before the Balneological Society [5]. The paper excited a good deal of criticism, as it presented facts in opposition to accepted views. I referred, however, to the Registrar-General's statistics and to the geological map of London, and there I left the matter. The fallacy, for such I regard it, as to the supposed superiority of a gravel soil is no new thing. Recently I came across the following couplet in Garth's "Dispensary" (Canto iii), published in 1699:—

"The sick in hundreds sooner shall repair  
And change the gravel pits for Essex air."

This couplet is written in satire, and infers that it is better to live in the Gravel Pits region of Notting Hill than in the bracing air of Essex, whereas the experience of any tuberculous patient will teach us the contrary.

The clay is capped in some places by Bagshot sand left by a primeval flood. There is a small patch of this sand at Hampstead. Formerly this sand was carried down to London and spread upon the floors, but it was found that it was washed into the river, which it tended to silt up, to the extent, it is said, of 2 ft. in fifty years, and the practice was stopped by Act of Parliament in 1760.<sup>1</sup>

Below the clay, as already stated, is a layer of *Lower London Tertiaries*. This is a layer 80 ft. thick. Its composition varies. It usually consists of Reading and Woolwich beds, but towards the estuary of the Thames there is also an addition of Thanet sand. It is into the Lower London Tertiaries that an artesian well must be sunk to collect the water that has gravitated down from the Chiltern Hills on the north

<sup>1</sup> *Notes and Queries*, January 15, 1916.

and the North Downs on the south, and lies upon the chalk. An artesian well, therefore, to pass through the clay must be at least 400 ft. deep, and is said to cost £1 a foot to make.

The *chalk*, the lowest of the Thames basins, is about 800 ft. thick in the London area, but less thick elsewhere. It rises to the Chiltern and Cotswold Hills in the north and to the Wiltshire and North Downs in the south. Its structure varies. In the Chiltern Hills it is laminated, but in the Cotswolds it resembles masses of fish roe, and so is called "oolite."

The *width of the Thames Valley* varies considerably from the narrow gorge of Streatley and Goring, which is less than half a mile, to the wide expanse of 40 miles between Dorking and Dunstable. The river was formerly much wider than it is now and extended to the chalky crests of its watersheds. At a period still more remote, the Thames was a tributary of the Rhine.

As the centre of the earth's gravity has shifted, so the Thames has been drawn nearer the North Pole at one time and repelled from it at another. Hence in some of its deposits are found the remains of Arctic animals, and in others the vestiges of tropical plants. The presence of peat in some places is evidence of submerged forests. Pepys, the diarist, states that among the things "which did surprise him" was that workmen in digging a dock at Blackwall came upon a tree standing upright with nuts upon it.<sup>1</sup>

Oscillation of the Thames Valley is still going on. In 1660 the magnet in London pointed due north and south. Then there was a gradual declination to the west, which reached an extent of 25° in 1818. Since 1818 the needle has been gradually returning to its usual position.

The amount of *rainfall* varies. As might be expected, it is greatest in the western part owing to the close proximity of the hills, and lowest in the eastern part, where the surface is flat. The amount of rainfall at various places in the Thames Valley is shown in the accompanying diagrams (*see p. 39*).

*Tides*.—Below Teddington—that is, for a distance of 77 miles—the water is tidal. The water flows and ebbs twice in the twenty-four hours, about five hours being spent in the flow and about seven in the ebb. Vagaries in the tides, however, are on record. In 1638, for instance, the river flooded twice in three hours, three times in four hours on March 22, 1682, and again twice in three hours on

<sup>1</sup> "Diary," September 22, 1665.

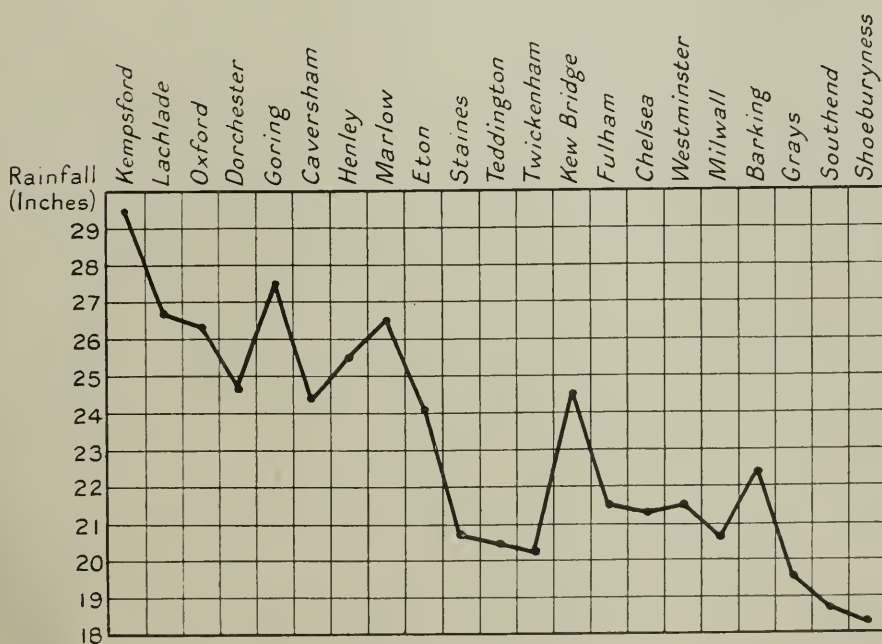
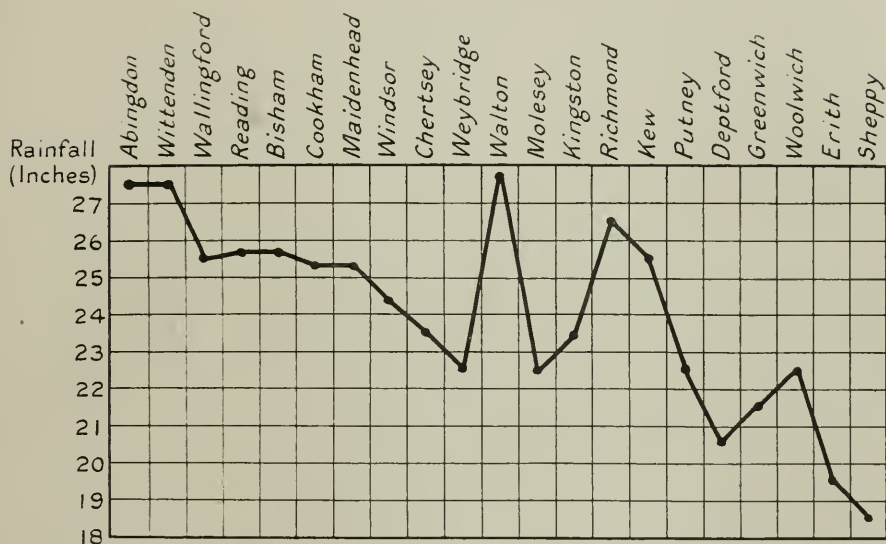


FIG. 2.

Diagrams showing Thames Valley rainfall.



November 24, 1777. The tides vary twice monthly, and are known as "spring-" and "neap-" tides—the former, in which the water is highest, occur at the new or full moon, the latter, in which the water is lowest, occur during the moon's second and fourth quarters; the spring-tides being due to the attractive forces of the sun and moon acting in a straight line, the neap-tides when these forces act at right angles.

*Floods, Frosts and Fogs.*—The Thames, like other rivers, has often been guilty of eccentricity in these matters. It has often overflowed its banks, especially during a spring-tide with a heavy rainfall. The earliest recorded flood was in 1235, when lawyers and litigants had to be taken from Westminster Hall in boats. Other historical floods have occurred in 1736, 1748, 1762 and 1791. Since the formation of embankments, however, the river, although it occasionally rises to a great height, does not overflow its banks so far as London is concerned. In the western part of the river, however, disastrous flooding occurs. In the eastern part of the river the water is kept from the Essex marshes by a sea wall, stated by Sir Walter Besant [2] to have been put up by the Romans. Frosts were formerly more common than in recent times. In 1063 the Thames was frozen over for fourteen weeks. In 1434 it was frozen from London to Gravesend. In 1684 the ice on the river was 11 in. thick, a fair was held on the river, and forty coaches plied daily up and down its course. In the winter of 1716 an ox was roasted on the river. The last severe frost of this kind occurred in 1814. Fogs, although they arise from the surface of the river, are often denser on the hills around; the reason is that on ascending the hills they are kept down by a layer of dense cold air. Fogs, however, are now comparatively rare.

*Flora and Fauna.*—There is not much to record with regard to the flora except that at certain parts the temperature of the Thames Valley is so mild that subtropical plants can be grown. The river banks produce the usual poisonous plants, foxglove and others, so carelessly eaten by children and so carefully avoided by cattle. Upon the banks of the Thame, woad (*Isatis tinctoria*), says Mr. Cornish in his interesting book [8], flourishes to an extent sufficient to stain a whole British tribe. Watercress flourishes in the streams which come down from the chalk. Of the birds, the most interesting scenically are the beautiful swans, black or white, which add so much to the beauty of the river. They are all alien birds, and are carefully protected by the Crown. Seagulls, as is well known, find their way up the river at times through stress of

weather. They fly over the bridges, never through them, and settle themselves upon the fresh-water reservoirs at Barnes. Edible fish, especially trout, is found in great quantity in the upper part of the river and its tributaries. The shrimp is interesting as indicating, by the colour of the shell, the condition of the water. At the mouth of the river this colour is pink, at Gravesend it is brown and sometimes black. Before the pollution of the water, salmon flourished in great abundance near London, and the fishermen were wont to offer it as a tithe upon the high altar at Westminster. Since the repurification of the river many fish have reappeared, and whitebait has had the temerity to appear off Greenwich. Recently a porpoise made its appearance in the river, but, says Mr. Cornish, like a true London porpoise it halted opposite a public-house and was arrested.

#### HEALTH.

It is, I think, generally admitted that those who live in valleys are at some disadvantage as regards health. The reason of this may be difficult to trace. We are all of us conscious that throughout life we are under the influence of two forces, the "*vis vitæ*," which urges us to energy and the "*vis inertię*," which leads us to inaction. We feel the former force more before and the latter force more after middle age. The former asserts itself when we are upon a bracing hill, the latter when we are in a relaxing valley. Metabolism of our tissues is greater upon a hill than in a valley, consequently our power of resistance is increased and our opsonic index raised. In this way, it may be that dwellers in valleys appear to be more prone to illness than those who live upon hills. The Thames Valley, in common with all other river valleys, appears to exemplify this, and in the following brief accounts I will mention certain maladies which appear to be so influenced.

*Adenoid growths* and enlarged tonsils in certain parts of the Valley are above the average rate.

*Ague*.—In the last paper I had the honour of reading before this Section [7] I stated there was a malady formerly prevalent known as "London ague," and that Oliver Cromwell, Charles II, William III and Queen Anne had suffered from it. The disease, however, with the introduction of arterial drainage and the closing in of many small streams which formerly traversed London, has disappeared. More recently ague was common on the Essex marshes, and when, in 1877,

I was House Physician at the London Hospital we admitted several cases of it. Dr. Thresh, the Medical Officer for Essex, however, informs me that through the better drainage of these marshes the disease has almost disappeared.

*Anæmia*, independent of organic disease, as might be expected, is common in some of the more closed-in parts of the Thames Valley.

*Cancer*.—The incidence of cancer is a difficult subject. We are all acquainted with the views and statistics of Alderson [1], Clemow [3], Haviland [12] and Hirsch [13]. Two medical officers in the western part of the Valley and two in the eastern part report not only super-normal frequency of the disease, but also a tendency to its increase. Some of my correspondents attribute the high mortality from cancer to the great longevity prevalent in their respective districts, but I must confess I was not aware that cancer was a malady especially frequent in old age. In a paper upon the Chiltern Hills which I read before this Section some years ago [6], I stated I had found cancer very frequent in the Chiltern valleys, and that it seemed to be of a tribal or family character. I would venture to suggest that this may be the case in certain parts of the Thames Valley. The question of the so-called "cancer houses," often not disinfected, may also be considered.

*Enteric Fever*.—Dr. Thresh, in his medical report for Essex, states that typhoid fever used to be most prevalent in South Essex, bordering the Thames, but it was proved to be due to people eating shellfish collected on the foreshore. When this was known the typhoid-rate dropped until it is little, if any, higher than elsewhere in that county. On the other hand, Dr. Pugh, Medical Officer for the independent borough of Southend-on-Sea, complains that he still has this difficulty to contend with, and that people eat largely of the cockles, mussels and other mollusca which are found at Leigh and around Canvey Island.

*Fibrositis*, so well described by Dr. Jones Llewellyn, is prevalent in both the upper and the lower parts of the Thames Valley; supposed, in the upper part, to be on account of the frequent inundations, and in the lower part to be on account of exposure to the keen winds of the North Sea.

*Goitre*, as might be expected, is prevalent in the valleys of both the Chiltern and the Cotswold Hills.

*Tuberculosis*.—Perhaps one of the most instructive papers upon the

ætiology of phthisis was that contributed to the *British Medical Journal* a few years ago by our President, Dr. Gordon, upon the influence of rain-bearing winds in developing phthisis. His paper [11], I think, only referred to Devonshire, but his observations apply equally to the upper part of the Thames Valley, which is traversed by the same rain-bearing winds.

All my correspondents in the upper part of the Valley report a prevalence of phthisis apart from the question of infection, seeming to show that the prevalence is due to local and not to personal causes.

Towards the Thames Estuary, however, the disease becomes rarer, due no doubt to the purer air breathed by the inhabitants: air that has been purified by its contact with the ice around the North Pole and charged with the salt it has picked up in passing over thousands of acres of ocean. In this connexion it is interesting to note that the Essex marshes, "saltings" as they are called, produce excellent beef and mutton, which may be compared with the superior mutton which the French describe as "*pré salé*."

*Violent deaths* are above the average in all parts of the Thames Valley, the reason being, no doubt, as pointed out by Dr. Middleton Martin [16], the presence of the river as conducive to death by drowning, accidental, homicidal, or suicidal.

#### THERAPEUSIS.

In a district the climate of which varies so greatly as does that of the Thames Valley in its long range of 250 miles, it is obvious that cases which are suitable for one part of it are unsuitable for another. For the anæmic or tuberculous, residence near the mouth of the river is preferable, or failing residence, frequent trips down to the estuary. But for the overworked student or brain-fagged man of business a stay at one of the quiet villages up-stream or frequent trips upon the upper river steamers will be found to provide a very effective scenic and atmospheric "bromide."



## REFERENCES.

- [1] ALDERSON, JAMES. *Trans. West Lond. Med.-Chir. Soc.* (1895-86), 1887, ii, p. 81.
- [2] BESANT, Sir WALTER. "The Thames," 1903.
- [3] CLEMON, FRANK G. "Geography of Disease," Camb. Univ. Press, 1903.
- [4] CLIPPINGDALE, S. D. "West London Rivers, Extant and Extinct," *West Lond. Med. Journ.*, 1909, xiv, pp. 1-21.
- [5] *Idem.* "Clay and Gravel Soils of London and the Relative Advantages of Dwelling upon them," *Brit. Balneo. Journ.*, 1902, vi, pp. 14-24 (Discussion, pp. 44-57).
- [6] *Idem.* "The Chiltern Hills and Dales," *Proc. Roy. Soc. Med.*, 1910, iii (Balm. Sect.), p. 45.
- [7] *Idem.* "London as a Health Resort," *Proc. Roy. Soc. Med.*, 1914, vii (Balm. Sect.), pp. 33-42.
- [8] CORNISH, C. J. "A Naturalist on the Thames," London, 1902.
- [9] DAVIES, A. H. "Geology of the Thame Valley," *Proc. Geol. Assoc.*, xvi.
- [10] EWART, WILLIAM. "Climates and Baths of Great Britain" (Royal Medical and Chirurgical Society), 1902, ii, pp. 1-80.
- [11] GORDON, WILLIAM. "Influence of Strong Prevalent Rain-bearing Winds on the Prevalence of Phthisis," 1910.
- [12] HAVILAND, ALFRED. "Geographical Distribution of Disease," Lond., 1892.
- [13] HIRSCH, AUGUST. "Geographical and Historical Pathology," New Sydenham Soc., 1886, iii, p. 502.
- [14] HUXLEY, T. H. "Physiography" (Thames Basin), Lond., 1878.
- [15] JORDAN, JAMES. "Geological Model of London and Suburbs" (Stanford), Lond.
- [16] MARTIN, J. MIDDLETON. *Proc. Roy. Soc. Med.*, 1915, ix (Epid. Sect.), p. 27.
- [17] MILL, H. R. "British Rainfall," Lond., published annually.
- [18] PHILLIPS, JOHN. "Geology of Oxford and the Thames Valley," Oxford, 1873.
- [19] PRESTWICH, JOSEPH. "Water-bearing Strata around London," Lond., 1895.
- [20] SUNDERLAND, SEPTIMUS. "Old London's Spas, Baths, and Wells," Lond., 1915.
- [21] WHITAKER, WILLIAM. "The Geology of London," Lond., 1901.
- [22] WOODWARD, HORACE B. "Soils and Subsoils of London," Lond., 1897.

See also Cassell's "Rivers of England" and "The Thames and its Story, from the Cotswolds to the Nore"; S. C. Hall's "Book of the Thames"; Charles Mackay's "Thames and its Tributaries"; the "Victoria County Histories," and Salter's "Guide to the Thames."

(*March 2, 1916.*)

## **Report of Committee of Council: A "Combined Physical Treatment."**

ON behalf of the Council Dr. FORTESCUE FOX reported as follows:—

A memorandum on the value of medical baths in the treatment of wounded and invalid soldiers was addressed to the Army medical authorities on December 26, 1914. It did not emanate from this Section, but from a number of representative medical men. It pointed out in the first place the extensive use of medical bath establishments for the military in Continental countries. It showed the great need for hydrological and other physical treatment for the men leaving hospital; that large numbers of these, even those labelled "incurable," could be, if not cured, at least greatly relieved, by physical methods; and that bath treatments in particular were suitable both for surgical and medical cases, and could be employed with advantage not only at the spas, but at the military hospitals and elsewhere in the great towns. The signatories also called attention to the scientific value of a simple and uniform system of case records, which should record the physical treatment of disabled soldiers—a work which had not hitherto been attempted in the health resorts of this country.

In January, 1915, the matter was discussed at a special meeting of this Section, at which Surgeon-General Russell, D.D.G., A.M.S., was present. Agreeably to his proposal the Council thereafter proceeded to appoint a Committee to prepare information for the Army Medical Service. To its great advantage it numbered among its members Surgeon-General Russell and the President of the Society, and Dr. Frederick Taylor. On behalf of my colleague, Dr. Campbell McClure, and myself, I have now to report the result of its work during the last fourteen months. As regards finance, the out-of-pocket expenses of the Committee amount to about £60.

In the first place a pamphlet was prepared for circulation among the military hospitals. It comprised: (1) a brief statement of the hydrological treatment appropriate to military cases, both medical and surgical, and (2) an account of the British spas and health resorts, showing the disorders which they benefit. In the preparation of this pamphlet, of which 300 copies were circulated, and in other matters, the Committee have had the assistance of many of the Fellows resident at the health resorts. Local medical committees were formed in the principal localities, and representatives appointed in others, to co-operate with the Committee in London. It was thought that this organization would help to solve the problems that

confront us in the war emergency, both by way of co-ordinating hydrological work in Great Britain and by assisting in the proper distribution of military cases. The Committee regret that these good results are so far not fully realized, partly, no doubt, from the novelty of their proposals and partly in consequence of the absence on military duty of so many of our more energetic and experienced Fellows.

The next undertaking was the preparation of a simple system of records by means of military case record cards; 10,000 of these were distributed among the health resorts in March, 1915, and a second issue of 10,000, with improvements suggested by experience, has been recently made. In the preparation of these cards Dr. Brownlee, of the Statistical Department of the Medical Research Committee, kindly made valuable suggestions, in order to bring the health resort records into conformity with the official records for the forthcoming "Medical History of the War." The Committee believe that a section of this history dealing with the physical treatment of military cases at the British health resorts and elsewhere cannot fail to be of permanent value as a contribution to British hydrological medicine. Here again they were opening new ground, and the response from some localities has hitherto been disappointing. Whilst all the health resorts with a praiseworthy effort have freely offered their treatment to wounded soldiers, and very large numbers have been already treated with much success and often free of charge, we have reason to fear that records from some of the most important localities will not be available for a large proportion of these cases. It is stated, for example, that at Harrogate 700 men were treated up to the end of 1915, but no case record cards have up to this date been returned from Harrogate to the Royal Society of Medicine.

In April, 1915, the Committee were informed that a system of "combined physical treatment" for wounded soldiers had been installed at the Grand Palais in Paris. Thereupon I visited Paris, and with the courteous assistance of the officials, and particularly of our colleague Dr. Quiserne, examined the hydrological and other methods employed at the Grand Palais Hospital. The application of very high temperatures to the limb in a "whirl bath" (*balnéation à l'eau courante*) appeared to me to be a method of much value, especially as a preparation for massage and mobilization of the joints. Full accounts both of the methods and of the results obtained have since been furnished to us from time to time, with statistics, and these form the basis of the report which was published by the Committee in the *Lancet*, February 5, 1916 (p. 311). The President of our Section has since that date made a special visit of inquiry to Paris. If only a part of the happy results that are claimed to follow from the "combined physical treatment" of wounded soldiers is in fact realized, it appears to the Committee that these methods are justified, and would prove of inestimable value in this country, not only from the medical and humanitarian but from the economic point of view. The saving to the State in pensions and gratuities by the reduction of disability of disabled men in France is stated to amount to a very large sum.

In recommending physical treatments, we have been asked why we have departed from the province of balneology or hydrology. It is a sufficient answer to indicate that this Section has never regarded the scientific study and use of physical agencies as alien to its scope and purpose. Health resorts exist for the application of physical treatment, and in addition to waters and baths many methods are employed accessory or adjuvant to hydrological and climatic treatment. Therefore, in recommending to the Army medical authorities the systematic employment of combined physical treatment for disabled soldiers this Section is on familiar ground. No section of medicine can operate in a water-tight compartment, and least of all one devoted to the interests of hydrology.

At the present moment there are within the hospitals, or leaving the hospitals, in numerous convalescent homes and in their own homes, thousands of disabled men, for whom we believe a combined physical treatment affords a hopeful and the only hopeful means of recovery and of return to peaceful avocations. To meet this great need an altogether unique extension of what may perhaps be called "orthopædic" work has become necessary. Without forgetting the achievements of surgery, we believe that in this particular field physical treatment ought to play a predominant part. It should take up the work which comes to an end in the hospital, and in multitudes of cases should prevent or minimize permanent disability. As regards the British health resorts, they are doing a magnificent work, but the lack of records is, and will prove to be, in our opinion a serious evil. But in the present emergency we are all feeling our way, and I am sure there is no disposition to neglect what can be shown to be a necessary duty. We also acknowledge with much satisfaction that most valuable physical treatment is and will be available for disabled men at the large convalescent camps and command depots, in regard to some of which we have been asked to advise.

But, when the full extent of this provision has been made there still remains a great residuum (how much greater may it not become?) of men who will not have the benefit of the health resorts nor yet of the convalescent camps, many of them being discharged and returned to their own homes. For all these, both officers and men, the Committee are of opinion that a combined physical treatment should be provided, and that it might be well done, as in France, in institutions devoted to that purpose. Such institutions would not be hospitals for in-patients but rather clinics or out-patient departments, where whirl and other baths, electricity, massage and mechanical treatment would be applied under the best auspices and for the requisite period of time. In extending their view beyond the health resorts proper, and recommending as they have done that the fullest possible advantage should be taken of physical remedies for disabled officers and men in London and in the country generally, the Committee are confident that they have the approval of the Section.



(March 2, 1916.)

## Some Observations on the Treatment of Disabled Soldiers by the Physico-therapeutic Methods now being used in Paris.

By WILLIAM GORDON, M.D. (President).

OUR Secretaries have asked me to give you some brief account of a recent visit to the Grand Palais Hospital and to other establishments in Paris where physico-therapeutic methods are being used for the reduction of disability produced by wounds.

Hearing that I wished to go to France to see medical cases in our own military hospitals, Dr. Fortescue Fox suggested that I might be of service to the Section if I visited the Grand Palais and its Annexe in Paris to ascertain officially certain facts for our Special Committee. This I willingly undertook to do. Thanks to the courtesy of the French authorities, and to the untiring kindness of Dr. Quiserne and Dr. Camus, the medical officers supervising the treatment at the Annexe and the Grand Palais respectively, I was afforded every facility which time permitted for observing the working of both institutions.

### CONCLUSIONS.

It should be clearly borne in mind that this system of treatment—for it is a system—aims at three objects—viz. :—

(a) To hasten the return of the wounded to their units at the Front.

(b) To reduce expense to the State by lessening the disability entailed by wounds.

(c) To reduce impairment of civil industry, after the War, occasioned by the numbers of seriously and permanently crippled men.

Into (a) and (b) I inquired very carefully, and am convinced that on these two counts alone the undertaking has been amply justified. For (a) half the patients treated for actual disability entailed by wounds return to their units at the Front—the duration of a “good case” being perhaps one to two months, the average reduction of disability for all cases being 20 per cent. And as to (b) there has already been a great saving effected by the State in pensions and gratuities, so that if this continues at its present rate (and there is no apparent reason to the contrary) the annual economy effected should amount to about 50 million francs.

(c) What the reduction of impairment of civil industry after the War will be may be reasonably deduced from our conclusions regarding (a) and (b).

Now if these conclusions are sound it will be at once apparent to the Section that we are dealing with a practical problem of no common sort, carrying with it important consequences—military, financial, and social. I shall therefore carefully lay before you the grounds upon which my conclusions rest.

#### GROUND FOR CONCLUSIONS.

Special facilities, as I have said, were afforded me for inquiry and observation, and the courtesy of the authorities enabled the officers entrusted with the treatment to talk to me freely regarding their methods, results, expenses, and economies. These officers were unanimous in claiming for the system a value which no other mode of treatment possesses for this class of case, and (with the aid of the all-important mensuration which the system includes) a reduction of disablement and consequent reduction of expense to the State from pensions and gratuities not otherwise obtainable. Abundant confirmation of these views, if such were needed, exists in the steady and rapid extension of the system throughout France. The Grand Palais began by treating 200 patients a day, now 650 electrical treatments alone are given daily; in and around Paris other installations have been set up, and in the provinces there are already seven fully equipped and satisfactory institutions of the same sort, as well as four which are less complete. At a time when economy and efficiency are vital national needs, public authorities do not extend work in this way for imaginary advantages.

The system in its essentials appears to me to consist in, broadly speaking:—

- (a) A preparation by heat, moist or dry;
- (b) A thorough course of massage, manipulation, mechanical treatment, and electricity;
- (c) And, finally, a re-education of the affected muscles by exercises and training.

The whole system is checked in its results by careful measurement of initial defects and ultimate gains.

Regarding the advantage of systematized massage, manipulation, and electricity, we need not stop to discuss what all recognize. A re-education of lost or weakened movements by exercise and drill under skilled instructors will, I think, appeal to the common-sense of everyone. So that the points on which I may perhaps say something are:—

- (1) The advantage of heat as a preparation for other modes of treatment.
- (2) The use of mechanical instruments for active and passive movements.
- (3) The use of mensuration of defect.

*Preparatory Heat.*—My own past experience has led me to place a high value upon it. I well remember my first case, which was that of a girl with double hydrarthrosis, whom I treated with back splints and Scott's dressing, with disappearance of effusion and fixation of both knees resulting. An able surgeon advised chloroform and forcible breaking down of the recent adhesions.

Acting on other advice, I packed both knees in hot dry sand for twenty minutes daily, with manipulation, and in a week this treatment had completely restored their mobility. Those were recent adhesions. The following demonstrates the value of heat in the kind of case we are discussing, but this was one of some standing: A soldier, whose arm had been torn by a shell, had rigid, painful fingers, and could not bear much massage or manipulation, so that little progress was being made under excellent surgical supervision. I advised baking the hand daily for twenty minutes in hot dry sand before manipulation. The massage became at once much less painful, the joints more supple, progress much more rapid, and ultimately a very considerable degree of movement was regained. There need, therefore, be no surprise at a claim that a system including preparatory heat gives better results than the too common practice of neglecting it.

*Mechanical Movements, Active and Passive.*—The so-called Zander system is probably known to everyone here. It has two obvious merits: First, it isolates for exercise or passive movement certain muscles or groups of muscles, and thus carries out a series of useful movements with a minimum of general fatigue. Secondly, it measures exactly the existing defect and the degree of exertion or passive movement prescribed.

*Mensuration of Defect.*—This is clearly of great importance from the point of view of assessment of claims, and when reinforced by an ingenious apparatus, devised by Dr. Camus for detecting malingering, that records on a revolving drum the tremor of exertion, a satisfactory basis for regulating pensions and gratuities is obviously afforded.

We have, of course, already effected considerable saving by ordinary methods in this country. But with the Grand Palais system I believe we should effect still more. Even if the system only accounted for 25 per cent. extra improvement, that would mean a very great saving of money annually, provided we avail ourselves of the methods of mensuration employed in Paris.

Lastly, the few results I saw in Paris seemed to me better than those usually produced at home. But here I would at once guard myself by pointing out that the observation of a few selected cases can of itself prove nothing. A few days' rapid survey of several institutions is not the way to acquire an experience worth recording. To indicate, however, the sort of thing I did observe: I saw one case of a recently healed wound with adhesion to the tibia and greatly swollen edges, which had been acutely tender before treatment, becoming quite free from tenderness after two weeks of radiant heat and light. I saw a scar on the arm which had been adherent to the bone, but had become quite soft, pliable, and freely movable over the bone beneath; I saw rigid, painful fingers massaged easily and with little pain under violet light and heat. Perhaps I was specially shown a dozen cases. In the time at my disposal I do not see how more could have been specially demonstrated, because my mission was not to see cases. All I would suggest is that what I saw bore out what I was told.

There is, therefore, nothing inherently unlikely in the claim made that this system of treatment produces better results than our old-fashioned universal "massage and battery," and that it constitutes an important money-saving factor so far as the State is concerned.

#### DEDUCTIONS.

Lastly, if it should seem to you that this system is what it has been represented to be—the most powerful and rapid means of reducing disabilities entailed by wounds, two deductions appear to be unavoidable—viz., first, that we, as the chief scientific body in this country within whose scope the system seems to come, will have to decide whether we should not press upon our own military authorities the desirability of giving it an adequate trial; and secondly, that if we decide to do so we should lose no time in doing it.

#### DISCUSSION.

Dr. BUCKLEY (Buxton): Upwards of 1,200 cases have been treated at the Devonshire Hospital in Buxton and a large number in addition at the Red Cross Hospital, with very good results. We have found the case card issued by the Sub-Committee very useful, and it has been warmly commended by visiting medical officers. I think, however, that the value of the records compiled from these cards will be increased if some uniform nomenclature can be devised and adopted by the various hospitals. The President's report on the methods in use at the Grand Palais in Paris is of great interest and value, and I hope that the methods there used with such benefit to French soldiers may become more widely employed in this country. As to the *eau courante* treatment, it is quite a new thing in England, but the under-current douche, as used at Buxton, and I believe also at Bath, has something in common with it. From a wide experience of this particular hydrological method I am quite satisfied that the *eau courante*, which is a method easy to instal, will be of very great value in the class of cases for which it has been devised.

Surgeon-General CARLETON JONES (Director of Medical Services, Canadian Contingents), after describing the method adopted by the Canadian authorities for the treatment of joint and nerve cases at the Granville Canadian Special Hospital, Ramsgate, said: The Canadian authorities are opening a hospital at Buxton, at the Peak Hotel, which will accommodate about 300 patients, thus having the advantage of the municipal baths. I am strongly of opinion that special hospitals for this class of case are very necessary, and will be most economical, as a large number of the men who would otherwise be invalided from the Service will be able to return to the fighting ranks.



Dr. LIONEL CALTHROP (Woodhall Spa): The President's report is very interesting and valuable. The use of heat in one form or another to prepare cases for subsequent treatment, such as massage, is not new ; on the contrary, it is well known to many to be exceedingly effective. But the systematic use of the whirl bath is unusual, and I am glad to know that it has been found so successful in producing a larger proportion of cures in bad cases than has been obtained by other methods.

## Balneological and Climatological Section.

President—Dr. WILLIAM GORDON.

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### Report on the Reduction of Crippling from Wounds.

THE prevention or lessening of crippling from wounds is a matter of national concern. Nothing which can be effected to lessen the permanent damage which wounded men have to face should be left undone while it can be efficacious. Public attention should therefore be drawn to a system of "Combined Physical Treatment," for which remarkable success has been claimed, yet which hitherto has been given no adequate trial in this country. Capital injuries, such as loss of limbs, do not of course come within its scope.

What is actually a combination of different forms of physical treatment has been in use for nearly eighteen months at the Grand Palais in Paris. This familiar building has been converted for this purpose into a *Hôpital Complémentaire* under the military government of Paris. It has now become a very large centre or clinic for the outpatient treatment of wounded men by means of physical remedies. Several similar institutions following the same methods have been set up in and around Paris, whilst in the provinces at least seven fully equipped, and four more or less complete, establishments have been opened for the use of soldiers.

The object of this physical treatment of the wounded is threefold, and may be thus stated. In the first place it hastens the return of the wounded to their units at the front. Secondly, it effects an economy to the State by reducing the disabilities for which pensions and gratuities are granted. Lastly, it minimizes the impairment of civil industry after the War, occasioned by the numbers of seriously and permanently crippled men.

The elements of this combined and systematic treatment are some of them familiar, whilst some are but little known in this country. It must be clearly understood that it is a system, and depends for its success on the completeness and regularity with which it is carried out, under expert supervision. Six essential departments are enumerated by Dr. Camus, the Director of the

"Corps de Ré-éducation Physique" at the Grand Palais. They are as follows: *Preparation* by heat in some shape or form, moist or dry, but especially by moving water, as in the *Eau Courante* bath; a thorough course of *manipulation* and massage; *mechanical treatment* by means of apparatus; *electrical applications*; *re-education* of the affected muscles by special exercises and training; and, lastly, a system of careful *mensuration* of the defect in each case. This latter is carried out before the commencement of treatment, at weekly intervals throughout the treatment and at its termination. The results are therefore checked not by personal impressions, but by recording and measuring apparatus designed for the purpose.

The latest published Report deals with the six months up to the end of last February. In that period many complete treatments were carried out at the Grand Palais alone. Of these 80 per cent. were returned to their depots as "cured" and fit to resume their duties. Of the remainder, about 15 per cent. were recommended for the auxiliary services, the remainder being discharged from the Army.

The average reduction of incapacity for work, carefully measured in every case after the treatment was completed, varied from 20 to 30 per cent. for the several months.

The total financial saving of pensions and gratuities for the entire six months from the Grand Palais work is estimated at 16,667,720 francs. Considering that during this period many patients were sent away elsewhere for the completion of treatment, the actual saving cannot have been less than twenty millions of francs (£800,000). It should be borne in mind that this sum does not represent something that was fairly due to the wounded man, and of which he has been deprived. It means something that is of far more value to the individual than any amount of possible money compensation, the actual removal or diminution of permanent crippling which would otherwise have been his lot.

Obviously such a Report as this, which more recently unpublished information more than confirms, has no small bearing not only upon the military service, but also upon the financial and social problems of the moment. It has therefore a special claim on the attention of the Army Medical Authorities and the Treasury, and of those who are called upon to deal with the question of pensions and the position of discharged soldiers.

Up to the present time physical remedies have not been used with the same thoroughness and precision in England. Heat, moist and dry, massage and electricity are of course in use throughout the country, and in a few cases baths and apparatus for mechanical movement are now being added. What is wanting is the methodical combination and association of these powerful physical agencies, under skilled direction, and checked by an accurate and periodical mensuration of defect. We think that a clear case has been made out for the introduction of physical treatment upon an adequate scale into this country for the benefit of disabled and discharged soldiers.

It is true that quite recently the *Eau Courante* bath of the French, or the analogous whirlpool bath devised in England, has been adopted at some British hospitals. This, however, is but a single item in the treatment to which we refer, although its adoption may be regarded as a tacit admission of the good results that are being obtained by our French and Belgian allies.

The medical service of the Empire, under which title may be included the whole medical profession, has played a part in this war of which every member may be proud. A reasonable delay in adopting new methods is doubtless evidence of a wise caution, but it would be unfortunate in the highest degree if such remarkably successful methods of treatment as those now adopted in France were not given at least an adequate trial in this country before it is too late. It cannot be too strongly emphasized that whatever is to be done should be done quickly, since every month that elapses between the healing of the wound and the skilled use of physical remedies materially lessens the advantage to be derived from them.

Finally as regards the discharged soldier, surely those who have been so seriously crippled as to be of no further service to the State in war ought not to be deprived of the privileges enjoyed by their less injured comrades in obtaining the greatest possible amelioration of their condition which science renders possible. To restore to them, even partially, the power of doing useful work will be in truth one of the greatest of war economies. Such expense as might be entailed by the setting up of cliniques for physical treatment of a systematic kind, would, on the evidence, be far more than covered by the great saving in men and money.

*For the Committee of the Section of Balneology and Climatology,  
Royal Society of Medicine,*

WM. GORDON, *President of the Section.*

S. SUNDERLAND, *Chairman of the Committee.*

R. FORTESCUE FOX, *Hon. Secretary.*





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## Section for the Study of Disease in Children.

President—Dr. ROBERT HUTCHISON.

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(November 26, 1915.)

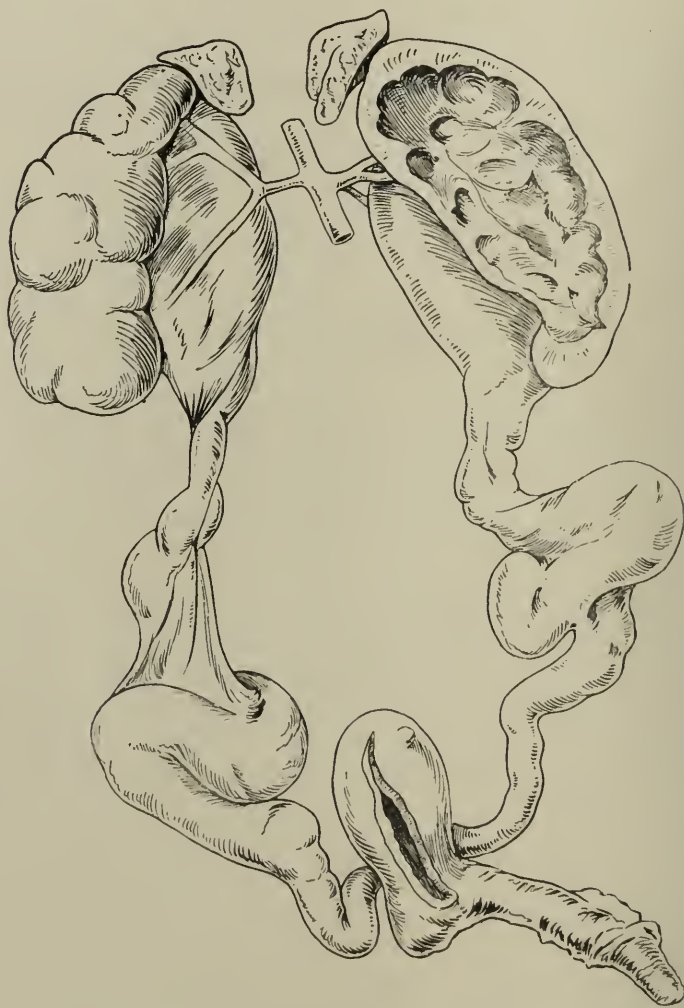
### Congenital Hydronephrosis of both Kidneys ; both Ureters dilated, with Hypertrophy of Bladder.

By E. CECIL WILLIAMS, M.B.

THIS specimen was removed from a male child, aged 6 months, who was sent into hospital as an urgent case for operation. Both ureters could be distinctly felt, and the right side ureter had a superficial resemblance to an intussusception ; there was, however, a complete absence of any other symptom of that condition. Waves of intestinal peristalsis were observed from time to time. There was almost complete suppression of urine. Death occurred about forty-eight hours after admission. Post mortem, both kidneys were found to be hydronephrotic, the left kidney showing almost complete absence of kidney substance. Both ureters were much dilated and tortuous ; in some parts they were as large as the small intestine. The bladder wall was much hypertrophied. I could not detect either in the urethra or ureteral orifices any folds of mucous membrane which could offer any mechanical obstruction. If the condition were due to mechanical obstruction in the urethra, one would expect to find some degree of dilatation of the bladder. I am inclined to think the condition is due to developmental error in both kidney and bladder, and that the hypertrophied musculature of the bladder tended to constrict the ureteral openings, and so helped to accentuate the

## 2 Williams: *Congenital Hydronephrosis of both Kidneys*

condition. There was no other abnormality in this case of any other organ. A condition very similar to my case was described by Fortescue-Brickdale in 1904.<sup>1</sup>



Congenital hydronephrosis, showing dilatation of renal pelvis and ureters, also hypertrophy of bladder wall.

<sup>1</sup> *Rep. Soc. for Study of Dis. in Child.*, 1904, iv, p. 94.



## DISCUSSION.

Dr. LEONARD GUTHRIE: I would like to ask whether Dr. Williams noticed any deficiency in abdominal muscles in this case; also what the condition of the urachus was, whether there was adhesion between the summit of the bladder and the umbilicus, or any abnormality in that direction. A considerable number of cases of this kind are associated with deficiency of certain portions of abdominal muscles, sometimes to such a degree that you can see the dilated ureters through the abdominal wall. I published a case of the kind a good many years ago,<sup>1</sup> and some years back Dr. Garrod<sup>2</sup> had one similar in many respects. The muscles which are deficient vary in different cases; sometimes the recti, sometimes the lateral muscles, but very frequently the recti, in one or more segments. I do not know the explanation. In my case I think the bladder was unable to contract downwards, as it was adherent to the inner side of the umbilicus and became hypertrophied in consequence, and there was back pressure which caused dilatation of the ureters and ultimate hydronephrosis of the kidneys. But I do not think that explanation applies to all. I do not know how to account for them all, unless we call them a congenital abnormality.

Dr. J. D. ROLLESTON: A very similar case to Dr. Williams's is described in *Guy's Hospital Reports*<sup>3</sup> by J. H. Bryant and Hale White; that also was in a child, aged 6 months. The peculiarity was that it was associated with calcification of the arteries, and I would like to know whether there was anything abnormal about the arteries in this case.

Dr. WILLIAMS (in reply): I did not notice any deficiency in the abdominal muscles, nor that there was any thickening of the arteries.

(November 26, 1915.)

## Optic Neuritis, (?) due to X-rays.

By EDMUND CAUTLEY, M.D., and N. BISHOP HARMAN,  
F.R.C.S.

Boy, aged  $8\frac{3}{4}$  years, one of three children, the others being healthy. He had measles in infancy, diphtheria three years ago, and double otorrhœa last June. On July 22 he attended the out-patient clinic of

<sup>1</sup> *Trans. Path. Soc.*, 1896, xlvii, p. 139.

<sup>2</sup> *Med.-Chir. Trans.*, 1905, lxxxviii, p. 363.

<sup>3</sup> "A Case of Calcification of the Arteries and Obliterative Endarteritis associated with Hydronephrosis in a Child, aged 6 Months," J. H. Bryant and W. Hale White, *Guy's Hosp. Rep.*, 1901, lv, p. 17.

#### 4 Cautley and Harman: *Optic Neuritis, (?) due to X-rays*

Dr. Prentice at the Belgrave Hospital for pains in the head of five to six months' duration. The pain was frontal, usually at night and preventing sleep, occasional during the day. He had been treated for ringworm by X-rays one week previously.

*Abstract of Dr. Prentice's Notes.*—Marked neuro-retinitis. Vision,  $\frac{6}{6}$ . No paralysis. Some uncertainty, almost amounting to inco-ordination, in right hand, and tendency to point to the right with that hand. Sudden jerking or twisting of the limbs; jumpy but not clumsy. Gait and reflexes normal. July 29: Has had one attack of headache and vomiting. Superficial and deep reflexes active. Vision,  $\frac{6}{6}$ . September 2: Has had no headache or vomiting, except for one sudden attack of pain across the eyes a few days ago. Normal reflexes. Hearing normal. No otorrhœa. Marked swelling and striation of optic disks. Vision,  $\frac{6}{6}$ .

On September 30 he was admitted as an in-patient. Since then he has had no headache or vomiting. Muscular control has recovered. In thirty-eight days he has gained 54 oz. in weight, and, except for the eyes, he is apparently in good health. No focus of infection has been found; there is no tuberculous history, and the Wassermann reaction is negative. The action of the X-rays caused complete baldness, but the hair is growing again. According to the history the pains in the head began before the ringworm was treated. He has been taking small doses of potassium iodide and liq. hydrarg. perchlor. since admission, although there is no reason to suspect syphilis. The case is shown in order to elicit the opinion of members of the Section as to the ætiology, prognosis, and treatment.

*Notes by Mr. Bishop Harman.*—September 21: Chronic optic neuritis in each eye. It is rather more marked in the right eye, the elevation of the disk being about 4D., exceeding that in the left eye by 1 to 2D. There is 1D. of hypermetropia, as measured by retinoscopy, at the macula of each eye. In the left eye there are a few small spots of exudate between the disk and macula. The degree of intensity is more marked in the right eye, but, as far as the age of the neuritis can be judged, there is no appreciable difference between the two eyes. Vision: Right  $\frac{6}{9}$ , left  $\frac{6}{9}$ . October 5: Right swelling only 2D., disk distinctly clearer; vision,  $\frac{6}{12}$ . Left eye shows much more swelling, elevation 6D., vision,  $\frac{6}{36}$ . No exudate seen. The eye conditions are not sufficient to determine the primary disorder. October 12: Neuritis beginning to subside. Right swelling 2 to 3D.; vision,  $\frac{6}{9}$ . Left swelling 5D.; vision,  $\frac{6}{18}$  slowly. October 19: Right swelling 2D.; vision,  $\frac{6}{9}$ . There is now exudate in the macular region, distributed in a roughly

radiate fashion. Left swelling 3D., suspicion of commencing exudate in macular region; vision,  $\frac{6}{60}$ . October 26: Right unchanged. Left slightly less swollen; vision,  $\frac{6}{24}$ . November 2: Right swelling under 2D., disk a fair colour; vision,  $\frac{6}{5}$ . Left swelling 2D., disk looks like becoming atrophic; vision,  $\frac{6}{36}$ . November 9: Right swelling diminishing, vessels of new formation clearer; vision,  $\frac{6}{8}$ . Left disk whitening, shrinking of inflammatory deposit on the disk; some small fresh spots of exudate between the disk and macula, like those seen on September 21; vision,  $\frac{6}{18}$ , attempts. November 16: The swelling in each eye disk is 1D. or less. The right disk has a fair colour but is ragged, vessels of a good contour; vision,  $\frac{6}{6}$ . Left disk white and ragged; vision,  $\frac{6}{24}$ . November 22: No swelling of disks; vision  $\frac{6}{5}$  and  $\frac{6}{18}$ . Probably the left eye will not recover fully. The eyes have presented throughout the signs of a pure neuritis. The fluctuation in the degree of swelling, and the changes in the fundus generally, have given no clue to its origin. On no occasion has there been any tenderness on pressure or photophobia. In the absence of any clue to its origin, one is inclined to speculate on the possibility of the irritating influence of the dose of X-rays to which the child's head had been subjected. When first seen by me on September 21 the head was quite bald. Seeing that it is well established that the rays exert a profound influence on the testicles and thyroid gland, it may well be asked whether the optic nerves may not similarly be liable to irritation in susceptible subjects.

Dr. CAUTLEY (in reply to a question put by the President): I brought the patient in order to elicit opinions; I know of no recorded cases of the kind. There is an extraordinary sequence here. I saw the boy before he was treated with X-rays, and apparently there was nothing amiss with him. I did not take him into hospital because of his ringworm. He had been treated for his ringworm very thoroughly, as he was quite bald when he came to the hospital. When his eyes were examined, it was found that he had neuroretinitis. There may be no connexion with the rays, but the course of events is suggestive in a boy who had no other symptoms—nothing suggesting tumour of the brain, for instance, nor syphilis, though, of course, the mere absence of a Wassermann reaction does not exclude syphilis. Still, there is no family history nor sign in the boy to make one think of that disease. I thought it worth while to bring the case forward as an instance of idiopathic optic neuritis, in which there was a possibility of it being due to X-rays. We know that X-rays have a powerful influence on other organs. I will report the case later if there is any further development.

(November 26, 1915.)

### Case of Friedreich's Disease.

By EDMUND CAUTLEY, M.D.

MALE, aged 10 years, the seventh of nine living children, two sisters being younger than the patient. Three children are dead. Four years ago he fell about 6 ft. and injured his head. His gait is said to have been affected a month later. He is intelligent and in Standard II.

On admission to the Belgrave Hospital his gait was somewhat ataxic and he walked with his legs wide apart. The knee-jerks are absent. An extensor reflex of the big toes is present. There is very marked pes cavus, with some equinus, and extension of the big toes on both sides. The grasp is stronger on the right side, and there are somewhat claw-like movements on prehension. There is no nystagmus and the fundi are normal. The boy is somewhat unsteady with the eyes shut. Wassermann's reaction is negative. He has improved in general health, has gained weight and in control of his muscles since admission.

(November 26, 1915.)

### Case of (?) Myositis.

By A. S. BLUNDELL BANKART, M.S.

GIRL, aged 12 years. First noticed swelling of right thigh on July 12, 1914, and said that it felt "tight" when she sat down. No history of any previous illness. Occasionally had "rheumatic" pains. Had been taking active part in games and training for school sports, but was not conscious of any strain or other injury. Four brothers, aged 14, 10, 5, and 3 years respectively, all quite well. No history of paralysis or myopathy in family. The patient was first seen on July 14, 1914. Right thigh and calf obviously larger than left. Swelling rather firm and (?) a little tender on deep pressure. No pain. Circumference of right thigh,  $13\frac{1}{2}$  in.; left thigh, 12 in.; right leg,  $10\frac{1}{2}$  in., left leg, 9 in. Musculature of upper extremities and trunk rather poorly developed, but equal on two sides. No atrophy of hand, arm, scapular muscles, or



elsewhere. No weakness. All movements of trunk and limbs performed normally. No lordosis or scoliosis. No nystagmus or strabismus. Reflexes normal. Electrical reactions normal. X-ray examination of right thigh shows nothing abnormal.

During the last seventeen months the condition has remained unchanged, except in so far as the child has grown. The right thigh and calf have increased in size, but not disproportionately. Also, the swelling is a little softer than it was at first. There is no disability of any kind. The child still occasionally complains of "rheumatism" in the right thigh.

#### DISCUSSION.

Dr. PORTER PARKINSON: I think there is a general enlargement of the whole of the right leg, and of the foot also. The foot enlargement is more one of bone. The right foot, I think, is distinctly larger in circumference than the left. The buttocks on the two sides are equal. The right breast is a little larger than the left. I thought it seemed like a case of hemi-hypertrophy confined to a portion of the right side of the body. Against that is the absence of any lengthening of the limb. I do not know whether skiagrams of the case have been taken. Dr. Parkes Weber pointed out to me that there was some over-action on the right side of the face. On the whole, the case appears to me to be one of a partial hemi-hypertrophy, of which several examples have been recorded.

Dr. FEARNSIDES: In this case I think we are dealing with a disease which affects the muscles primarily (a myopathy) rather than with an affection of one half of the body. The distribution of the hypertrophy, if bilateral, would not have been an unusual one for a case of myopathy; but though one frequently sees in pseudo-hypertrophic muscular dystrophy some asymmetry in the degree of affection on the two halves of the body I have never personally seen a case or read any record of a case where the disease had been limited to one half of the body.

Dr. GUTHRIE: It would be interesting to clear up the point raised by Dr. Porter Parkinson, as to whether the right foot is larger than the left. I did not notice a difference myself. If there is I should be inclined to agree with Dr. Parkinson, that it is hemi-hypertrophy. Otherwise I am of Dr. Fearnside's opinion, that it is a most unusual case of unilateral pseudo-hypertrophic paralysis affecting one lower limb only. I have not seen such a case before.



(November 26, 1915.)

**Case of Congenital Word- and Letter-blindness—Alexia  
congenita.**

By T. R. WHIPHAM, M.D.

THE patient is a physically well-developed girl, aged 8 years. In appearance she is not unintelligent, but her manner is shy and reserved, and she is not inclined to associate with other children. She is of a placid disposition and shows no signs of nervousness, and, except for a certain degree of mental impairment, she appears to be perfectly normal. She was born at full term after a normal labour, and was breast-fed until aged 10 months. Three years ago she had measles, followed by pneumonia and whooping-cough. About the same time her tonsils and adenoids were removed, and she suffered from left otorrhoea for three months. She has never had any fits. There are two other children in the family—one, aged 9 years, who is perfectly normal, and one aged 4 years. No miscarriages. One child died at the age of 1 year 8 months from tuberculous meningitis, and the grandfather and one aunt on the maternal side were also tuberculous.

The girl went to school two years ago, and she is now only in the class above the infants, her defective powers having always been commented upon by her teachers. Her general intelligence is in some ways below the normal standard for her age, but on the whole she is fairly bright. She cannot say the alphabet, but can write it perfectly on rare occasions, though generally she stops about E or F and then goes wrong. She cannot write a given letter except occasionally A, and can only at times pick out one or two letters from copies or from what she has herself written. She can count up to twenty, but has no knowledge of numbers above that. She can write the numerals up to 12, but frequently misses one out and usually stops at nine. Occasionally she can write a given numeral and pick one out, and is most likely to succeed with the first four. She has no knowledge of Roman numerals. She cannot spell words of even two letters, and cannot read easy words like *is*, either from print or writing. She cannot write from dictation simple words with certainty—e.g., *at* is at times written correctly and at others *ta*; *on* is a failure; *cat* is mostly written

correctly, but when following immediately after *dog*, the word *dog* is sometimes repeated, and *dog* itself at times becomes *gdo*.

Her writing appears to consist merely of a limited number of ideograms, which may or may not be correctly performed. Thus she can write her own name and the name of her school correctly, but when told to write either *R . . e* or *E . . . y* separately, she still writes her full name *R . . e E . . . y*. In the same way, when asked to write *school*, she writes *Picardy school*, and the same thing occurs when she is told to write *Picardy* alone. Her powers of concentration flag after a time, and on one occasion, when getting tired and told to write *Picardy*, she wrote *R . . e school*.

She will write voluntarily a complicated word like *transcription*, evidently from the top of a copy-book, but transposes the last two letters and produces *transcriptino*. On the other hand, she cannot write *description* or either of the syllables *trans* or *scription* separately. Another voluntary effort is to write *copy writing*, which appears as *copy writino*, the *tin* in the final syllable being evidently the same mental picture as the mistake in *transcription*. These two errors she uniformly perpetrates. When, after an interval, she is asked to read words which she herself has written, she is quite unable to do so.

She can copy correctly anything that is written for her, even unknown letters like Greek. Printed words are generally transcribed in printing characters, but occasionally she will copy them in her ordinary handwriting, though quite unaware of their meaning. She will copy accurately simple free-hand drawings, and can draw a cross and a square when asked to do so, but she does not know a triangle. Attempts to copy black-and-white illustrations, and even outline drawings, are very feeble, but crude efforts are made at drawing out of her head objects such as an engine, and she can name the component parts, such as the wheels and funnel.

She can interpret illustrations fairly well, but did not know an elephant, a donkey, or a goat. On the other hand, when shown a picture of a Chinese woman, she was nearly right in describing it as a Japanese girl.

There is no word-deafness; she understands what is said to her, and her vocabulary, so far as can be judged, for she speaks but little, is fairly extensive for a child of her age and disabilities, and she can repeat "pieces" which she has learnt by heart. There is thus no verbal amnesia nor motor aphasia. She can name ordinary objects and specify their uses, and is so far observant that, when shown a

## 10 Whipham: *Case of Congenital Word- and Letter-blindness*

broken toy watch and asked if there was anything peculiar about it, she said what it was and remarked that it had lost its hands. She knows the use of a watch, but cannot read the time.

Her sight is good, and there is no hemianopia. There is no nystagmus nor intention tremor, and all the reflexes are normal. The Wassermann reaction is positive. Cases of word-blindness are probably not so uncommon as the number recorded would lead us to suppose. I think there are only sixty-four cases in the literature, but doubtless a search among elementary schools would reveal many more. The cases seem to be more common among boys than among girls, as forty-seven of the sixty-four published were in males. The condition seems sometimes to be familial, in that more than one member of a family is affected. In one instance six cases were observed in a single family. Inability to read is not uncommon amongst the feeble-minded, but those cases come under a different category. Some people object to the terms word- and letter-blindness, as implying defective visual perception; therefore I have given an alternative title to this case—namely, alexia congenita. Word-blindness means a mental blindness. The patient does not appreciate written characters owing to a defect in the visual memory centre, without there being impairment in the centres for primary vision, and so without there being blindness in the literal sense of the word. The word- and letter-visual-centre is situated in the left parietal lobe, in the neighbourhood of the supramarginal and angular convolutions, and this is connected by commissural fibres with the primary visual centres in the occipital lobes. The auditory letter-centre is in the posterior part of the first temporo-sphenoidal convolution on the left side. As this child is apparently letter- and word-blind and letter-deaf, in that she cannot repeat the alphabet and cannot spell, the defect in her case seems to lie around the posterior end of the fissure of Sylvius on the left side. Now comes a difficulty in these cases. If the centres on the left side are impaired through some congenital defect, how is it that the corresponding areas on the right side do not come into play, and take their place in the same way as the third right frontal is supposed to act with regard to speech in left-handed persons, or when Broca's convolution on the left side is put out of action? Must we suppose that in these cases there is a symmetrical lesion on both sides? That seems to be rather a wide hypothesis. And if the lesion is situated so as to affect the fibres running from the primary visual centres to the angular and supramarginal convolutions in both hemi-

spheres, they would almost certainly involve the optic radiations running to the occipital lobes, and so cause hemianopia or some other form of blindness. In this girl there is no impairment of vision, so we must suppose that the congenital defect is somewhere round the posterior end of the fissure of Sylvius on the left side, without it being possible to give an explanation why the centres on the right side do not come into play. In this case the word-centre is not entirely functionless, as the patient is able to recognize one or two letters at times, and has some power of writing, which shows that the visual word-centre is capable of stimulating the graphic motor-centre, otherwise absolute agraphia would result.

#### DISCUSSION.

MR. SYDNEY STEPHENSON: It is true that the first two cases of word-blindness with which I am acquainted (W. P. Morgan and Bastian) were not reported by ophthalmic surgeons, but the majority of cases which have been reported have been from ophthalmic surgeons, for the very reason that Dr. Whipham hinted at—namely, that the condition is mistaken in many cases for defective sight. When such cases are brought to us we find no error of sight, and if we are aware of the condition we recognize it as being a more or less severe grade of congenital word-blindness. The most important contribution to the subject was the work of Dr. James Hinshelwood, of Glasgow, who is an ophthalmic surgeon. I think the cases are common, but they frequently occur under conditions which escape recognition. Since the attention of the profession has been widely directed to the condition, I think it is known in educational circles, at any rate, that the condition is not uncommon. Some remarkable figures were published a few years ago by Dr. C. J. Thomas, attached to the Educational Authority of London. Speaking from memory, I think he made out that about 1 in 2,000 of the ordinary elementary school children who were examined suffered more or less considerably from word-blindness. With regard to the prognosis, there is a well-known case reported by the late Mr. Nettleship many years ago, in which a severe case of word-blindness made so much improvement as ultimately to enable the patient to become a lawyer, though how far that can be regarded as a sign of real progress I will not undertake to say. A patient of my own is a fairly successful accountant. But children with any marked defect who are subjected to the ordinary routine school discipline do not do well. They must be taught by sympathetic people who do not think word-blindness to be a manifestation of sheer wickedness. Given these good conditions, we do find that the majority of these children come to be able to read fairly well, and eventually occupy a respectable place in the commonwealth.

DR. SHUTTLEWORTH: I rise not to follow Dr. Whipham into the domains of pathology, but because it has fallen to my lot, as a school medical officer,



## 12 Whipham: *Case of Congenital Word- and Letter-blindness*

to see a good number of cases of alexia which are brought for examination for special schools, and which, I judge, are not fit for admission because they are not mentally deficient in the plenary sense of that term. Dr. Thomas, as has been said, has demonstrated that the defect, in greater or less degree, is very common in the elementary school children of this country. From ten to twelve out of about 200 children who are brought to me annually come because the teachers complain they can make no progress in reading; there is no fault found in regard to their progress in other subjects. But they cannot, because of this one defect, pass to a higher standard. In spite of the abandonment of payment by results, there is still the red tape system of promotion by standards in elementary schools. Sometimes the teachers are so importunate that one at length gives way, and the child is admitted to a special school for defectives, for I compound with my conscience that the child has a partial cerebral defect, and it is likely to get on better in the special than in the ordinary school. Where individual attention has been given, I have seen these children learn to read slowly, but surely; I am speaking more especially of children of the better social class, who are able to spend a good deal of time at school, and have a nice home in which the school results are reinforced. I have known children of that class (where time can be spared to bring about kinæsthetic associations with words) read and write very fairly at the end of a few years' persevering instruction. In the case of elementary school children, where such attention is impracticable in large classes, I think it would be wise, where word-blindness has been ascertained, to let the reading go; let them get on with their sums and copying, which they can usually do, and more especially with manual occupations and industries likely to be of service in after life. The girl now shown is physically well developed, but I doubt whether she is quite average in intelligence. She told me she would be 10 years of age on Boxing Day. She is really aged 8 years, and she had no idea when Boxing Day occurs. The girl's father said that his father, who was an engineer, was an extremely bad scholar, and they could not teach him much reading at school; but he turned out to be a very good worker, and made his living without much knowledge of reading. The mother said her own sister was also a very dull girl at school, but I afterwards learned that she was tuberculous. I do not think a knowledge of reading is indispensable in lower-class education; I know some people who have got on well in the world who were very bad readers. When asked to write a simple sentence, this girl wrote nonsense (i.e., jumbled letter-forms); but when written for her she made a fair copy. Though these "word-blind" children cannot tell you the names of letters, they can usually recognize Arabic figures, but not Roman numerals. I do not know why there should be a different storage brain area for Arabic figures as distinct from letter-forms, but this peculiarity has been observed in numerous cases. Consequently sums are often worked with facility by "word-blind" children.



(November 26, 1915.)

### Case of (?) Juvenile Tabes.

By SYDNEY STEPHENSON, C.M.

R. K., AGED 9 years, was first seen on August 24, 1915. The lad's sight has been noticed to fail during the last month. The reality of the fact cannot be questioned. The handwriting, after being quite good for a boy of the patient's age, began to go off badly in July, and a sample made on August 23 and brought with the lad to the hospital for my inspection had become deplorable. The patient's health meanwhile has not failed in any other way. In April last he had a "cold in the head" and slight "running" from the right eye. He is dull of hearing on the left side, and in August, 1914, a mastoid operation was performed on that side.

Family history: The father, when aged 37 years, developed fits, and has had two such "fits" during the past twelve months. They are said to be of an epileptic nature. The mother enjoys good health. Two miscarriages and six children born at term. Of the last-named, five are still alive. Patient is the second in the family. A maternal aunt is stated to have lost the sight of one eye when aged about 9 years. This woman, now aged about 39 years, was examined, and found to be dull-sighted as regards one eye, in consequence of a convergent squint. One of the brothers of the patient was operated upon by me for squint on June 28 last.

On admission: A healthy-looking and well-nourished lad. The pupils are equal and active to light. Right vision,  $\frac{3}{60}$  and No. 16 J. Left vision,  $< \frac{1}{60}$  and  $<$  No. 20 J. Both optic disks are pale, but without signs of past neuritis. The retinal vessels are of fair size. No changes in gait. Intelligence quick. No history of pains in legs or arms, or of sphincter trouble. No loss of sensibility in lower limbs or about face. The knee-jerks cannot be elicited, even on re-enforcement. No clonus, Babinski's, or Romberg's sign. No personal evidence of syphilis (the Wassermann reaction, taken at a later stage, was negative).

Treatment and progress: Potassium iodide, 5 gr. twice a day. Two weeks later: Right vision,  $\frac{5}{36}$  and No. 12 J.; left vision,  $< \frac{1}{60}$  and  $<$  No. 20 J. The iodide was increased on September 7 to three doses a day, and later (October 11, 1915) to four doses a day. When

examined on October 12 last, the condition was as follows: Pupils 3.5 mm. Both react to light, but the right better than the left, and the contraction of the former is much better maintained. Right vision,  $\frac{6}{60}$  and letters of No. 6 J.; left vision,  $< \frac{1}{60}$  and letters of No. 20 J. The retinal vessels appear to be shrinking. The pallor of the optic disks remains apparently without alteration.

### DISCUSSION.

Mr. SYDNEY STEPHENSON: I disclaim the diagnosis "(?) tabes." I put that in the title only in order to elicit discussion.

Dr. FEARNSIDES: In the Lorain type of dyspituitarism optic atrophy may occur without any preceding optic neuritis, and in these cases an enlargement of the sella turcica is usually revealed by X-ray examination. In this case I think that a skiagram of the skull might be helpful; if an enlargement were found a certain diagnosis could be made.

Mr. SYDNEY STEPHENSON: I am obliged to Dr. Fearnside's for his suggestion; I will have a skiagram taken of the case and report results.

(November 26, 1915.)

## Case of General Tremor and Left Cortico-spinal Interference; Lesion of the Mesencephalon.

By E. G. FEARNSIDES, M.D.

(For Dr. LEONARD GUTHRIE).

GIRL, aged  $4\frac{3}{4}$  years.

Family history: There is no family history of tuberculosis or neuropathy. The patient is one of two living children, and her brother, aged 10 years, is healthy. One sister and one brother died from "bronchitis and pneumonia" in babyhood, and another brother, after ailing for some months, died of "tuberculous meningitis" at the age of 6 years.

Personal history: The child was healthy and suffered from no illness until the age of 3 years. She was forward in talking and walked well at the age of 16 months. In December, 1913, she had an attack of "bronchitis" and was laid up for a few days, and in August, 1914,

developed "whooping-cough." After this attack she remained well until May, 1915, when the cough returned and she "began to whoop again." Between May and July, 1915, the paroxysmal cough persisted. Early in July, 1915, she had a "fainting attack," after which she "went off her feet, lost all her strength, and began to tremble." On October 20 she suffered from a "second faint." She was brought to the Maida Vale Hospital on October 25, and admitted under the care of Dr. Leonard Guthrie.

The patient is a fairly well developed child, aged  $4\frac{3}{4}$  years. There are no abnormal signs in the heart, lungs, abdomen, or urine. The temperature and the rate of the pulse have been normal throughout. With every attempt at movement a dynamic tremor of the head, trunk, and upper extremities occurs; this is greater in the left upper extremity than elsewhere. Muscular tone is not grossly affected, and there is no muscular wasting. Ocular movements are normal and nystagmus is not present. The pupils react normally. The face is somewhat asymmetrical in movement, and there appears to be some slight right facial weakness. The tongue, on protrusion, comes out straight. The ocular fundi appear normal. Hearing is not affected, and there is no otorrhœa. The knee-jerks are obtained, ankle-clonus is not present, and the plantar response on the right side is flexor, on the left indefinitely extensor. The abdominal reflexes are obtained. The sphincters are relaxed incontinently. The hands and feet are always cold, blue, and tend to be sodden. The patient is backward, but intelligent and irritable, and cries when approached. Speech is somewhat jerky and explosive. Under observation she has improved considerably.

(November 26, 1915.)

### Case of Anterior Poliomyelitis affecting the Distal Portion of the Left Upper Extremity.

By E. G. FEARNSIDES, M.D.

(For Dr. LEONARD GUTHRIE).

GIRL, aged 8 years. At the age of 9 months this patient suffered from an acute illness which affected the left upper extremity. At the present time she shows a general regression and atrophy of this limb affecting chiefly the muscles supplied by the eighth cervical and first

thoracic spinal segments. Movements at the left elbow are paretic, and the joint itself cannot be fully straightened. The left wrist is flail, and the fingers of the left hand are unaligned. All the small muscles of the left hand are poorly developed. The muscles on the flexor surface of the left forearm are better developed on the outer than the inner side, and than those of the extensor surface. The knee-jerks and ankle-jerks are unaffected, and both plantar reflexes give a flexor response. The abdominal reflexes are normal. There is no scoliosis. Measurements from acromion to internal condyle: Right 9 in., left  $8\frac{7}{8}$  in.; internal condyle to pisiform, right  $7\frac{1}{2}$  in., left 7 in.; wrist to tip of fingers, right  $5\frac{1}{4}$  in., left  $4\frac{3}{4}$  in.

(November 26, 1915.)

### Case of Polio-encephalo-myelitis.

By LEONARD GUTHRIE, M.D.

A GIRL, aged 5 years, was taken ill in March, 1914, with sore throat which lasted three days and was followed on the fourth day by measles. The sore throat was attributed to diphtheria, but as the child was nursed at home and antitoxin was not given, the diagnosis may be considered doubtful. Ten days later she became unconscious without any convulsions, and sweated profusely. For three weeks she did not speak, and the only sign of consciousness shown was that her eyes would follow a lighted match. She then gradually came round, and six weeks after the commencement of the illness she attempted to speak but could only say "Dad" and "Mum." Previously she had spoken fluently. The voice was not nasal, and there was no regurgitation through the nose. At the same time her eyes became "turned," and when she tried to sit up she "flopped." Gradual improvement occurred, and by November, 1914, she could stand when supported, and could walk about the room holding on to chairs. She could now say any words and express herself distinctly, but speech was slow and drawling. Since November, 1914, she has made but little progress.

Condition on admission (November 1, 1915): A well-nourished child. The face lacks expression, but intelligence is not defective. Eyes: There is marked internal strabismus of the right eye (alternating convergent nystagmus). The left eye is also at times diverted strongly



inwards. Pupils are equal, reaction, disks, and fundi are normal. No paralysis of other cranial muscles, but slight weakness of lower left face. Speech is slow and hesitant, but articulation is clear. She is mostly taciturn, but at times talks volubly and to the point. Upper extremities: Some quasi-voluntary "pawing" movements are noticeable in the right arm, and also at times there are coarse involuntary tremors on both sides, but voluntary movements are fairly well co-ordinated. The muscles are not wasted nor weakened. A slight degree of hypotonia is present in fingers and wrist. There is no rigidity, and the arm-jerks are not exaggerated. The involuntary movements have decreased considerably since admission to hospital. Lower extremities: Wasting and rigidity are absent, and also hypotonia. Knee-jerks are absent on both sides. Ankle-clonus is absent. Plantar response is extensor (Babinski type). Equilibrium: She can stand alone but sways slightly in all directions, though chiefly backwards after a few moments. Gait is halting and unsteady, and the right leg is often abducted at the hip in order to secure a wider basis. She cannot walk alone for more than a few steps without swaying backwards or from side to side, and would frequently fall if not supported.

*Remarks.*—The case resembles the cerebellar type of polio-encephalitis. But the absence of knee-jerks and the extensor plantar response show that the disease has affected the connexions of the postero-lateral columns as well as the cerebello-spinal apparatus. The onset of the nervous symptoms in association with measles needs comment, for the association has been questioned by those who regard polio-encephalitis and poliomyelitis as identical diseases. Certainly the association of poliomyelitis with measles and other exanthemata is most uncommon, but it is otherwise with polio-encephalitis. Probably cases like the present have a different causation, and only resemble clinically and anatomically those of epidemic poliomyelitis and polio-encephalitis.

#### DISCUSSION.

Dr. J. D. ROLLESTON: I am glad Dr. Guthrie has given me an opportunity of expressing my views. For the last fifteen years I have been on the look-out for these nerve symptoms which are described as following measles, but I have never yet encountered such a case. I was recently discussing the point with Dr. Ker, of Edinburgh, who has had a vast experience of measles, and he said he had never seen such a case either, though he was familiar with the literature on the subject. In this case, I suggest that what was called measles was not measles, but a morbilliform eruption; and that the sore throat was a



tonsillitis, not diphtheria. It is well known that in poliomyelitis, sore throat is one of the initial symptoms, and that morbilliform and scarlatiniform eruptions are also among the initial phenomena. These nerve affections, if they do occur after measles, are very rare. In some cases there is a distinct doubt about the diagnosis of measles, and in the second place, the nerve symptoms sometimes occur too long afterwards for the measles to have any ætiological significance. Thirdly, there is probably in some cases some other concomitant affection which has more influence on the nervous system, such as diphtheria or, more important, syphilis, which is overlooked.

Dr. TRAVERS SMITH: I have been, for the last twenty-eight years, medical officer to a charity in North-west London, the Kilburn Dispensary, and every year I see a great many cases of measles—in some years the figure might be hundreds—but I have never yet seen poliomyelitis following measles in any one case.

Dr. C. O. HAWTHORNE: I think that in such a discussion something must be allowed for the possibility of mere coincidence. At the present moment I have in hospital a child with evidences of an intracranial tumour, and the history places all the symptoms in immediate relation to an attack of measles. The extensive negative evidence quoted by Dr. Rolleston and Dr. Travers Smith is very impressive, but of course does not exclude the possibilities Dr. Guthrie proposes.

Mr. SYDNEY STEPHENSON: Optic neuritis is a nervous sign of some importance. Many years ago, when I was in charge of a large institution for children, I had occasion to examine the eyes of inmates with the ophthalmoscope, and a certain child was included in my examination who afterwards developed measles, not particularly severely. He made a good recovery, and I had occasion to examine a group of children, of whom he was again one, and he had well-marked double optic neuritis. He was under observation many months, but no other sign developed. So, as an extremely rare thing, optic neuritis may follow measles, as effect from cause.

Dr. GUTHRIE (in reply): I am obliged to Dr. Rolleston and other speakers. The general view is that these cases do not follow measles at all. But as the medical man reported a sore throat and rash on the fourth day, it is difficult to exclude measles altogether from reasonable possibility. I agree that these cases may be very rare after measles, but I do not see why they should not occur sometimes. The history seems to my mind fairly reliable. However, I have been very glad to elicit opinions on the point, which may be worthy of further consideration.

(November 26, 1915.)

### Single Pelvic Kidney.

By J. D. ROLLESTON, M.D.

THE specimen is from a mentally defective boy, aged 14 years, who died of septic scarlet fever on the eleventh day of the disease. There was a trace of albumin in the urine during the nine days that he was in hospital, but there were no other renal symptoms. Post mortem no kidneys were found in the usual situation, but on the right side and within the brim of the pelvis was a single kidney, lying in apposition to the posterior wall of the bladder. Its shape was irregular and by no means reniform. The surface was lobulated. The organ measured  $3\frac{1}{4}$  in. long by  $1\frac{3}{4}$  in. broad at its upper part and  $2\frac{5}{8}$  in. in its lower part, and 1 in. thick. The weight was 4 oz. On section, the kidney showed some dilatation of the pelvis and calices, but no marked degree of hydronephrosis. The hilus was situated anteriorly, and from it issued a single ureter, considerably dilated and hypertrophied, which measured  $7\frac{1}{2}$  in. long,  $\frac{1}{2}$  in. broad at its upper part close to the kidney, 1 in. broad at its widest part, and  $1\frac{1}{4}$  in. above the bladder. At the lower end it became narrow again and the lumen of the vesical end barely admitted a pin's head. No trace of a left kidney or ureter could be found. There was no malformation of the genital organs nor other obvious abnormality. The brain, unfortunately, was not examined.

Single kidney is a rare occurrence. According to Sir Henry Morris, it occurs in only one in 2,400 autopsies[5]. I showed a case two years ago at this Section[6], but there the kidney was in the ordinary situation; here it is in the pelvis. Congenitally displaced kidney is not so rare as single kidney; it occurs in from 1 in 500 to 1 in 1,000 autopsies (Girard)[2]. The combination of single and pelvic kidney, however, is very rare indeed. According to Girard[2], only fifteen cases of single dystopic kidney have been recorded, and in only six of them was the kidney in the pelvis. Nine were in children, six in adults. As one was in an old woman the condition is compatible with advanced life. The affected side is usually the left; here it is the right. The form is usually altered, and in this it is more like a cake than anything else (*rein en galette*). Symptoms are usually absent, and the case is either discovered post mortem by chance, as in this case, or during an operation for some



Single pelvic kidney and dilated ureter with constriction at vesical end.

other condition. Pelvic kidney was well known to the anatomists of the sixteenth century.

Another interesting feature about the case is the association of mental with renal defect. At least three other cases have been recorded (Albrecht[1], Hochenegg[3], Israel[4]), and one urologist (Israel) does not regard it as a mere coincidence, but regards them both as developmental errors and stigmata of degeneration. I did not do the post-mortem examination myself, and did not pay special attention to the blood supply. As a rule, the blood supply is peculiar in these cases. The arteries may come from the bifurcation of the aorta, or internal iliac, or inferior mesenteric, or median sacral. Most of these cases have been recorded by gynæcologists and obstetricians.

#### REFERENCES.

- [1] ALBRECHT, P. *Zeitschr. f. Urol.*, 1908, ii, p. 413.
- [2] GIRARD, J. H. *Thèse de Par.*, 1910-11, No. 439.
- [3] HOCHENEGG, J. *Wien. klin. Wochenschr.*, 1900, xii, p. 4.
- [4] ISRAEL. "Chirurgische Klinik der Nierenkrankheiten," 1901, p. 3.
- [5] MORRIS, Sir H. "Surgical Diseases of Kidney and Ureter," 1901, i, p. 39.
- [6] ROLLESTON, J. D. *Brit. Journ. Child. Dis.*, 1913, x, p. 161.

(November 26, 1915.)

## Polydactyly and Deformity of the Right Leg.

By EDMUND CAUTLEY, M.D.

THE specimen is from a girl, aged 6 months, who weighed only  $6\frac{1}{2}$  lb. three days before death from zymotic enteritis. She was the fifth child, and her mother had had one miscarriage. There is no family history of deformity. The right foot and leg show the following conditions: The leg is shortened and curved, and the foot has seven toes. On the inner side of the great toe are two toes, identical in appearance with normal second and third toes. The second and third toes are closely united, almost to the proximal end of the terminal phalanges. Similarly, the skiagram shows metatarsals and phalanges of the extra toes—practically identical with those of the normal second and third toes. There is no fibula, and the tibia is very much curved. The left foot closely resembled the right in appearance, presenting similar extra toes but

united up to about the middle of the second phalanges. The normal second and third toes were also united for about the same extent. Possibly other members of the Section have seen somewhat similar cases. In my experience extra toes have generally been on the outer side of the foot.

Dr. GUTHRIE: One has heard a great deal about deformities due to amniotic compression. There is no fibula here, and one might possibly assume the result of pressure was to deprive the child of its fibula; but it could not give it six toes. Therefore when people speak so much of pressure *in utero* and its results, they ignore the important point that, where there is deficiency in one direction, there may be hypertrophy in another.

(November 26, 1915.)

### Specimens from Congenital Syphilis.

By C. O. HAWTHORNE, M.D.

Boy, aged 1 year 7 months. History of good health until July, 1915, when he had a "stroke" affecting left side; case later called "infantile paralysis." On admission to hospital (November, 1915) no physical signs other than considerable enlargement of liver, and with this anæmia (hæmoglobin, 50 per cent.; red corpuscles, 3,240,000; white corpuscles, 19,600, with relative excess of lymphocytes) and a positive Wassermann test. Considerable cystic formation in lower parietal cortex of the right brain, and extending deeply. Evidence of disease in liver, kidneys, and lungs, and an adherent thrombus in left ventricle of heart.



## Section for the Study of Disease in Children.

President—Dr. ROBERT HUTCHISON.

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(January 28, 1916.)

### Case of Dyspituitarism in a Girl, aged 15 Years.

By SYDNEY STEPHENSON, C.M.

H. C., AGED 15 years. First attendance, Queen's Hospital for Children, July 9, 1914.

The girl was brought to the ophthalmic department with the complaint that the sight had been failing for three or four months, and that for one week she had suffered from headaches. Right vision,  $\frac{5}{5}$  J.1. ; left vision,  $\frac{5}{36}$  J.1. ; weak atropine drops (1 gr.) were prescribed, to be applied to each eye three times a day, and when the girl was examined the following week she was found to have in the right eye + 0.5D. cyl., axis 180°, and in the left eye + 0.5D. sph. A note was made to the effect that the media were clear.

I first saw the patient on August 27, 1914, and found that the sight of the right eye had become reduced from normal to "shadows," while that of the left eye remained as it was on admission some seven weeks before—namely,  $\frac{5}{36}$ . The sight of the right eye was stated to have failed suddenly two weeks ago. The pupil of that eye had lost its direct response to light. The media were clear. Both optic disks were pale, but the retinal vessels were of normal dimensions. The girl's personal appearance was somewhat peculiar, since she was inclined to stoutness and her eyes seemed rather prominent.

For closer observation the patient was admitted to the hospital on August 27, 1914, and remained until the following October 24. She was found to be a bright, intelligent, and good-natured girl, whose every movement appeared to be normal. There was no tremor nor alteration in sensation. The facial movements were normal, and there was no

## 24 Stephenson: *Dyspituitarism in a Girl, aged 15 Years*

nystagmus. The functions of all the cranial nerves, with the exception of the second, were normal. During her stay in hospital there were occasional headaches, but no vomiting nor vertigo. Successive weights during the two months' stay in hospital were: 79 lb., 79 lb., 79 lb. 10 oz., 81 lb., 80 lb. 4 oz., 80 lb. 8 oz., 80 lb., and 74 lb. 4 oz. The temperature was usually between 98° F. and 99° F.; but on thirteen occasions during her sojourn in hospital it rose to between 99° F. and 100° F.

An examination of the blood, made a few days after admission, gave the following results: Red blood corpuscles, 3,770,000; white blood corpuscles, 6,400; hæmoglobin, 65 per cent.; index, 0·86. The differential count was: Neutrophiles, 55; basophiles, 1; eosinophiles, 2; large lymphocytes, 9; small lymphocytes, 29; hyaline, 4; total, 100.

The knee-jerks were brisk; there was slight ankle clonus. Flexor-plantar response. The abdominal reflexes were present; the reflexes of the upper arm were easily elicited. No jaw-jerk. No facial irritability. The Wassermann reaction negative as regards both the blood serum and the cerebrospinal fluid. No increase of pressure in the cerebrospinal fluid, which, from other points of view, was normal. The sphenoidal and other sinuses were reported as normal.

The girl was next seen by me on May 13, 1915, that is to say, after an interval of almost nine months, when sight remained much as it had been on the date of the last note. The optic disks were pale, with no evidences of antecedent neuritis. The retinal vessels were of good size. The field of vision of the right eye could not be taken, but that of the left eye, estimated for a small white object, was restricted in the temporal region, although tolerably full elsewhere.

Soon after this (June 17, 1915) a note was made to the effect that no pubic hair was present, and that the girl had not begun to menstruate. She was placed on thyroid, 1 gr. twice a day. A negative report was received at about this time respecting an X-ray examination of the patient's skull.

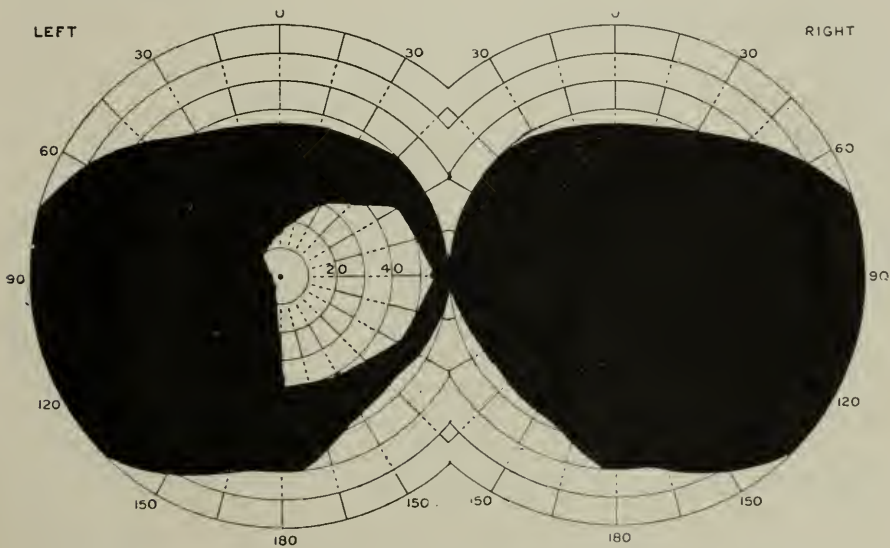
September 9, 1915: Right vision, perception of light; left vision,  $\frac{3}{60}$ . Dose of thyroid increased to 4 gr. a day.

October 7, 1915: Left vision,  $\frac{3}{60}$ , No. 20 J.

December 4, 1915: This bright and intelligent girl has a peculiar personal appearance, and is something more than merely well nourished. Her eyes convey the idea of prominence, and the left eye turns slightly outward and upward. Although within a few weeks of 15 years of age, no signs of menstruation have yet made their appearance. No headache, vomiting, or vertigo. Pupils 6·5 mm. in diameter. Direct action

of the right eye almost lost, while that of the left one is as yet almost intact. A very slow hippus is present. The optic disks are pallid, well defined, and the physiological cups are not filled in or concealed in any way. Retinal vessels of good size. The visual field of the left eye, as tested for a 10 mm. white square, is entirely confined to the nasal half. The fixation point is as yet intact. Right vision, ? perception of light ; left vision,  $\frac{1}{24}$  (letters) and No. 18 J. (letters).

A report from Dr. Williams, Radiographer to the Queen's Hospital, states: "The sella turcica is greatly enlarged, the antero-posterior diameter being 17 mm. There appears also to be some thinning of the clinoid processes."



Visual fields taken on December 4, 1915. The field of the right eye is lost ; that of the left eye, as tested for a 10 mm. white square, is entirely confined to the nasal half.

*Comments.*—The diagnosis of "dyspituitarism" can now be made with tolerable confidence, seeing that the following points are present : (1) The existence of bilateral simple optic atrophy, otherwise unexplained ; (2) loss of the temporal field in the left eye (temporal hemianopsia) ; (3) X-ray evidence of enlargement of the sella turcica ; and (4) the general appearance of the girl. As to treatment, large doses of thyroid have been tried, but apparently without particular benefit. It would appear that nothing short of surgical treatment is likely to do good, a point upon which expressions of opinion are requested. The risk of impending blindness is, in itself, the justification for such a view.

## DISCUSSION.

Mr. ZACHARY COPE: The newer method of removing these tumours seems to be the best—namely, going through the roof of the orbit, depressing the eyeball temporarily, and opening the dura near the sella turcica. This produces comparatively little shock. One surgeon had eleven cases last year in which this was done, without a death; another had four without a death, and I have had three of them without a death. These statistics of approach by the frontal method are certainly encouraging. If the tumour in this case is a cyst, which I think probable, much improvement in sight may be looked for after the operation.

Mr. A. S. BLUNDELL BANKART: I may say that the patient is to be submitted to operation as soon as possible.

(January 28, 1916.)

**Two Cases of Hypertrophic Cirrhosis of the Liver.**

By T. R. WHIPHAM, M.D.

*Case I.*—A boy, aged 9 years, who was brought to the Prince of Wales's Hospital on November 24, 1915, with a history of jaundice during the previous three weeks. He vomited at the onset of the jaundice, but had no further nausea until just before he was seen. During the same time he had been getting thinner and had lost his appetite and spirits. The urine had been dark-coloured and the motions light "like putty." There is one younger brother in the family, who is healthy, and between his birth and that of the patient the mother had had one miscarriage. When first seen the boy was well nourished and presented a general icterus of moderate severity. There was no irritation of the skin and the pulse was not diminished in frequency. The liver was greatly enlarged, extending to within  $1\frac{1}{4}$  in. of the umbilicus, and was firm and uniform to the touch. There was no enlargement of the spleen or of the lymphatic glands. The thoracic viscera were normal. A blood count showed red corpuscles 4,500,000, white cells 6,000 per cubic millimetre; the hæmoglobin value was 100 per cent. The urine contained much bile pigment, and the bowels were constipated. The Wassermann reaction is negative. Treatment with calomel and salines brought about a gradual diminution in the jaundice, and at the present time no coloration is visible. The liver, however, remains approximately the same size.



*Case II.*—A boy, aged 6 years, who attended the hospital on January 10, 1916, on account of jaundice of ten days' duration. He was said to have been getting thinner and to have lost his appetite. He had had no pain, but had vomited once on January 9. The urine had been of a dark colour, but, according to the mother's definite statement, bile had never been entirely absent from the stools. The patient has one brother who is healthy and there have been no miscarriages. The boy showed a marked icterus of a deep yellow colour, rather more intense than that in Case I. He had no itching of the skin and the pulse was not slow. The liver was very similar to that of the first case, but was a trifle larger in proportion, the lower edge being felt within  $\frac{3}{4}$  in. of the umbilicus. In this case also there was no enlargement of the spleen nor of the lymphatic glands, nor any abnormality in the thoracic organs. The blood count likewise resembled that in the former case, the red corpuscles numbering 4,500,000 and the leucocytes 10,000 per cubic millimetre, while the hæmoglobin value was also 100 per cent. The urine was deeply pigmented with bile, but the fæces have not been observed. The Wassermann reaction in this case, too, is negative. The patient has been treated with calomel, but the deep jaundice still persists and the liver has not altered in size.

#### DISCUSSION.

Dr. WHIPHAM: In the case of the smaller boy it is stated in the notes that the stools were not acholic, but his mother tells me that his motions have recently become colourless, in spite of the fact that the jaundice is now disappearing. I show the cases with some doubt as to the diagnosis, a doubt which is not lessened by the fact that in the first case the liver has decreased in size since the boy was last seen.

Dr. EDMUND CAUTLEY: I can scarcely agree with Dr. Whipham in the view he takes of these cases, especially that of the older boy, whom I regard as an instance of ordinary catarrhal jaundice, in which the liver enlargement is rapidly subsiding. The younger boy's case is more difficult. He is not normal, and is unduly small for his age. He has had rickets, and the liver is enlarged somewhat, independently of the jaundice. I do not think the liver will regain its normal size, even on complete disappearance of the jaundice. It may be that it is merely depressed on account of the rickets, or enlarged from some other factor. I do not think there is sufficient evidence in the case to warrant calling it hypertrophic cirrhosis, an extremely rare condition in children.

Dr. HUGH THURSFIELD: I would like to call the attention of the Section to the fact that in both these cases the Wassermann reaction was negative.



## 28 Bunch: *Papillomatous Growths in Old Operation Scar*

We are constantly being told, in this Section and elsewhere, that bile-stained serum of itself causes the Wassermann reaction to be positive.

Dr. F. PARKES WEBER: I think both these cases may possibly be instances of catarrhal jaundice. With regard to the diagnosis of hypertrophic cirrhosis of the liver, if by that is meant the biliary hypertrophic cirrhosis of the liver which was described by Hanot, I think that that diagnosis cannot be ventured on in a case of such short duration; that disease might be thought of if the symptoms had existed two or three years. I was surprised to hear Dr. Thursfield's remark about the Wassermann reaction, because I do not think it has ever been maintained that the Wassermann reaction in cases of jaundice is necessarily positive, though I believe that, as stated by many, it is sometimes positive in cases of jaundice apart from syphilis.

(January 28, 1916.)

### Case of Papillomatous Growths in Old Operation Scar.

By J. L. BUNCH, M.D.

THE boy, aged 13 years, was operated on eight years ago for a fatty tumour on the left side of the chest. Some months after the scar had healed, small reddish growths began to appear on and around the scar, and such growths have recurred intermittently ever since. There are now about a dozen of these growths present; they are soft, sessile, and red to purplish in tint. Under the microscope they have a definite papillomatous character, with a well-marked cortex (slide exhibited). No tubercle bacilli have been found in them, and the boy gives a negative von Pirquet reaction. There is nothing abnormal in the patient's lungs.

(January 28, 1916.)

### Multiple Tumours of Molluscum Contagiosum.

By J. L. BUNCH, M.D.

THE patient, a girl, aged 13 years, has a number of typical molluscum contagiosum tumours on both arms. One or two have undergone degeneration. They have been present a month or so.

(January 28, 1916.)

### Case of Chronic Patchy Dermatitis.

By J. L. BUNCH, M.D.

THE girl, aged 11 months, has a number of scattered erythematous patches on the trunk and thighs. These patches made their appearance soon after birth, the first one being on the side of the neck, and they have gradually increased in number. When once a patch has made its appearance it never disappears, but after some weeks or months it develops a yellow tint which persists. No patch is definitely scaly, but some of the earliest ones show a slight roughness of the skin, the recent patches being quite smooth.

#### DISCUSSION.

Dr. BUNCH: The first case was sent to me by Mr. Bankart, and I brought it here in case any member of the Section had seen anything similar. One or two members, after looking at the microscopic slide, suggested there was a definite downgrowth of epithelium, but I cannot for a moment think that the case is malignant. The third case presents some interesting peculiarities. The lesions appeared shortly after birth, and have persisted absolutely; though fresh patches come, the old ones do not vanish, and the lesions soon begin to take on a yellow colour. I have seen a similar disease in adults, from time to time—a variety of parakeratosis—but never before in a child so young as this one. All the lesions were devoid of scales. The treatment which the child has so far had has not made any notable difference in the disease, and this is, of course, characteristic of the disease in adults (xantho-erythrodermia perstans).

Dr. PORTER PARKINSON: I suggested there was a downgrowth of epidermis in the specimen, yet I would not, for that reason, consider the case as necessarily malignant, as one knows how deceptive sections are in that way; it depends entirely on the way in which the sections are cut. One sees many innocent growths which show a similar downgrowth of epidermis; in fact, an ordinary wart will show it. The difference between that and what takes place in the early stage of malignant disease is such that it requires a skilled person to decide; certainly it is beyond my own capacity.

Dr. HUGH THURSFIELD: I would suggest to Dr. Bunch that the section he is showing under the microscope to-day has not gone through the centre of one of these papillæ; there must, I think, be more to be seen in a section of such lesions than the slide he exhibits shows. Would it be possible to have

another excised and a section made? Or perhaps Dr. Bunch has more material? I should expect to find, somewhere in some section, the giant cell formation of tubercle. I suggest that the third case is urticaria pigmentosa—possibly an atypical form; but there is apparently some pigmentation which the light does not enable us properly to appreciate. The distribution and general characters, and the fact that it is congenital, all correspond with such a diagnosis.

Dr. BUNCH (in reply): I have had examined a number of sections from the case referred to, but only one nodule was excised. The sections have been carefully stained for tubercle bacilli, and the von Pirquet reaction has been done once or twice, and though I do not lay much stress upon that, it has been negative each time. With regard to the third case, I do not agree with Dr. Thursfield, the differences are so obvious; but I will have a small portion of the skin excised and will have sections cut. Of course, urticaria pigmentosa has an absolutely characteristic picture.

(January 28, 1916.)

### Double Optic Neuritis.

By C. O. HAWTHORNE, M.D.

W. H., GIRL, aged 7 years. Usual good health until Christmas, when she vomited on two or three occasions in the early morning and before breakfast; there was also some complaint of headache, but the child was never regarded by her mother as in any sense seriously ill. As the vomiting was repeated on several occasions, her doctor advised admission to hospital. The girl is bright and cheerful, and makes little or no complaint; there is marked double optic neuritis but no other evidence of nervous disease, though some degree of alternating convergent squint is noted; vision,  $\frac{6}{6}$  each eye. Cerebrospinal fluid negative; Wassermann's test inconclusive. No evidence of visceral disease.

I think the case presents three features worthy of attention: First, it illustrates the advisability of making the use of the ophthalmoscope a routine procedure in every clinical examination; if this practice had not here been adopted there would have been nothing in the clinical history to encourage the view that the child was in any sense seriously ill, and it is noteworthy that vision is of full standard. Secondly comes the question of diagnosis—is double optic neuritis with an occasional attack of vomiting in the early morning sufficient to justify a diagnosis

of intracranial tumour? Personally I am disposed to answer this question in the affirmative. In the third place, what treatment ought to be adopted? I have not found repeated lumbar punctures to produce any benefit in such cases; nor have I been much encouraged to advise decompression. On the other hand, I have recorded a number of cases—and several similar series have been published—in which, without surgical interference, all the symptoms have subsided, but with the serious qualification that the patients have become blind from consecutive optic atrophy. I shall be much interested to hear the views of members of the Section on the diagnosis and treatment of the case.

#### DISCUSSION.

Dr. PORTER PARKINSON: In such a case, it seems to me, one might very well follow out the expectant treatment at present, noting whether there is any increase in the symptoms or whether fresh ones develop. Should that happen, then a decompression operation should be done without further loss of time. Like the exhibitor, I have not found lumbar puncture of use; indeed, I think that procedure is attended with some danger, especially if the tumour be beneath the tentorium. Cases of sudden death have been recorded, due to some derangement of the intracranial tension. Two years ago I showed before the Society a case in which the result of a decompression operation was extremely good. When the child was in hospital she vomited every day, suffered from severe headache, and, owing to the intense optic neuritis, her sight had become so dim that she could not count fingers held close in front of her eyes. Almost every day also she had fits. After the operation the vomiting, the fits, and the headache ceased, and the sight eventually improved so much that she could read. The optic neuritis cleared up considerably. Eventually she died, however, because the tumour was malignant. Still, the operation gave her several months of comfortable existence which she would not otherwise have had.

Dr. EDMUND CAUTLEY: I would like to refer Dr. Hawthorne to the notes of a case which I showed at the last meeting of the Section, one of double optic neuritis of prolonged duration, which gradually subsided under simple methods of treatment. That case did not present signs indicative of cerebral tumour, unless you so regard optic neuritis. I do not think Dr. Hawthorne's case presents indications which warrant surgical interference. It should be treated on ordinary medical lines, on the chance that it may be toxic and subside, allowing the child to retain a considerable degree of sight. This child does not seem to have any signs of intracranial pressure; there would not be such increased pressure in a child without the production of headache and probably a greater degree of vomiting. And unless there is increased intracranial pressure, the benefit to be derived from a decompression operation is problematical.



Dr. HAWTHORNE: In reply to Dr. Thursfield, the cerebrospinal fluid flowed freely until a test-tube was about two-thirds full, and then it came in drops. I am not sure that the ease or rate of flow of the fluid is always a correct index of the degree of pressure in the cerebrospinal space. I am much indebted to Dr. Porter Parkinson and to Dr. Cautley for their suggestions, and though I feel very doubtful whether so severe an optic neuritis—associated as this is with morning vomiting—can be regarded as of toxic origin, I quite agree that there is no necessity for immediate operation.

(January 28, 1916.)

### Case of Solid Œdema.

By C. O. HAWTHORNE, M.D.

A. C., GIRL, aged 15 years, six years ago fell and cut her left leg below the knee; the limb became red and inflamed and she was confined to bed for a month. Two years later the limb was noticed to be swollen, and this condition has persisted, and even increased, to the present date. There is pitting on pressure over the dorsum of the left foot, and the soft tissues of the left leg and of the lower thigh are thickened but without evidence of dropsy. Measurements show the left calf to have a circumference greater than the right by  $2\frac{1}{2}$  in., while 5 in. above the upper border of the patella the left thigh measures  $19\frac{1}{2}$  in., and the right  $16\frac{1}{4}$  in. The limbs are of equal length, and skiagrams show nothing abnormal in the left tibia or fibula.

The case is, I think, justifiably called one of solid œdema, because there is substantial thickening of the soft tissues of the lower limb (left), though since the girl has left the hospital and has been walking about some œdema of the ordinary form has been super-imposed on the chronic thickening. My view was that the case probably belonged to the group described by the late Sir Jonathan Hutchinson as due to blocking of the lymphatics as a result of repeated attacks of erysipelas, and that the inflammatory condition leading to this had been in this instance a cellulitis following the injury to the limb. The appearance of œdema in the other limb (right) is a new fact since the girl has left the hospital, and I am rather staggered by it. None of the ordinary causes of œdema are present, and I am driven to wonder whether, after a long rest in bed, the balance of the circulation may not be re-established.



## DISCUSSION.

Dr. G. A. SUTHERLAND: The explanation given by Dr. Hawthorne is plausible in regard to the left leg in this case, but I see a difficulty in attributing the œdema of the right leg to the fact that the patient has got up and is walking about. I should be surprised to find œdema of such extent arising under such circumstances.

Dr. F. PARKES WEBER: In regard to the present patient one, of course, thinks of the possibility of so-called "trophœdema," of which I know that our President has seen several examples. But the child says that seven years ago she had scarlet fever, and after that there was some "blood-poisoning," and the left leg was swollen and was black and blue. About two years later the leg was practically all right again; but from that date it has been swollen, off and on, and lately the swelling has never completely subsided. With such a history I hesitate as yet to diagnose the case as one of trophœdema. Trophœdema is an apparently idiopathic disease, which may affect more than one extremity, and more often occurs in females than in males; occasionally it is met with in more than one member of a family. After an attack of venous thrombosis in one leg it is not very rare to find permanent enlargement of that extremity, but not persistent progressive œdema (as in the present case). The present case is not the result of venous thrombosis, but may later on turn out to be an example of "trophœdema."

Dr. HAWTHORNE (in reply): I fully admit the force of Dr. Sutherland's comment. The explanation I proposed of the œdema in the right limb is untenable—that advanced by the President is a much more reasonable one. The cases in which both limbs are affected are familiar to me, and I have figured such a case some years ago. I quite agree that too much stress may be laid on a history of recurrent inflammatory attacks, for, manifestly, these may be, not the cause of the thickening of the integuments, but merely accidental infections in tissues of poor nutrition and exposed to slight traumatisms.

(January 28, 1916.)

### Cardiac Case for Prognosis.

By HUGH THURSFIELD, M.D.

E. S., GIRL, aged 13 years, has had four attacks of chorea: the first in 1912, the last in May, 1915. She now has her cardiac impulse 1 in. outside the left nipple line with some dilatation of the right side, and systolic and early diastolic murmurs. The chief feature of the case,

however, is the pulse irregularity, which varies considerably, but is, according to the electro-cardiogram, due to ectopic contractions of the right ventricle. The cardiac affection does not seem to have checked her growth. She now weighs 7 st. 11 lb. in her clothes, and is unusually muscular and big for her age. What may be expected to be the course of the disease? And what suggestions can be made as to treatment?

The child appeared to be unusually well developed for 13 years of age after the four attacks of chorea: and I thought the irregularity of the pulse was a sufficiently interesting feature to justify my bringing her before the Section.

### DISCUSSION.

Dr. G. A. SUTHERLAND: The irregularity of pulse in this case appears of great interest. The question is, what is its significance? I felt the irregularity of pulse, but by the time I had got to the heart the action had become regular, and during the time I was auscultating there was no trace of irregularity; evidently it comes and goes. The electro-cardiogram shows there is an extrasystole. But when one felt the pulse something more than that came into evidence; a big beat was felt, then a small beat, then a premature contraction. I suggest that the basal sound is the extrasystole, and that after each extrasystole comes first a strong beat, then a small beat of the pulsus alternans, and that this causes the double irregularity in the pulse. I do not think we could have told what was going on there without a pulse tracing. With regard to the irregularity of the heart, I do not think it is of any importance in regard to the future prospects of the child. There does not seem to be sufficient disease to affect the child's growth. She appears to have some cardiac hypertrophy, and to be suffering somewhat from the effects of a valvular lesion. The prognosis depends on whether she can keep free from rheumatic infection.

Dr. EDMUND CAUTLEY: On the whole, I do not agree with Dr. Sutherland. I think the heart in this case is very seriously damaged; there is considerable hypertrophy. I do not attach much importance to the irregularity. There is a remarkably small pulse for such a degree of hypertrophy. Such a combination implies mitral stenosis, and that there is, or has been, considerable myocarditis. In either case I do not think the prognosis is particularly good. The fact that she is well grown is much in her favour. Still, with the conditions I have mentioned, I think that in course of time, or under a severe strain, the heart is likely to give way and dilate.

(January 28, 1916.)

## **Tumours over Manubrium and in Left Calf.**

By EDMUND CAUTLEY, M.D.

MALE, first-born, aged 15 months. Mother died twelve months ago from tuberculosis. A cystic swelling, size of a marble, has been situated over the manubrium for seven weeks. Two weeks ago it was freely movable on the bone and under the skin. During the last few days it has become red and tender. In the outer half of the left calf there has existed a diffuse swelling for six weeks. Two weeks ago the superjacent skin, to which it was adherent, was a little red. At present there is no redness nor tenderness, and the tumour presents no definite edges.

### DISCUSSION.

Dr. G. A. SUTHERLAND: It would be well to open the swelling in the neck and evacuate the pus. There may be pus also in the leg swelling. The probable diagnosis is either a tuberculous abscess or a broken-down gumma.

Dr. HUGH THURSFIELD: I agree with Dr. Sutherland. When I saw the case I was not aware of the tuberculous history, but the family history seems practically to clinch the diagnosis. In my experience the prognosis in these cases of deep-seated tuberculous abscesses is appallingly bad; such children get well for a month or six weeks, and then have generalized tuberculosis. I have seen half-a-dozen go like that in the last four or five years; I do not think any of them are alive now.

Dr. J. D. ROLLESTON: I would like to know from Dr. Cautley whether the von Pirquet test has been tried, because it is generally agreed that it is of value at this age.

Dr. E. CAUTLEY (in reply): I have not had the von Pirquet test applied. I have had the Wassermann done, but have not yet received the report. The surgeons who saw the case took the view that it is gummatous; my own opinion is that it is tuberculous. I do not think it is likely that a child so healthy has syphilis, and there is no evidence of it; the child is not backward, there is no enlargement of liver or spleen, no scarring about the mouth, and no history of that disease in the family. I agree with Dr. Thursfield that it is a low form of tuberculous abscess over the manubrium, and perhaps the disease

in the leg is of similar causation. I do not take such a gloomy view of the prognosis as he does, because I have seen multiple tuberculous nodules in the skin subside and get well, even without scraping. I saw one such case twelve years ago, and the patient is still living, though, perhaps, not quite sound mentally. Undoubtedly some cases which apparently recover die of general tuberculosis later on. In such cases I have sometimes found scattered throughout the brain small yellow caseous tubercles, varying in size from that of a millet seed to that of a pea.

*Note.*—On incision a few days later, thick, non-caseous pus was evacuated from the tumour over the manubrium. No fluid could be obtained on exploration of the tumour in the leg. The Wassermann and von Pirquet's tests were negative.

(January 28, 1916.)

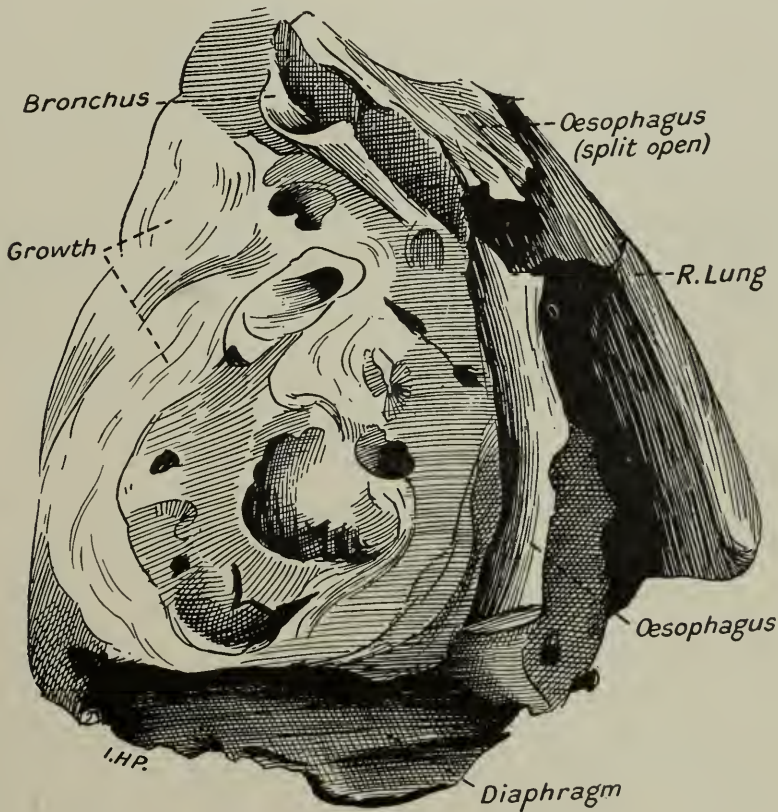
## Heart showing Infiltration by a Large Round-celled Sarcoma.

By E. CECIL WILLIAMS, M.B.

THE specimen was removed from a boy, aged 8 years, who was admitted into hospital with a history of pain in the left shoulder-joint; he had been seriously ill at home for six weeks with cough and much expectoration; two weeks before admission he suffered from vomiting, swelling of the eyelids and orthopnœa. On admission he was cyanosed; respirations 44, pulse 112. The area of cardiac dullness was increased, and the heart sounds were muffled, but there was no distinct murmur. Rotch's sign was present. There was no marked præcordial bulging. Two weeks after admission he was much less cyanosed and could lie down on his pillow, after  $1\frac{1}{2}$  oz. of blood had been removed by venesection. The area of cardiac dullness had diminished and he remained much better for a couple of weeks, when a friction sound (pericardial) was heard over the sternum, also left pleural friction. He then began to get gradually worse, with increasing dyspnœa and some œdema of the feet. There was never any difficulty in swallowing, nor was there any paralysis of the vocal cords. The urine only contained occasionally a trace of albumin. The right lung was dull at the base and there were scattered moist sounds heard behind; the left lung was very dull at the



base, the breath sounds were weak, and 2 oz. of yellow fluid were withdrawn with an exploring needle. There were no enlarged veins over the abdomen, the liver could be felt about four fingers' breadth below the costal margin; there was no free fluid in the abdomen. The boy died on November 26, about seven weeks after admission. The diagnosis was one of pericarditis with effusion and possibly an adherent pericardium.



Drawing showing invasion of heart by large round-celled sarcoma.

At the post-mortem, however, the anterior mediastinum was shown to be full of a hard white growth, adherent to the sternum and invading the visceral and parietal layers of the pericardium, so that the two layers were indistinguishable. Below, the growth invaded the diaphragm. The heart itself was encased in the hard growth, which invaded the heart muscle itself. The mitral valve was thickened and



appeared to be infiltrated; there was no stenosis. There were some scattered areas of growth in both pleuræ, and in each there was blood-stained fluid. In the left pleural cavity there were many tough adhesions. There was no enlargement of either the cervical or axillary glands. Professor Walker Hall kindly examined a section of the growth and declared it to be a large round-celled sarcoma, probably originating in the sternal periosteum.

In Osler and McCrae's "System of Medicine" it is stated that primary malignant growth in the pericardium is very rare; a case of sarcoma of pericardium reported by Broadbent is quoted,<sup>1</sup> where the symptoms were pain in shoulder, dyspnœa, and pleural and pericardial effusions; also a case by Williams and Miller.<sup>2</sup> It is also stated that secondary growths, arising in the mediastinum or other parts, are scarcely more frequent, and that malignant growths of the heart itself are usually secondary to malignant disease of neighbouring or distant parts, and that in a few cases the growth invades both peri- and myocardium. A case is quoted in which the left ventricle was almost entirely invaded by new growth, and the author says there are no symptoms characteristic of myocardial new growth, and that the diagnosis is either impossible or else a matter of good fortune.

(January 28, 1916.)

### Congenital Double Hydro-ureter.

By ERIC PRITCHARD, M.D.

THE specimen shows both ureters to be greatly enlarged, with double hydronephrosis; at the autopsy they were found to be full of thickly purulent urine. The kidney substance is reduced to a shell, and the ureteral openings of the ureters into the bladder are greatly stenosed. The bladder is enlarged and the walls hypertrophied; the urethral orifice appears normal.

The patient, a girl, aged 12 years, was admitted to the Queen's Hospital for incontinence of urine and ingravescient drowsiness. She

<sup>1</sup> *Trans. Path. Soc. Lond.*, 1882, xxxiii, p. 78.

<sup>2</sup> *New York Med. Journ.*, 1900, lxxi, p. 537.

appears to have had good health till within a year of admission, when symptoms of incontinence commenced. Four days before admission pus was discovered in the urine with albuminuria, and the child complained of a sensation of cold down the back.

On admission, the child was in a comatose condition and breathing was slightly stertorous. There was a large, rounded swelling in the abdomen, which extended to the umbilicus. The child had a strong smell of urine and incontinence was continuous. The temperature was normal, the pulse and respirations regular; the blood tension was 125 mm. Hg. The fundi were normal. The urine contained a large amount of pus, with a large number of staphylococci and a few coli bacilli. The uræmic symptoms gradually increased, and the child died four days after admission.

The interest of the case lies in the fact that the child enjoyed good health until the age of 11 years, that the bladder was hypertrophied and enlarged without apparent cause, and that the infection which resulted in the death of the child was staphylococcic, without the production of a rise in the temperature.

*(January 28, 1916.)*

### **Nephritis without Albuminuria.**

By J. PORTER PARKINSON, M.D.

As far as I can discover not a single case of this variety of nephritis has as yet been reported before this Society, and it is quite possible that many members, like myself, have not paid much attention to it. For that reason I thought it would be of interest to relate a case recently under my care in the wards of the Queen's Hospital for Children.

A boy, aged  $3\frac{1}{2}$  years, was admitted on November 13 last year suffering from pneumonia. He had been ill for three weeks previously with cough and fever. The family history was unimportant. He had previously had pneumonia at the age of 9 months. There were a few bronchitic râles scattered over the chest, and signs of consolidation in the left axilla. The temperature rose daily to  $103^{\circ}$  F., till it fell by crisis on November 19, after which it was normal. The urine, examined

every third day, showed nothing abnormal. By November 25 the chest was quite clear and the boy to all appearances well. On November 27 slight œdema appeared on the face, and the next day the face was extremely swollen, as also were the arms, hands, feet and legs, and the wall of the trunk. There was no evidence of fluid in the chest or abdomen. From now the urine was examined daily, but no albumin was ever found. Microscopically a few granular and hyaline casts, and an odd red corpuscle or two, were found on centrifugalizing. The amount of urine passed daily was normal, and fairly abundant urates were deposited on standing. On November 29 the patient got very stupid, but not exactly drowsy. The blood-pressure on this date was 90 mm. By December 1 the œdema had begun to disappear under hot-air baths and saline purgatives, and by December 9 all casts and blood had disappeared from the urine, only a few epithelial cells from the bladder being seen on centrifugalizing. The child was discharged on December 18 in perfect health.

I believe this child to have had an attack of acute nephritis following but not due to pneumonia, causing œdema and the presence of casts and blood cells in the urine, but without albuminuria. What the cause was I do not know, but I do not think it was the pneumococcus, as in such cases I have usually seen severe hæmaturia and abundant organisms in the urine, which was not the case here. There was no sign leading one to suspect intestinal toxæmia, as the motions were regular, not offensive, and appeared normal.

The majority of cases of nephritis, with œdema and without albuminuria, seem to be the result of scarlet fever, and in those in which the patients have died the kidneys have shown typical acute nephritis. But in my case there was no history of any acute specific or other cause, except perhaps acute pneumonia. The absence of albuminuria has probably led to many such cases being overlooked in the absence of careful microscopic examination of the urine, and this, too, is rendered difficult and uncertain by the presence, as a rule, of a large deposit of urates.

Henoch, in his lectures on "Children's Diseases," records several cases, and says that in his opinion even casts may occasionally be absent from the urine. Ballico, in an article on "Albuminuria and Nephritis in Infancy,"<sup>1</sup> states that out of seventy urines examined there were four which had no trace of albumin and showed renal cells

<sup>1</sup> Abstracted in the *Brit. Journ. Child. Dis.*, 1906, iii, p. 163.

and casts, and in two blood cells also. These had no œdema nor any symptoms of nephritis whatsoever. Herbst, in an article on "Latent Nephritis in Children,"<sup>1</sup> reports the examination of the urine of nine children between the ages of 8 and 14 years. Only one had a trace of albumin, and the urine was normal in appearance, quantity, and specific gravity. The sediment contained blood corpuscles, hyaline, granular, and blood casts. The clinical symptoms were only headache, pallor and fatigue, but no œdema or heart complications or retinal changes. In 282 healthy children he found in 43 per cent. more or less the same elements, and so concludes that a few hyaline casts have no pathological significance, and the same with granular casts if in small numbers. Herringham has reported a case<sup>2</sup> of œdema without albuminuria; this he considered to be toxic, due to the same poison that is responsible for the œdema of nephritis.

It is possible that latent nephritis is much more frequent than is suspected, and as the matter has not previously been brought before this Section for discussion, I thought the case worthy of record, and it will, I hope, provoke some remarks on the subject.

Dr. J. D. ROLLESTON: This is a very interesting paper. I have seen a very few cases such as Dr. Porter Parkinson has described following scarlet fever, though I have seen very many cases of scarlatinal nephritis. In one case, at all events, I verified the existence of nephritis by post-mortem examination. Curiously, there has been comparatively little written on the subject in recent literature—and I have taken an interest in the subject and been on the look-out for references—whereas in earlier works, such as the classical treatise by Hensch, to which Dr. Porter Parkinson has referred, one finds much more notice of it. I was recently looking at the article by Jaccoud on albuminuria in his "Dictionary of Medicine."<sup>3</sup> He there quotes Philippe, of Berlin, who said he had sixty patients affected with scarlatinal dropsy, in none of whose urines did he find albumin. It sounds incredible, but it is so stated. A short time ago I read a very interesting Paris thesis,<sup>4</sup> by F. Léonetti, on his experiences as a French medical officer under captivity in Germany. He gives a description of various epidemics, and, among others, one of nephritis, at the camp of Güstrow. "The English," he said, "struck me by the fragility of

<sup>1</sup> Abstracted in the *Brit. Journ. Child. Dis.*, 1908, v, p. 505.

<sup>2</sup> *Proc. Roy. Soc. Med.*, 1908, ii (Clin. Sect.), p. 55.

<sup>3</sup> "Nouveau Dictionnaire de médecine et de chirurgie," 1864, i, p. 547.

<sup>4</sup> "Souvenirs de Captivité: les Épidémies dans les camps de prisonniers d'Allemagne," "Thèses de Paris," 1915-16, No. 8.

their kidneys." They were especially exposed to cold because they had been deprived of their overcoats and body linen. Although the existence of nephritis *a frigore* had been disputed, he had seen twenty cases among these Englishmen with all the symptoms of nephritis, and though in some of them the test-tube was solid with albumin on boiling, in others who were examined there was no albumin to be discovered in the urine.



## Section for the Study of Disease in Children.

President—Dr. ROBERT HUTCHISON.

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(*March 24, 1916.*)

### Rickets ; Multiple Fractures.

By HUGH THURSFIELD, M.D.

THE patient, a girl, aged  $2\frac{1}{2}$  years, has old fractures of both clavicles, both femora, and both humeri, and recent fractures of the right humerus and the left fibula and tibia. She has marked rickety deformity of the thorax.

I am twitted with having implied that the multiple fractures are due to the rickets: that is not my meaning. I think the child has rickets and also has multiple fractures. From time to time one comes across cases of children with rickets, at various ages, who, for some reason, have an undue fragility of their bones, with the result that multiple fractures occur. I think this condition cannot be of the same class as that properly called *fragilitas ossium*, a disease which usually exhibits multiple fractures, either during foetal life or immediately after birth. I brought this child in order to obtain the opinions of those who have seen more of such cases than I have as to whether the condition can be definitely related to rickets.

(*March 24, 1916.*)

### Osteogenesis Imperfecta.

By H. C. CAMERON, M.D.

I. K., AGED 5 months, was admitted to Guy's Hospital. Three other children in the family. No history of fractures in any relatives. Born at full time. The baby is small, though not ill-nourished. On admission,

there was tetany of the hands, and the spasm could be made very marked by rendering the hand cyanosed by the pressure of an elastic band round the upper arm (Trousseau's sign). In a few days this disappeared. Until admission the baby was breast-fed. The shape of the skull is typical of the disorder. The anterior fontanelle is large and continuous anteriorly with a persistent frontal suture, which, in part at least, is widely separated. The posterior fontanelle is also very large. The bones of the skull generally are very thin, and in many places yield on pressure. The ossification is most defective in the lateral parts of the skull, which are formed largely in membrane, making persistent lateral fontanelles of considerable size. In the temporal region, only marked on the left side, there is a bulging outwards above the ear, the apex of which is displaced a little downwards. The antero-posterior axis of the eyeball points downwards as a result of changes in the shape of the orbit. The skull presents a superficial resemblance to that of hydrocephalus of mild degree. The limbs are somewhat curved and a little short. A thickening due to callus can be felt on the shaft of the right femur. The sclerotics are somewhat blue.

I have one or two points in connexion with osteogenesis imperfecta—none of them perhaps of the first importance, but some of them novel—which I am desirous of bringing before the Section. I have the Secretary's permission to show a number of photographs and radiograms of the condition, without which I can hardly make my meaning clear.

The first point which I wish to make is *that in osteogenesis imperfecta, properly so called, we have to deal with a condition characterized not only by deficient ossification and fragility of the long bones but also by a typical and peculiar formation of the skull.* The appearance of this characteristic type of skull differs according to the age of the patient. As seen in infancy its condition is well represented by the child I have brought to-night and by the skull of the little skeleton which I have before me. I believe that the condition has in the past been generally confused with hydrocephalus. In reading the published descriptions of cases, I have been struck by the repeated statement that the disease had been accompanied by "hydrocephalus of mild degree." The skulls which I show to-night have in common with hydrocephalus that the cranium is formed very largely in membrane and, as in the case of the living child, changes exist in the shape of the orbit that have given to the axis of the eye that downward inclination which is so constant a feature of hydrocephalus. Here, however, the resemblance ends. There is no

enlargement of the skull, the fontanelles, although large, do not bulge, and the persistent and wide frontal suture running almost to the root of the nose is a constant feature not found in hydrocephalus of mild degree. In the case of the skeleton the post-mortem examination showed that hydrocephalus was not present and that the ventricles of the brain were not distended. A radiogram of the skull of the skeleton shows other individual peculiarities. The bones are not formed of a single sheet of bone, but are composed largely of a mosaic of tiny, irregular,

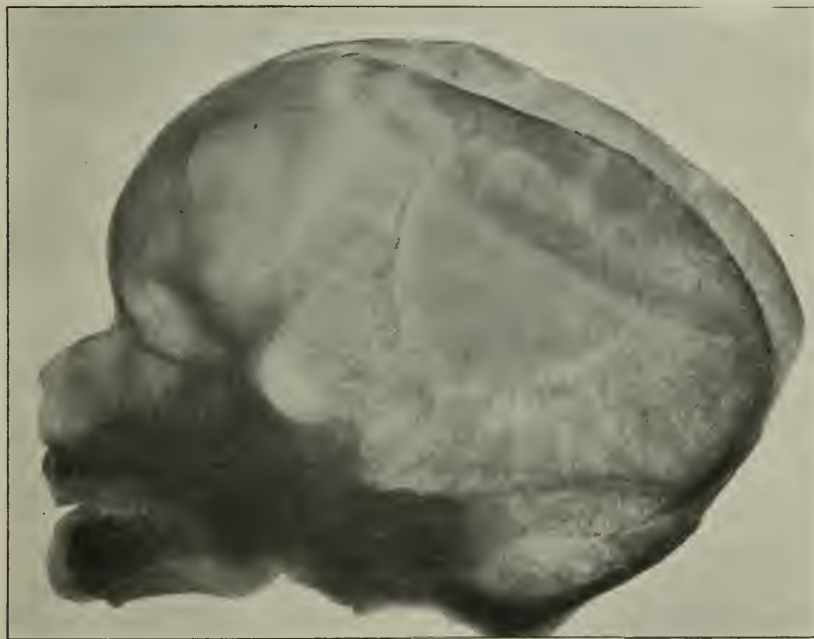


FIG. 1.

Photograph of the lateral aspect of the skull of an infant, aged 2 months, with osteogenesis imperfecta, to show the deficient ossification of the skull and the extensive mosaic of small Wormian bones which has already formed in the temporal region.

separate bones. This is especially apparent in the lateral aspect of the skull.

In later childhood, in cases in which life was prolonged, the most striking characteristic is a marked bulging in the temporal region sufficient to turn over the upper part of the ear. I show photographs of this condition; two of them have been lent me by Dr. John Thomson,

of Edinburgh, who has been long aware of the significance of this characteristic symptom—indeed it was from him that I first learnt of its occurrence.

The bulge and the displacement of the ear are already present on the left side in the infant I have brought to-night, and the bulge is well shown in the very interesting adult skeleton which the kindness of Professor Keith has enabled me to bring from the Museum of the Royal College of Surgeons to which it has been recently added. I have found no mention in the literature of this characteristic feature, but it is of interest that Dr. Poynton some years ago brought a case before the Clinical Section in which he had noticed a remarkable bilateral temporal bulging. Relying on a history of trauma, Dr. Poynton at that time had inclined to regard the prominences as being of the nature of bilateral hæmatomata. I did not see Dr. Poynton's case, and so it is with all diffidence that I mention it and suggest that the bulging was of the type now being described.

A cursory examination of the living child brought to-night might not suggest that there is much at fault with the bones of the legs. I show, therefore, the radiograms of the limbs, which demonstrate a recent fracture in the right femur and typical expansion of the shaft. The shafts of the long bones generally are tortuous, and the left tibia and fibula show a marked antero-posterior curve. I show also a chart of the adult skeleton lent by the kindness of Professor Keith, which illustrates a point which has been noticed by him, *that the shortening of the limbs is more marked in the proximal than in the distal portions of the limbs*. In this, osteogenesis imperfecta resembles achondroplasia, and it is interesting that the infant's skeleton which I have before me was one of four specimens of micromelos awaiting description in the Guy's Hospital Museum, all of which had been classified provisionally under the heading achondroplasia. Three proved to be true achondroplasia, the fourth is the present specimen of osteogenesis imperfecta.

The third point to which I wish to draw attention is the *characteristic expansion and the tortuous, sinuous outline of the shafts of the long bones*. I look on these changes as characteristic and think they are not to be matched in any other condition of mollities ossium. Time does not permit me to show radiograms illustrating the contrast between these curves and those of rickets or osteomalacia.

I am indebted to Mr. Shenton and Dr. Eccles for all these radiograms and to Dr. Nicholson for the histological sections of the bones.

These last show several interesting points. By many writers doubt has been cast upon the nature of some of the so-called fractures in osteogenesis imperfecta. The callus is laid down in a curious annular fashion, and it has been thought that there is sometimes a suspicious symmetry in its appearance. In one of the sections shown, the section has gone through an aberrant mass of cartilage lying under the periosteum, and this mass has been cut twice and lies symmetrically



FIG. 2.

Photograph of an older child, aged 6 years, with osteogenesis imperfecta, who has suffered from at least twenty fractures of the long bones. The photograph shows the characteristic bulging in the temporal region, which is sufficient to turn the upper part of the ear downwards.

disposed under the periosteum both on the right and on the left hand of the section, as though there had been a complete cartilaginous ring around the shaft under the periosteum. The ossification of this ring of cartilage would, no doubt, have resulted in an annular thickening, such as is shown in many places in the infant's skeleton.



(March 24, 1916.)

**Skeleton from a Case of Osteogenesis Imperfecta.**

By H. C. CAMERON, M.D.

E. B., AGED 2 months, admitted into Guy's Hospital. The baby died within a few hours of admission. The birth had been premature and the deformities had been noticed from the first. The skull closely resembles that of the infant I. K. The frontal suture is persistent and widely separated. The anterior posterior and lateral fontanelles are all widely patent. The bones of the skull are very thin. The orbits are oval, with the long axis vertical. In the temporal region the walls of the cranium are formed almost entirely of membrane, and the position of this membranous area corresponds with that of the bulging above the ear which is so marked a feature later in life. Almost every rib is fractured once, and some are fractured twice. The fractures are situated for the most part just in front of the angle of the rib, and form two vertical rows, one on either side of the vertebral column. The position has probably been determined by the pressure of the mother's hands in the axillæ of the child in lifting it from the recumbent position. The pelvis is small and triradiate. The limbs are much deformed and curved. The curves generally seem to exaggerate and perpetuate the posture which the child adopted *in utero*. There are numerous fractures situated generally on the apex of one of the curves. The hands and feet are slender but normal.

**DISCUSSION.**

Dr. G. A. SUTHERLAND : Hitherto we have had isolated specimens of so-called osteogenesis imperfecta, but now Dr. Cameron has linked them up together for us in a way that has not been done before. Perhaps he will not mind me giving the impressions which come to my mind from the clinical standpoint. In the case he showed there is a much more marked change in one part of the child as compared with the rest. In the specimen he showed of the child which died when a month old, the changes affecting the skull and long bones were very generally distributed, whereas in this baby, apparently, the great changes have occurred in the skull, and the remainder of the body has escaped. There is some sinuosity about the humerus, which did not seem to me, however, to be very unusual; and the only other definite lesion is a

fracture of the right femur. Dr. Cameron referred, also, to the bulging over the ears, but in his case it is not a bilateral bulging: it is limited to one side. In a general affection like that, one would have expected that both sides would have been affected. From the clinical point of view, too, the patient shown looks very much like an ordinary case of hydrocephalus. He says cases have been mistaken for that in the past, and I have perhaps fallen into the error myself. The parchment-like crackling of the skull in the temporal region is frequent in hydrocephalus, and the rotation downwards of the axis of the eyes is so markedly associated with hydrocephalus as to be almost diagnostic.

[I wish to add that, having had an opportunity of studying Dr. Cameron's series of photographs of this patient, the generalized changes in the long bones are so marked as clearly to prove his point.]

Dr. F. J. POYNTON: I would like to say a word about the swelling which has been mentioned and insisted upon by Dr. Cameron, because I have had an opportunity of watching the formation of such a swelling. I have had a good number of these cases altogether, have followed up some of them for years, and among them was the patient to whom I refer. She was a Jewish child, who had all the signs of osteogenesis imperfecta; she had had many fractures and was unable to walk. For a long time, over the right ear there was one of these bosses. One day the child fell and hit the side of her head—not a severe blow—and developed a large swelling in a similar position on the left side. This swelling at first was exactly like the cephalhæmatoma such as we see in babies. It lasted for many weeks, and finally settled down into a boss. Somewhat hastily, perhaps, I concluded that these bosses were the results of some slight injury to this peculiarly soft bone, and I am not even now sure this may not be so; trauma may well be at the bottom of their formation, for they are, you see, not necessarily symmetrical at first. Another point is, that in these cases the lower jaw gets very under-hung in at least some of them, and it is a very striking feature in osteogenesis imperfecta: it was very marked in this child. I noticed in another case there were frequent outbreaks of fever just before the fractures were detected, and that fact I have not seen mentioned by others. So at one time I thought there must be an active process in the bone preceding the fracture. The fever was perfectly definite: the case was in hospital, and the fact was noted time after time. It is, I admit, quite possible an injury had occurred resulting in fracture, and that a rise of temperature took place immediately *after* this, for it is very difficult to know exactly when a fracture occurs. What strikes us most in these cases is fracture with bending, but, at the same time, we also meet with a fragility of bone exactly like that seen in fragilitas ossium. In one of my cases, whenever the nurses lifted the child the ribs went “crack”—a most unpleasant sound. Sometimes the condition runs in families. A little boy who was under my care died of the disease, the next child was healthy, and the succeeding child had the disease in lesser degree. I have thought that in a considerable number of these cases there has been definite neglect; not merely a matter of feeding likely to produce rickets,

but general neglect. With regard to treatment, like Dr. Cameron, I had one of these cases early under my care, and I treated it as for syphilis for a long time—it was before the discovery of the Wassermann reaction—and another case was treated by me and the late Dr. Cheadle for rickets, but I never saw any response to treatment. I have also tried various other substances, such as the gland extracts and phosphorus, but none of them proved of use so far as I could recognize.

Dr. J. D. ROLLESTON : Dr. Poynton alluded to such cases running in families. I noticed in this case that the sclerotics are blue, so it is possibly a case of the association of blue sclerotics and brittle bones of which I showed a case five years ago before the Section,<sup>1</sup> and Dr. Cockayne showed one more recently.<sup>2</sup> I would like to ask Dr. Cameron whether there were other cases in the same family showing this association.

Dr. H. C. CAMERON (in reply) : There must be many conditions characterized by multiple fractures of bones, and I would rather suggest that we have, so far, identified one particular condition which should be described as *osteogenesis imperfecta* proper, in which we find not only softening and multiple fractures of long bones, but also those changes in the skull which I have described, and the tendency to affect the proximal more than the distal parts of the extremities. With regard to Dr. Sutherland's criticism, I admit that when I first saw that baby in the out-patient department my first impression was that it was suffering from hydrocephalus, but as I was examining this little skeleton at the time, and as it seemed to me that the case reproduced all the features in the living skull, I thought it would be worth while to take a skiagram of the bones, to see whether there was anything which corresponded with the expansion and twisting of the shafts. The result showed the fracture of the femur and the changes in the bone, which I must maintain are characteristic, although of that lesser degree which is compatible with continued life. With regard to blue sclerotics, I think the familial type, with which they are so constantly associated, must be of a different nature from *osteogenesis imperfecta*. I think the cases which have been described do not date back to foetal life and early infancy, and many of the fractures are found only in later life. There are other conditions too, such as acute rickets and osteomalacia proper in children, associated with fracture, but I think it is clear that in the somewhat rare disorder to which the name *osteogenesis imperfecta* should be confined the clinical features are constant and well defined and will repay closer study.

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1911, iv (Child. Sect.), p. 96.

<sup>2</sup> *Ibid.*, 1914, vii (Child. Sect.), p. 101.

(*March 24, 1916.*)

### **Papulo-necrotic Tuberculide.**

By J. L. BUNCH, M.D.

PATIENT, a youth, aged 17 years, has been under my care at the Queen's Hospital for Children for nine years, and previously under Dr. Adamson at the same Hospital. He was first shown at the Dermatological Society of London in 1906. The eruption had begun when the child was aged 4 years, as a single red patch at the navel, on which small red nodules had developed later. The nodules were slightly raised, somewhat papular in character, and distinctly infiltrated. They had a tendency to necrose, and always left a superficial, shallow scar about  $\frac{1}{8}$  in. to  $\frac{1}{3}$  in. in diameter.

In 1906 there were about thirty of such scars round the umbilicus, and scattered among these were about a dozen millet-seed- to split-pea-sized raised red papules. During the succeeding years similar necrotic papules made their appearance in the inguinal region, on the thighs, on the upper part of the buttocks, in front of and behind both axillæ, and on the shoulders and back. Such papules were succeeded by superficial, depressed scars, similar to those previously described, so that there were then a very large number of such scars on the areas already mentioned—so many that they were not counted. The disease was evidently still in a progressive stage, for, when shown at the meeting there was a well-marked raised red papule on the abdomen which had made its appearance during the past week, of a kind that always went on to the formation of a depressed, atrophic scar.

The boy attended school regularly, and was not inferior in physique to other boys of his own age; there were not, and never have been, any signs or symptoms of lung trouble, but occasionally there have been enlarged glands. Since 1906 the disease has made continuous progress, always in much the same manner, but more recently definite ulcers have been present on the legs and an unusual form of alopecia has shown itself.

Attention may be called to the fact that the nodules and scars are always preceded by a circumscribed, irregular, dry, scaly, red dermatitis, such as was described in 1906 on the inner side of the thigh and arm,



where there are now the characteristic scars. Similar appearances have preceded the atrophic tuberculide elsewhere, and there are, at the present moment, some patches of a similar dermatitis which will go on through the usual cycle.

Guinea-pig inoculations were positive.

All kinds of treatment have been tried ; tuberculins of various nature and in various doses, X-rays, and many local applications.

(March 24, 1916.)

### Multiple Lupus Vulgaris.

By J. L. BUNCH, M.D.

THE patient, a girl, aged 5 years, developed forty-four patches of lupus vulgaris after an attack of measles. These patches were of various sizes, and situated on the face, trunk, and limbs. The lesions have been scraped and X-rayed in addition to other local treatment, and are now cured.

Dr. BUNCH: With reference to the little child with lupus vulgaris, the mother has a scar of old lupus vulgaris on her face. I have not looked up the statistics of the heredity of the condition, but, from the cases I have seen, I should be inclined to say offhand such heredity is extremely rare. The patient with the extensive tuberculide I show in the hope that I may hear some suggestions as to treatment. The boy has been under my care nine years, and under Dr. Adamson before that for four years, and though practically everything has been tried, I cannot say at present that the disease has been even arrested. The treatment which seemed to give better results than anything else consisted in injections of Rosenbach's tuberculin, which is highly thought of both in Japan and this country, but better known in Japan. It consists essentially of a glycerine extract of a symbiotic growth of the tubercle bacillus with a certain trichophyton. Rosenbach suggests a biochemical process taking place in the mixed culture as an explanation of the undoubted diminished toxicity. But after a time it seemed to lose its effect on the case, and the boy got worse. He has a spreading scaly dermatitis, which results in a complete loss of hair, and the history is that the scaliness, when situated on the trunk or limbs, is followed by the formation of these necrotic papules.



(*March 24, 1916.*)

### **Case of Molluscum Contagiosum.**

By J. L. BUNCH, M.D.

THE boy has a number of typical molluscum lesions, but in an unusual position. They are situated in the right groin and are in various stages of evolution. The usual positions—hands and face, &c.—are quite free from lesions.

(*March 24, 1916.*)

### **Transient Hemiplegia in Diphtheria and Diphtheritic Onychia.**

By J. D. ROLLESTON, M.D.

A BOY, aged 5 years, was admitted into hospital on October 6, 1915, with severe faucial diphtheria, on the third day of the disease. 20,000 units of antitoxin were given on admission, 16,000 on October 7, and 12,000 on October 8. The throat became clean on October 11. An urticarial rash appeared at the injection site on October 12, and persisted till October 15. The same day the heart, hitherto normal, showed some slight left-sided dilatation and an occasional extrasystole, and the child vomited. On October 16 triple rhythm developed and the liver became enlarged. On November 2 the voice became nasal. In the morning of November 3, the thirty-first day of the disease, a circinate rash appeared over the limbs. Temperature 97·8° F. During the day he complained of abdominal pain, and at 10 p.m. became very pale and almost pulseless. At 1.30 a.m., the face, eyes, and arms twitched, and between 4 a.m. and 6.40 a.m. he had general convulsions. At 11.20 a.m. he was found to have complete loss of power in the right upper and lower extremities. There was slight facial palsy. The right abdominal reflex was less marked than the left. Both plantar reflexes were flexor. There was no ankle clonus. The knee-jerks were absent. He seemed to understand what was said to him, but could not speak. The left

index, middle, and ring fingers each showed a bulla on the terminal phalanx. At 7 p.m. he had recovered some power in the right upper limb, but the lower limb was still paralysed. The right plantar reflex was now extensor. He could speak a few words. The following morning he had quite recovered his speech, and could move his right arm and leg freely. The right plantar reflex was still extensor, but on the next day became flexor. The voice remained nasal until November 29, but no other nervous symptoms developed, and when he was allowed up, on December 3, the sixty-first day of his illness, he could walk without support.

In spite of a variety of local applications the lesions on the fingers showed no tendency to heal and sloughing took place, worse on the middle finger. No membrane was seen at any time. There was no constitutional disturbance, nor any rise of temperature.

On January 19, a culture taken from the lesion on the middle finger showed numerous groups of diphtheria bacilli, and 8,000 units of antitoxin were injected. Within a week, the lesions, which had been in existence for two and a half months, had completely healed. On his discharge from hospital the left index, ring, and especially the middle finger showed some deformity and ulnar deflection of the terminal phalanges. There was marked limitation of movement of the terminal phalanges of the index and middle fingers, from which nearly all the nails had disappeared. The nail of the ring finger showed only a deficiency on the ulnar side.

Transient hemiplegia in diphtheria is a very unusual condition. Some years ago I reported to the Clinical Section<sup>1</sup> eighty cases of hemiplegia occurring in diphtheria, including six I personally had seen. In all of them the hemiplegia persisted until death or, in those which recovered—the majority—it became permanent. I have never before known of a transient case of hemiplegia in diphtheria, and the question arises as to the cause. I think it is not due to arterial spasm, like transient strokes in adults, nor that it is uræmic hemiplegia. I have reported transient hemiplegia in scarlatinal uræmia,<sup>2</sup> in which it lasted barely twenty-four hours, but have never known hemiplegia in diphtheria to last so short a time. I think it was due to embolism, as in most reported cases of diphtheritic hemiplegia in which there was a necropsy; possibly, as Dr. Hawthorne suggested to me, it was the result of a shower

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1913, vi (Clin. Sect.), p. 69.

<sup>2</sup> *Rev. of Neurol. and Psychiat.*, 1908, vi, p. 530.

of small emboli, because the lesions were easily reparable. About twenty years ago there was an interesting paper by Achard and Lévi<sup>1</sup> on transitory paralyses of cardiac origin. Both the cases there reported occurred in adults, but I think the present case is an instance in point.

There is a question whether the onychia in this case was due to primary or secondary diphtheritic infection. When first seen the lesions were bullæ, like those in a bad burn, and it was many weeks before I thought of the possibility of a diphtheritic infection: there was no membrane. The child had had severe diphtheria, and had been treated with large doses of antitoxin. I then found the diphtheria bacillus present.

In the literature I find many cases of diphtheria of the skin in which the lesions have been bullous from the first, including diphtheritic whitlow. The first man to describe the latter was a Frenchman named Hau,<sup>2</sup> who also says that in many cases there was no membrane present. One might compare them with cases of syphilitic chancre on the fingers, which are often treated for a long time as ordinary whitlows, but when their nature is discovered they rapidly yield to antisymphilitic measures. Dr. Tylecote reported, some years ago,<sup>3</sup> a somewhat similar case in a colleague of his in a fever hospital. He had a troublesome whitlow, which lasted for weeks, in spite of all kinds of local treatment. A culture was then made, and the diphtheria bacillus found, and there was rapid healing on administering antitoxin: in that case no paralysis followed.

Dr. Guthrie<sup>4</sup> and Dr. Paterson<sup>5</sup> have recorded cases of paralysis following diphtheritic whitlow.

This child was under observation a long time, because shortly after the nature of the whitlow was recognized the patient developed chicken-pox, but no further paralysis developed.

*(Discussion follows next case.)*

<sup>1</sup> *Bull. et Mém. Soc. méd. des Hôp. de Par.*, 1897, 3 sér., xiv, p. 1139.

<sup>2</sup> *Lyon méd.*, 1900, xciii, p. 109.

<sup>3</sup> *Brit. Journ. Child. Dis.*, 1913, x, p. 211.

<sup>4</sup> *Lancet*, 1894, ii, p. 1025.

<sup>5</sup> *Med. Times and Gaz.*, 1866, ii, p. 608.

(March 24, 1916.)

**Gangrene of the Leg following Diphtheria.**

By E. B. GUNSON, M.D.

A BOY, aged 6 years, was admitted into hospital on January 5, 1915, with severe faucial diphtheria on the fourth day of disease. He was given 20,000 units of antitoxin on admission, and again on the following day. On the seventeenth day the pulse became markedly irregular (tracings), the onset of arrhythmia being followed by marked increase in cardiac and liver dullness. On the eighteenth day there was a sudden onset of acute pain, referred to the right popliteal space. On the following day a ring of discoloration appeared below the knee and the leg became cold. Gangrene of the right small toe and outer aspect of the foot developed on the twenty-second day, and gradually involved the whole of the leg below the level of the knee-joint. Sloughing of the skin appeared at the line of demarcation on the forty-ninth day. Palatal, pharyngeal and diaphragmatic palsies developed during the fifth and sixth weeks. Amputation of the right leg was performed by Mr. H. S. Clogg, the patient ultimately making a complete recovery.

**DISCUSSION.**

Dr. GUNSON : The point which specially interested me in this case was the fact that before the embolus occurred—to which the gangrene apparently was due—the pulse became markedly irregular, and shortly afterwards the heart dilated, and the child showed signs of cardiac paralysis. Among the number of cases of diphtheria patients I was watching at that time I never saw an instance of cardiac paralysis which was not preceded by cardiac arrhythmia.

Dr. E. CAUTLEY : I think it is a bold diagnosis to say this child had an embolus, seeing how rapidly recovery ensued in the right upper extremity, practically on the same day. And I think Dr. Rolleston has not explained why the attack was associated with the circinate rash on the limbs and with abdominal pain. The condition suggests the possibility that there had been a recrudescence of so-called serum disease, that there had been some exudation of serum into the substance of the brain, which was fairly rapidly reabsorbed. I ask Dr. Rolleston, who has more experience of serum disease than anyone here, perhaps, whether he considers this is a feasible explanation.

Dr. F. J. POYNTON: The point which has been raised in connexion with Dr. Rolleston's case is very interesting, owing to the enormous doses of antitoxin which are given now, though I know this is not the time to discuss that general question. It touches the question of some far-reaching secondary effects.

Dr. G. A. SUTHERLAND: I think Dr. Gunson's point about embolism in association with marked cardiac disturbance is interesting. I have not much experience of emboli occurring in the course of diphtheria, but here, apparently, there was a definite alteration in the condition of the heart, which one may probably attribute to auricular flutter or auricular fibrillation; the blood was kept back in the auricle, and there was a tendency to clotting.

Dr. BOX: Quick recovery from an embolic hemiplegia is quite possible; I have seen this happen in rheumatic endocarditis. A child got embolism and transitory hemiplegia, and was well in a couple of days, with no signs of residual paralysis. With regard to antitoxin, it is true that larger doses of it are now given than formerly, but we see nothing like the severe serum effects which we did in earlier days. I was dealing with cases of diphtheria when the antitoxin was first introduced, and sometimes the serum results from even small doses of antitoxin were terrific; I have had to keep a patient in hospital six months for recurring attacks of rash with pain in the joints. If we use concentrated antitoxin we now see results that are even less serious than when the ordinary serum is used. I think that increased dosage is not responsible for an increase of serum phenomena.

The PRESIDENT: I have had a case somewhat like Dr. Gunson's. The child had diphtheria, and some time after it came with a very dilated heart with an irregular action. It suddenly developed gangrene of one leg, and this spread upwards. The child's condition would not permit of an operation and it died. We found a large embolus impacted in the iliac artery, with clotting in the left ventricle. The valves were sound.

Dr. LEONARD GUTHRIE: I would like to support Dr. Rolleston's view with regard to the causation of the hemiplegia in his case. I see no reason why emboli should not be lodged in some such place as the Sylvian vessels, and then move on. Transient hemiplegia of this kind is not uncommon in heart disease, and I know no other explanation which seems to cover the facts. With regard to the diphtheritic sore on the finger, I ask Dr. Rolleston whether he considers that sore a diphtheritic manifestation or whether he thinks the bullæ were subsequently infected by diphtheria. I did not know of diphtheria occurring in that form, though cases of burns and wounds which have subsequently become infected with the disease are not infrequent. Dr. Rolleston alluded to a paper which I wrote many years ago<sup>1</sup> on the

<sup>1</sup> *Lancet*, 1894, ii, p. 1025.



occurrence of diphtheritic paralysis without previous faucial affection. In that paper I collected all the cases I could in which diphtheria had primarily affected the skin surface without a history of previous throat infection, and also cases in which the primary affection was on the vulva. The true nature of the vulvar cases may be unsuspected, because a membrane is not a prominent condition. There may be slight superficial grey-coloured excoriations and signs of acute inflammation rather than any other appearance of diphtheria, but a subsequent development of diphtheritic paralysis reveals the nature of the original affection. I should like to ask Dr. Rolleston's opinion as to the frequency of primary vulvar diphtheria, and also whether diphtheria may occur in a bullous cutaneous form, or whether a "bullâ" or whitlow, as in Dr. Rolleston's case, becomes subsequently infected by the *Bacillus diphtheriæ*.

Dr. J. D. ROLLESTON (in reply): Dr. Cautley's suggestion is a fascinating one, but I have never, either in my own experience or in the literature, come across any account of a nervous disturbance, especially hemiplegia, as one of the manifestations of serum disease. With regard to Dr. Sutherland's objection, all I can say is that I think the embolism was the result of cardiac thrombosis, which is so apt to occur in severe diphtheria. In answer to Dr. Guthrie, at first I was inclined to think the bullous affection was secondary, but on looking into the literature, I see that cases have been recorded by G. C. Garratt,<sup>1</sup> by G. W. Dawson,<sup>2</sup> and by Hau, and there is reference to it in a recent review by Knowles and Frescoln.<sup>3</sup> So these lesions in my case may have been diphtheritic from the first. Diphtheria of the vulva is, I think, extremely rare; in my 2,600 cases I came across only two in that situation. It used to be more frequent. Dr. Gunson's case is a rare condition. Five years ago<sup>4</sup> I showed a similar case, and I now show the photographs of it. I collected records of ten others. During the five subsequent years, only four others have been recorded,<sup>5</sup> making a total of sixteen. The prognosis, though grave, is not hopeless, because at least eight of them have recovered. The question is, was the gangrene due to an embolus or to a thrombosis due to arteritis? I agree with Dr. Gunson that it is due to an embolus. I have seen a few cases of arterial occlusion in the limbs which would have proceeded to gangrene had the child lived; and other authorities have recorded similar cases.<sup>6</sup> And there have been reported cases of Raynaud's disease occurring after diphtheria,<sup>7</sup> and

<sup>1</sup> *St. Bart. Hosp. Journ.*, 1904-05, xii, p. 35.

<sup>2</sup> *Brit. Med. Journ.*, 1910, ii, p. 859.

<sup>3</sup> *Journ. Amer. Med. Assoc.*, 1914, lxiii, p. 398.

<sup>4</sup> *Proc. Roy. Soc. Med.*, 1911, iv (Child. Sect.), p. 10.

<sup>5</sup> Ransome and Corner, *Lancet*, 1911, i, p. 94; Kramer, *Brit. Med. Journ.*, 1911, ii, p. 505; Aviragnet, *Bull. Soc. de Péd. de Par.*, 1912, xiv, p. 142; Bailly, *Thèses de Par.*, 1913-14, No. 100.

<sup>6</sup> Auché, *Rev. mens. des Mal. de l'Enf.*, 1904, p. 326; Escherich, *Wien. med. Wochenschr* 1907, lvii, p. 474.

<sup>7</sup> Powell, *Brit. Med. Journ.*, 1886, i, p. 203; Chevron, *Thèses de Par.*, 1899-1900, No. 112.

they are rarer still. All these cases of gangrene on record have occurred in children, unlike the gangrene of typhoid or typhus, which have been in adults.

Dr. GUNSON (in reply): The arrhythmia as seen here is mainly a very rapid change in the pulse-rate, and that is often difficult to determine by means of the polygraph. The only radiograms of the condition I have seen have been taken abroad, and in a number of them no arrhythmia was present, and none showed auricular flutter or fibrillation.

*Postscript by Dr. Rolleston.*—I have since come across a paper by Leede<sup>1</sup> containing an account of four cases of diphtheritic hemiplegia, personally observed by him, in one of which—a male, aged 18—complete right hemiplegia developed on the twenty-first day of a severe attack of diphtheria, but completely disappeared within twenty-four hours. Death took place on the twenty-third day of severe myocarditis. Post mortem, there was fatty degeneration of the myocardium, but no thrombi were found in the heart. There were no macroscopic changes in the brain or vessels at the base to account for the hemiplegia.

(March 24, 1916.)

## Rapid Respiratory Rhythm apart from other Evidence of Disease.

By C. O. HAWTHORNE, M.D.

THE patient, a girl, aged 8 years, was under observation in hospital for some six weeks, and no evidence of organic disease was discovered. The respiratory rate was constantly quickened (60 to 80 per minute), and it occasionally exceeded the pulse-rate; when the child was asleep, however, it fell to 20 to 25. There were no other symptoms, and save for the rapid breathing the child seemed to be in a normal state of health. When she left the hospital the respiratory rate was about 40 per minute.

(Dr. Hawthorne submitted a series of tracings showing the rapid respiratory rhythm in relation to the pulse-rate.)

<sup>1</sup> *Zeitschr. f. Kinderheilk.*, 1913, viii, p. 88.

## DISCUSSION.

Dr. G. A. SUTHERLAND: It was a very striking case. I took tracings of the patient, and I have never seen anything like it. The respiratory rate was, at times, faster than the pulse-rate. Once the respirations were 108 per minute and the pulse about 80. In one of my tracings there seemed to be one large wave, followed by a certain number of small ones, and that sort of thing went on pretty regularly. The large waves were 20 to 30 per minute. It seemed that one full inspiration was taken, followed by a succession of smaller movements, which probably did not empty the chest thoroughly, but produced movements of the chest wall. Underlying the whole process there was still the ordinary rhythm of respiration in the background. I ask whether members have seen anything of the same kind, and whether Dr. Hawthorne means "hysterical" when he speaks of functional conditions?

Dr. J. D. ROLLESTON: Personally I have not come across such cases, but recently I have read of instances of hysterical tachypnoea, as Charcot called it, which were characterized by this extraordinary cardio-pulmonary synchronism, in which the pulse and respiration were at the same rate, 112 per minute. One of them occurred in a naval electrician,<sup>1</sup> one in a soldier,<sup>2</sup> and one in a child aged 14 years.<sup>3</sup> In each case it seems to have been a mono-symptomatic hysteria. In some new cases reported there has been a slight organic lesion, such as diaphragmatic pleurisy. In a most interesting paper by Galliard,<sup>2</sup> in which he records tachypnoea in a soldier, one gathers he suspected simulation, but he could not prove it, and one wonders whether there is simulation in this girl's case.

Dr. H. C. CAMERON: In cases of rapid respiratory rhythm which I have seen it has not been difficult to detect its functional character. For example, the apparent dyspnoea has not affected the speech. It is said, too, that in these cases the respiratory rhythm is always reversed, and that such rapid breathing is impossible if the pause follows expiration in the ordinary way.

<sup>1</sup> Plazy, *Gaz. hebdomadaire des Sci. méd. de Bordeaux*, 1915, xxxvi, p. 69.

<sup>2</sup> Galliard, *Bull. et Mém. Soc. méd. des Hôp. de Par.*, 1915, 3 ser., xxxix, p. 1295.

<sup>3</sup> Zanelli, *Riv. di Patol. nerv. e ment.*, 1915, xx, p. 78.

(March 24, 1916.)

### Acute Nephritis without Œdema ; Recovery.

By C. O. HAWTHORNE, M.D.

THE patient, a girl, aged 9 years, was sent into hospital as "probably a case of intestinal obstruction"; the recent history was that of constipation with persistent vomiting of forty-eight hours' duration. Physical examination negative, but for several days the urine was scanty (8 oz. to 10 oz.) with albuminuria (Esbach 3.5) and numerous epithelial, granular, and hyaline tube casts; no blood. There was early cessation of vomiting and the urine was normal at the end of a fortnight. There was no œdema at any time; the maximum sphygmometric reading was 85 to 90; there was no evidence of cardiac hypertrophy; the brachial arteries were palpable, but not in excess of those in other children of same age; and the ophthalmoscopic examination was negative. No clinical evidence of specific disease, but the Wassermann reaction was positive. The personal history was unreliable, and was said to include diphtheria, scarlet fever (two attacks), and measles (three attacks).

The interest of this case lies in the fact that the symptoms did not suggest renal disease in any of its more ordinary forms. I do not say that the case is a unique one, on the contrary, it belongs to a recognized class, in which nephritis manifests itself entirely by gastro-intestinal symptoms, and which possibly would be more often identified were the examination of the urine in children made a matter of routine practice.

#### DISCUSSION.

Dr. BOX: Some years ago Dr. Sidney Philipps drew attention to a form of nephritis arising as a sequel to scarlet fever and simulating intestinal obstruction. I have seen one or two such cases myself in the London Fever Hospital—indeed, one often sees something of the same sort, of mild grade, in scarlatinal nephritis. There is extreme difficulty in these cases in getting the bowels open.

Dr. WALTER CARR: Dr. Hawthorne's patient illustrates the almost insuperable difficulty of giving anything like a definite prognosis when a case of this kind is first seen. I have met with several cases more or less similar to his, and with others in which, in addition to the symptoms of acute

nephritis, there was also slight œdema, cases which form a connecting link with those of ordinary Bright's disease with well-marked œdema. Some of these children get well with extraordinary rapidity. At first the child seems to be seriously ill, and is passing rather scanty urine which contains blood, much albumin, and a number of casts, a condition which it appears likely will take many weeks or even months to clear up, if, indeed, it ever does so. Consequently, an unfavourable prognosis is given, and then after two or three weeks the urine has become perfectly normal and the child appears to be quite well. In other cases, however, apparently quite similar at first to those just described, the disease becomes chronic and perhaps recovery never takes place. We badly need some means of distinguishing at the outset those cases which are likely to recover quickly from those which will probably become chronic and perhaps end fatally.

Dr. EDMUND CAUTLEY: Acute nephritis in children is an extraordinarily variable affection, and there are different stages which we do not fully recognize, I feel sure. There is a type of acute congestion of the kidney—I do not think we should call it nephritis. In this condition, blood in considerable quantity is passed, albumin is passed, and one finds hyaline, and sometimes granular, casts. Yet there may be no œdema, and the condition may clear up quickly, or the albuminuria may persist for two or three months. In some respects it is analogous to "trench nephritis," such as we have seen lately. There is a further stage, more analogous to that in Dr. Hawthorne's case, in which there are many casts, sometimes gastric symptoms, and yet little or no œdema. I am certain that œdema in acute nephritis in children is nothing like so common or pronounced as it is in later life, except in some cases of scarlatinal nephritis. I think there is an acute congestion of the kidney, and a further stage, which may be called acute nephritis; but it is difficult to say that one is justified in making such an arbitrary distinction. The amount of albuminuria varies considerably, and depends, probably, on the degree of acute nephritis and the general condition of the child.

(March 24, 1916.)

### Congenital Sclerodermia and Sclerodactylia.

By E. A. COCKAYNE, M.D.

PATIENT, a male, aged 2 years. First child. Parents healthy. The mother was quite well during pregnancy and has had no miscarriages. The child was born prematurely at the eighth month and is said to have weighed only  $2\frac{1}{2}$  lb. at birth. When first seen at the age of 7 months he



weighed 9 lb. 4 oz. There was hydrocephalus, the head measurement being  $16\frac{1}{4}$  in. The Wassermann reaction was negative. The condition of the skin has changed very little since birth, though a slight increase in the thickening of the skin on the outer aspect of both thighs occurred in February, 1915. There are dilated veins over the scalp, which is almost devoid of hair, and the eyebrows and eyelashes are almost entirely absent. The skin of the face is thickened, shiny and atrophic, especially on the ears and *alæ nasi*. The skin of the abdomen and chest is sclerodermatous and pigmented, that of the back, buttocks, inguinal regions, penis and scrotum normal. The upper arms are unaffected, but there is marked scleroderma from the elbows downwards, and the fingers are small and fixed in a position of flexion. The nails are atrophic, though a little growth takes place. In the lower limbs, the outer aspect of the thighs and the whole of the skin below the knees is thickened. The toes are fixed in the "hammer-toe" position and no growth takes place in the nails. Sweating takes place, especially on the palms and soles. No teeth are present. Growth has been very slow, the weight having been 10 lb. 8 oz. in March, 1915, 11 lb. 2 oz. in August, 1915, and 11 lb. 8 oz. in February, 1916. There is persistent conjunctivitis, and during last summer there was inflammation of the cornea, with subsequent formation of *nebulæ*. Nystagmus is now present.

Treatment first with mercury and then with thyroid extract was carried out for some months without appreciable effect.

(*March 24, 1916.*)

### **Paroxysmal Hæmoglobinuria.**

By E. A. COCKAYNE, M.D.

PATIENT, a boy, aged 7 years, lived in India until March, 1915, where he suffered slightly from malaria. There is a history of a miscarriage before the patient was born, and of a stillborn premature child two years ago. The first attack of hæmoglobinuria occurred the day after landing in England. He was free in the summer, but in the autumn with the onset of cold weather attacks have occurred fairly often. Previous to the attack the ears and feet feel very cold, but there are no definite

phenomena of Raynaud's disease. The child is thin, red-haired, and very anæmic. No definite stigmata of congenital syphilis. The urine during an attack was coffee-coloured and contained hæmoglobin and methæmoglobin, but no red corpuscles and no casts. The Wassermann reaction is strongly positive.

Dr. Browning, who examined the serum of the boy, says: "The serum gives a weak positive Eason's reaction—i.e., there is hæmolysis after exposure of the blood to a low temperature when complement has been added. The boy's serum itself is deficient in complement. The source of the added complement was another human serum, which by itself did not give rise to any hæmolysis of the boy's corpuscles. The deficiency in complement in the boy's serum was further proved by its failure to hæmolyse sensitized ox corpuscles, though the other human serum in the same concentration used as a control caused complete hæmolysis."

The case falls into line with the ordinary cases of paroxysmal hæmoglobinuria, exhibiting in life this curious condition of the blood which can be shown in this way *in vitro*. Apparently there is a body which combines with the red blood corpuscles under the influence of cold, and when the blood is warmed up again the complement combines with this and hæmolysis takes place.

I brought up the case in order to ascertain whether members regard it as an ordinary one due to congenital syphilis, or whether it may be due to the malaria he had when in India, and whether a positive Wassermann can be obtained after attacks of malaria. Should one treat him with antisyphilitic remedies, such as galy, or simply keep him warm?

#### DISCUSSION.

DR. MORLEY FLETCHER: I certainly advise active antisyphilitic treatment in this case. For the last eighteen months I have had under my care a striking example of paroxysmal hæmoglobinuria in a boy. He had considerable enlargement of the liver and spleen. The attacks of hæmoglobinuria could be readily produced by sending him out of doors on a cold day, and they were ushered in by vomiting, dyspnoea, and increased pulse-rate. He gave a strongly positive Wassermann reaction. He was given repeated injections of neo-salvarsan, as well as mercury and potassium iodide by the mouth. He made a rapid recovery, and the attacks have ceased during the past six months. The spleen has diminished to its normal size.

Dr. F. J. POYNTON: I should like to ask whether Dr. Morley Fletcher advocates strenuous antisymphilitic treatment in this boy. Ordinary mercurial treatment does not seem to me to have much effect in these cases. I think many of the milder cases tend to get well of themselves. It is a very important question whether this patient should be given strong drugs. I want to feel, should I start such a treatment, that there is solid evidence of a good result, and Dr. Morley Fletcher would be doing a service if he can encourage us on this point.

Dr. MORLEY FLETCHER: The patient had fairly large doses of mercury ( $\frac{1}{2}$  dr. of the perchloride) and 5 gr. to 10 gr. of potassium iodide for some months, and under this treatment the attacks became less frequent, but did not completely cease until after the administration of neo-salvarsan. After this, exposure to cold no longer caused attacks of hæmoglobinuria. When he was last seen the Wassermann reaction was still positive.

(*March 24, 1916.*)

### **Brain of a Mongolian Imbecile.**

By J. D. ROLLESTON, M.D.

THE brain is from a female infant, aged 12 months, the youngest of eight children, who died of severe faucial and laryngeal diphtheria, complicated by broncho-pneumonia. The brain of a normal infant of the same age who also died of faucial and laryngeal diphtheria complicated by broncho-pneumonia is exhibited to show more clearly the following features of a mongol's brain:—

(1) Reduction of the antero-posterior diameter. The brain, as a whole, is short and oval.

(2) The large and flat character of the convolutions.

(3) The shallowness of the secondary sulci.

(4) The deficient vascularity.

(5) The small size of the pons, medulla, and cerebellum.

The brain, as a whole, is a good size for an imbecile's brain. Its weight is 836.32 gm., as compared with 816 gm.—the normal weight of a female infant aged 1 year. There is no obvious asymmetry nor any localized macroscopic lesion.

(March 24, 1916.)

**Report on a Case of Hemi-hypertrophy, with Post-mortem Examination.**

By ROBERT HUTCHISON, M.D.

SOME years ago I published<sup>1</sup> an account of a case of hemi-hypertrophy in an infant, with a description of the appearances found after death. I have now to report upon another case of the same kind. The patient, a boy, aged 5 months, was admitted to hospital for pyelonephritis, probably due to the *Bacillus coli*. He was illegitimate, and no accurate family history was obtainable. On examination it was at once evident that the right arm and leg were appreciably larger than the left. This was borne out by the following measurements: Length of right arm from acromion to styloid process,  $5\frac{1}{4}$  in.; left,  $4\frac{1}{2}$  in. Circumference of right upper arm, 5 in.; left, 4 in. Circumference of right forearm,  $5\frac{1}{4}$  in.; left,  $4\frac{1}{2}$  in. Length of right leg from anterior superior spine to external malleolus,  $11\frac{1}{4}$  in.; left,  $10\frac{1}{2}$  in. Circumference of right thigh,  $7\frac{1}{2}$  in.; left,  $6\frac{1}{2}$  in. Right semi-circumference of chest at level of nipples,  $8\frac{1}{2}$  in.; left, 8 in. The right semi-circumference of the abdomen at the level of the umbilicus was also greater than the left, but the exact figures do not appear in the notes. There was no apparent asymmetry of the skull or face. In the abdomen the kidneys could both be felt greatly enlarged, but otherwise the viscera were normal upon examination. The urine was acid, and contained pus and a little albumin. The deposit showed large quantities of epithelial cells. The temperature during the few days the child was under observation ranged from  $103^{\circ}$  F. to  $101^{\circ}$  F. The baby went rapidly downhill in spite of treatment by full doses of citrate of potash, and died about ten days after admission.

The following is a summary of the post-mortem examination by Dr. H. M. Turnbull, Pathologist to the London Hospital:—

<sup>1</sup> *Brit. Journ. Child. Dis.*, 1904, i, p. 258.

*Descending Purulent Pyelo-nephritis (Coli Bacilli) ; Mucous Catarrh of Intestines ; Asymmetrical Development of Trunk, Limbs, and Paired Organs ; Hypertrophy on Right Side, with exception of Thymus.*—Markedly enlarged kidneys (right, 70·87 grm.; left, 63·78 grm.) with purulent infiltration in pale cortex and medulla. Pus in both renal pelves. Slight dilatation of right renal pelvis. Dilatation of right ureter (lumen 1·2 cm. diameter) to within 0·5 cm. of cystic orifice. No narrowing of right cystic orifice. Hypertrophy, with great crenation, of right suprarenal body (5 cm. by 3 cm. by 0·5 cm. at thickest; weight, 7 grm.). Left suprarenal (4 cm. by 2·5 cm. by 0·2 cm. at thick margin, and less than 0·1 cm. thick in remainder); weight, 2·5 grm. Three right accessory suprarenal cortical bodies, two with old hæmorrhage replacing foetal cortex (each 1·2 cm. diameter), and one non-hæmorrhagic (0·3 cm. diameter). One left accessory suprarenal cortical body (0·5 cm. diameter), with old hæmorrhage replacing foetal cortex. Right undescended testicle (2·2 cm. by 1·3 cm. by 1·2 cm.; weight, 2·5 grm.) in inguinal canal of congenital hernial sac. Left testicle (1·5 cm. by 0·9 cm. by 0·5 cm.; weight, 1·98 grm.) in scrotum. Right side of tongue, 2·2 cm. broad and 1·6 cm. thick; left side, 1·9 cm. by 1·2 cm. Right lobe of thyroid, 2·2 cm. by 1·3 cm. by 0·5 cm.; weight, with half isthmus, 1·75 grm. Left lobe of thyroid, 2·1 cm. by 1·1 cm. by 0·5 cm.; weight, with half isthmus, 1·66 grm. Postéro-inferior parathyroids: right, 0·5 cm. by 0·25 cm. by 0·25 cm.; left, 0·15 cm. diameter. Postero-mesial parathyroids: right, 0·2 cm. diameter; left, 0·2 cm. by 0·15 cm. by 0·15 cm. Right faucial tonsil, 0·142 grm.; left, 0·130 grm. Right submaxillary salivary gland, 0·870 grm.; left, 0·402 grm. Circumference of lumen of common carotids: Right, 1·05 cm.; left, 1·00 cm. Right lower limb, 28·9 cm. long; left, 26·35 cm. Right thigh, 15·8 cm. circumference; left, 12·9 cm. Right calf, 12·7 cm. circumference; left, 10·7 cm. Right upper limb to styloid, 19·85 cm. long; left, 18·9 cm. Right forearm, 11·9 cm. circumference; left, 10·4 cm. Right femur, 14·3 cm. long; circumference at centre shaft, 4·1 cm.; corticalis, 0·25 cm. broad. Left femur, 13·2 cm. long; circumference at centre shaft, 3·5 cm.; corticalis, 0·20 cm. broad. Right posterior border of scapula, 5·5 cm. long; left posterior border of scapula, 4·5 cm. long. Right thoracic cavity and lung larger, and osseous ribs longer (measurements taken) than left. Right pinna, 4·9 cm. by 2·9 cm.; left, 4·9 cm. by 2·6 cm. Thymus (weight, 5 grm.): left lobe, 7·4 cm. by 2·2 cm. by 0·4 cm.; right lobe, 6·4 grm. by 1·8 grm. by 0·35 grm.; posterior accessory lobe on left side (3 cm. by 1·1 cm.), passing obliquely on to



right lobe. No asymmetry in measurements of lower jaw, skull, orbits, cerebrum, or cerebellum. Anterior fontanelle patent. No premature closure of sutures of skull. Spinal column straight. Exomphalos. Accessory fissures on posterior surface of liver. Foramen ovale patent (0·2 cm. diameter). Umbilical vein and ductus arteriosus closed. No abnormality in metaphyses. Commencing eruption of left lower incisor. Orbits sunken. Wasted child. Pituitary gland, 1·5 cm. by 0·9 cm. by 0·5 cm.; weight, 0·2 gm. Heart, 35·437 gm. Liver, 219·712 gm. Spleen, 14·17 gm. Brain, 694·575 gm. Body, 64·77 cm. long; weight, 4·3659 kilos. Post-mortem Wassermann reaction negative (Dr. James McIntosh).

Dr. Turnbull is of opinion that the asymmetry in this case was due to a right-sided hypertrophy because of the obvious hypertrophy of the right suprarenal, the left being apparently of normal size. He thought also that the right testicle was abnormally large.

## Section for the Study of Disease in Children.

President—Dr. ROBERT HUTCHISON.

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(May 26, 1916.)

(Chairman—Dr. EDMUND CAUTLEY.)

### Two Cases of Optic Neuritis (? Cerebral Tumour).

By EDMUND CAUTLEY, M.D.

#### CASE I.

MALE, aged 6 years 8 months, attended as an out-patient at the Belgrave Hospital for Children on April 17, under Dr. Prentice, who admitted him the following week with these notes:—

“April 10: Headache for two weeks. Wakes up screaming at night. Anorexia, furred tongue, offensive breath, (?) vomiting, B.O.

“April 17: Not so well; headache, retching, drowsiness. Arm reflex greater on the left than right side; normal abdominal reflexes; knee-jerks equal and brisk. Gait unaffected. Optic disks swollen, and hæmorrhage on the left disk. He vomited during the afternoon but did not complain of headache.”

Since he has been in hospital he has not suffered from headache or vomiting.

Mr. Bishop Harman's report on eyes (May 9): “Bilateral papillitis, 1.5D. elevation. Fine interstitial hæmorrhages at left disk, no exudation. No disorder of fixation.”

There is no albuminuria. The gait is a little stiff.

#### CASE II.

Girl, aged 11 years, admitted to Belgrave Hospital on April 27. One month before she had two fits. Since then she has had almost constant headache and vomiting the result of the headaches. On

April 26 she had another fit. She is a strong, high-coloured, healthy-looking girl. Her abdominal reflexes are practically absent; deep reflexes depressed, the left rather more marked than right; plantar reflexes absent. Her head is tilted somewhat towards the right shoulder, which is a little raised. Gait somewhat unsteady, (?) cerebellar, and she tends to fall backwards and to the left. Her left side is a little weaker than the right and shows slight inco-ordination, probably sensory; and for the same reason the left movements are clumsy. The eyes show nystagmoid movements, lateral and rotatory, and slight left convergent squint. Perhaps there is a little weakness of the left facial nerve; and there is double optic neuritis.

Mr. Bishop Harman's report (May 9): "Nystagmus on extreme movement, tendency to left convergent squint; well-marked papillitis in each eye, about 2D. elevation, a trifle more on right side; no hæmorrhages or exudation; slight silkiness of right macular region."

Since being in hospital no special symptoms have been noted and she has not suffered from headache or vomiting.

The probability is that the first patient has a cerebral tumour, but as to the region occupied by it I am not certain. Dr. Fearnside suggests it is in the pontine region. In the other case the tumour is probably cerebellar. I brought them as interesting examples of optic neuritis with remarkably few symptoms. Dr. Hawthorne showed a similar case at the last meeting, and has shown another to-day. Last November I showed a case of well-marked prolonged double optic neuritis in which there were some cerebral symptoms, but the optic neuritis is now almost well, the child never having developed signs of cerebral tumour.

(May 26, 1916.)

### Case of Double Optic Neuritis.

By C. O. HAWTHORNE, M.D.

Boy, aged 9 years, the subject of double optic neuritis of moderate degree, without other evidence of intracranial or nervous disease or of disease in the thoracic or abdominal viscera. Some degree of anæmia, but by no means extreme, and Wassermann reaction negative. Has been under observation in hospital for fourteen days, and except for

some headache on the first day has seemed quite well and happy. Acuity of vision,  $\frac{6}{5}$  each eye. History of occasional attacks of headache commencing eight months ago, sometimes very severe, and in recent weeks often accompanied by vomiting. No admission of injury to the skull or of illness other than the above. There is a history of "consumption" in the mother's family and of "fits" in that of the father; a sister has a "tubercular knee."

The point of special interest in this case is the well-marked double optic neuritis, with not only an entire absence of physical evidence of disease, but also with very slight symptomatic disturbance. I am indebted to my colleague, Dr. Scott Pinchin, for being able to show the case. The boy was brought to the hospital on account of headache, and was referred, after a time, to the ophthalmic surgeon, who prescribed glasses for him, with a view to correct some hypermetropia and astigmatism. But the glasses did not relieve the headache, and vomiting became an occasional symptom. Since his admission to hospital there has been some headache on one occasion, but during the rest of the time he has been running about the ward and enjoying himself. Yet, in spite of this, it is immensely probable that the case is one of intracranial tumour. Some of these children develop, often in an intermittent fashion, symptoms which leave no doubt on the score of the diagnosis, and post-mortem examination brings confirmation. In other cases, though the symptoms may become alarming, they gradually subside, and nothing is left except double optic atrophy consequent upon the neuritis. This is the natural history of cases of double optic neuritis occurring in children as an early symptom, and perhaps the earliest symptom, of intracranial tumour. Toxæmia is the magic formula of the moment for explaining many ills, but I do not know of any justification for applying it to such cases as those here submitted by Dr. Cautley and myself. The presumption must be always in favour of tumour unless some alternative explanation can be established.

#### DISCUSSION.

Dr. MORLEY FLETCHER: I should like to ask how it is proposed to treat these cases, assuming that they are cases of cerebral tumour. Is it proposed to carry out any kind of decompression operation in order to try to save the sight? And I should like to ask what is the exhibitors' experience of the results of operative interference to relieve pressure, and so to check the degeneration of sight in these cases occurring in children, which may otherwise get well.

Dr. CAUTLEY (in reply): Both these children are improving with merely negative treatment. I cannot yet say whether the eye condition has improved or not, I think, from what Dr. Hawthorne tells me, that the eye condition is somewhat worse in the first case. The general condition of both children is better, and the physical signs of nerve disturbance from the tumour are less marked in the girl than when she came in; her nystagmus was very bad, and her gait was more unsteady than now. I propose, therefore, to pursue a policy of masterly inactivity at present. I have no experience of decompression except in one case; in that instance it appeared to do good, but later the child gradually got worse.

(May 26, 1916.)

### Case of Infantilism.

By EDMUND CAUTLEY, M.D.

MALE, aged 6 years 8 months, sixth child. He is said to have been a puny baby, but to have grown up to 3 months of age. At the age of 9 months he only weighed  $4\frac{1}{2}$  lb., and from 13 to 18 months of age he remained stationary at 6 lb. 1 oz. He was entirely breast-fed for one and a half years and partially so for another six months. Proprietary foods were given up to the age of  $4\frac{1}{2}$  years, and since then he has had ordinary diet. His mother suckled her previous baby for eighteen months, up to her confinement with this child. During the first year of life he was remarkably somnolent; and at the age of 3 years he had diphtheria. At times he is troubled with constipation, and occasionally with cramps in the hands and feet. He has had no such troubles during the two months he has been under observation.

His present condition (May 18): Height,  $26\frac{1}{2}$  in.; weight, 16 lb. 14 oz.; head, 17 in. He is pot-bellied, markedly rachitic, and the liver has dropped well below the costal margin. There are enlarged tonsils, adenoids, and moderate adenitis. The skin is elastic and the complexion rather dull. He speaks fairly clearly, and mentally appears to be about 4 to 5 years of age. He has twenty good teeth, the first having been cut at the age of 2 years. His appetite is poor.

On April 22 he developed pertussis. This induced convulsions, once on April 24 and four times on April 25, and the pertussis was followed by general bronchitis. On admission on March 25 he weighed 15 lb. 12 oz. and on April 20 he had gained 28 oz. Since then the pertussis and bronchitis have caused loss of weight. Wassermann reaction negative.

There appears no special cause for his backward development,



beyond malnutrition and unsuitable feeding during early life. He has been taking polyglandin and his condition is now improved, but the improvement may be due to food and nursing.

It was due to Dr. Mitchell Smith that this case came under my care. That the boy possesses marked intelligence is revealed by his answers to the nurses, and he pronounces difficult words plainly. Though he is backward, judged by ordinary standards, I think it is no more than is attributable to his mode of life.

I do not quite know in what group to place this case. It is infantilism in the sense that he is small and infantile. That may have been due to deprivation of nourishment before birth and defective nursing afterwards, the mother having nursed the previous child eighteen months up to the time of her confinement with this one. I expect that when this child gets older, and has a more rational diet, he will grow more quickly. In the case of animals, when they get "set fast," as it is called, as the result of cold and insufficient diet, they remain small and puny, and the coat is in a bad condition. In this patient I have not been able to make out a deficiency in any particular organ.

#### DISCUSSION.

Dr. MORLEY FLETCHER: I can only make here the remark I uttered when examining the child, that it is a case of secondary infantilism so-called, not primary. In my opinion his impaired growth is due to some disease, which I believe to be congenital syphilis; he has the face and complexion which are associated with that disease. I admit there are no other evident signs of that complaint: no glandular enlargement, enlarged liver or spleen, no choroiditis, and the Wassermann test is negative. I think the Wassermann test should be repeated.

Dr. MITCHELL SMITH: I agree with the opinion of the Chairman. This child has not had a fair chance since its conception. The mother is not a robust woman, and the last four babies were born within a period of three years and four months. In addition to suckling the previous child up to the date of this child's birth, the mother shared her supply between the two children for some time. From 18 months to 3 years old the child was fed on Nos. 1 and 2 Allenbury, and was also given 20 minims of brandy daily to assist its growth. So far as I could ascertain, he had no fresh milk till he was over the age of 3 years, and since then he has had one pint or less *per diem*. The child has been equally unfortunate as regards a proper supply of fresh air and sunlight. He has never been in the country, and has spent practically all his life indoors at his home, which is in a poor, low-lying district in the Potteries. The home is clean, but ill-lit and overcrowded, and he is only in the fresh air when his mother can find time to take him out in the perambulator.

(May 26, 1916.)

### Case of Giantism.

By W. MITCHELL SMITH, M.D.

GIRL, aged 12 years 9 months.

Family history: Patient is the seventh of ten pregnancies; two miscarriages; five children died at or soon after birth; three living and



FIG. 1.

Case of giantism.

well: brother, aged 20 years, height 5 ft. 9½ in., weight 16 st.; brother aged 11 years, normal; and present patient. Father suffers from diabetes; his weight, once 17 st., is now 14 st. Mother healthy. Maternal grandmother reputed to be 32 st. at time of her death.

Present condition : Height, 5 ft.  $4\frac{1}{8}$  in. ; weight, 17 st. 7 lb. Very obese, with equal distribution of fat ; large framework, especially across the hips. Pubic hirsuties. Has been menstruating regularly since last Christmas. Blood-pressure 125 mm. Hg. ; urine normal ; optic disks normal. Mentality normal for a girl of her age (Standard VII) ; excellent memory, placid disposition, gross eater.

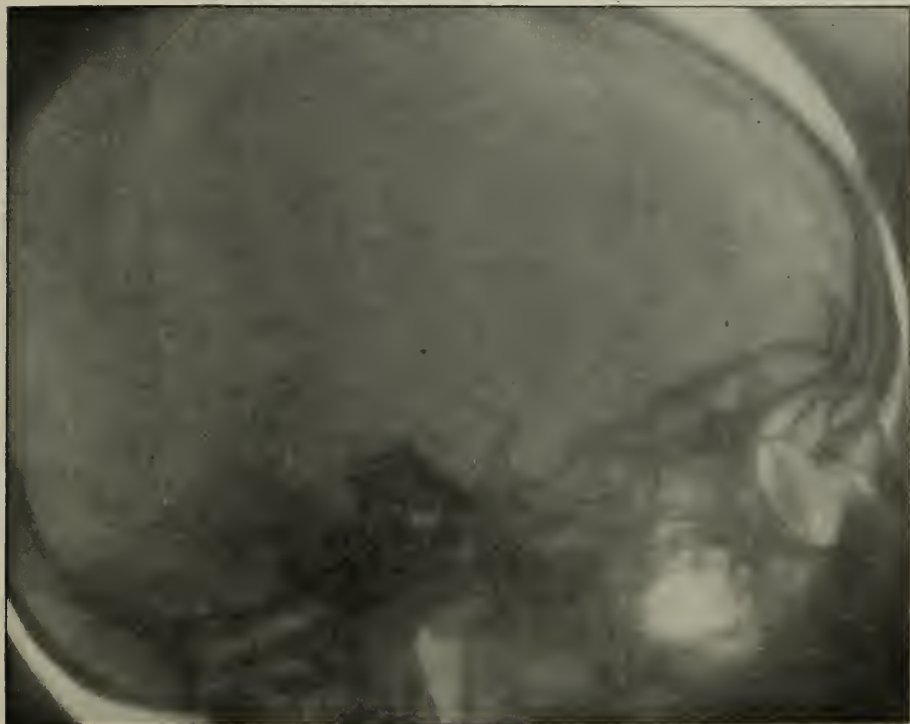


FIG. 2.

Skiagram of sella turcica from case of giantism.

Skiagram of sella turcica (Dr. A. H. John) shows that—(1) it is very well developed for a girl of her age ; (2) there is no erosion of the anterior or posterior part to indicate a tumour of any size.

I have not estimated the sugar tolerance. The father is suffering from diabetes, and I thought it wiser not to run any risks. The parents are very proud of the size of their daughter, and would not welcome any attempt to retard her growth.

## DISCUSSION.

Dr. E. G. FEARNSIDES: This case and one which I brought before the Neurological Section last night have many features in common, but, on the other hand, show striking differences. My patient, a girl, aged 18 years, wears gloves of size 6, boots size 4, and corsets 48. Her hands and feet, in contrast with her body, are extremely small, and the trunk is covered with a great excess of adipose tissue, so that when viewed from in front or from behind her body resembles that of an old and obese woman. In addition she shows a well-developed double chin and hirsuties of the beard and moustache areas, and, although 18 years of age, has never menstruated. Moreover, in her case there was evidence of gross organic nervous disease: double ankle clonus, exaggerated knee-jerks, and doubtful extensor plantar responses, but no evidence of any definite affection of the pituitary gland, thyroid, or suprarenals. In Dr. Mitchell Smith's case the limbs and body are not disproportionate and all the bones seem large, so that, bearing in mind the history of her family, I would tentatively suggest that she is an example of what Sir James Goodhart once called a "normal abnormality"—that is to say, she is a person who is "built large," and as she grows older will continue to be fairly proportioned.

Dr. MITCHELL SMITH (in reply): In support of Dr. Fearnside's opinion, I may add that the doctor who attended at the birth said this was the largest baby he had ever seen born, and the history is that she has continued to grow proportionately ever since.

(May 26, 1916.)

**Case of Juvenile General Paralysis.**

By J. WALTER CARR, M.D.

Boy, aged 8 years. There is nothing in the family history to suggest syphilis, and the patient does not present any of the stigmata of the disease. He has not had any previous illness of importance. For about six months his walk has been getting increasingly jerky, and he has sometimes fallen down. For the last three months his pupils have been noticed to be unequal. Recently his speech has been getting indistinct and mumbling. Formerly he was rather bright at school, but of late has ceased to make any progress; has been unable to write, and has lost his memory. He has not had any convulsions. He is a healthy-looking, rather stout boy. He shows marked mental

impairment, with distinct delusions of exaltation—for instance, he talks of having knocked down a policeman and killed two Germans. He has incontinence of urine and often of fæces as well. The left pupil is larger than the right and reacts very sluggishly to light; the right pupil reacts normally. The left optic disk is very considerably paler than the right. He has a distinctly spastic gait, with double ankle clonus and exaggerated knee-jerks. The triceps and supinator jerks are also increased. It is not possible to be sure whether the plantar reflexes are flexor or extensor. There is no ataxy. Wassermann reaction of blood positive.

Dr. E. G. FEARNSIDES: Whether we call this class of case juvenile paralysis or not, in my opinion it is advisable to treat them heavily with antisypilitic drugs, because in a certain number of cases which come under my notice a good deal of improvement has followed injections with the salvarsan preparations. Certainly one usually finds a greater response to treatment in cases of juvenile general paralysis so-called than one does in adult examples of dementia paralytica. In one boy, aged about 12 years, after injections with neo-salvarsan I was able to abolish completely the positive Wassermann reaction in the cerebrospinal fluid which was at first present, and to diminish the cellular contents of this fluid to average normal counts. The boy then began to learn rapidly at school, and is now able to earn his living. He was injected intravenously with 0.4 gm. of neo-salvarsan at weekly intervals for three doses, and then given 0.6 gm. six weeks later and another 0.6 gm. three months after the inception of treatment.

(May 26, 1916.)

### **Case of Hypertrophy of the Right Side of the Face, Right Half of the Tongue, Left Upper Extremity, and Right Lower Extremity—Hemi-hypertrophy (?).**

By A. S. BLUNDELL BANKART, M.C.

Boy, aged 6 years. Asymmetry of face and limbs was noticed soon after birth. The condition has remained stationary, except in so far as the child has grown. The right side of the face, right half of tongue, right thigh, and right leg are decidedly larger than the corresponding parts on the left side. The left forearm is decidedly, and the left arm is slightly, larger than the right forearm and arm respectively. Skiagrams



of the forearms and legs show that the bones on the enlarged side are slightly thicker than those on the smaller side (?). The right lower extremity is  $\frac{1}{4}$  in. longer than the left. The left testicle is slightly larger than the right.

#### DISCUSSION.

Dr. F. PARKES WEBER: I suppose (as far as the four limbs are concerned) this is a case of "crossed quarter hypertrophy," in which you get one arm and one leg on opposite sides of the body enlarged, so that there is a "crossed hemi-hypertrophy."

Mr. P. LOCKHART MUMMERY: I showed a case of this condition at this Society for three or four years, during which time it came up periodically, and, when reporting it, I collected the records of many more such cases. In regard to the present case, I think the really important requirement is that Mr. Bankart should show it again in two years' time. One is very loth to accept a case of this kind as belonging to the class of ordinary hemi-hypertrophy; the present case is not a hemi-hypertrophy, it is an enlargement of the face and leg on one side, and on the opposite side an enlargement of the arm. If this case is one of hemi-hypertrophy, as it is declared to be, it upsets the theory on which these cases are based—namely, a lesion of the brain. In a couple of years' time its nature will have become more obvious, and there should then be no uncertainty as to its nature.

Dr. F. PARKES WEBER (in further comment): We have had shown at London medical societies various cases of "crossed quarter hypertrophy," one quarter (limb) being enlarged on one side, and the other quarter (limb) being enlarged on the other side. I think that such cases are of the same nature as hemi-hypertrophy.

Mr. BANKART: I would ask Dr. Parkes Weber to explain what is the nature of the real hemi-hypertrophies.

Dr. F. PARKES WEBER (in reply to Mr. Bankart's request): By "true hypertrophy of the type of typical hemi-hypertrophy" I mean hypertrophy of all the tissues of the limb or part involved—more or less general—without any condition of elephantiasis, or disproportionate increase of nerves and the fibrous tissue of nerve trunks (in the way of plexiform neuroma), or disproportionate hypertrophy and dilatation of veins and arteries, or diffuse angiomas (hæmangiomata) or lymphangiomata: I mean a genuine hypertrophy, in both length and breadth, and fairly equally divided between the different constituent tissues of the part involved. Half the body or only one quarter (one limb) of the body may show this "true hypertrophy of the type of typical hemi-hypertrophy," or there may be a crossed hypertrophy—an

arm being involved on one side and a leg on the other side of the body. And I have little doubt that one might meet with a non-symmetrical case of *double hemi-hypertrophy*, in which there is "double hemi-hypertrophy" in so far as the limbs on both sides of the body are hypertrophied, though the hypertrophy on one side is greater than that on the other side. Other combinations of "quarter hypertrophies" can be imagined.

(May 26, 1916.)

### Case of Ulceration of the Soft Palate.

By E. D. D. DAVIS, F.R.C.S.

A GIRL, aged 13 years, was sent to Charing Cross Hospital by Dr. J. D. Rolleston with the history that she had been admitted to the Fever Hospital for diphtheria on April 14. One week before admission to the Fever Hospital she complained of sore throat but was otherwise quite well. Cultures of the fauces showed no diphtheria bacilli and direct smears showed no Vincent's organisms.

When seen by me on May 19 there was a large irregular ulcer involving the left half of the uvula, soft palate, and left pillar of the fauces, with considerable loss of the left half of the palate. The ulceration was atypical of lupus, tubercle, or syphilis. There is no other lesion of the nose, pharynx, larynx, or ear. A piece of the floor of the ulcer was removed under cocaine anæsthesia for microscopy. A bacteriological examination is being carried out.

The Wassermann test has yielded a positive reaction, and the patient is already on iodide of potassium and mercury. I propose to give her an injection of novo-arseno-benzol.

Dr. J. D. ROLLESTON: I saw this case before it was sent to Mr. Davis. Two years ago the child was treated for tuberculosis of the upper arm. The question is as to whether that might have been syphilitic bone disease. In view of the excellent family history—there is a large family and all the other children are healthy—it is possible that this is a case, not of inherited, but of acquired, syphilis.

Note by Dr. ROLLESTON, July 1: Mr. Davis informs me that the ulceration has healed with mercury and potassium iodide, and an injection of novo-arseno-benzol (3.75 mg.), and that he has obtained a definite history of infection of both parents.

(May 26, 1916.)

### Case of Lupus of the Hard Palate.

By E. D. D. DAVIS, F.R.C.S.

THIS case is shown for comparison with that just exhibited. It is the ordinary type of lupus, except that its distribution is a little unusual. The lupus which one sees is usually on the fauces, the uvula, soft palate, pharynx, occasionally on the larynx, but more commonly still in the nose.

(May 26, 1916.)

### Case of Recurrent Jaundice.

By P. HAMILL, M.D.

(Shown by EDMUND CAUTLEY, M.D.)

GIRL, aged 13 years, has been under observation since November, 1914, suffering from recurrent attacks of jaundice. From then up to the present time she has not been entirely free from pigmentation. During the exacerbations bile is present in the urine, the fæces are clay-coloured, and the liver is enlarged. In the intervals her general health is good. Since January, 1916, the spleen has begun to enlarge, and has extended more than 1 in. below the costal margin. During an exacerbation the blood count was found to be: Red blood cells, 3·78 million; white blood cells, 10,800 per cubic millimetre; hæmoglobin, 70 per cent.; colour index, 0·9 per cent. Differential count: Polymorphonuclears, 52 per cent., total number 5,620; lymphocytes, 45 per cent., total number 4,860; eosinophiles, 1 per cent., total number 100; basophiles, 2 per cent., total number 220. Very slight anisocytosis and poikilocytosis, no nucleated red cells seen. Corpuscular fragility normal. Loewi's pancreatic reaction is negative.

The Wassermann reaction is slightly positive. Apparently it is not

a case of recurrent acholuric jaundice; possibly it depends on some hepatic condition. The positive Wassermann suggests it is syphilitic. She is the only child, and, apparently, is healthy except for the jaundice. Dr. Hamill will be glad of suggestions on diagnosis or treatment.

(May 26, 1916.)

### **Case of Unilateral Enlargement of the Tongue.**

By P. HAMILL, M.D.

(Shown by EDMUND CAUTLEY, M.D.)

GIRL, aged 5 years, was brought to hospital with unilateral enlargement of the tongue. It is with difficulty that the tongue can be retained in the mouth. The enlargement appears to affect the right side only, and involves the floor of the mouth. The papillæ are enlarged but do not resemble the minute vesicles commonly seen in cases of lymphangiectasis. Skiagrams do not reveal any corresponding hemihypertrophy of the cranial bones. It looks like a simple hypertrophy. It does not seem to be a lymphangiectasis.

(May 26, 1916.)

### **Congenital Defect of Left Ulna.**

By PAUL BERNARD ROTH, F.R.C.S.

GIRL, aged 9 years. The case was described in the *Lancet*, May 23, 1914.

I show this case because of its rarity. I meant to show it in 1914, but was prevented by the War. The chief differences noticeable after two years are: (1) A diminution in the amount of ulnar deflection of the hand, due to the long straight splint which has been worn every night; and (2) the free projection of the upper end of the radius under the skin on the outer side of the humerus; it articulated before with the

front of the humerus in the radial fossa. This effect must also be due to the splint; the radius has lengthened; as it has not been allowed to deflect the hand, it has had to push itself somewhere, and it chose the line of least resistance and pushed past the humerus. If its further projection later on causes annoyance I propose to cut off an inch or so without in any way damaging the functions of the limb.

The radiogram by Dr. Ironside Bruce shows—(1) that the curvature of the radius has lessened; (2) that there is now an ossific centre for the head of the radius; (3) that the two ossific centres about whose identity there was previously some doubt are respectively for the lower end of the ulna and for the tip of the olecranon; (4) that there are now



Radiogram showing congenital defect of left ulna. Note (1) the curvature of the radius, and how its upper end lies subcutaneously to the outer side of the humerus; (2) that the lower half of the shaft of the ulna is missing, though the epiphysis for its lower end is present; (3) that only two digits are present—viz., the thumb and (?) little finger.

three carpal bones—the os magnum, unciform, and cuneiform. These cases cannot be as rare as Wierzejewski<sup>1</sup> supposed in 1910, when, in addition to his own case, he was only able to collect twenty-two others from the literature; for since publishing this case I have come across three others, about which I hope to make a complete communication later. The first of these was shown by my father, the late Mr. Bernard Roth, at the Clinical Society<sup>2</sup> in 1888. The patient was a girl, aged 11½

<sup>1</sup> Wierzejewski, *Zeitschr. f. Orthop. Chir.*, 1910, xxvii, pp. 101-131.

<sup>2</sup> Bernard Roth, *Trans. Clin. Soc. Lond.*, 1888, xxi, pp. 283 and 284.



years, and her right ulna was defective. The second case is in a doctor friend of mine, again the right ulna being affected. And the third case, published by Dr. Metcalfe<sup>1</sup> last year, occurred in a Russian clerk, aged 24 years, in whom both ulnæ were deficient.

(May 26, 1916.)

### **The Teeth in Rickets.**

By J. LAWSON DICK, M.D.

THESE observations on the teeth in rickets form part of a general examination of 1,000 school children attending L.C.C. schools in the East End of London. Without entering into the larger results it may be stated that 80 per cent. showed distinct evidence of rickets. In other words, practically all children living under conditions such as prevail in the East End of London have to struggle through a rickety phase of their existence in the first two years of their life, and this struggle leaves its marks in certain definite signs or stigmata which persist during the whole of the child's school career. A special point was that the children were mostly Jewish children. They were purposely taken to show the relationship between nutrition and rickets. As a rule the nutrition, especially the fat nutrition, of Jewish children is very good, and over 80 per cent. of the children were breast-fed for from twelve to eighteen months.

Defect in the calcium metabolism, especially in the laying down of calcium salts throughout the skeleton, is the most prominent feature in rickets, and the one responsible for most of the physical signs which mark the disease. Nowhere is this factor more marked than in the teeth. In rickets caries is exceedingly common, and hypoplasia, or defective calcification of the enamel, is well marked. A hypoplastic condition of the teeth is characterized to the naked eye by a defective formation of the enamel and frequently stunted growth of the teeth. There may be only a pitting, producing a honeycombed appearance of the enamel, or the enamel covering is slight, and the cutting edge of the teeth presents sharp points, giving a characteristic appearance to the

<sup>1</sup> J. Metcalfe, *Arch. Radiol. and Electrol.*, June, 1915, p. 18.

tooth. The defect usually extends from the cutting edge and may, in severe cases, involve the whole crown. As in syphilis, the condition is found typically in the permanent dentition, but whereas notched incisors and the contracted first molars of syphilis are but rarely met with in school children, the hypoplastic teeth of rickets are common. In everyday life one constantly sees these teeth and notes how well they often last. Thus frequently a smoker is seen whose teeth have been worn down in depth, and only short stumpy teeth are left, with a layer of enamel all round the cutting edge and the dentine exposed in the centre of the biting edge.

The calcification of the teeth begins about the fifth month of intra-uterine life, and the following diagrams (*see* p. 85) give the rate of progress of calcification at various periods for both the temporary and permanent teeth.

The only teeth of the permanent set which show any signs of calcification at birth are the cusps of the first molars. Fig. 3 shows the portion of the enamel which has undergone calcification at the end of the first two years of life, and the parts affected by the commonest form of hypoplasia. In the typical form of hypoplasia commonly met with, the teeth affected are the central and lateral incisors, the tips of the canines, and the crown of the first molars. The condition is symmetrical, affecting both jaws. Usually the depth of the defect is greater in the enamel of the central incisors than of the lateral incisors, and it will be noted that the enamel affected is identical with that laid during the first two years. This condition is almost pathognomonic of rickets. Rickets is the only condition which interferes with the deposition of calcium over this prolonged period.

In cases of acute illness, especially fevers, the finger-nails are apt to show grooves marking the state of depressed growth of the cells of the bed of the nail during the progress of the disease. In the case of the teeth, a groove more or less broad, or even a succession of grooves with healthy enamel in between, may mark attacks of grave illness in the child. Frequently without a history, a shrewd guess can be made at the period of occurrence of some serious illness by the part of the enamel of the teeth which is affected. But it is usually a grave and prolonged illness which thus leaves its mark, more especially measles, followed by whooping-cough. Scarlet fever is less likely to have this effect. The internal secretory glands apparently play a large part in the defence of the body during attacks of infectious disease, and probably no gland plays such a large part as the thyroid. The thyroid apparatus,

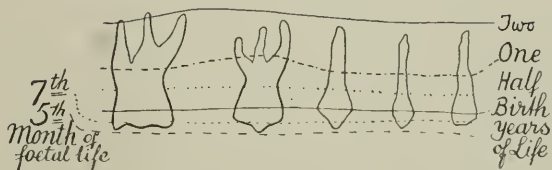


FIG. 1.

Showing calcification of temporary teeth at various periods

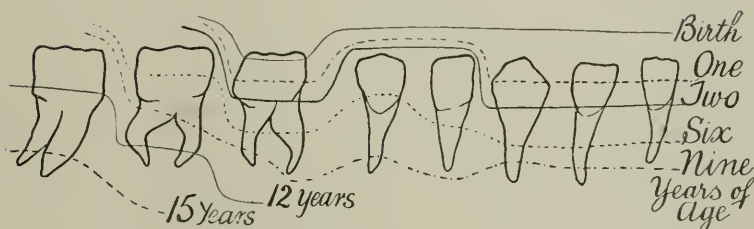


FIG. 2.

Showing calcification of permanent teeth at various periods.

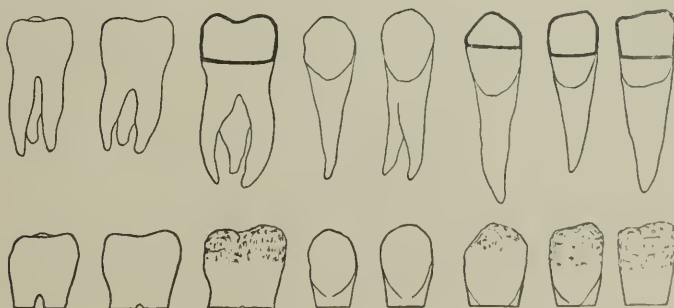


FIG. 3.

Permanent teeth, showing the parts calcified in the first two years. Below :  
to show the parts affected by commonest forms of hypoplasia.

more especially the parathyroid, as pointed out by Loeb, is largely concerned in governing the calcium metabolism of the body, and any depressing cause which makes extra demands on the gland over a prolonged period, when calcium is being actively assimilated and laid down in the bones and teeth, is liable to show itself in defects of the enamel.

Hypoplasia due to congenital syphilis is rare, whilst the form due to rickets is exceedingly common among school children. At birth calcification has involved the crowns of the deciduous incisors, the cusps of the deciduous canines and deciduous molars, and the tips of the cusps of the first permanent molars. The portions of the temporary enamel not yet calcified at birth in the temporary set frequently suffer from hypoplasia. Any disturbance in the function of the ameloblasts, the cells which, according to Tomes, either secrete the enamel or in which the calcium salts are deposited, will lead to the enamel being thin in parts and to the lime salts being imperfectly laid down, so that the compact enamel is more easily disintegrated. In other words, the agents which determine the tendency to decay are those which affect the soft enamel organ in the earliest history of the tooth and not those which affect the enamel after the tooth has been erupted. Ewan Waller holds that alterations in the quantity and quality of the saliva in hypothyroidism are prime factors in the causation of decay. Lactic acid produced by fermentation of carbohydrates is generally held to be the chief factor in producing caries. Waller believes that in thyroid inadequacy the parotid saliva is rendered less alkaline, or actively acid, from deficiency of calcium bicarbonate in the saliva, and that therefore the saliva fails to neutralize the lactic acid. Possibly this is a factor, but the chief factor is probably the manner in which the calcium salts are deposited in the soft enamel organ.

Seafaring communities are said to have good teeth, and this is generally attributed to the action of ozone and pure air. But more likely causes are the favourable conditions under which the children of such communities are brought up in early life, as regards fresh air, sunlight and ventilation, and the consequent better balance of the internal secretions controlling the processes of growth throughout the body. Professor Keith, in his fascinating book on "The Antiquity of Man" (p. 12), speaking of the Coldrum collection of Neolithic men, draws a comparison between Neolithic and modern man as regards their teeth :—

"Amongst modern Kentish folk, as is the case all over modern Britain, there is a tendency to crowding and irregularities of the teeth; the palate and jaws do not grow and expand sufficiently in youth to give room for a symmetrical eruption of the teeth. . . . The nose is narrow and the palate contracted, and its vault is high. The teeth are not worn down as in Neolithic men; they are very liable to be attacked by caries. The front teeth, when the jaws are closed, do not meet edge to edge as in primitive races; like the blades of scissors, they overlap, the lower passing behind the upper. In the Neolithic people all these modern characters are absent. Abscesses or gum-boils at the roots of the deeply ground teeth were, however, common; but there is not a single carious tooth to be seen in the Coldrum collection. The teeth are regular in their arrangement, the palates were well formed, but in actual size the teeth possess the same dimensions as those of modern English people. All these changes, which are appearing in the teeth and jaws of modern British people, arise, we suppose, from the soft nature of our modern diet. We believe that were modern men to resume a Neolithic diet their teeth and palates would again be moulded in the ancient manner."

The difference is, however, hardly capable of such easy solution. Rickets was probably a disease unknown among Neolithic infants, as is largely the case among native races to-day, notwithstanding frequent privations as regards food. Contrast the life of Neolithic men living under primitive conditions, rising with the sun and going to rest at sunset, and living in small communities scattered over wide areas, with that of the slum conditions under which so large a proportion of our population lives to-day. It is not a question of food, but of the deprivation of fresh air, exercise and sunlight, which profoundly alters the metabolism of the child and produces aberration in the growth not only of the bones and teeth, but in probably every tissue of the body. Natural selection is not always beneficial in its action. Nature is ever mindful of the continuity of the race, and if the slum-dweller is essential for its continuity an individual capable of adapting himself to such conditions will undoubtedly be produced.

In judging of the presence of hypoplasia, it was found practically impossible to make accurate observations on the temporary teeth of infants of school age. Caries was so universal and extensive that it completely masked the hypoplasia. It was evident that a hypoplastic condition of the teeth was common in the temporary set, and was, in all probability, the chief factor in bringing about premature decay. For the purposes of statistics only the records of the permanent dentition have been taken, and in marking a case as one of hypoplasia, only the severer forms are admitted, and cases of slight pitting, chalky-looking



patches in the enamel, and so on have been excluded, although microscopically such teeth would be found markedly defective and liable to disintegration. Of the 586 rickety cases in which a record of the permanent teeth could be taken, 42 per cent. had normal teeth and 58 per cent. had defective teeth; 20 per cent. of these showed hypoplasia frequently combined with decay, and 38 per cent. had decayed teeth. This is not equivalent to saying that 42 per cent. of school children have normal teeth. As already pointed out, all infants with a record only of temporary teeth have been excluded, because these teeth were so universally decayed as to make accurate observations on the structure impossible. Again, most of the children with records of the permanent teeth were about the ages of 12 or 13 years, when all the permanent teeth have been erupted except the third molars, and caries has least time to make its appearance. It is a somewhat quaint commentary on the general state of the teeth of the community to have to explain why only 58 per cent. of the individuals are given as having defective teeth.

Of the cases with carious teeth, the lower first molar was decayed in 80 per cent., the upper first molar was decayed in 30 per cent., one or more lower premolars in 30 per cent., and one or more upper premolars in 12.5 per cent. The incisors, canines, and second molars were seldom decayed. The shape of the incisors and canines must protect them from many of the causes of decay to which the flattened grinding molars are subjected. The lower first molars decay out of all proportion to the others, and their earlier eruption is not a sufficient explanation of this. It is to be attributed rather to the main part of the enamel of the crown having been laid down in the first two years of life, when rickety conditions are operative. Naturally the lower teeth, which lie in the well of the mouth, will suffer more seriously from deleterious influences which surround the teeth.

Even though no macroscopic change indicative of hypoplasia is to be found in these teeth, in all probability their microscopic character is distinctly affected. One practical point may be noted. Frequently on looking into a child's mouth, the two milk molars or the first permanent molar will be seen with blackened exposed dentine and the enamel on the surface of the crown removed. Closer examination will show that these are still effective teeth and that there is no pyorrhœa or gum irritation around them. The surface of the tooth can be freely touched with the spatula. The dentine of the tooth seems to have become consolidated, and usually, though the teeth do not look well, they can be

safely left, the process of decay having become arrested. Frequently it is not a process of decay in the ordinary sense, for the thin defective enamel on the surface of the crowns of the grinding teeth can sometimes be seen breaking off in distinct flakes. Again, many of the conditions conducive to rickets are present in the parents of these children, and *a priori* it might be expected that this would frequently be a congenital defect. In such a case in a certain proportion of children the milk incisors should show hypoplastic changes. The enamel of the crown of the milk incisors begins to be laid down about the seventeenth week, half-way through intra-uterine life, and has progressed to a considerable extent at the time of birth. At birth, half the crowns of the incisors, the tips of the canines and the cusps of the molars are calcified in the temporary set, and by about six months after birth the calcification of the crowns is completed. Careful search for a hypoplastic condition of the milk incisors during a long period both in school children and in the babies who have attended the Hackney Mothers' Centre has failed to detect a case. It is strong presumptive evidence that rickets is not a congenital condition, and is another instance of the care that Nature takes that, whatever else suffers, the germ at least is protected during the intra-uterine life. If in achondroplasia the milk incisors showed signs of hypoplasia, this would be excellent evidence to link it up as a congenital form with rickets.

A later form of hypoplasia is every now and then seen in which the two premolars and the second molars are affected, whilst the incisors, canines and first molars are not affected. This later form of hypoplasia must be due to influences acting on the child from the second to the sixth year. Association has been recognized between lamellar cataract and hypoplastic teeth. Mr. Norman Bennett collected twenty-two cases of lamellar cataract, all of which showed well-defined hypoplasia in the permanent teeth, and in thirteen cases there was a history of convulsions whilst teething.

#### DISCUSSION.

Dr. ERIC PRITCHARD: I should like to ask Dr. Dick, as it concerns the paper which I shall read immediately, why he selects the first two years as the particular period in which the conditions exist which he thinks are responsible for rickets; why they should be operative in the first two years, and not subsequently? I ask, because it seems to me that the children still continue to live under the same bad conditions—bad air, lack of sunshine, and restricted exercise. One would imagine that the same conditions would be

operative till at least the seventh year or later, and influence the development of the permanent teeth during the whole of this time.

Dr. J. SIM WALLACE: I should like to remind the author that there have been other papers written on this subject, the authors of which have arrived at very different conclusions from those of Dr. Dick. I allude to such papers as those by Mr. Denison Pedley and Mr. Norman Bennett, and both of them seemed to indicate that rickets and caries are not specially associated. There is much in Dr. Dick's paper into which time prevents me from entering properly. He notes two kinds of hypoplasia, describing one as due to rickets, the other as due to scarlet fever or other severe illness. The author also said that the form due to rickets is very common, and yet there are only about 7 per cent. of children with hypoplastic teeth altogether; and it is generally believed that the majority are due to some of the exanthemata which occur at the time of the formation of the enamel. It seems to me, moreover, that some points have been completely lost sight of in this paper. The author does not seem to have compared the amount of caries of the teeth among rickety children with the amount of caries among those who are not rickety. They are extremely bad in both cases. He leaves out of consideration the temporary teeth, yet the temporary teeth, according to his own observations, are formed before rickets has had time to affect the calcification of the enamel. Nevertheless, at the age of 6 years—that is, after the temporary teeth have been but a few years in the mouth—there is an average of six or more decayed teeth in the mouths of all children; and, as a matter of fact, caries is more common and more rapid in temporary teeth than in the permanent ones. Moreover, caries frequently does not affect hypoplastic teeth or teeth due to rickets—if that is the cause. Indeed, it has been a matter of astonishment to dentists that they should so frequently remain free from caries, even though, to outward appearance, they seem insufficiently calcified and liable to decay. Of course, when the pits are deep, so as to induce the lodgment of fermenting carbohydrates, they are specially liable to decay.

Dr. DICK (in reply): With regard to Dr. Eric Pritchard's remark, certainly I think it is agreed by all authorities that rickets is a condition operative chiefly from the third to the sixth month to the end of two years; it is the time when overwhelming growth is going on throughout the child's body. In any infant centre, from the third month in almost all cases, you can make out some signs of deviation in a rickety way. Why they should be affected at that time it is difficult to say, because there are many conditions operative. Taking school children, probably the average family would be six; add the father and mother; and, recollect, these usually live in only two rooms. That, I think, is the essential factor in explaining the causation of rickets. These children especially London children, get very little exercise, fresh air, or sunlight, and my opinion is that it produces a strong hypothyroid phase, of which rickets is simply a manifestation. These people say all their children have been like this,

but at the age of 5 or 6 years they have improved immensely ; that is because, at the later period, the child lives a more independent life, has a greater variety of diet, and there is a more vigorous growth. The mother's milk is probably affected by hypothyroidism, and there is a lack of some internal secretion there. The second speaker (Dr. Sim Wallace) has himself laid down no very definite ideas as to the causation of decay in teeth. I did not say hypoplasia was absent in the temporary teeth ; it is markedly present in those teeth. After all, it is only a certain proportion of the enamel of temporary teeth which is laid down at the time of birth. With regard to hypoplastic teeth being sound, that is not reasonable ; the good defence of teeth is to have the enamel efficiently laid down. There are various forms of hypoplasia. There are the transverse bands, generally as the result of illness ; there is the projecting dentine and thin layer of enamel, and there are the honeycombed and pitted teeth. Dr. Sim Wallace said 7 per cent. showed hypoplasia ; I gave about 10 per cent. of all school children showing hypoplasia.

(May 26, 1916.)

## **The Treatment of Rickets, based on a New Theory as to the Pathogenesis, with an Illustrative Case.**

By ERIC PRITCHARD, M.D.

THE case of rickets which I show this afternoon is brought before the Section for the purpose of illustrating the good results which can follow when the treatment is based in accordance with certain principles which I venture briefly to outline.

In accordance with the view of the pathogenesis of rickets which I suggest, the symptoms usually ascribed to the condition may be regarded as representing the results of the calling into play, by the victims of the disease, certain protective mechanisms. The troubles from which they protect the organism, in the case of rickets, are those which arise in connexion with the disposal of an excess of nutritive material.

In order that an individual may remain in health, sufficient food-material must be provided for the maintenance of the normal temperature, for the performance of such mechanical work as may be required for the ordinary purposes of life, as well as for growth and



maintenance of the tissues, and also for the elaboration of secretions. If the total calorie value of the food exceeds these requirements, it follows that the excess must be disposed of in some manner which, inasmuch as it does not subserve any required or necessary purpose, may be regarded as pathological. Recognizing the strictly conservative character of the organic reactions in the animal body, we may assume that under any given circumstances the method of disposal adopted will be that which will inflict the least possible degree of injury on the organism as a whole.

By what methods can excess of food be disposed of? There are many alternative methods, of which the following are examples: Classified in order of physiological advantage we may assume that the most economical expedient is to store up the excess in the form of a food reserve—for instance, as glycogen or fat. The limits of storage—especially as far as the fixation of nitrogen is concerned—are, however, under all conditions extremely circumscribed, and occasionally quite negligible.

A second method is that of combustion or oxidation to the normal end-products, carbonic acid gas, urea, and water. This method is economical in that it entails little strain on the mechanisms of elimination; on the other hand, it implies a waste of energy in certain other directions, as, for instance, in the dissipation of the heat produced and in the supply of the necessary oxygen.

A third alternative depends on the short-circuiting of the oxidation processes, in fact, on the production of incompletely burnt-up products of combustion. This method has the advantage of saving oxygen and of limiting heat production, but the disadvantage of flooding the blood with acid bodies of large molecular size. I mean such bodies as lactic, oxalic, uric, glycuronic, di-acetic,  $\beta$ -oxybutyric, and certain other organic acids. Such acid bodies are not allowed to remain in the blood without neutralization, for acid reactions are fatal to cell activities; hence they are neutralized almost before they are formed at the expense of alkalis or basic elements derived from the floating reserves or from the tissues themselves. After neutralization such bodies are, of course, removed from the system by the ordinary processes of elimination. In this way the reserves of carbonates in the blood are drawn upon, ammonia is extracted from protein sources, while calcium, sodium, magnesium, and iron are withdrawn or withheld from various important organic combinations.



The injuries inflicted on the organism by this method of disposal are those which are commonly covered by the comprehensive term "acidosis." The symptoms by which we recognize an acidosis are as follows: In serious cases we observe "acyanotic hyperpnœa" due to the depletion of carbonic acid carriers and concomitant stimulation of the respiratory centres; there may be serious cyclic vomiting, possibly due to an associated stimulation of the pneumogastric centres, as well as a great variety of nervous symptoms dependent on analogous causes. Among the more chronic results are disintegration of the red blood corpuscles, interference with the oxygen-combining powers of hæmoglobin, and the demineralization of the osseous tissues. One of the most interesting results of the hæmolytic disturbances thus set up is the compensatory activity of the blood-forming organs; the hæmogenetic functions of the red marrow being stimulated in a corresponding degree, the external evidences of this functional and compensatory activity are the enlarged epiphyses of long bones so common in rickety conditions.

The resources at the disposal of the organism for dealing with excess of nutritive material are by no means exhausted by the methods already enumerated, but they represent the most important and include among their results the symptoms which are generally regarded as making up the clinical picture of rickets. In any particular case of rickets it is impossible to predict what precise method or combination of methods will be adopted to dispose of the excess, and herein lies the protean character of the symptoms of the disease. But however much they may be disguised or obscured, or however incompletely we may be able to follow the intermediate stages, we may feel perfectly confident that in the final issue the metabolic processes will follow the rule of physiological expediency, obey the laws of the conservation of energy, and conform to the economic necessities of supply and demand. The fulfilment of these laws and conditions must result in the manifestation of the kind of symptoms I have described, which make up the clinical picture of rickets.

If this theory of rickets is true, as I firmly believe, it is necessary to prove that in every case of rickets there is some excess of food, otherwise the acidosis and the other manifestations of the disease cannot be explained on the lines suggested. If by "excess" we simply mean "excess" in the popular interpretation of the term, and as estimated by the usual arbitrary standards, it must be admitted that the theory

falls to the ground. But if by the term we mean "relative excess"—that is to say, a redundancy over and above that which is required for the physiological purposes—then I submit that the theory holds good in every detail. I mention this because most people appear to think that rickets is due to some deficiency in the food and not to excess.

The physiological food demands of many infants and children are negligible. If they are kept in hot, stuffy rooms, if they are wrapped up in multiplicity of clothes, if they are seldom taken out of doors, and if they are given no opportunities for muscular exercise, they will create no demand for food, and consequently any dietary, however small, may be *relatively excessive*, and if excessive must be disposed of by one of the protective methods already described. These are the usual conditions which surround the victims of rickets. Under such conditions we could predict with complete confidence that the child would, if he could, lay up stores of glycogen and fat and become obese; that he would show evidence first of excessive combustion by sweating and vascular dilatation of the superficial capillaries of the face and other exposed parts and possibly by disturbances of the heat-regulating centres, and then of suboxidation with the symptoms of an acidosis, with enlarged epiphyses and demineralization of bone; and finally of acyanotic hyperpnœa with other serious nervous manifestations.

I have for many years treated all cases of rickets on the assumption that this is the true pathogenesis of the disease, but I have selected this particular case to show because it has responded peculiarly well to the expedients which such an assumption suggests.

The following is a short history of the case:—

W. H., a girl, aged 2 years, was admitted to the Queen's Hospital for Children on February 28, 1916, for spinal disease, supposed to be tuberculous.

Family history: Mother married twenty-four years; fifteen pregnancies, four miscarriages; ten children alive and healthy. W. H. was born ten months after the previous child, who is strong and healthy. Mother contracted gonorrhœa from father two months before birth of W. H. No family history of tuberculosis.

Previous history: Very small at birth, weight not recorded. Ophthalmia neonatorum. Child always delicate. Easily caught cold; has had cough on and off since birth: severe attack of bronchitis two months before admission.

Feeding: Breast-fed four or five months, then Nestlé's milk and barley water; then goat's milk, and finally Glaxo. Solid food given at the age of

nine to ten months. Recently, before admission, had cow's milk, gravy, potatoes, cream, eggs, bread and milk—altogether a very liberal dietary.

Patient has never had diarrhoea nor vomiting: bowels have been fairly regular; motions normal. At about the age of 6 months mother noticed the back was becoming greatly curved, the curvature increasing. The child was not nursed much, but was allowed to lie at full length on her back in a perambulator which was long enough for the purpose. At the age of 1 year the child began to say a few words, and to make attempts at taking steps if held, but has never stood alone. The legs have always appeared "strange."

Condition on admission: Child markedly anæmic and flabby, lies absolutely motionless with legs fully extended; makes no effort to move, and cannot be made to turn over or use legs by any ordinary method of stimulation. Patient has been kept on her back for some months owing to spinal curvature. The bones show rickety changes in marked degree. Fontanelles not closed, bossing of frontals, marked Harrison's groove, marked curvature of humerus and femur; some bulging of the temporal bones just above the ear. Marked kyphosis in lower dorsal and lumbar region with considerable prominence of spinous processes. All epiphyses of long bones greatly enlarged and well-defined rickety rosary. There was no tenderness of spine, no rigidity, and apparently no pain in jarring. Heart small and flabby in action. Abdomen pot-bellied. Lungs: No obvious physical signs.

Pathological report: Urine acid; small deposit on centrifugalizing, consisting of starch, granular and bladder epithelium, numerous Gram-negative motile bacilli; no pus. Blood: red blood corpuscles, 3,640,000 per cubic millimetre; white blood corpuscles, 12,400 per cubic millimetre; hæmoglobin, 40 per cent.; colour index, 0.55 per cent. Differential count—lymphocytes, 62 per cent.; polymorphs, 37 per cent.; eosinophiles, 1 per cent. No alteration in size or shape of red blood corpuscles. No nucleated forms. Wassermann test negative.

X-ray report: Spine: All the bones appear poorly mineralized. No indication of tuberculous deposits in spinal vertebræ; all epiphyseal lines irregular.

Progress of the case in hospital: The general condition of the child shows enormous improvement since admission. The temperature, however, has never been steady, but has fluctuated daily between 101° F. and 98° F. The pulse fluctuates—rate between 116 and 130. The weight on admission was 1 st. 3 lb. 14 oz., and is now (May 20) 1 st. 1 lb. 12 oz. Movements are much more active, and the child can now (three months after admission) sit up by herself and stand up by holding on to the head of the bed. The colour of the child has completely altered, and she no longer appears anæmic. Since admission there has been one attack of rather severe bronchitis, lasting a few

days with high temperature, due to an infection (influenza) which attacked most of the children in the same ward. The general course of the complaint suggests that in addition to the very marked symptoms of rickets there is an underlying tuberculous process. But the fact that this serious complication has not interfered with the general improvement of the typical rickety symptoms only strengthens the view of the pathogenesis of rickets which I have sketched.

The treatment since admission has been based on the assumption that the marked and typical rachitic symptoms have been caused by a relative excess of food, which has called into play certain protective mechanisms, which have brought about a short-circuiting of the oxidation processes and the production of an acidosis. The acidosis has led to an anæmia and the organism has attempted to counteract this by compensatory over-activity of the hæmogenetic centres in the long bones. Hence the great enlargement of the epiphyses of the long bones. The acid toxæmia has further led to the paresis of muscles, the general debility and the demineralization of the bones. The kyphosis is probably postural. Previous to admission the child had been treated by liberality of diet and enforced rest in the recumbent position on the assumption that the spinal curvature was of a tuberculous character, and hence all the conditions were provided for establishing a "vicious circle" of relative over-feeding. Since admission the treatment has been to create a demand for food by massage, resistance exercises, cold douches, and, as far as conditions in hospital allow, by open-air treatment. The acidosis has been specifically treated by the exhibition of large doses of alkalies, but the alkali tolerance has been very high, and the urine has rarely been alkaline in spite of the alkali administration. The presence of the Gram-negative motile bacilli in the urine may be explained by its acid reaction. To compensate for the anæmia carbonate of iron has been given, and phosphorated cod-liver oil (1 in 10,000) has been supplied with the view of building up the damaged nervous system. The diet has been simply of milk, restricted in quantity.

The general result has been that, although the child has lost weight owing to the muscular exercise and loss of fat, the general condition has improved immensely.

I cannot help thinking that the general improvement of the rickety symptoms under this line of treatment, in spite of some underlying condition of an infective and probably tuberculous character, proves that the hypothesis is correct and that the pathogenesis of the rickety condition in this case at least, and probably in all cases, is due to causes which may be explained in accordance with the views I have ventured to express.



## DISCUSSION.

Dr. WALTER CARR: Dr. Pritchard says that the child suffered from a spinal curvature which was erroneously diagnosed as spinal caries, that she was consequently kept for some time absolutely at rest, that during that time she was relatively over-fed and as a result developed severe rickets. Surely, however, the spinal curvature is to be regarded as a definite symptom of well-developed rickets? and if so, the disease must have been present for some time before the rest treatment, which Dr. Pritchard regards as having caused it, was even commenced. Furthermore, I note that the patient was the fifteenth child, and that her immediate predecessor was only ten months older. I wonder if Dr. Pritchard regards these facts as of any importance, and especially whether, under such circumstances, it is likely that she was ever even relatively over-fed?

Professor ADAMI: Let me confess that thus far I have learnt to distrust absolutely any theory of rickets that has come before me, and to regard the disease as one of the hopelessnesses of pathology, and let me admit that in the paper which we have just heard, at least there is a reasoned course of thought on the part of Dr. Pritchard. Everyone knows how difficult it is to discuss on first hearing any new line of thought and inquiry. I cannot go so far as to accept immediately and unreservedly what Dr. Pritchard has told us, but would say that he has brought forward some points of extraordinary interest. His suggestion, for example, that the swelling in the region of the epiphyses is compensatory, due to the need for increased production, not of bony elements proper, but of the blood-forming constituents of the marrow, well deserves consideration, while I am prepared to go very far with him in finding a connexion between acidosis and rickets and to see in acidosis an explanation of the using up of calcium and other minerals of the body to supply bases which act as an antidote thereto. That finally, acting along the lines of this theory of causation, as due to relative excess of nutrition (and I would place emphasis on the word "relative"), Dr. Pritchard is able to bring before the Section a case showing notable improvement, entitles his contentions to respect; the theory must not be put aside without consideration.

Dr. ERIC PRITCHARD (in reply): I did not expect I should be able to convert the greater number of members of this Section to a revolutionary view on first hearing. The inference I hoped would be drawn was that the term "rickets" is a very unsatisfactory one, and the very protean nature of the symptoms suggests the unsuitableness of the term. There is every variety of symptom produced by one or other of the many protective mechanisms which are called into play. One cannot say, in any given case, what will be the line of least physiological resistance, the most expedient line for the



organism to follow. One feels that the organism is so magnificently conservative that in each case of relative over-feeding it will take its own best line: it may be one of acidosis, of storage, or of oxidation of food. There are cases of rickets in which the food is completely oxidized and the child displays much heat production: it is red about the cheeks and has sweating of the head. In others there are acidosis and poisoning, with nervous symptoms. One can never say when an acidosis is going to ensue, though one knows it generally does occur.

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By F. PARKES WEBER, M.D.

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## Clinical Section.

President—Surgeon-General H. D. ROLLESTON, C.B., M.D.

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(December 10, 1915.)

### Sequel to the Case of Chronic Splenomegaly of Uncertain Origin, with Persistent Leucopenia, shown on January 12, 1912.<sup>1</sup>

By F. PARKES WEBER, M.D.

WHEN the patient, A. N., a young Jewish married woman, aged 23, was seen in January, 1912, the spleen reached a good hand's breadth below the left ribs, but was not hard. The patient otherwise appeared well, though rather pale. The history was that after a confinement in August, 1910, she had suffered from pains in the loins, headache, and giddiness. The splenic enlargement was first detected on October 6, 1910. The blood count then showed leucopenia, the red cells being 4,850,000 and the white cells 2,575 to the cubic millimetre of blood. On October 30, 1910, owing to sudden severe abdominal pain and the presence of free fluid in the peritoneum, an exploratory laparotomy was performed, but nothing abnormal was discovered excepting some ascites and enlargement of the spleen and liver; the capsule of the spleen was adherent to the surrounding parts; the peritoneum looked very hyperæmic.

The patient recovered from the operation and the ascites, but, whilst in the hospital, occasional moderate fever was noted. She left the hospital on November 19, 1910. The Wassermann reaction and

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1912, v (Clin. Sect.), p. 113.

Pirquet's cuti-reaction were both negative. Blood counts were made on various occasions and always showed more or less leucopenia. In February, 1913, the Wassermann reaction was again found to be negative. The patient remained fairly well till the end of 1914, when she had three severe attacks of hæmatemesis in three days. At the commencement of January, 1915, she began to suffer from shortness of the breath, dryness of the throat and swelling of the legs, and about two weeks later she was admitted to the London Hospital, where she died on the following day (January 28, 1915).

Dr. Weber is indebted to Dr. Hubert M. Turnbull, Director of the Pathological Institute of the London Hospital, for the following report on the findings after death:—

*Post mortem 50. January 29, 1915. Recent, upon Old, Organized Thrombosis of Portal Tract; Failure of Right Heart; Chronic Emphysema of Lungs.*

Occlusion of lumina of contracted portal, splenic and gastro-coronary veins by canalized fibro-elastic tissue. Occlusion of lumen of some intra-hepatic portal veins by vascularized fibro-elastic tissue. Ridges of organized thrombus upon intima of wide hypertrophied superior mesenteric veins. Recent red thrombus in canalized portal vein, and in a collateral portal vein which passed from the superior mesenteric vein over the anterior aspect of the head of the pancreas to the left side of the hilum of the liver, anterior to the hepatic artery and bile-duct. Recent red thrombus in very wide tributaries, in hilum and substance of spleen, of occluded splenic vein. Collateral circulation in veins (0.5 cm. diameter) between spleen, diaphragm and retroperitoneal tissues. Varicose lower œsophageal and cardiac veins. Marked atrophy of pancreas (head, 3 cm. by 3 cm. by 2 cm.; body, 6 cm. long by 2 cm. by 1.5 cm.). No abnormality in duct of Wirsung. Recent ill-defined venous infarcts and a few pigmented areas of old hæmorrhage in enlarged, greatly congested, slightly fibrotic spleen. Dense fibrous perisplenic adhesions. Fibrous adhesions between great omentum and anterior abdominal wall. General chronic thickening of peritoneum. Distension of intestines. Bile-stained ascites (70 oz.). Red marrow in lumbar spine. Red cellular and cellulo-lipomatous marrow throughout whole of right femur with exception of condyles. Congestion, œdema and chronic atrophic emphysema of small lungs. Very great hypertrophy of dilated right ventricle and auricle (right ventricle

0·8 cm., left ventricle 0·9 cm. thick). Considerable hypertrophy of pulmonary arteries. Very slight atheroma in commissure and arch of small thin aorta. Great tortuosity of wide splenic artery. Congestion and extensive back-pressure atrophy of liver. Back-pressure kidneys. Petechiæ in visceral pericardium, visceral pleura, peritoneum and skin of left side of neck. Blood-clot in uterus. Edema of legs. Marked cyanosis of lips. No clubbing of fingers. Polypoid fibroadenoma in anal canal. Operation scar (14 cm. long) in mid-line above pubes. Operation scar (13 cm. long) in mid-line above umbilicus with lateral extension (5 cm. long) to left. Well-developed, stout young woman.

Examination of head not permitted.

Length of body, 5 ft. Weights: Body, 143 lb. 12 oz.; heart, 12¼ oz.; spleen, 3 lb. 2 oz.; liver, 2 lb. 6½ oz.; kidneys, 11¼ oz.

A microscopical examination was made of the following tissues: lung, liver (including occluded intra-hepatic portal vein), portal vein, spleen, marrow of femur.

Dr. Paul Fildes kindly applied a Wassermann test to the serum of blood obtained at the necropsy; the reaction was negative.

*Remarks.*—The structure of the organized thrombi, the advanced development of the collateral venous circulation, the remarkable dilatation and hypertrophy of the superior mesenteric veins, and the atrophy of the pancreas, show that the obstruction of the portal and splenic veins was an old lesion. I do not think that there can be any doubt that the splenomegaly was the sequel of obstruction, by thrombosis, of the portal and splenic veins. In the excess of cellular marrow in the femur there was no appreciable abnormality in the relative number of the different hæmatogenous elements.

#### DISCUSSION.

The PRESIDENT: With regard to the possibility of distinguishing clinically between chronic splenomegaly due to splenic and portal thrombosis and chronic splenic anæmia, a history of an infection or of an abdominal injury shortly before the onset of splenomegaly suggests, but does not prove, that there is thrombosis; the association of ascites also is in favour of thrombosis, but as ascites may be caused by chronic perisplenitis this again is not pathognomonic. The question of splenectomy in thrombotic splenomegaly is suitable for discussion; it may be argued that it is a reasonable form of treatment for recurrent hæmatemesis, for owing to obstruction of the splenic vein the blood brought by the splenic artery will mainly escape from the spleen through the vasa brevia veins which

pass from the cardiac end of the stomach to the splenic vein in the hilum of the spleen. The blood current in these veins will thus be contrary to that in health. The increased flow through the vasa brevia veins will lead to dilatation and varicosity of the gastric and œsophageal veins and so favour hæmatemesis. Removal of the spleen should therefore prevent recurrent hæmatemesis.

Dr. GALLOWAY: I am much interested in hearing the concluding part of the history of Dr. Parkes Weber's case, previously reported to the Section. The additional information which is now accumulating should help us to make an earlier diagnosis of portal thrombosis in cases of splenomegaly otherwise simulating the condition still commonly described under the title "splenic anæmia."

I take the opportunity of adding to the discussion the later history of the boy J. N. whom I brought before the Section at its meeting on February 12, 1915. Members present will recollect that at the time the boy was shown to the Section it was under consideration whether it would be wise to remove the spleen or not. I had the advantage of the opinion of my colleague, Mr. Stanley Boyd, who had experience of the operation of splenectomy, and also the opinion of my friends Dr. Parkes Weber and Dr. G. A. Sutherland. On account of the fact of the lad's condition, and especially as there was evidence of a certain degree of ascites, I determined at the time that operation would be inadvisable. During the following month the lad's condition remained very much the same as when shown to the Section. There was naturally a gradual failure of strength, perhaps a slight increase in the amount of abdominal fluid effusion, but very little change otherwise; one or two slight attacks of hæmatemesis occurred. Then on March 19 there occurred a sharp rise of temperature to 102° F., falling to normal the same day; rising to 101·5° F. on March 20, falling to normal on the morning of March 21; rising to 100·6° F. on March 22. On March 23, 24, and 25 the temperature did not rise above 99·6° F. During this pyrexial attack, for which no definite cause could be ascertained, the lad remained remarkably well; except for the rise of temperature there was no evidence of any change occurring. On March 26, there was a sudden rise of temperature between 6 and 10 o'clock from 97·8° F. to about 106·2° F., with rigor. The lad complained of a certain amount of pain in the abdomen, and he vomited a little, but relative to the remarkable rise of temperature very little evidence of illness was present. The abdomen remained flaccid, though a small quantity of fluid was still present; the spleen did not enlarge, the liver remained palpable below the costal margin. The temperature fell suddenly on the morning of March 27 to 102·4° F., rising in the afternoon of that day to 104° F. On March 28 it fell to 99° F.; on March 29 it was subnormal and remained normal subsequently during his stay in the hospital. The day after the rigor and sudden rise of temperature the lad seemed to be almost in the same condition as before. Those in attendance were very much struck with the fact that in spite of the very severe febrile reaction the patient showed



so little sign of illness. Within a few days the abdominal fluid was observed to increase in amount, and as by this time the diagnosis had become fairly obvious, and as at any rate the question of operation could no longer be considered to be advisable, the boy was taken home by his parents. He was seen by me on one subsequent occasion at his own home about a fortnight after discharge from the hospital. He had then obviously lost strength very markedly, and the abdomen was much distended with fluid. A few days afterwards he died. Unfortunately a post-mortem examination could not be obtained.

There can be no doubt that in the case of this boy the symptoms—splenomegaly and anæmia—were actually associated with portal thrombosis, and that almost certainly the portal thrombosis was the primary cause of the disease. From the point of view of diagnosis especially, the type of pyrexia deserves attention. The degree of fever would certainly, in other cases, have given rise to symptoms of severe toxæmia or septicæmia, but in this case the patient's general condition was remarkably little disturbed. I have very little doubt that the febrile attack was associated with the increase and spread of the portal thrombosis—probably widely in parts of the portal circulation hitherto unaffected. In the dissection of a previous case (reported before this Section) I found that the stage of thrombosis associated with organization of the clot and of fibrous occlusion of the veins was most marked about the junction of the portal vein with the splenic, superior and inferior mesenteric veins, and that the more recent clot was in the peripheral portions of these veins, especially of the splenic.<sup>1</sup> It appears probable, therefore, that the remarkable rise of temperature in the case of the boy J. N. was associated with a spread of the thrombosis, possibly of an extensive nature, through portions of the portal circulation at some distance from the portal vein itself. The increase of the abdominal effusion would readily be accounted for by such a lesion. The actual cause of the thrombosis is still a matter for investigation, but it is highly improbable that the thrombosis in such cases is due to a bacterial infection resulting in any of the forms of abdominal or intestinal inflammations with which we are familiar. From the point of view of diagnosis the occurrence of fever of this character in such cases would probably assist in the recognition of portal thrombosis.

Dr. DAVID FORSYTH: Now that the distinction between an enlargement of the spleen from splenic vein thrombosis and other forms of splenic enlargement has been recognized, it becomes a matter of importance to identify the symptoms of splenic thrombosis at its onset, and also to ascertain its causes. It may be anticipated that the pathological causes will be found in the rectum, sigmoid and descending colon, and in the stomach—i.e., in the areas drained by the inferior mesenteric vein and other tributaries of the splenic vein. In one case in which at the autopsy the splenic vein was filled with recent clot and the spleen had swollen considerably, the primary condition was a chronic gastric ulcer.

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1914, vii (Clin. Sect.), p. 138.



## 6 Weber: *Chronic Splenomegaly of Uncertain Origin*

Dr. PARKES WEBER (in reply) : In these splenomegaly cases of thrombotic origin the cause of the thrombosis and the site of the commencement of the thrombosis are not yet really known. I think that the apparent recovery of a patient, when it occurs, though the enlargement of the spleen persists, suggests a thrombotic origin for the splenomegaly. In such cases of apparent recovery a leucopenia may remain even when the anæmia disappears (a condition suggesting "anæmia splenica sine anæmiâ"). The operation of splenectomy in definitely thrombotic cases has, I believe, never been known to do good, but it is quite conceivable that the manipulation connected with an operation may lead to an extension of, or more complete, portal thrombosis and therefore to increased pouring out of ascitic fluid into the peritoneum, such as happened after the operation in the patient of Dr. Galloway and Mr. Stanley Boyd.<sup>1</sup>

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1914, vii (Clin. Sect.), p. 138.

(December 10, 1915.)

**Note on a Case of Anæmia and Purpura, with Greenish Coloration of the Bone-marrow, and its Bearing on the Question of the Causation of the Green Colour of Chloroma and the so-called Chloro-leukæmia.**

By F. PARKES WEBER, M.D.

THE patient was an anæmic woman, aged 34, a Russian Jewess, who died on December 16, 1914, about an hour after spontaneous delivery of a dead child, apparently nearly at full term. The immediate cause of death seemed to be exhaustion, as the loss of blood connected with the delivery was not excessive. The patient's urine had contained some albumin and granular casts. A blood count had given: red cells 1,280,000 to the cubic millimetre of blood; white cells 9,200; in stained blood films many nucleated red cells were seen; the erythrocytes showed a little poikilocytosis and polychromatophilia; no differential count of the white cells was made. The Wassermann reaction was slightly positive, but there was no history of syphilis. The patient, who likewise suffered from favus of the hairy scalp and of the glabrous skin of the back of the thorax, was said to have been always weakly and subject to headaches and to have had a slightly yellowish tinge of skin. But she had been especially weak and ailing for the past two years, during which time she had had four or five bad attacks of epistaxis, the last attack only about two weeks before her death. She was married ten years ago. Her first child, a girl, was born nine years ago and died after two weeks. Her second child, a girl, was living and healthy, aged 7. The patient was said to have had an abortion at the third month of pregnancy about two and a half years ago, and a year or two ago she was delivered of a child who died after six hours.

During the last two days the patient's temperature had varied between 98·5° F. and 99·4° F.; her pulse had varied between 86 and 100 per minute, and her respirations between 28 and 36. There

was a systolic (doubtless so-called "anæmic") murmur at the apex of the heart. There were some spots of purpura about the wrists and other parts and some patches of ecchymosis on the lower extremities.

At the *necropsy* all the viscera had an anæmic appearance. The heart weighed 13 oz.; there was no valvular disease. The lungs appeared healthy, but there were some pleuritic adhesions. The liver was enlarged, weighing 74 oz. The spleen was a little enlarged, weighing 12½ oz. The kidneys were rather large than small; the capsules stripped readily. The state of the uterus was characteristic of recent delivery. Nothing special was noted in the pancreas, suprarenal glands and other viscera. The microscopic examination of the liver, kidneys and spleen showed nothing abnormal.

A remarkable feature was that the bone-marrow in this case (the sternum, one rib, and the shaft of the left humerus were examined) was found to have a dirty dark greenish colour. It was at first supposed that this might possibly be due to a post-mortem change, but in reality that explanation was quite out of the question, for the weather was cold and the necropsy had not been unduly delayed. Moreover, the microscopic examination of sections of the bone-marrow only showed a cellular excess. This excess was more of a leucoblastic than of an erythroblastic kind. In the section from the shaft of the humerus the marrow consisted chiefly of cells of the myelocyte series and in a lesser degree of erythroblasts; there was a good sprinkling of eosinophile cells, and a few scattered giant cells were seen.

I have to thank Dr. H. Schmidt for much help in the examination of this case.

From the pathological point of view it is the green colour of the bone-marrow which chiefly interests me at present, and I think that this finding tends to confirm some suggestions made by C. H. Treadgold (in a paper to which I shall further on refer) respecting the causation of the green colour of chloroma tumours. From this green colour a group of cases derives its manifold names, including chloroma, chloro-lymphoma, chloro-myeloma, chloro-sarcoma, chloro-myelosarcoma, chloro-lymphosarcoma, chloro-leucosarcoma, chloro-leukæmia, &c.

In regard to the green colour of chloromatous infiltrates, very little as to its nature, &c., is known. Reynolds (quoted by Treadgold, *see* below) attempted an analysis and came to the conclusion that the hypothetical pigment was of the nature of a fatty acid combined with iron.

H. Lehndorff<sup>1</sup> described a case of atypical chloroma (in a boy, aged  $4\frac{1}{2}$ ), in which the green colour was confined to the cervical lymphatic glands, the other infiltrates not being coloured. C. H. Treadgold<sup>2</sup> reported a case (patient Miles) in which the lymphatic glands were green, but the other tumours and infiltrates were not green. L. Johansson and O. Moritz<sup>3</sup> described a case of so-called chloroleukæmia (in a man, aged 42) in which the green colour was confined to the bone-marrow; there were no real tumours or periosteal growths at all. Firth and Ledingham<sup>4</sup> discussed a case of atypical chloroma in a child, aged 12 months, in which there was no greenish coloration excepting in some of the skin of the scalp and orbital regions, and that only appeared in the late stages of the disease. In E. Trevithick's case of chloroma,<sup>5</sup> not only were the obvious growths of a green colour, but some of the bone of the skull situated between tumours—i.e., between a tumour on the outer surface and one on the inner surface—was stained green. H. M. Turnbull<sup>6</sup> has seen green bone-marrow in a patient who died of purpura. He at first thought this was due to some post-mortem change in the medullary fat. However, microscopic examination of the tissue only showed a tremendous hyperplasia of marrow cells, many of which contained neutrophile and eosinophile granules.

Dr. Turnbull has very kindly furnished me with some details of the case in question. It was that of a girl, aged 19, with a history of previous chorea and rheumatism. The purpuric spots appeared about two weeks before her death. A few days before her death the white cells were estimated at 3,125 per cubic millimetre of blood, but no differential count was made. The post-mortem examination summary was: septicæmia, anæmia, purpura; hæmorrhages into the skin and gastric mucosa, abdominal muscles, myocardium and diaphragm; hæmolytic staining of all the blood-vessels; gangrene of the left tonsil; enlargement of the left cervical glands; chronic endocarditis; thickening of (competent) aortic valves; fatty degeneration of the liver and myocardium; red septic spleen; green bone-marrow

<sup>1</sup> *Folia Haematologica*, 1910, ix, pt. 1, p. 309.

<sup>2</sup> *Quart. Journ. Med.*, Oxford, 1903, i, p. 239.

<sup>3</sup> *Folia Haematologica*, 1908, vi, p. 243.

<sup>4</sup> *Proc. Roy. Soc. Med.*, 1911, iv (Path. Sect.), p. 60.

<sup>5</sup> *Lancet*, 1903, ii, pp. 158, 530.

<sup>6</sup> Quoted by Treadgold, loc. cit.

in the upper three-quarters of the femur. The microscopical examination showed septic left tonsil, inflamed left cervical glands, and great myeloid activity, with recent bacterial invasion, of the green bone-marrow. Dr. Turnbull likewise kindly informs me that out of thirteen recent consecutive necropsies on cases of myeloid leukæmia green coloration was noted in the leukæmic infiltrates in five cases. The green coloration affected one or more of the infiltrates; it did not necessarily affect all the infiltrates in any of the organs affected. The green colour faded on exposure to air, but could be restored by ammonia. In one case, however, the colour did not fade in the kidneys.

Treadgold<sup>1</sup> suggests what seems to me to be a probable theory of the causation of the green colour in chloroma, &c. He writes: "It seems certain that the green colour is not present from the beginning. . . . It therefore seems certain that cellular degeneration plays a part in the formation of the colour. Accordingly in some cases the age of the growth seems to be responsible, while in others an organic toxæmia facilitates cellular degeneration. On the whole, it seems very unlikely that the green colour is derived from ordinary blood-pigments. The bright pea-green colour of the chloromatous lesions is much against this view. . . . We have already seen that abnormal myelocytes and myeloblasts are the pathogenic cells in chloroma. . . . Possibly, a degeneration of the granular or pre-granular protoplasm of these cells, or an abortive attempt to form granules, is the real source of the colour, aided by the broken-down products of hæmoglobin."

The present case, like Turnbull's case, which it appears in some respects to resemble, may, I think, be accepted as supporting Treadgold's suggestions. So does a case reported by Mary Wetter.<sup>2</sup> The patient was a man, aged 44, sent into hospital with the diagnosis "hæmorrhagic diathesis," or "scorbutus." No tumours (that is to say, chloromata) were discovered elsewhere. The erythrocytes were at first estimated as 3,140,000 to the cubic millimetre of blood, but they rapidly fell to 1,700,000. The white cells were at first 5,000, and fell to 1,800, reaching up to 8,000, however, two days before death. Of the white cells about 85 per cent. were of the "large lymphocyte type." At the post-mortem examination the greater part of the bone-marrow was found to have a green colour; the spleen, which was scarcely double

<sup>1</sup> Loc. cit., p. 275.

<sup>2</sup> *Frankfurter Zeitschr. f. Path.*, Wiesbaden, 1909, iii, p. 541.



the size of a normal spleen, showed on section a slight dirty green-brown coloration.

I cannot see anything pointing against the probability of Treadgold's suggestion in Kurt Ziegler's statement,<sup>1</sup> that a green coloration may likewise sometimes be present in the tumours of Hodgkin's disease (lymphogranulomatosis maligna), for the granulomatous tumours of Hodgkin's disease are often rich in granular white cells (notably eosinophiles<sup>2</sup>), and are very liable to degenerative changes. But I have never myself observed this colour in any case of Hodgkin's disease, and I have never been shown a specimen from that disease in which it was present.<sup>3</sup>

A question which arises in regard to the present case is whether it, and some other cases in which purpura, severe anæmia, and green coloration of the bone-marrow are combined, are not in reality cases of so-called chloro-leukæmia; that is to say, whether they are not really cases of atypical (lymphoid or myeloid) leukæmia, combined with chloroma-like green coloration of the bone-marrow, but without any definite tumours or periosteal growths anywhere—in fact, cases similar to the one described by L. Johansson and O. Moritz, to which I have referred in a preceding paragraph. It was certainly unfortunate that no differential count of the white cells was made in the present case. A positive Wassermann reaction has, it may be noted, been found in cases of chloro-leukæmia (as it was in the present case), and, I believe, in the absence of any other evidence or history of syphilis.

<sup>1</sup> "Die Hodgkinsche Krankheit," Jena, 1911, p. 203.

<sup>2</sup> In regard to eosinophilia in the tumours, bone-marrow, and blood of Hodgkin's disease, see W. T. Longcope, *Bull. Ayer Clin. Lab., Pennsylv. Hosp.*, 1906, No. 3, p. 86; also O. H. P. Pepper, *ibid.*, 1907, No. 4, p. 22. The degree of eosinophilia varies considerably in different cases. See Parkes Weber and Ledingham, "The Mediastinal Form of Lymphadenoma (Hodgkin's Disease)," *Proc. Roy. Soc. Med.*, 1909, ii (Clin. Sect.), pp. 66-86, especially figs. 6, 7 and 8.

<sup>3</sup> It must be remembered, however, that the boundaries between granulomatous cases (Hodgkin's disease) and lymphosarcomatous cases are not always so obvious as has been supposed. Hard fibrotic (apparently neoplastic) masses may, in Hodgkin's disease of long duration, extend very much beyond the lymphatic glands. The *causa morbi* in such cases may be the same as that in typical Hodgkin's disease, and the difference may depend on the vital reaction (the response) of the patient's tissues.

The PRESIDENT: I believe that cases of greenish coloration of the bone-marrow are all examples of chloro-leukæmia. In the author's case there was no leucocytosis, but in the absence of a differential leucocyte count the disease may have been an aleukæmic form of leukæmia and, from the absence of chloromatous tumours and of visceral infiltration, possibly in an early stage. I am not aware that this green marrow occurs in lymphadenoma. The partial or irregular distribution of the green colour in the tumours and bone-marrow of chloro-leukæmia is difficult to explain as an essential feature of the lesion, and suggests the co-operation of some external factor.

I

# PROCEEDINGS

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DERMATOLOGICAL SECTION



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## Dermatological Section.

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## Dermatological Section.

President—Dr. J. H. STOWERS.

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(October 21, 1915.)

### Case of Erythrodermia.

By J. H. SEQUEIRA, M.D.

W. B., AGED 60, was admitted to the London Hospital on October 1, 1915, suffering from a universal red eruption. The family history was unimportant. There had been no affection of the skin until two years ago, when a red patch appeared on the shoulders and gradually extended over the body. The skin had itched intensely from the onset. The colour had become deeper red during the past twelve months.

The patient was a characteristic "homme rouge," the skin everywhere being a bright red colour. The surface was smooth, and some superficial oedema was present in the lower extremities, where there was slight scaling. The only parts of the skin which were not bright red were the orbital regions, hands and feet. The lymphatic glands on both sides of the neck, in both axillæ and groins were enlarged, movable, painless and not tender. Neither the liver nor the spleen showed any increase in size, and a radiographic examination failed to show evidence of enlarged glands in the thorax. The urine presented no abnormality.

The blood was examined by Dr. Panton on three occasions, and at each examination there was an increase in the lymphocytes. A count on October 4 was as follows: Erythrocytes, 4,500,000; leucocytes, 8,000. Percentage of polynuclear neutrophiles, 38; of polynuclear eosinophiles, 1; of small lymphocytes, 42; of large lymphocytes, 13.5; of large hyaline, 2; of coarsely granular basophiles, 0.5; of neutrophile myelocytes, 1; of myeloblasts, 2—100.

The Wassermann reaction was negative.

It was proposed to remove one of the glands for microscopical examination and also to cut sections of the affected skin.

The patient was shown as a case of erythrodermia with lymphocytosis. Dr. Sequeira believed it to be a case of mycosis fungoides, but in view of the persistent high percentage of lymphocytes the possibility of an erythrodermia of leukæmic or pseudo-leukæmic origin required consideration. A further report would be presented when the microscopical examination had been made.

### DISCUSSION.

Dr. F. PARKES WEBER asked whether Dr. Sequeira had made a biopsy of the skin, as he thought it probable that the case would afterwards become an instance of Kaposi's lymphodermia perniciosa. This was not the same as true mycosis fungoides; it might be described as a kind of leukæmia commencing in the skin, but during life it had been almost certainly sometimes mistaken for true mycosis fungoides, for the blood need not necessarily show great excess of white cells.

Dr. ADAMSON said the patient was under his treatment at the out-patient department of St. Bartholomew's Hospital for some time, and the diagnosis he made was mycosis fungoides; the biopsy made confirmed that view. He was treated by X-rays.

Dr. GRAHAM LITTLE was struck with the essential similarity between this patient and the lady with an undoubted and very severe form of mycosis fungoides shown by him at a recent meeting. Here, too, there was a universal redness and an enlargement of glands which preceded the eruption of tumours. This patient, he might mention in passing, was rapidly becoming worse, notwithstanding that, in conformity with the general sense of the discussion on the case, he had sanctioned the further application of X-rays by a specially skilled radiographer, Dr. Harrison Orton.

Dr. PERNET said this case came into the category of the "hommes rouges," as they were known at the Hôpital Saint-Louis, and he agreed with Dr. Sequeira and Dr. Adamson. He did not consider this case would become one of lymphodermia perniciosa of Kaposi, which condition was much rarer than mycosis fungoides, and of a different type.

The PRESIDENT asked Dr. Sequeira to exhibit, or report upon, the case at a later date, because of the chance of the further development alluded to.

Dr. SEQUEIRA agreed to do so.

(October 21, 1915.)

**A Boy, aged 15, of English Parentage, showing very Septic  
Ulcerations of Undetermined Nature.**

By E. G. GRAHAM LITTLE, M.D.

AT the first inspection—and the exhibitor apologized for having been unable to investigate the case, which had come under observation only the previous day—a diagnosis of sporotrichosis was suggested, chiefly because of the history and the course of lesions in the line of the lymphatics; this suggestion was now abandoned. At present, however, it was impossible to come to any decision, and the case was exhibited in this incomplete stage because it was feared a further lapse of time—until the next meeting of the Section for example—might result in too much alteration in the present extraordinary appearance.

The history was as follows: Ten weeks ago the first lesion appeared as a “blister” at the tip of the left thumb; this developed into a suppurative inflammation. The case was seen in the surgical out-patient department at St. Mary’s Hospital and a diagnosis of “whitlow” made, and it was treated accordingly. This site healed within eight weeks, leaving a scar, but in the meantime several other and similar lesions appeared on the contiguous fingers of the left hand, and later on the right thumb and forefinger, the toes of both feet and several other positions which would be described later in detail.

Present condition: When seen by the exhibitor there was no vesicle or any approach to a “blister,” and the eruption consisted of two types of lesion: (*a*) a number of purulent granular elevations very like the bromide honeycomb patch (but there was no history of drug-taking), and (*b*) a number of roughly circinate, somewhat superficial ulcerations, the ulceration being limited by a raised yellow chamois leather slough with an extraordinary and very foul smell. In size the ulcerations varied from that of a shilling to a five-shilling piece. More recently a typical “blister” type of lesion had been seen, and one such which appeared on the foot was examined in the early vesicular stage, and cultures and films prepared from its contents proved to be sterile. There were now numerous scabs, rather like the scab of a varicellar lesion, distributed over the back of the trunk. The boy said that all the

lesions commenced in the same way with blister formation. There was no itching, and the temperature had throughout been normal.

Distribution: There was a large and foul ulcer on the anterior surface of the right wrist, and there were three large fleshy elevations of the bromide type in near proximity to this up the line of the inner and lower third of the forearm. Between the elbow and axilla there was a scar of a large healed ulcer in the same line. There were scars of old lesions about the fingers of the left hand, and on the axillary surface of the upper arm there was a large serpiginous ulceration some  $2\frac{1}{2}$  in. in diameter. On the dorsum of the left foot there were several bromide-like elevations running in a line from the toe to the front of the ankle. There was a large ulceration covering the whole heel, and a smaller ulcer on the plantar surface. On the right foot there was an ulcer the size of a florin in front of the internal malleolus and a small granular elevation on the anterior surface of the ankle. There were a number of small, partly healed, scabbed lesions on the inner side of the left knee, on the front aspect of the left thigh, and on the middle of the right thigh. The mouth was at present entirely free, but there was an earlier history of a very foul septic condition there. There was a small patch of herpes simplex on the front of the lower lip. In other respects the general health seemed to be but little impaired.

Film preparations from the foulest ulcer, that on the wrist, showed a preponderating diphtheroid organism and a very varied bacterial flora, with no specific indications at present.

#### DISCUSSION.

Dr. PRINGLE said that the Section must feel greatly indebted to Dr. Graham Little for bringing so interesting a case before them in the rough state, but thought that it could be more profitably discussed at the next meeting, when the exhibitor would have had the opportunity of studying it more fully. His own impression was that the essential lesions in the case were bullæ and not the granulomata which constituted the primary elements of sporotrichosis. When published, he hoped the case would be fully illustrated.

Dr. PERNET suggested this case might turn out to be one of pemphigus vegetans rather than a case of sporotrichosis.

*Note.*—Since the case was shown the *Bacillus pyocyaneus* had been grown from the lesion to a degree which made it probable that this organism was present in preponderating numbers. The streptococcus and staphylococcus were also found in large quantities.

(October 21, 1915.)

### Case of Granulomata.

By J. H. SEQUEIRA, M.D.

E. A., A MARRIED woman, aged 53, came to the London Hospital for treatment of a crop of nodules on the arms and face. The patient's father died from the effects of a "chill" when she was a child; her mother died from "bronchitis and dropsy." The patient had five children: two died in infancy from "teething and bronchitis." The eldest son, aged 34, was rheumatic, the others were well. There had been no miscarriages. The eruption was first noticed in May, 1915. It apparently began as a crop of red spots on the arms and face. The nodules increased in number. The patient had taken no medicine. She had enjoyed good health with the exception of headaches until the eruption appeared. Since then she had felt sick, and complained of frequency of micturition. She got her living as a washerwoman and perspired freely at her work, but not at night. She had lost weight, but was not wasted.

The eruption consisted of a large number of papules about 4·5 mm. to 5 mm. in diameter on both arms, from the knuckles to 6 in. above the elbows. Each lesion was raised above the level of the surrounding skin, hard, and with a rounded smooth surface. The colour was reddish-brown fading to a brown in the older lesions. In a few instances there was a small scale at the summit. The papules were deep in the skin. None of the lesions had disappeared and there had been no ulceration or necrosis. On the face there were numerous lesions of the same type, the forehead and the areas in front of the ears being affected. All the papules were discrete except at the left elbow, where, near the olecranon, there were three raised brown patches about  $\frac{3}{4}$  in. by  $\frac{1}{2}$  in. without scaling, apparently due to the coalescence of a group of papules. On the fronts of the legs there were discrete areas of erythema about 1 in. in diameter covered with fine scales. These lesions alone caused itching. The papules caused no subjective symptoms, there was no evidence of visceral disease, and no enlargement of the lymphatic glands nor other sign of tuberculosis.

The Wassermann reaction was negative (Dr. Fildes).

The blood examination showed some excess of large lymphocytes (21 per cent.), while the polynuclear neutrophils were only 4 per cent. The urine was quite normal both in quantity and character.



A nodule was excised and the sections showed an encapsulated mass of what were apparently fibroblasts. There was very little inflammatory reaction of round cells from the blood.

The condition was certainly an unusual one. At first sight the lesions suggested a late syphilide, but the absence of mucous membrane lesions, the character of the sections, and the negative Wassermann reaction might be taken to exclude lues. The lesions also differed from the usual types of tuberculide, as they had not undergone central necrosis or ulceration. The sections also excluded xanthoma. In the absence of other indications the eruption appeared to be more closely related to the tuberculides or the small form of sarcoid than to other conditions.

The PRESIDENT said that Dr. Sequeira was naturally in the strongest position; he had had the case under observation and investigated it. If he (the speaker) had seen it without outside influence, he would have regarded it as a syphilide, and even now he thought the grouping of some of the tubercles was very suggestive of that disease. It was the borderland cases which were so interesting and instructive. He asked if Dr. Sequeira would give a further report on the case. During the last few years he had been impressed by the varying opinions sometimes given by bacteriologists as to the result of the Wassermann reaction. During the last six months he had seen a case which was submitted for the test, and was said to give a positive Wassermann; but he did not think the case was syphilitic on clinical grounds. The test was repeated by another equally expert pathologist, who declared it to be negative. He thought, therefore, one should be cautious about accepting as decisive one Wassermann reaction alone, seeing that it was a very serious matter and meant a prolonged course of treatment.<sup>1</sup>

(October 21, 1915.)

### Result of Arsenical Intoxication Ten Months after One Injection of Novarsenobenzol (Billon).

By HAROLD SPENCE, M.D.

(Introduced by Mr. McDONAGH.)

MALE patient, aged 25, contracted syphilis early in August, 1914, but had no treatment until October 31, when he came to the Lock

<sup>1</sup> Since the patient was shown the examination of the blood has been repeated, and the Wassermann reaction was again negative. Antisyphilitic treatment has also failed to influence the eruption.—J. H. S.

Hospital with well-developed "secondaries," including a profuse maculopapular rash and mucous membrane lesions. Intramuscular injections of 40 per cent. grey oil were commenced, but after the sixth weekly injection it was discontinued. Three weeks later, namely, on January 3, 1915, he was given 0.6 grm. Billon intravenously. This was accomplished without incident; there was no phlebitis or induration at the site of injection and no immediate reaction of any kind, although he had a severe headache most of the following day. Immediately afterwards he felt well and the oral lesions improved, but on his return to the out-patient department six days later a diffuse erythema was noticed. When next he attended, which was on January 23 (twenty days after the injection), he had an established dermatitis, with œdema of the face and extremities, and pyrexia; consequently he was admitted into the hospital. He remained an in-patient from January to June inclusive, nearly six months, with a most pronounced and versatile general dermatitis, intense erysipelatoid erythema being first associated with vesicles, blebs, pustules and impetiginous areas about the hands, nostrils and lips, and then a copious and prolonged universal desquamation. Concurrently he developed severe nasopharyngeal inflammation and a muco-purulent conjunctivitis with photophobia and smarting pain; he became weak and emaciated, his general condition for some time being such as to give rise to considerable anxiety. Every vestige of hair was shed and all of his finger-nails and toe-nails. Gastro-intestinal symptoms were absent, no kidney insufficiency was discovered, and there was no evidence of wrist-drop or lesions of special nerves. He left the Hospital in June and for the following two months was semi-invalided at home, then returning to work.

It was now ten months since the injection had been given, and the members would observe the present condition. His hair had returned for the most part, although on the head there were a few small patches of cicatricial alopecia and a general thinning. The finger- and toe-nails had all returned and were perhaps as good as they were before. In places they seemed thickened, in others somewhat irregular. The extensive pigmentation of the trunk and extremities was quite characteristic; the muddy or greyish-brown mottled staining which they associated with arsenicalism, usually described as "raindrop pigmentation," was seen in quite a typical form. There was some xeroderma and still a fine branny desquamation, more obvious in some places than in others, but present pretty generally if looked for. The face had for the most part escaped except for an irregular blotch on one

cheek, the area of which would add up to that of a half-crown. There was moderate keratosis of the palms and soles.

#### DISCUSSION.

Dr. PRINGLE believed it to be a generally recognized clinical fact that some people were immensely more susceptible than others to arsenic, however administered. He remembered when, about twenty years ago, there was a revival of arsenical treatment, many cases of arsenical keratosis were brought forward as the result of quite small doses of the drug administered internally. He had not previously seen anything like this patient's condition from the intravenous use of any arsenical preparation, but he did not question the accuracy of the diagnosis.

Dr. BOLAM asked whether any œdema appeared immediately after the administration. This did not seem to be the kind of pigmentation one used to see following arsenic; and though he had lately witnessed a good many administrations of these drugs he had not seen such a result. The patient's condition was more like the pigmentary disturbance following a general exfoliative dermatitis.

Dr. PERNET said he had seen very severe pityriasis rubra in a young man following the intravenous injection of an arsenical organic preparation. He did not know what the exact substance was. He agreed with Dr. Bolam's remarks concerning pigmentation following pityriasis rubra; when pityriasis rubra cleared up, it was common to find very marked pigmentation, according to the complexion of the patient. In one dark subject the pigmentation was extremely marked, but after having examined the present patient he was of opinion it was an arsenical pigmentation.

Dr. J. H. SEQUEIRA thought that the pigmentation in the present case was the result of the erythrodermia. When the salvarsan treatment was started, the late Sir Jonathan Hutchinson expressed a fear that there would be cases of arsenical keratosis, and this Section appointed a Committee to inquire into arsenical poisoning, but so far no meetings had been held.

Dr. ALFRED EDDOWES said he once saw an extensive secondary syphilitic eruption which had been mistaken for psoriasis. The patient had had small doses of Fowler's solution, and a tar preparation for application to the skin. The result was that in a few weeks the patient was much pigmented and spotted like a leopard; there were dark spots wherever there had been roseola. The patient also had paronychia. The arsenic had aggravated the eruption.

Dr. ADAMSON regarded the pigmentation as typical of that seen in cases of chronic arsenical poisoning. It showed the characteristic "raindrop spots," or pale areas around the hair-follicles.

Mr. McDONAGH said that he had asked Dr. Spence to exhibit this case as it was a typical instance of the form of arsenical poisoning that had occurred only too often following the use of the English and French substitution products for "606" and "914." He had seen nine similar cases, two of which had ended fatally. In no case had more than two injections been given, and in some only one. In one case the dermatitis did not appear for nine weeks after the injection. Before the War he had only seen one case of exfoliative dermatitis following the use of salvarsan. He considered the pigmentation to be typical of arsenical poisoning. The occurrence of nine cases in so short a period excluded them from being coincidences, and pointed to the increased toxicity of the substitution over the original arsenical compounds.

Dr. DOUGLAS HEATH agreed that this patient had typical arsenical pigmentation. Pityriasis rubra and various other skin diseases produced a general bronzing, but not a white picked-out point over the hair-follicles. The pigmentation on the abdomen in this case he regarded as characteristic; he had seen the same appearance in people who had taken long courses of Fowler's solution.

Dr. SPENCE, in reply, said that on the sixth day after the injection the patient had erythema. When on the twentieth day he was seen again, he had well-marked dermatitis and fever, with œdema of face and extremities. There were no gastric or nervous phenomena. He had had to depend on other notes, but he did not think there had been any kidney disturbance. The patient was admitted into the Hospital on account of his severe general symptoms, and afterwards had a great deal of desquamation; as one of the older nurses described it, one could almost shovel the scales off the bed, and shortly afterwards it was as bad again. He gradually improved, and at the end of six months it was considered safe to let him leave Hospital, but he was in bed another two months at home.

(October 21, 1915.)

### **Case of Lymphangioma.**

By DUDLEY CORBETT, M.D.

THE patient was a female child, aged 12 months. There was one other child in the family, a boy, aged 4, without blemish, nor was there any history which could be connected with the case. The child was



perfectly healthy and experienced no inconvenience from the tumour, nor had there been any attacks of inflammation associated with it. The lymphatic glands were not enlarged anywhere. The tumour itself dated from birth, and the parents did not think that it had grown at all. It was situated in the left leg, extending from the middle of the thigh to the ankle, and rendering that limb notably larger than its fellow; even the left thigh above the tumour was thicker than that on the right. At its upper part, over an area the size of the palm of the hand, was a nævoid patch which faded on pressure. Over it were scattered numerous dark blue venous tufts of varying size. Some showed signs of recent hæmorrhage and some were quite hard to the touch like an angiokeratoma. Besides these venous tufts were a few transparent vesicles containing a clear fluid, which flattened on pressure, only to fill up again when the pressure was removed. Similar vesicles were present all over the tumour, some quite clear, and some just tinged with red. In the calf of the leg the tumour was very bulky, and when grasped gave the sensation of a bundle of varices, though this point was not quite so marked as it was when the child first came under treatment. There was no tenderness anywhere.

The mother stated that the little venous tufts frequently bled, and the vesicles also exuded a clear fluid, but there was never any prolonged bleeding nor much leakage from the vesicles. The tumour never varied in size during the day, nor was its fluid content affected by gravity. Clinically the case seemed to be one of lymphangioma circumscriptum associated with hæmangioma. From the literature it appeared unusual to find one of such great bulk.

Large doses of radium were being applied, and as a result the tumour seemed to be much less tense than it was, and the nævoid patch was paler in colour.

Dr. Corbett said that he would be glad to hear from any members their experience of the effect of treatment in such cases, and their opinion as to the exact group of lymphangiomata to which the case properly belonged.

#### DISCUSSION.

Dr. ADAMSON said he showed a case somewhat similar to this some time ago in which the nævus was blood-vascular and verrucose.<sup>1</sup> It was difficult to say how much of the condition in the present case was of lymphatic and how

<sup>2</sup> *Brit. Journ. Derm.*, 1910, xxii, p. 263.



much of blood-vessel structure. In his own case diminution in the size of the nævus had resulted from repeated and prolonged applications of radium through a lead screen. He suggested the trial of X-rays applied through a 3 mm. aluminium filter.

Dr. PARKES WEBER remarked that the case might subsequently become complicated by more or less permanent or intermittent lymphorrhagia (lymphorrhœa).

(October 21, 1915.)

### Case of Urticaria Pigmentosa in an Adult.

By S. E. DORE, M.D.

THE patient was a healthy looking man, aged 31. He had suffered from itching and a skin eruption for eight years. The main features of the case were intense pruritus, marked factitious urticaria, and a macular, pigmentary eruption on the trunk and limbs of a permanent character. The itching was more or less general and was worse at night, considerably interfering with his sleep. Scratching was followed, almost immediately, by a copious eruption of small circular or oval wheals similar in size and shape to the permanent macules, which also became turgid as the result of rubbing. The factitious urticaria lasted from a quarter to half an hour, generally about twenty minutes, and disappeared without leaving any trace on the skin. The patient stated that sometimes wheals appeared spontaneously. In addition to the evanescent factitious eruption there were scattered over the trunk numerous oval or rounded macules varying in size from that of a pin's head to that of a pea or small bean. They were reddish-brown to dark brown in colour and displayed no particular arrangement except a slight tendency to follow the lines of the ribs on the sides of the trunk. They were more sparsely distributed on the upper arms and thighs, and almost absent from the forearms, legs, hands and feet. No intermediate stage was apparent between the evanescent wheals and the permanent macules, but the patient thought the latter were gradually increasing in number. He had enjoyed good health with the exception of some recent dyspepsia, bronchitis during the past three winters, and an attack of tonsillitis twelve years ago. He had also had

gonorrhœa, but there was no history of syphilis, and the Wassermann test was negative. His mother was said to have suffered from a skin disease, but he did not know of what nature, and one of his children suffered from nettle-rash.

One of the macules had been excised for microscopical purposes, and sections stained in polychrome methylene blue showed large numbers of mast cells situated around dilated blood-vessels in the papillary and subpapillary layers of the skin.

The only internal treatment had consisted of fairly large doses of calcium lactate, but this drug had exercised no influence upon the disease, a fact which the exhibitor thought was in harmony with other similar cases in which the coagulation time of the blood had been found to be hastened rather than prolonged.

(October 21, 1915.)

### **Angiokeratoma.**

By W. KNOWSLEY SIBLEY, M.D.

THE patient (I. H.) was a fairly healthy-looking girl, aged 12, who had for many years suffered from chilblains on the hands and feet. Ever since she was three years old nævus-like puncta had been gradually appearing on the fingers and toes. The eruption was stated to have commenced on the outer side of the little fingers and the small toes. Of recent years the lesions had become more prominent, and some on the toes, especially those which had appeared on the adjacent surfaces of the dorsal aspect of some of the toes, had become hard and warty in appearance and feel. At times the lesions were slightly painful with a pricking sensation, and they occasionally bled. They varied in size, from that of a pin's head to that of a small pea, occurring singly and in groups scattered over the dorsal surface of all the fingers, and especially on the toes, and were persistent.

A section of one of the lesions from the right big toe revealed a thickening of the stratum corneum and the stratum Malpighii, with large cavernous spaces filled with blood corpuscles in the rete, together with a dilatation of the papillary and other blood-vessels.

## DISCUSSION.

The PRESIDENT said he had seen cases of this kind benefited by electrolysis.

Dr. PRINGLE feared that the results of electrolysis might be found somewhat unsatisfactory; he had done a large number of such needlings, and in some the results had been rather disappointing. His further experience had not fully confirmed the cheery views he expressed on the subject when he published, in 1891, the first English case in which electrolysis was tried. The keratomata often disappeared in a marvellous way, but when the blood-vessels were large, frequent repetitions might be necessary to cause their obliteration.

Dr. SIBLEY, in reply, said that he agreed that electrolysis should be applied to each of the lesions in order to produce some shrivelling of them, and this he proposed to carry out.

(October 21, 1915.)

**Case of Extensive Tertiary Syphilis of the Face improved  
by Galyl.**

By GEORGE PERNET, M.D.

THE patient, a man, aged 29, first attended at the West London Hospital on September 7, 1915, for an extensive scarred and ulcerated condition of the face of three years' duration. This had started about the right nostril and had gradually extended in a serpiginous manner. The diagnosis of syphilis was made and confirmed by a large amount of scarring on the under surface of the penis, the result of a sore dating ten years previously, evidently primary and of phagedænic nature. The man had had no specific treatment as the condition had apparently been mistaken for lupus vulgaris. Nor had he been treated with X-rays or other local treatment likely to lead to scarring. The condition was not the result of a burn. He was ordered hyd.  $\bar{c}$  cretâ and 0.40 gm. of galyl, but as only 0.25 gm. of the latter was available on the morning (September 11) he came for the intravenous injection, that was administered. The results were immediate, for though unhealthy when seen on September 14, the ulcerated areas about the chin and elsewhere were rapidly healing. Since then some hyd.  $\bar{c}$  cretâ had been administered in a desultory way, owing to his infrequent attendance. The exhibitor had not seen him for some weeks until that day, when the face was found to be quite healed as far as the ulcerated areas and foci were concerned; the patient was feeling better than he had done for ten years.

(October 21, 1915.)

### Case for Diagnosis.

By H. W. BARBER, M.D.

THE patient, a soldier, went to the Front in December, 1914. His duties were to take rations to the reserve trenches and to groom his horses on his return. As far as he knew the horses were healthy. In April of this year, when at Ypres, he noticed a rash on both legs extending from the knees to the ankles. He stated that the rash began as "red spots," which became white in the centre and then burst. Owing to this eruption he was admitted to hospital at Ypres, and remained there ten days. He was treated first with a sulphur and then with a white ointment, and he recovered. Afterwards he was shifted to the Base, where he had to look after a horse with ringworm. Three months later an eruption appeared—similar to the previous one—first round his wrist and later on his legs as far down as his ankles. The pain caused by the rash was so great that he could hardly walk, and he was taken into hospital at Rouen, where he improved under treatment, and afterwards was transferred to the Second London Hospital at Chelsea. He appeared fairly healthy; he had acne on his face and upper part of his back. From his iliac crests downwards his skin was covered with numerous scars, some of them quite 2 in. in length. The skin in this region was also very pigmented.

Dr. Barber said that he was indebted to Captain H. Sharpe, R.A.M.C., for permission to show the case.

His own view was that the cause was some parasite, possibly animal scabies or pediculosis, though he had not previously seen a case of either of these conditions in which there had been so much scarring. The markings on the buttock partook of a linear arrangement.

### DISCUSSION.

The PRESIDENT thought the condition was primarily dermatitis herpetiformis. That did not necessarily exclude the view already expressed by others.

Dr. PRINGLE regarded the case as one of a primary pus infection, aggravated by persistent scratching and rubbing.

(October 21, 1915.)

### **Horny Growth on Arm.**

By W. KNOWSLEY SIBLEY, M.D.

E. H., A HEALTHY married woman, aged 60, presented on the anterior surface of the left arm just above the bend of the elbow a horny growth, which measured  $1\frac{1}{2}$  in. in length and was slightly curved, pointed and shaped like a bird's beak. It was hard and horny in consistence, and dark brown to black in colour, but had a fleshy base, and was stated to have commenced some six months ago as a moist wart to which the patient had applied caustic. It was growing entirely upon the soft tissue and was not situate over a bony surface, as was usually the case with such growths.

(October 21, 1915.)

### **Case of Erythromelalgia and Raynaud's Disease.**

By GEORGE PERNET, M.D.

THE patient was a woman, aged 60, who first attended at the West London Hospital on August 2, 1915, when she exhibited a condition of erythromelalgia of the fingers, some of the Raynaud phenomena being also present. The condition was very painful and kept the patient awake at night. There was a history of a miscarriage, and a subsequent Wassermann reaction was returned as "suspicious." Hyd.  $\bar{c}$  cretâ was ordered, and high-frequency current was administered by Dr. McDougal. This led to relief of the pain and the patient was able to sleep. Until a fortnight ago she was generally much better, but after that the pain and sleeplessness returned. Though the erythromelalgia retroceded at first, the Raynaud phenomena became more accentuated, actual necrosis of greater or less extent occurring about the ends of some of the fingers. This might have been due to a stay at Margate, where the patient felt the cold very much.



(October 21, 1915.)

**Case of Lichenoid and Nodular Tuberculide.**

By H. W. BARBER, M.D.

THE patient was a male child, aged  $4\frac{3}{4}$  years. The condition had already lasted for six months. The child had been fed in infancy on Nestlé's milk and barley-water. The father had died from perforated gastric ulcer, but had previously been healthy. The child's mother and one brother were both healthy. In February, 1915, the patient had measles followed by bronchitis, and some weeks later the eruption appeared on the hands and knees, and afterwards on the trunk. He had lately had a brassy cough, and the physical signs in his chest indicated pressure on the left bronchus with fibrosis and collapse of the left lung—presumably from caseous bronchial glands. The lesions on the trunk were lichenoid, and those on the hands and knees nodular.

(October 21, 1915.)

**Herpes Zoster with Paralysis of Arm.**

By F. PARKES WEBER, M.D.

THE patient, G. G., a waiter, aged 64, had, he said, enjoyed good health till suddenly, on June 16, 1915, between 2 and 4 p.m., whilst he was having a little afternoon sleep, his left upper extremity became paralysed. About June 23 an eruption of typical herpes zoster, the marks of which could be still just made out, was first noticed. It involved the left side of the head and neck, the left clavicular region and shoulder, and the upper front of the left side of the chest. There were no noteworthy pains accompanying the eruption. The paralysis had considerably diminished and the eruption was drying up when the patient was admitted to hospital on August 2, 1915. At that time there was very little if any muscular atrophy of the affected extremity. There was some contracture of the paralysed hand, with a tendency to turgid cyanosis—a condition frequently seen in the hand in cases of hemiplegia of cerebral origin. Both knee-jerks were exaggerated. Ankle clonus could be obtained on the left side, but not on the right side. The plantar reflex was slightly

of the extensor type on the left side and of the flexor type on the right side, but later on it was sometimes of the normal flexor type on the left side also. There was no paralysis elsewhere and no anaesthesia was noted. The blood serum on August 4, and again on September 14, 1915, gave a strongly positive Wassermann reaction for syphilis. Antisymphilitic treatment was tried.

Movement in the affected limb was now (October, 1915) somewhat better and the hand showed less acrocyanosis and contracture, but it was doubtful whether the improvement, such as it was, could be ascribed to the antisymphilitic treatment. The muscles were only very slightly wasted. No evidence of disease could be found in the thoracic or abdominal viscera. The urine was free from albumin and sugar and was of normal quantity. The brachial systolic blood-pressure was 150 mm. Hg. When the patient was excited or nervous there was considerable tremor in the affected (left) arm. The knee-jerks were still greatly exaggerated, and ankle clonus could be obtained with the left foot but not with the right. The plantar reflex was normal on the right side, but still variable on the left side. In regard to sensation, there was slight hypo-aesthesia on the ulnar side of the left forearm. The patient complained of a little pain in the region of the left shoulder. The pain might be of "post-herpetic" nature. He had not noticed any real pain before or during the actual eruption of herpes zoster.

Very many examples of herpes zoster of the forehead and face in association with oculo-motor or facial paralysis had been recorded, but relatively few of herpes zoster of the neck, arm, and thorax connected with paralysis of the arm. In the *Archives de Physiologie* for 1882 (Paris, 2 sér., ix, pp. 170 to 173), A. Joffroy described two cases in which herpes zoster of an upper extremity was associated with muscular atrophy of the affected limb. In the *Nouvelle Iconographie de la Salpêtrière* (Paris, 1914-15, xxvii, pp. 251 to 256), Souques, Baudouin and Lantuéjoul published an account of the case of an old man, whom they had shown before the Paris Société de Neurologie on May 7, 1914. The patient in question said that on March 10, 1914, he was feeling a little tired, when the paralysis in the left upper extremity commenced (with a sensation of heaviness and clumsiness). There was no real pain. Two or three days later the herpes zoster of the affected extremity showed itself. The authors regarded the paralysis as a radicular one.

The positive Wassermann reaction in the present patient was a feature apparently not noted as yet in these rare cases.

Several other references to the literature of the subject were given in Doucet's thesis, entitled "Le zona associé aux paralysies et aux amyotrophies" (*Thèse de Paris*, Année 1906, No. 23). Doucet considered that the paralyses were generally of peripheral origin and incomplete; the paralysed parts were mostly in the neighbourhood of the distribution of the herpes zoster, only rarely were they at a distance.

Amongst published cases one which nearly resembled the present case was that of a man described by C. Handfield Jones in 1882.<sup>1</sup> The patient, a solicitor, aged 64, was seen on August 28, 1872. He had been attacked by herpes zoster in the beginning of April, 1872, the left arm being the part affected, the eruption extending up from the hand to the posterior fold of the axilla. The eruption was not at first attended by a notable amount of pain, but after the eruption got better long-lasting neuralgia commenced. The skin was hyperæsthetic in the tract formerly occupied by the eruption. The whole arm was considerably wasted. Sir William Broadbent, in 1866,<sup>2</sup> described the case of a woman, aged 74, in whom herpes zoster in the distribution of branches of the brachial plexus was followed by partial paralysis in corresponding motor nerves. Other very interesting papers on the subject were those of Wilhelm Ebstein,<sup>3</sup> Stanley Barnes,<sup>4</sup> Alexander Bruce,<sup>5</sup> Frederick Taylor,<sup>6</sup> E. Farquhar Buzzard,<sup>7</sup> S. Vere Pearson,<sup>8</sup> Fage,<sup>9</sup> Norman Sharpe,<sup>10</sup> Weidner,<sup>11</sup> G. Waller,<sup>12</sup> and John Duncan.<sup>13</sup>

<sup>1</sup> C. Handfield Jones, *Med. Times and Gaz.*, Lond., May 6, 1882, i, p. 468.

<sup>2</sup> W. H. Broadbent, *Brit. Med. Journ.*, October 27, 1866, p. 460.

<sup>3</sup> W. Ebstein, *Virchow's Archiv*, Berl., 1895, cxxxix, p. 505.

<sup>4</sup> Stanley Barnes, *Trans. Clin. Soc. Lond.*, 1903, xxxvi, p. 236; also *Lancet*, 1902, ii, p. 1197.

<sup>5</sup> Alexander Bruce, "Unusual Sequela of Herpes Zoster (? Posterior Poliomyelitis)," *Rev. of Neurol. and Psych.*, Edin., 1907, v, p. 885.

<sup>6</sup> Frederick Taylor, *Guy's Hosp. Reports*, Lond., 1896 (for 1895), lii, p. 37.

<sup>7</sup> E. Farquhar Buzzard, *Brain*, Lond., 1902, xxv, p. 299.

<sup>8</sup> S. Vere Pearson, *Trans. Clin. Soc. Lond.*, 1903, xxxvi, p. 268.

<sup>9</sup> Fage, *Recueil d'Ophthalmologie*, Paris, 1909, 3 sér., xxxi, p. 209.

<sup>10</sup> Norman Sharpe, "Herpes Zoster of the Cephalic Extremity, with a special reference to the Geniculate, Auditory, Glossopharyngeal, and Vagal Syndromes," *Amer. Journ. Med. Sci.*, Philad., 1915, cxlix, p. 725. See also J. R. Hunt's various papers.

<sup>11</sup> Weidner, *Berl. klin. Wochenschr.*, 1870, vii, p. 321.

<sup>12</sup> G. Waller, *Weekblad*, of Amsterdam; abstract in *Brit. Med. Journ.*, 1885, ii, p. 560.

<sup>13</sup> John Duncan, "On Herpes Zoster," *Journ. Cutan. Med.*, Lond., 1868, ii, p. 211. He recorded two cases of hemiplegia, with herpes zoster of the same side of the body, in old

B. J. Vernon<sup>1</sup> and Sir J. Hutchinson<sup>2</sup> were perhaps the first to draw attention to the cases of oculo-motor paralyses associated with ophthalmic herpes zoster. Cases of herpes zoster (rarely the mouth and buccal mucous membrane had been involved in the herpetic eruption), accompanied by paralysis of extrinsic eye muscles or of the face, and sometimes by gustatory and auditory disturbance, had likewise been recorded by H. A. Spencer (1894), N. B. Darabseth (1894), Arthur Hall (1903), P. H. Mules (1903), E. Hewat Fraser (1904), T. Grainger Stewart (1909), Fage (1909, loc. cit.), Leplat (1910), H. Claude and H. Schaeffer (1911), Souques (1914), F. Ramond and Poirault (1914), and Laignel-Lavastine and Mlle. Romme (1914), and J. R. Hunt (various dates).

C. Achard and J. Castaigne (1897) described a case in which permanent paralytic dilatation of the left pupil followed an attack of left-sided ophthalmic herpes zoster,<sup>3</sup> and in their paper they referred to two other cases, in which (permanent ?) mydriasis of one pupil followed ophthalmic herpes zoster of the same side.

Dr. Weber wished to point out that, just as it was highly probable that unexplained attacks of paresis or paralysis in isolated muscles or groups of muscles (with or without much sensory disturbance or muscular wasting)—for instance, in the deltoid or other muscles of the shoulder-girdle and arm, or in the face, or in the extrinsic muscles of the eye—might be of the nature of zona (that is to say, of the nature of *herpes zoster sine herpete*, if one might express oneself thus), just so some cases of unexplained mydriasis of one eye might really be the manifestation of an attack of zona without any herpetiform eruption. Possibly even cases of unilateral mydriasis like those recorded by C. Markus<sup>4</sup> might be accounted for in the same way.

persons. In both cases recovery was good. Charcot, in one of his writings, referred to a case of hemiplegia with herpes zoster of the lower extremity on the paralysed side. E. Schwimmer ("Die Neuropathischen Dermatosen," Vienna, 1883, p. 139) mentioned the case of a man, aged 35, who had an attack of herpes zoster of the shoulder after a fall on the side in question. The herpes was followed by sensory disturbance and incomplete paralysis in the arm.

<sup>1</sup> B. J. Vernon, *St. Bart.'s Hosp. Reports*, Lond., 1868, iv, p. 121, Case V.

<sup>2</sup> Sir Jonathan Hutchinson, *Ophthal. Hosp. Reports*, Lond., 1869, vi, p. 181 ("A Second Report on Herpes Zoster Frontalis seu Ophthalmicus").

<sup>3</sup> C. Achard and J. Castaigne, *Gaz. hebdomadaire de médecine*, Paris, 1897, N.S., ii, p. 1177.

<sup>4</sup> *Trans. Ophthal. Soc.*, 1906, xxvi, p. 50.

(October 21, 1915.)

### Case of Granulosis Rubra Nasi.

By A. WINKELRIED WILLIAMS, M.B.

THE patient, a girl, aged 13, had suffered from the condition since early childhood. Her nose was red and the skin wet with sweat. There were hyperidrosis and lumpy swellings (sweat gland abscesses) in the armpits. The hands and feet were cold and moist. The localized hyperidrosis of the cartilaginous part of the nose was very marked until about two months ago. A few deep-seated papules, under the diascope, gave the appearance of something between a hydrocystoma and lupus. The patient was now much better, in fact she was getting towards the age when recovery usually took place.



## Dermatological Section.

President—Dr. J. H. STOWERS.

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(November 18, 1915.)

### A Boy, aged 15, of English Parentage, showing very Septic Ulcerations of Undetermined Nature.<sup>1</sup>

By E. G. GRAHAM LITTLE, M.D.

THE case, first shown at the last meeting, remains very obscure in causation. We have had the patient in St. Mary's Hospital since the last meeting, and the lesions have steadily grown larger and more numerous. There is the same sloughy ring surrounding an extremely foul ulcer, which was characteristic of some of the lesions seen at the October meeting. A certain number of blisters have been noted from time to time—isolated, thick-walled blebs. The contents of one of these was examined, by film and culturally, but nothing was found; it was sterile in the unbroken stage. The bacteriologist at St. Mary's Hospital has also examined the ulceration, and reported nothing specific from that, except the presence of a surprising number of pyocyaneus organisms. We have regarded that as a possible factor in causation, and have given the patient a vaccine made from his own streptococcus and from his own pyocyaneus. I am sorry to say the boy's condition is very septic, and he is obviously getting worse; but for that I would have brought him again to-day. I have to-day examined the section of one of the granulomatous lesions, but found nothing at all characteristic. There is some acanthosis and a good deal of ordinary infiltration round a prolongation of some of the lesions, but there is no specific organism to be found in the skin. There is certainly no evidence of sporotrichosis, and, as I said last time, that was a tentative diagnosis, founded

<sup>1</sup> Exhibited at meeting of October 21, 1915; see *Proceedings*, 1915, ix, p. 3.

## 22 Little: *Septic Ulcerations of Undetermined Nature*

chiefly on the way in which it was reported to have originated and spread. Blisters are infrequent, and are not at all necessary predecessors of the other lesions. There has been no elevation of temperature, and, except that the ulcerated lesions are in a very foetid and foul condition, the boy is in much the same condition as you saw him. Some of the ulcers now look like large medallions, raised above the skin level. These lesions are spreading, especially on the fingers, hand and the wrist, and one of them is three times the size it was when he was exhibited. I have tried to stop their spread by painting with pure carbolic and then wiping over with alcohol, and we have tried all the



Case of boy with chronic septic ulcerations. The illustration shows the flat ulceration and raised granuloma-like plaque.

resources which Sir Almroth Wright has been introducing into the work of our hospital, in the matter of irrigation and otherwise, on which he has recently spoken before this Society. There is a history that the boy's mouth was very foul before admission, but I have not seen any involvement of the buccal mucous membrane. He is a London resident, and has never been out of England.

The PRESIDENT: I hope Dr. Little will give us a further report of the case later on.

(November 18, 1915.)

**Case of Chronic Ulceration (*Ulcus molle serpiginosum*),  
probably due to Inoculation with Ducrey's *Bacillus* of  
Soft Chancre.**

By E. G. GRAHAM LITTLE, M.D.

THE history, which was given with great frankness and detail, is as follows: The patient, a British solicitor, aged now 43, had sexual intercourse in a British South Coast seaport with a street-walker on January 26, 1902. Within the three days following this there developed on the penis two soft sores, which healed within the week. Some three weeks later a bubo developed in the right groin; its nature was not at first recognized, and it finally burst spontaneously, in March, 1902, and a large quantity of matter was evacuated from it. In view of the subsequent happenings it is of interest to note that the woman from whom the infection was derived was a native of this country, and the patient had not been and has not subsequently been in any tropical country, and, in fact, has spent the last eight years in British Columbia. On the site of the burst bubo an ulceration remained, and from this focus a serpiginous, very slowly advancing ulceration spread in a manner which has always been the same; that is, the central areas healed while the disease spread at the periphery, and in this way the whole surface of the thigh, the right lower half of the abdomen and the back of the body up to the level of the midriff on the right side is seamed with healed scars, with curious trabeculae of normal tissue running over the affected area. All the surfaces of the thigh are affected, with the exception of a small area on the inner aspect of it just below the groin. At the present time the ulceration is confined to the lower margin of the great sheet of scarred skin bounded by the limits named above, and a complete ring of contiguous but discrete ulcerations, averaging each the size of a walnut and arranged in a sinuous line, encircles the leg just below the knee. The ulcer presents an undermined edge, is moderately deep, hardly at all painful, and seems to heal partially as mentioned above, in the part in contact with the region scarred by previous ulceration. The ring of ulcerations thus forms a demarcation between healthy tissue and the scarred region. The patient has never been free

of some ulceration in some part of this area in the past thirteen years, and he has used absolutely no treatment except that of local dressings of lint cut in small circles to cover each individual ulcer, to which it is rendered adherent by ointment. He has had no constitutional treatment whatever. He has been and is in otherwise excellent health, though he seems to have roughed it considerably in Canada. His blood gives a negative Wassermann reaction.

#### DISCUSSION.

MR. McDONAGH: I think the case is a typical one of *ulcus molle serpiginosum*. The case interests me particularly, since my article<sup>1</sup> on the subject was, I think, the first that appeared in the English language, and because this is the first case brought to my notice in which the whole course of the disease has been run in this country. I have had seven cases under my care, and all occurred in men who had been in the Tropics. The disease appears to start either from a bubo which has burst of its own accord, or from one into which a big incision has been made; in both instances a certain amount of local necrosis of the skin is very liable to occur. Under ordinary circumstances, Ducrey's bacillus is an extracellular organism, but in the cases of *ulcus molle serpiginosum* which I have carefully examined, the organism has not only taken to an intracellular habitat, but it has also changed its form. Possibly the local necrosis above referred to is the starting point of this change, since I have never seen a case develop in which only a small nick was made into the bubo to let the pus out.

DR. PRINGLE: Although the general circumstances in this case undoubtedly suggest the diagnosis of syphilis, a close examination of the scar at once shows that it is not a syphilitic one; there are a large number of islets of apparently moderately healthy tissue in the midst of the scars. I remember Mr. McDonagh's paper on the subject of "*Ulcus molle serpiginosum*," and from it was enabled to make the diagnosis which he has confirmed. I have never seen a similar case, and I am sure they must be extremely rare in this country. I would ask Mr. McDonagh whether a diagnosis could be made from a bacteriological examination of the spreading margin of the ulcers at this period of the disease; and whether scraping or destruction by caustics can be successfully practised.

DR. GRAY: I would like to know whether in the cases which were scraped a strong caustic was applied afterwards.

DR. PERNET: When buboes are unhealthy, breaking down, phagedænic and undermining as a result of mixed infection, I have observed that thorough scraping makes matters much worse.

<sup>1</sup> McDonagh, *Brit. Journ. Derm.*, 1914, xxvi, p. 1.



The PRESIDENT: I should like to know whether 200 gr. daily of iodide of potassium is the maximum amount given by Mr. McDonagh, and whether he thinks a corresponding advantage is conferred upon the patient by such large doses.

Dr. DORE: Has X-ray treatment been tried in cases of this description? Many chronic ulcerations yield to applications of the rays, and it seems reasonable to infer that they might prove beneficial in this case.

Dr. KNOWSLEY SIBLEY: I would ask whether Mr. McDonagh has tried freezing in these cases, such as by means of snow. We know how satisfactorily soft sores heal up under freezing processes. With regard to the giving of large doses of iodide of potassium, I am very sceptical as to whether anything over 4 or 5 gr. doses is of any use; anything beyond that I look upon as waste. I have heard of dermatologists prescribing 1,000 gr. doses of iodide of potassium. I think it is the opinion of those who have worked at the matter that 4 or 5 gr. is as much as is absorbed in a dose. I have been interested in the principle of giving nascent iodine: 20 gr. are given once a day immediately after breakfast, and four, six, and eight hours afterwards chlorine water is given; that liberates the iodine, and you get a much greater effect. On that principle everything that is given over 4 or 5 gr. acts as a hindrance by keeping the iodine imprisoned and preventing it liberating itself. In this case I would suggest painting with acetone and  $\text{CO}_2$ , and filling the little excavated ulcers with the freezing mixture.

Mr. McDONAGH (in answer to questions asked): The organism can easily be found under the spreading edge of the ulcer. The undermining of the edge is typical of the condition, and it exactly resembles the original soft sore, only naturally on a larger scale. Under the undermined edge from that part of the ulcer which is spreading the organisms can be readily demonstrated in section. In my experience all forms of operative treatment have made matters worse, even careful excision. X-rays and radium have proved ineffective. The only treatment I have found to do any good is to give iodine internally and externally. I always give increasing doses of potassium iodide internally, and apply camphor phenol and iodoform externally, being very careful to get well under the undermined edges. Intravenous injections of antimony and ionization with zinc chloride have also proved useful.

*Postscript.*—Since the case was shown Dr. Fleming and Dr. Colebrook, of the Inoculation Department at St. Mary's Hospital, have seen the patient and recognize the strong similarity clinically to a case of chronic ulceration which was treated in the Department some years ago, and was then demonstrated to be due to an inoculation with Ducrey's bacillus. An attempt will be made to obtain a vaccine from this patient's ulcers, and to try this therapy.



(November 18, 1915.)

Case of *Lupus vulgaris exuberans* complicated by  
Epithelioma.

By GEORGE PERNET, M.D.

THE patient is a woman, aged 50, who states that at the age of 6 a "wart-like" growth appeared about the root of the nose. As a



Case of *lupus vulgaris exuberans* complicated by epithelioma.

result of a knock some fifteen years ago (or more), the disease has spread slowly from that point until it has attained its present dimensions. The *lupus vulgaris* now occupies the face in a mask-like symmetrical manner, which is well brought out in the figure. About

the original focus there is a certain amount of scarring, but the greater part presents the apple-jelly granuloma *en nappe* in a very typical way; and this is well raised above the level of the skin, especially in the region of the chin and mouth, where the growth terminates in a definite border. The end of the nose was occupied by a large adherent crust. Four weeks previously to her being first seen at the West London Hospital a "little head" made its appearance just above the left orbit, and rapidly enlarged. It was flatly hemispherical, raised more than  $\frac{1}{2}$  in. above the level of the skin, and measured 2 in. in diameter. The patient has not had any previous X-ray treatment. The growth has been removed by Mr. Souttar, and a flap brought down to cover in the gap.

The case is shown (1) on account of the unusually high degree of the exuberance of the lupus vulgaris growth, and (2) on account of the epitheliomatous complication.

A photograph of a case of epithelioma in lupus vulgaris under the care of the late Dr. Radcliffe-Crocker in 1890,<sup>1</sup> that is, before the days of the X-rays, is also shown. The growth in that case was very like the present one, but it had taken three months to grow; whereas, if the present patient's account can be quite trusted, her epithelioma has only taken one month. I hope later to report on sections.

#### DISCUSSION.

Dr. MACLEOD: Has the patient had treatment by means of the X-rays which may have been responsible for the epithelioma? I have reported a case of this nature in the *British Journal of Dermatology* in 1906.<sup>2</sup> The occurrence of epithelioma in old-standing lupus, which was occasionally met with before the introduction of the X-rays, seems to me to have become more frequent since their employment. To Dr. Heath's types of lesions which may develop on lupus tissue, I would add a fourth which I have met with in one or two instances in the scar tissue of healed lupus—namely, red angiomatous patches, irregular in shape, level with the surface or slightly raised. I wish to raise a point with regard to the treatment of lupus vulgaris by means of the X-rays. My experience has led me to doubt whether it is possible completely to eradicate the lupus tissue by the X-rays without producing a dangerous X-ray scar, and I have never seen a case in which eradication has been accomplished. Of course I admit the utility of the X-rays in the healing up of ulceration in lupus patches, and I am accustomed so to employ them.

<sup>1</sup> See also Radcliffe-Crocker's "Atlas," plate LX.

<sup>2</sup> *Brit. Journ. Derm.*, 1906, xviii, p. 104.

Dr. GRAHAM LITTLE: I should like to ask members what is their experience as to the malignancy of epitheliomata growing on lupus tissue. In the few cases which I have seen I have been struck with the comparative benignancy of the growth and the long periods of freedom which many patients have enjoyed after excision. I recall one such case in a patient who attended my department for at least ten years after removal of a growth, which had appeared on an old lupus, and was demonstrated to be epitheliomatous. During that period there was no recurrence.

Dr. PRINGLE: Has the exhibitor any theory as to what was the starting point or the determining factor in the ætiology of this epithelioma? And I would like to ask the Section generally if they have any views as to the particular tissues from which such epitheliomata spring. Some years ago<sup>1</sup> I published a case of multiple epitheliomata arising from lupus erythematosus; the case was a severe one, as I know of thirteen tumours being removed. In that case the tumour seemed to develop from the cicatricial tissue which resulted from treatment; such tissue frequently forms spontaneously in lupus vulgaris.

Dr. DOUGLAS HEATH: I think there are three kinds of tumours which arise on lupus vulgaris. In the moist stage it is fairly common to get a pyogenic granuloma. At a later stage, in the more chronic cases—whether they have been treated by rays or not—a warty epithelioma seems the most common. Thirdly, there seems to be a more rare condition, of which I have recently had an instance—namely, a sarcoma developing on the scar tissue of lupus vulgaris. This last bears on the point raised by Dr. Pringle as to the point of origin of the malignant growth. In my case, in a man with old lupus of the upper lip and nose, a tumour developed to the size of a large cherry. It was excised and the area treated with X-rays; but they did not check it, and the tumour continued to grow on the smooth scar surface. A surgeon removed the growth freely, taking out a piece of the entire thickness of the upper lip, and it proved to be sarcoma. It recurred. I took the man into hospital, and cross-fired the rapidly growing tumour with pure radium salt on each side, and the tumour shrunk up and fell off. Since then I have treated the base with radium, and all signs of the growth have disappeared. Four months after the treatment there was no recurrence. Radium treatment seems to do better for this class of case than anything else.

Dr. DUDLEY CORBETT: I think it is hardly right to deduce from the cases mentioned by Dr. MacLeod that we should not use X-rays in the treatment of lupus, altogether apart from the debatable point as to whether an actual cure is ever effected by their means. I have seen cases such as Dr. MacLeod describes, and if one looks up the records one finds that the rays were given twice a week until a reaction supervened, when they were stopped, re-applied when it had subsided, and the process repeated for months or years. Nowadays, if

<sup>1</sup> *Brit. Journ. Derm.*, 1900, xii, p. 1.

one uses unscreened rays, no more than one pastille dose is given per month whether the dose is divided or not, and they can only be given more frequently than this if aluminium filters are used, a method which seems to suit some cases. It is yet too early to judge from cases treated during the last two or three years as to whether telangiectases will result, but my principle has been to avoid an erythematous reaction by suitable periods of rest, and then it is reasonable to suppose that there will be neither atrophy nor telangiectases.

Dr. DORE: I agree with Dr. Dudley Corbett that the occurrence of telangiectases and atrophy of the scar in cases of lupus treated with X-rays depends upon the amount of inflammatory reaction set up by them. I have treated a good many cases over periods of several years without this result ensuing. In my opinion X-rays are the best form of treatment for the majority of cases of lupus vulgaris (Finsen light being reserved for small and superficial patches). I admit the difficulty of getting rid of residual nodules, and I think it is this that often leads to an increase of dosage, followed by erythema and dermatitis, and the subsequent development of disfiguring scars. With regard to epithelioma, I have seen it in several instances in which X-rays had not been employed, and I think it is important to remember that in most cases of lupus, when they are brought forward, many methods of treatment have already been tried, and that an epithelioma is more prone to develop upon damaged tissues.

The PRESIDENT: I think one of the most important points we can consider with regard to the treatment of these cases is that which Dr. Dore has referred to—namely, the influence of previous remedies. That will explain some of our failures and disappointments when we apply remedies which otherwise we believe to be good. As Dr. Corbett said, the moderate use of X-rays seems to be the most promising form of treatment, and I think many of the ill-effects we have seen are due to excessive application or to a misunderstanding on the part of those who administer the rays, or, possibly, an insufficient knowledge of the methods of application.

Dr. PERNET (in reply): The Radcliffe-Crocker case, of which I have shown a photograph, was observed before the days of X-rays. I am quite aware that epithelioma may complicate lupus vulgaris that has not been X-rayed; nevertheless I think that there is some ground for the caution expressed by Dr. Norman Walker. It is not wise to overdo the X-raying, as is sometimes done in a routine way by unqualified radiographers. Not long ago I saw an epithelioma (of two months' duration) with stony hard border in the centre of the cheek of an old lady aged 75, who had had lupus vulgaris from the age of 3. The whole of the left cheek was scarred. She had not had the X-rays, but galvano-cautery and some radium exposures. She had been under the care of Dr. Dubois-Havenith, of Brussels, for some twenty years.



(November 18, 1915.)

### Case of Granuloma annulare.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a girl, aged 10, one of three children. The distribution and type of the eruption bear a striking resemblance to the eruption described in the second of Dr. Colcott Fox's cases of ringed eruption.<sup>1</sup> Two forms of lesion are present. One, the earlier stage of the second, is a deep-seated nodule, the skin over which is reddened and the average size of which is that of a pea. The other lesion, which in most cases is described as having passed through the nodular stage, is a circular, raised, vividly pink patch, varying in size from that of a sixpence to that of a shilling, persisting *in statu quo* for several months, and giving rise to no subjective symptoms whatever, so that the child is unconscious of their presence except where she can see them. With the lapse of time the colour is apt to fade and the patch to flatten down somewhat. The lesion is nummular rather than circinate, that is, the centre has not involuted—there is not the granular rim which is so typical of granuloma annulare.

The number of lesions is unusually numerous. In Fox's case there were fifteen separate lesions, and that was an unusually extensive case. This number is exceeded here. The detailed distribution is as follows: The earliest lesions, and also now the largest, are four nummular patches, close together but quite discrete, on the skin covering the left calf. These patches are the size of a shilling. The sequence of the other lesions was not remembered. Left side: Left buttock—a single recent nodule slightly reddened. There are three nodules close together on the front of the upper third of the left thigh; there is one nummular patch on the inside of the left knee; there are two nummular patches on the outer and upper third of the left leg; there are two ringed patches on the middle of the posterior surface of the left thigh, and four patches over the calf. Right side: There are three nummular patches as mentioned above on the middle of the anterior surface of the right leg; there are four small nummular patches close together on the outer and middle third of the right leg; there is one nummular patch on the middle

<sup>1</sup> *Brit. Journ. Derm.*, 1896, viii, p. 15.



of the posterior surface of the right thigh, and one nummular patch over the right calf. There are thus about twenty-five separate lesions scattered on the limbs below the level of the pelvis. The onset dates from six months ago. Fresh lesions are still appearing.

One maternal uncle died of phthisis, aged 17, and one brother of the patient, aged 9, is at present under treatment at the Tuberculosis Dispensary for tubercle of the lung. The child herself has been examined by my colleague, Dr. Langmead, who reports that there is no evidence of tuberculosis or other constitutional illness.

I believe the case corresponds with the type of disease described by Crocker and Campbell Williams under the name "erythema elevatum diutinum," which I included in the generic group of granuloma annulare in a survey of the subject in 1908. I should like to ask some of the senior members present who had opportunities of seeing the original cases reported by Crocker whether, in their opinion, I am correct in regarding this case as of that nature. I admit that the distribution of Crocker's cases was somewhat different. It is obvious from mere inspection that the title "erythema elevatum diutinum" admirably describes the actual appearances in this patient, for the lesion is a fairly homogeneous pink elevation without the granular white ridge made up of separate granular nodules surrounding a depressed centre which is the characteristic aspect in the great majority of instances of cases described as granuloma annulare. The strong family history of tuberculosis lends countenance to the view I expressed in 1908, that tuberculous associations were somewhat suggestively frequent in this disease.

#### DISCUSSION.

Dr. PRINGLE: I think that if anything could prove that there is no hard-and-fast line between erythema elevatum diutinum and granuloma annulare, this is a case in point. In his observations Dr. Little has practically admitted my point, because he has shown that these characteristics are present which are really so typical of the old erythema elevatum diutinum, although not exactly of the kind of case which Dr. Radcliffe-Crocker described. That was a very much harder lesion and in a different position. But, on the whole, this lesion corresponds far more to the ordinary type of erythema diutinum, and, I think, shows there is no hard-and-fast borderline, either pathological or clinical, to be drawn between the two conditions. My impression is that the condition in this child will disappear spontaneously; it is my experience of similar cases, though I have not seen a large number of them. The disappearance of the lesions is probably hastened by mild X-ray treatment.

Dr. GRAY: Mild X-ray treatment also seems to prevent the recurrence of the condition. I remember the case of a man who had typical granuloma annulare lesions on both hands. We did a biopsy and tied one hand up afterwards, and found that the other lesions on that hand had disappeared under the bandage. The patches on the other hand were treated with X-rays, with success, and these had not recurred when I saw the patient some months after, but those on the bandaged hand had done so.

Dr. DORE: Is Dr. Little familiar with this condition affecting the face? Last week I saw a soldier who had a circular lesion, the size of a five-shilling piece, of six months' duration, near the right eye, and a nodule lower down on the cheek, which was not ringed and looked like a syphilide. The circinate patch had a semi-cartilaginous edge, and, the Wassermann reaction being negative, I have come to the conclusion that the case is one of granuloma annulare.

The PRESIDENT: I would ask Dr. Graham Little to include the ear when speaking of the face. A case of great interest was published in 1911 by Dr. Chipman,<sup>1</sup> in which the ears were involved. I have myself seen two cases in which the ears were implicated.

Dr. GRAHAM LITTLE (in reply): As regards the duration of the lesions, I saw a case which interested me greatly in the person of the small daughter of a medical man, aged 8, who had contracted the disease in India, and had had persistent lesions for at least twelve months before I saw her. These were also very numerous, with the same distribution as in this case, but were of the more usual granular white-ridged type. They disappeared completely under short exposures to freezing with carbon dioxide, and had not recurred a year after. But lesions may disappear almost spontaneously, as I found in one of my early cases, in which a number of typical lesions completely vanished, apparently as a result of covering the area on which they were situated with a dressing rendered necessary by a biopsy of a single nodule in the same neighbourhood. As to the distribution on the ear, I have not seen this, but in a case recorded by Dr. Grover Wende, who was good enough to send me an excellent photograph and a section from the skin, there were numerous lesions on the side of the face, the neck, and, if I am not mistaken, on the cheeks. I believe the histology to be sufficiently characteristic to allow of a diagnosis being made from inspection of a section of affected skin.

<sup>1</sup> *Brit. Journ. Derm.*, 1911, xxiii, p. 349.

(November 18, 1915.)

### Case of Folliclis (Papulo-necrotic Tuberculide).

By ALFRED EDDOWES, M.D.

THE patient, a female, aged 21, married, is a music-hall artiste. Her general health has been excellent.

Family history: Her father, who was a very strong, healthy man until a few months ago, died after a short illness of some disease which affected his mind. The rest of the family enjoy excellent health.

She took two bottles of a blood mixture five months ago. The skin eruption has existed for about a year and a half, and has gradually spread from the arms and thighs to the trunk. It has the typical appearance of the nodular cutaneous tuberculide, "folliclis." The cervical glands are greatly enlarged on both sides of the neck. The patient says they have been like that, at any rate since she was aged 10, and have never at any time caused discomfort.

The blood examination, made by Dr. Edward Back, shows: Total red cells, 3,000,000; hæmoglobin, 75 per cent.; colour index, 0.95. The red cells show slight vacuolation and poikilocytosis. The average size is normal. Total white cells, 11,000. Differential count: Small lymphocytes, 47 per cent.; polymorpho-neutrophiles, 52 per cent.; polymorpho-oxyphiles, 1 per cent. Blood test for Wassermann reaction: The reaction is fully positive.

The blood count would be consistent with either chronic tuberculosis or Hodgkin's disease. May we exclude the latter? If so, how do we account for the fully positive Wassermann reaction? So far—and the patient was carefully examined a week ago—there has not been found any roseola, any enlargement of axillary glands, or of glands other than those already described. There has been no headache nor even a suspicion of sore throat or mouth rash. I have never before met with or read of such a combination of conditions.

### DISCUSSION.

Dr. PRINGLE: I think there will be a consensus of opinion that Dr. Eddowes's diagnosis is correct; I agree with it entirely. But I cannot help expressing the hope that he and the rest of us will drop this unfortunate term

"folliclis." It is unnecessary, it is extremely inaccurate; and we now have the expressive term "papulo-necrotic tuberculide" to take its place, so that I think "folliclis," like "acnitis," may well be consigned to oblivion. The other observation I would make is one of caution in applying von Pirquet's test to cases such as these. Only lately, in my ward at the Middlesex Hospital, in a case somewhat similar but rather more acute, the von Pirquet test, carried out in the usual way, was followed by a most violent reaction and an erysipelatous condition which caused considerable alarm for some days. I do not think Dr. Eddowes has used the test in his case, and if that be so I congratulate him on his wisdom.

The PRESIDENT: I endorse the remarks of Dr. Pringle. At the moment I cannot explain the positive Wassermann result in this case, unless it occurs from a source apart from that we are considering. The question is whether the result of one Wassermann test should be accepted in circumstances like this. Personally, I much doubt it. My experience points to the need of confirmatory tests, and preferably by the original technique. I agree with Dr. Eddowes's diagnosis. It is a rare type of case, and we shall be glad to learn the later course of it, with the results of treatment.

Dr. EDDOWES (in reply): I had a definite Wassermann "fully positive" report in the case, but I trusted rather to clinical appearances and have not regarded the case as syphilitic, as I have not seen any signs or symptoms, such as a roseola or a mucous patch in the mouth, or heard of her suffering from headaches. In the belief that the eruption is a tuberculide, I am treating it locally with antiseptics, and I hope to see benefit from a course of arsenic, which I think will do her more good than iron. Mr. McDonagh pointed out to me that the patient has some nasal trouble, which I had not noticed. That shall be investigated. I did not suggest taking one of the nodules for examination or excising a gland, as I think that course would have resulted in the patient disappearing from our view.

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(December 16, 1915.)

### Case of Granuloma Annulare with a Widespread Follicular Eruption.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a young officer in good health. For about a year he has noted the eruption on his hands and knees. This is in the form of a ridge of white nodules closely aggregated together and arranged in the ringed distribution so typical of the disease described by Radcliffe-Crocker under this name. There are several such small rings on a patch at the base of the medius and ring-finger on the dorsum of each hand. The skin included within the ridge is of a bluish tinge, showing no atrophy or other change. On the knees the eruption is less diffuse but equally characteristic. Itching is very moderate. The case presents many dissimilarities clinically as compared with the patient whom I brought forward at the last meeting of the Section, a little girl, who showed the variety of the disease known as erythema elevatum diutinum. The follicular eruption in the present case, which is present on the thighs, legs, arms, and forearms, has been noted from early childhood, and might be regarded as an exaggerated lichen pilaris, but there are some lesions not peripilar, and showing a fine necrotic centre, rather suggestive of an abortive acneiform tuberculide. The tuberculous associations of granuloma annulare which have been recorded in a number of cases makes the occurrence of a follicular eruption, which also may be tuberculous, of some special interest. There is, however, no family or personal history of tubercle in this patient.



THE PRESIDENT: I do not see any necessary connexion between the lesions upon the hands and the condition of the legs of this patient. There are many persons whose skins present the same appearance as do the legs of this officer, apart from accompanying disease.

(December 16, 1915.)

### Case of (?) Lichen Planus.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a Hindu medical student, aged 22, in his third year at Edinburgh. He is of somewhat delicate physique, and appears to suffer from the cold. During the past twelve months the eruption now present has appeared in the form of bluish, somewhat verrucose inflammatory patches on the forearms, back of trunk, and scalp. Over the left eyebrow there is a disk-shaped warty patch the size of a sixpence. On the dorsum of the right foot and at its outer edge there is a linear warty ridge circumscribing a deeply red inflammatory patch covering the outer surface of the foot. On the plantar surface of both feet, but much more markedly on the right, there is a granular warty condition occupying the whole sole. On the abdomen, just above Poupart's ligament, on both sides there is an area covered by flat papular lesions very much like a retroceding lichen planus. The mucosæ show nothing abnormal. Sections were obtained from a lesion excised from the arm. These show a very extensive and uniform infiltration of mononuclear cells just below the rete, the interpapillary processes of which are flattened down by the pressure of the cells. The granular layer, which is so characteristic a feature of lichen planus, is not increased, and there is little or no acanthosis. There is no appearance of foci of deep-seated infiltration, and no giant cells or other indications of tuberculous causation. The disease had appeared only some years after his arrival in this country. The Wassermann reaction is negative. I do not think that either the clinical or histological features sustain the diagnosis of tubercle, but I will make further investigations, and in particular will obtain an experimental inoculation of a guinea-pig with tissue from one of the lesions and report later.

## DISCUSSION.

Dr. PRINGLE: I venture, with some confidence, to suggest that this is a tuberculous affection of the skin. The scars of the healed lesions, the spreading disease on the foot, and the outlying lesions on the forehead and other parts appear to me to be deposits in the true skin presenting all the characteristics of tuberculous tissue. I do not know whether the von Pirquet test has been done, but in the absence of syphilis, which the objective characters do not suggest to me, I do not see what else the disease can be.

Dr. ADAMSON: I agree with Dr. Pringle that the eruption is of tuberculous nature, and regard it as acute multiple lupus. About two years ago I showed a similar case of multiple lupus appearing in an adult. In that case the lupus patches entirely disappeared after six months' treatment by internal administration of iodogenol, prescribed by Dr. Abadie, of Paris.

Dr. A. EDDOWES: I am inclined to agree with the last two speakers. When I first saw the case the idea of lupus came into my mind. But Dr. Little remarking that this is somewhat similar to lichen in some respects recalls to my mind that I have at present a patient under my care who has a small patch which commenced first as lichen spinulosus; each spine was very prominent. The patch seems inclined to cease spreading, and the spinous processes are falling out. The condition might be described as lichenization, without the usual excessive thickening and hardening of the horny layer. If I can induce my patient to give me a section I will exhibit it, and will try to bring the patient also.

The PRESIDENT: A few lesions upon the feet of this patient are certainly suggestive of lichen planus, but I am in accord with those who regard them as tuberculous manifestations.

(December 16, 1915.)

**Case for Diagnosis (? Tuberculosis).**

By W. KNOWSLEY SIBLEY, M.D.

THE patient, A. A., is a married man, aged 27, a labourer on the railway. His mother died at the age of 42, from consumption. He has never had any serious illnesses. He has been married eight years, and has four children, all healthy. His wife has not had any miscarriages. The condition of the skin came on shortly after marriage, eight years ago, the first lesion appearing on the knuckle of the left hand, which he describes as a gathering full of pus, which after a time healed and left a scar. Shortly after, other similar lesions appeared on other parts of the body. He considers he has been incapacitated and on the sick list

for nearly six months each year. On one occasion he was ill and away from work for six months. The lesions are painless when they first appear, but they become painful after a few days and look like superficial abscesses, all of which leave distinct scars. Most of these are circular, about the size of a shilling, and look like vaccination marks. They are not pigmented. The condition has chiefly affected the arms, forearms, palms, legs, especially about the knees, also the soles. They are slightly distributed over the shoulders, more abundant over the buttocks, all being more or less symmetrically arranged. Scars are also present on the face and head. A few minute punctate vesicular lesions are present on the dorsum of the hands. There has never been any affection of the mucous membranes. The patient lost most of his teeth early in life from caries, otherwise he appears to be a well-preserved, healthy man.

The *Staphylococcus albus* was isolated from one of the recent lesions. The Wassermann reaction was negative, and the von Pirquet doubtful.<sup>1</sup> The blood count was as follows: Red cells, 4,650,000 per cubic millimetre; white cells, 4,800 per cubic millimetre. Differential leucocyte count: Polymorphonuclear leucocytes, 67·5 per cent.; small lymphocytes, 25·5 per cent.; large lymphocytes, 5 per cent.; eosinophiles, 1 per cent.; basophiles, 1 per cent. The urine does not contain either albumin or sugar.

When I first saw the case in the out-patient department it struck me as a recurrent bullous eruption on account of the regularity of the scarring, but investigation revealed no history of bullæ. What then occurred to me was that the process might be tuberculous, but against that is the fact that so many of the lesions get perfectly well with little or no treatment. Most of the lesions have got well, one or two have left typical scrofulous scarring, but their generally circular and clean-cut character is against their being tuberculous. Apparently they have never affected his health to any extent. I presumed it was not a chronic staphylococcus infection recurring over and over again. The question of its being an artefact also occurred to me, but I think that is excluded by the perfect symmetry of the lesions, very few of them being irregular or linear. Sporotrichosis must also be excluded, as I have not been able to find anything of that nature. I have not yet been able to make a biopsy, but one might well examine a portion from the thickening over the right knuckle. There has never been any affection of the nails, nor of the mucous membranes.

<sup>1</sup> A subsequent von Pirquet gave a violently positive reaction, vesication, and a subsequent ulceration.

## DISCUSSION.

Dr. TRAVERS SMITH: May I suggest the diagnosis of angio-neurosis here? I have had one case not dissimilar from this, and the subsequent scarring was very much like what we see in this case.

Dr. J. H. SEQUEIRA: I should like first to ask Dr. Sibley whether there is any seasonal variation in this eruption: whether it is worse in the winter than in the summer? [Dr. SIBLEY: He does not think it is; I asked him that question.] I think it is a tuberculous affection, and that it belongs to the group to which the name "tuberculide" has been given. That is, I believe it to be due to a reaction of the skin to circulating tubercle bacilli. The fact that these lesions have healed I do not consider to be against, but rather in favour of the view. I have under my care a woman whom I have shown here, who has hundreds of small scars on her upper extremities, and some large scars, very similar to those in this case, on her lower extremities. I have followed that case for several years; she has had the eruption eight years. Every lesion healed up practically without treatment, for she has usually had just a simple ointment given to her. In this type of case the lesions do tend to heal. Most cases of Bazin's disease can be healed by placing the patient for a few weeks in the horizontal posture. These cases are entirely different from scrofuloderma and some forms of lupus.

Dr. ADAMSON: I do not think this is a tuberculide condition. The patient has not the cold hands and feet that such cases usually have, and did not give a marked von Pirquet's reaction, as those cases always do. And I do not think that in tuberculide one gets such big, deep scars as are seen in this case without the breaking down of deeply seated nodules, which are absent in Dr. Sibley's case. I am inclined to regard it as dermatitis artefacta. The alternative seems to be a persistent erythema multiforme, such as Dr. Gray, Dr. Little, and others have shown, but the irregular shape of the scars seems more in favour of an artificially produced lesion.

Dr. GRAY: I do not agree with Dr. Adamson that this is dermatitis artefacta. I think it belongs to that group of cases which have been labelled "persistent erythema." This case is almost identical, except in the matter of degree, with that which I showed at the International Congress. The lesions were distributed in much the same way, and there is the same difference in the lesions on the upper and the lower extremities. In the upper limbs the lesions are not usually associated with extensive hæmorrhage into them, while the lesions in the lower limbs are so associated, and have a tendency to leave large pigmented scars behind. The initial lesions in the present case seem to be much smaller than those in my case. Some of them are small follicular papules, and there is a slight tendency in all of them to have a little central hæmorrhage. The lesions in the lower limb also tend to get damaged and to break down and become infected. In my case we were never able to produce



evidence of tuberculosis, and that is so here. I do not know what the aetiology is. I labelled mine "persistent erythematous eruption." The microscopical picture of the case was a very intense sub-epithelial cellular exudate, with much necrosis.

Dr. GRAHAM LITTLE: In answer to Dr. Gray, I do not think that this case can be regarded as erythema multiforme. The patient mentioned by Dr. Gray was under my observation several years, and though she was practically never free from some lesion during all that time, there was never any scarring. I think the presence of scarring presents a very important differentiation. I do not think such an amount of scarring would result from erythema multiforme, whatever type it might be supposed to be. It is, however, very like a case I showed to the Dermatological Society of London some fifteen years ago, which was then classed as a doubtful tuberculous process. The eruption was on the arms, and five years afterwards she came with typical lupus vulgaris, a sequel which may be regarded as a corroboration of the earlier diagnosis. The condition in the present case is evidently tuberculous.

Dr. PERNET: I cannot conceive of any erythema multiforme leaving scars of this kind. I am inclined to the opinion that the condition is of a tuberculous nature. Some forms of cutaneous tuberculosis may clear up spontaneously, to some extent at any rate. It is observed in lupus vulgaris, which may partially clear up and leave scar tissue behind. Material from the elbow lesions should be injected into a guinea-pig as a test.

Dr. PRINGLE: As I expressed a rather definite opinion upon this case in the other room, I feel bound to repeat it here. It was to the effect that this is a tuberculous affection. To my mind, this patient presents several well-defined different types of tuberculous manifestation in the skin; he has verrucose tuberculosis on the knuckles, elbows, knees, and points of pressure; he has typical papulo-necrotic lesions of "acnitis" type upon the forearms; and, although I have not had the chance of observing him very closely, I think he has tuberculides of the angio-keratoma type on the backs of the hands. With regard to the scars present, there is certainly nothing about them incompatible with the diagnosis of tuberculosis. I could show two cases at the present moment with distinct and emphatic tuberculosis of the skin, presenting absolutely similar scarring. I should like further to emphasize the very important point laid down by Dr. Sequeira, that tuberculous processes of the skin, even of intense severity, tend to spontaneous healing when the patient is kept at rest in bed and mild antiseptics applied. This man has admittedly been off work, presumably resting, many times during the last eight years. Those points establish, in my own mind, a fairly firm diagnosis of multiple tuberculous manifestations on the skin. I cannot conceive that any mere erythematous process would leave scarring of this depth and intensity, although I am familiar with the disease to which Dr. Gray has referred, which presents so close a resemblance in some respects to skin tuberculosis.



Dr. F. PARKES WEBER: I think the cutaneous affection is probably tuberculous, due to the presence of tubercle bacilli in the cutaneous lesions. Like Dr. Pernet, I would suggest that the Lister Institute be asked to inoculate some material from the lesions into guinea-pigs to see whether the tuberculous nature of the lesions can be thus proved. In that way the Lister Institute recently succeeded in demonstrating the tuberculous nature of a hospital case under my care, sent to the hospital as a case of "chronic pyæmia," but which was really one of the nature of multiple "tuberculous gummata." I suppose that cutaneous tuberculous lesions like the present one (if it really turns out to be tuberculous) bear a relationship to subcutaneous chronic tuberculous abscesses ("cold abscesses") and "tuberculous gummata," analogous to that which tertiary syphilitic lesions of the skin bear to ordinary subcutaneous gummata.

The PRESIDENT: The predominant opinion is certainly in the direction of tuberculosis, and I agree with it. I think that at the moment we lack confirmatory evidence, and it is to be hoped that Dr. Sibley will give us the result of his further investigation of the case. Perhaps he will make a biopsy. The remarks which have been made clearly show the importance of a case of this kind, and are helpful in many directions, one in particular, as indicated by Dr. Sequeira and confirmed by Dr. Pringle—viz., that tuberculous manifestations will, under certain conditions, heal spontaneously, without treatment.

Dr. SIBLEY (in reply): I am much obliged for the remarks which have been made, and I will do what I can to have a further investigation carried out.

(December 16, 1915.)

### **Sclerodermia occurring in a Case of Myxœdema while under Thyroid Treatment.**

By J. H. SEQUEIRA, M.D.

A. M., A MARRIED woman, came under my care at the London Hospital on October 25, 1900. She was then aged 42. She had six children, who are in good health, and one miscarriage. She had been in failing health for nearly two years before I saw her, and had been admitted into a provincial cottage hospital as possibly suffering from cancer of the liver. She came to the Skin Department of the London Hospital on account of a dry eczematous condition of the hands. It was immediately obvious that she was suffering from myxœdema. The skin was dry, the hair had been falling out freely and was very thin, there was a pink flush on each cheek and the rest of the skin had

a characteristic waxen appearance. The lips were thickened and the patient complained of feeling cold. She was rather deaf and her speech was slow. She stated that she had not perspired for a long time. On examination, the thyroid gland could not be felt, the fingers, on palpating, coming directly down on the trachea.

Progress: This patient has attended my department for the past fifteen years and throughout has taken thyroid extract. At first I began with a very small dose and increased it until she complained of flushing, palpitation, &c.—i.e., until I had demonstrated the reaction to thyroid. I then diminished the dose until I could keep the symptoms entirely under control, and for years the patient has taken 1 gr. of the thyroid extract three times a day. In passing, it is interesting to note that she shows a remarkable sensitiveness to the drug. During the long period she had been under my care she has twice complained of the tablets doing her no good, and on inquiry each time I have found that a temporary change has taken place in the source of supply of the drug. The patient has enjoyed good health, and her skin gave no trouble until about a year ago. She has complained from time to time of "rheumatic pains," in the lower extremities chiefly.

Present condition: The patient is now aged 56. She shows no sign of myxœdema. She is rather fresh-coloured, brisk in her movements and her speech is not affected. There is no evidence of pulmonary, cardiac, or hepatic disease. The bowels act only with the use of saline aperients. The urine is of normal character. The Wassermann reaction is negative.

Sclerodermia: For a great part of the past year the patient has noticed that the skin on the lower half of both legs has been very hard, and she has had "rheumatic" pains in those parts. An area of sclerodermia extends from below the middle of each leg to just above the instep. The skin is rather more yellow than the rest of the integument; it is very tough, feels like hide, and cannot be pinched up. There are no telangiectases, but below the sclerosed areas there is on each side of both feet a very large plexus of dilated veins, which I presume to be due to pressure of the sclerodermia on the venous trunks of the leg. The area affected on the right leg is more extensive than that on the left, but in both it forms a band which envelops the limb.

The obvious interest of this case is the development of sclerodermia in a patient suffering from myxœdema, a disease which has been controlled for many years by thyroid extract.

It is difficult here to account for the sclerodermia. Graves's disease may be associated with sclerodermia, and myxœdema may occur with sclerodermia. Here we have myxœdema which has been controlled for fifteen years by thyroid, and yet the patient develops a condition for which thyroid is strongly advocated as a remedy. Whitehouse published a paper in which he said he found a positive Wassermann reaction in four out of five cases of sclerodermia. I had the blood tested here, but it gave a negative reaction.

## DISCUSSION.

Dr. F. PARKES WEBER: Might not the rarity of cases of this kind be used equally well as an argument that sclerodermia is not directly dependent on any disease or alteration in functional activity of the thyroid gland? There may, of course, be some indirect relationship between thyroidal activity and the various forms of sclerodermia.

Dr. SEQUEIRA: In reply to Dr. Parkes Weber, there are on record cases of sclerodermia in which the thyroid gland has been completely atrophied, with symptoms of myxœdema. I can give the references to them, if desired.

(December 16, 1915.)

**Recurrent Herpes Zoster of the Face with Hemiatrophy.**

By WILFRED TROTTER, F.R.C.S.

(Shown by Dr. A. M. H. GRAY.)

THE patient is a girl, aged 11, and gives a history of repeated attacks of herpes on the right side of the face since infancy. The attacks began when she was aged 1, and have recurred every two or three months since; each attack is preceded by headache and fever, but local pain is not a marked feature. All attacks previous to the present one have involved only the second division of the fifth cranial nerve, the lesions being limited to the centre of the right cheek and right side of the nose. In this present attack, however, the lesions present are two in number, distributed over the areas of the buccal and submental branches of the third division of the fifth nerve. Two other points are of interest: firstly, very marked pitted scarring is present at the site of previous lesions; and secondly, there is marked atrophy of the cheek on the affected side. There are no appreciable sensory changes in the skin of the face.

The child has had measles, chicken-pox and scarlet fever, but otherwise has been in good health. There is no history of tuberculosis or syphilis in the family. The mother has had thirteen other children who are all alive and well, and has had no miscarriages. There is no evidence of any disease—no enlargement of tonsils, no adenoids, and no ear trouble; the teeth and mouth are in good condition and the child has never had any trouble with the eyes.

Cases of this type are of interest from two points of view: firstly as to ætiology, and secondly as to nomenclature. In this case the evidence appears to point to the lesions being in the region of the Gasserian ganglion and of an inflammatory nature. As regards nomenclature, I believe the case to be a true zoster and that it differs entirely from the cases of recurrent, irregularly bilateral herpes which occur around the mouth. The case was sent to Mr. Trotter as possibly due to some intracranial trouble; he would be glad if members could suggest any treatment which would stop these recurrent attacks.

#### DISCUSSION.

Dr. J. H. SEQUEIRA: I think this type of case is of great interest, and it is undoubtedly a source of great worry to dermatologists. I presume we must all have had cases of recurrent herpes from some unexplained cause, and I would like to mention two points which have occurred in my experience. In one case the small son of a medical friend had frequent recurrent attacks of herpes about the middle of the cheek, as in this child's case, and I was asked to see him. I prescribed simple remedies, but I could not suggest a cause for the recurrences. The affection was said to have been started by the child having been scratched on that part of the face. Apparently the slight trauma sensitized the area. Ultimately, the child had his tonsils and adenoids removed, and though that is two years' ago, there has been no further attack of herpes. I therefore have cases of recurrent herpes sent to the ear and throat department with the view of having the nasopharynx examined, and I also inspect the teeth for evidence of peripheral irritation. I think peripheral irritation is as important as a ganglionic lesion. In support of that opinion, I may mention the fact that I had a patient suffering from a small rodent ulcer on the lower part of the lobule of the left ear. Each time that patient had radium applied, she developed a patch of herpes over the mental area on the same side. As to the Gasserian ganglion being affected, I think it is on record that hæmorrhage has been found there in herpes associated with pneumonia.

Dr. GRAHAM LITTLE: I do not know whether Dr. Sequeira thinks with me that this is febrile herpes. I do not think it is herpes zoster. Recurrences in true herpes zoster are very rare. With regard to herpes



simplex, one has had a certain number of cases of what must be called herpes simplex, recurring from time to time in the same positions; Dr. Adamson has also reported some instances. One case of mine is extremely interesting—in a man who has had recurrent herpes simplex on the back of the right wrist, which has occurred again and again in the same position for ten years. This restricted localization I do not think is against herpes simplex, but the fact of recurrences is very much against herpes zoster, and I think the history in the present case is against it also. The pain here does not seem sufficiently severe; it is more a febrile attack than a painful one. Herpes zoster involving the fifth nerve is almost always an exceedingly painful condition.

Dr. PERNET: The difficulty in these cases is to know whether one is dealing with herpes zoster or herpes simplex (febrilis), and I agree that this is probably not true herpes zoster. Many years ago I published a case in the *Transactions of the Dermatological Society of Great Britain and Ireland*.<sup>1</sup> In that patient I carefully observed each attack. The lady had three attacks of herpes at intervals. It was a unilateral condition—one large group. I came to the conclusion then that it was a true recurrent herpes zoster; it seemed to fit in with all the clinical characters of that disease. But the point I want to bring out is that I thought a peripheral cause might be a factor, and I suggested that the eyes, which had never been examined, should be investigated. The report was that she had uncorrected astigmatism. She was given suitable glasses, and since then she has had no more facial herpes, a fact which seems to point to the case having been one of herpes simplex. Howbeit, up to the present time there have been no more recurrences.

Dr. PRINGLE: I think that a very important and, indeed, essential feature of this case has been somewhat neglected in the debate—namely, the marked atrophy of the parts. I pretend to only slight knowledge on such a subject, but I think that the atrophy strongly points to the probability of some disease of the Gasserian ganglion being the origin of the visible disease, to the exclusion of a peripheral cause.

Dr. F. PARKES WEBER: In the present case, unless we think there is genuine muscular atrophy, which I regard as doubtful, there seems to be no nervous complication of the herpetic eruption. If, however, in a future attack the patient were to develop facial paralysis or local anæsthesia one would then be bound to regard the case as being an example of “ganglionic herpes zoster.”

Dr. ADAMSON: I think the scarring left by the herpes is in favour of it being herpes zoster, although herpes zoster very rarely involves the second division of the fifth nerve. I do not think that such scarring occurs after herpes recurrens.

<sup>1</sup> Pernet, *Brit. Journ. Derm.*, 1897, ix, p. 151; *Trans. Derm. Soc. Great Brit. and Irel.*, 1897, iii, p. 101.



THE PRESIDENT : From the writings of Dr. Head, who published, in the seventh volume of Clifford Allbutt and Rolleston's "System of Medicine,"<sup>1</sup> some special observations on herpes, I think it is likely he would include this case as a zoster, at any rate, herpes ophthalmicus is counted by him as a zoster. In the twenty-two cases which Head published of the trigeminal kind, he noticed that the greatest frequency was in the first division of the ophthalmic—namely, eighteen cases—while of that involving the inferior and superior maxillary there were only two each. Therefore it is clear that the development in this case is most unusual.

DR. EDDOWES : I suggest a very careful examination of the teeth in the upper and lower jaw, and of the jaw itself, and the ear on that side.<sup>2</sup> Some years ago I treated a young lady, who apparently had incurable neuralgia on the right side of the face, and some inspissated wax in the right ear. I removed it, and her neuralgia was cured. Apparently slight causes of irritation in the ear will sometimes cause much disturbance. I was glad to hear Dr. Sequeira mention the curious effect of radium applied to his patient's ear. The observation was valuable.

DR. GRAY (replying on behalf of Mr. Trotter) : The discussion does not seem to have included one of the important points—namely, as to the difference between herpes zoster and herpes febrilis. If one says that herpes febrilis is a herpes which recurs, whereas herpes zoster is a herpes which does not recur, the matter is simple; but I do not think that is the way in which one should define these cases. I look upon herpes zoster as a herpes which is produced as the result of a nerve lesion, whether in the ganglion or in a peripheral nerve, or, possibly, in one of the tracts of the spinal cord. I think cases of herpes simplex are probably due to local peripheral irritation of nerve-endings; there is, as a rule, no restriction to nerve areas, and when they recur they tend to do so more or less irregularly. The cases of herpes of the buttock which Dr. Adamson collected I should regard as herpes zoster. Herpes about the mouth, preputial herpes, and possibly aphthous ulceration of the mouth, I should put in the latter group.

(December 16, 1915.)

### Case of Morphæo-sclerodermia (shown before<sup>3</sup>) with recent Acute Symmetrical Involvement of the Soles of the Feet.

By GEORGE PERNET, M.D.

A GIRL, aged 14, who was first seen at the West London Hospital on July 31, 1914, for morphæic manifestations on the outer side of the

<sup>1</sup> Allbutt and Rolleston, "System of Medicine," 1910, vii, pp. 470-492.

<sup>2</sup> See end of report of meeting, p. 50.

<sup>3</sup> *Brit. Journ. Derm.*, 1915, xxvii, p. 286; *Proc. Roy. Soc. Med.*, 1915, viii (Derm. Sect.), p. 236.

right thigh, quite typical, but with a very diffuse lilac discoloration extending for some distance around (instead of the usual ring). Thickening followed, and this sclerodermia has progressed on the limbs in a symmetrical manner. This was demonstrated at the time the patient was shown before the Section on the first occasion. She is now brought forward again on account of an acute symmetrical involvement of both soles, which was observed on November 12 last. The



To illustrate Dr. Pernet's case of involvement of soles in morphæo-sclerodermia.

affected area shown in the photograph were at that time pale and somewhat swollen and œdematous, with a well-defined purple-lilac border. They were very tender and painful to pressure. There was some hyperidrosis of the feet. Since then the pain on pressure has subsided to a great extent, and so has the swollen condition, giving place to a certain amount of toughness of the parts.

I consider the condition of the soles is part and parcel of the

morphæo-sclerodermic process, but I would like to hear the opinion of members on this point. If I am correct in my opinion, then the case is a very unusual one. The patient has been on small doses of thyroid on and off since she was first seen. But the sclerodermia has advanced notwithstanding.

#### DISCUSSION.

The PRESIDENT: I do not see any connexion between the condition on the soles of the feet and that on other parts of the body. I think it is accidental. The patient has flat-feet and hyperidrosis, and also callosities, which may account for the pain. I think there is a closer relation between the condition of the feet and the boots she wears.

Dr. ADAMSON: There is a good deal of leather-like thickening of the patches on the soles, and they have the sharp edge suggestive of sclerodermia. I think the patches on the soles are probably an early state of sclerodermia.

Dr. PRINGLE: I am inclined to share Dr. Adamson's opinion. Seeing such a condition by itself, one would have classified it without hesitation as an example of Besnier's keratoderma symmetrica or of Brooke's erythema keratosa, but the onset of sclerodermia is sometimes so bizarre that the possibility of the condition of the soles in this case being of that nature forces itself upon one's consideration. I asked whether the patient has been taking arsenic as that is a frequent cause of a somewhat similar condition, but apparently she has not. I hope the further progress of the case will be reported.

Dr. PERNET (in reply): The affected portions of the soles have become harder the last week or two. I will try to keep her under observation and show her again if possible. She has had no arsenic.

(December 16, 1915.)

#### Case of Actinomycosis.

By W. KNOWSLEY SIBLEY, M.D.

THE patient, W. J. T., a restaurant caterer, aged 37, was sent to me by Dr. J. Bremner. He was stated to have had syphilis ten years ago, for which he was under treatment for two years. The patient spent a fortnight in September last at a house next a farmyard, at the foot of the Pentland Hills, Midlothian. He spent some time in the hayfields, but he never handled any of the crops, nor had he anything to do with the cattle. He first saw his doctor in London on October 20,

on account of a painless swelling that commenced on the inside, and was then showing on the outside of the right cheek. The doctor removed two decayed stumps which seemed to have some connexion with the swelling.

He presented himself at the hospital on November 25, with a somewhat extensive irregular infiltration through the whole substance of the cheek, with a swelling the size of a walnut, which was felt and seen inside the mouth and a nodular pustular eruption on the outside. A thick creamy pus was exuding from three or four points, which was found under the microscope to contain the ray fungus. The Wassermann reaction was negative.

#### DISCUSSION.

Dr. PRINGLE: I think that big doses of iodide of potassium would cure this case, but I believe that nothing less than a drachm a day would have a curative effect. I do not attempt to explain the *modus operandi*, but it is a widely accepted fact that big doses of iodides have a different effect from small ones, and that large doses are frequently better tolerated than small ones.

I have an interesting experience of this disease which I have never before put on record:<sup>1</sup> I was called in consultation in September, 1898, by Dr. Tyson, of Folkestone, to see a gentleman, aged 59, who was suffering from ulcers of undetermined nature round the anus and on his face and right ear. Those near the anus had appeared in April of that year. These had been unsuccessfully treated by eminent surgeons with mercurials as being syphilitic. Subsequently a considerable portion of the lower end of the rectum and surrounding skin had been excised, and the condition was reported after microscopic examination to be indefinitely epitheliomatous. The lesions on the face and ear developed early in July, and, I have no doubt, were conveyed from the anal region by "picking," as the patient was difficult to control. When I saw him the ulcers about the anus had almost completely healed or had been removed by operation, but there was present on the centre of the chin a soft, boggy swelling as large as a walnut, the middle portion of which had broken down, leaving a deep crateriform ulcer, the base of which was bathed in bright sulphur-yellow granular pus, an impromptu microscopic examination of which at once revealed beautifully typical ray fungus. The pus from a similar ulcer on the antitragus of the right ear was of identical nature. The history of the case subsequently given by the patient was peculiar and interesting. He was a hunting man, and on one occasion late in the preceding season he had been attacked in the middle of a run with diarrhoea, and, after obtaining relief in a farm-yard, he had cleansed himself as best he could with a wisp of straw, shortly after which his trouble started round the anus.

<sup>1</sup> The speaker refreshed his memory subsequent to the meeting by reference to his private case-book.—J. J. P.



I do not think that there can be any doubt as to the cause and effect of this remarkable case. I may add that the patient recovered completely in about a year's time under iodide treatment, although it had frequently to be interrupted.

I think, therefore, that I have some reason for my belief that the origin of Dr. Sibley's case is connected with the patient's visit to a country farm, although many links are wanting in the chain of evidence.

Dr. EDDOWES: I had a case very similar to this, and managed to cure it very quickly. I made a strong solution of iodoform in sterilized vaseline, and injected it into all the sinuses, and I got the case well in a short time, without the internal administration of large doses of iodide of potassium.

The PRESIDENT: A one per cent. solution of iodide of potassium is sometimes injected into the part, in addition to the general treatment, with very good result.

(December 16, 1915.)

## Microscopical Specimen for Diagnosis.

By ALFRED EDDOWES, M.D.

A SHORT time ago a boy was brought to me with a small growth on his thigh about  $\frac{3}{4}$  in. long. It was spindle-shaped, running in the direction of the limb. In the dull light of the out-patient room I thought it was lupus vulgaris, but when I pressed it with a watch-glass there were no apple-jelly nodules to be seen. It was said to have been growing for two years, so I advised its removal. I took it out with a wide margin. It has been examined by two pathologists, but they are not sure as to the nature of it. There is a suspicion that it may be malignant. There was no glandular enlargement, and it has not the ordinary structure of lupus vulgaris.

### DISCUSSION.

Dr. ADAMSON, Dr. SEQUEIRA, Mr. McDONAGH, and Dr. EDDOWES (in reply) agreed that the specimen was a mole.

### ADDENDUM (to p. 46).

Dr. Eddowes wishes to add that in referring to the importance of examining the teeth, what he mainly had in mind was the possible disturbance caused by the growth, development, and eruption of the teeth. He recalls four interesting cases: (1) a case of odontoma due to the cutting of a wisdom tooth; (2) that of a well-known Scottish athlete, whose general health was much upset during the cutting of his wisdom teeth; (3) the son of a physician, who suffered recurrent attacks of erythema over the point of exit of the mental nerve, due to the cutting of the lower wisdom tooth on the same side; (4) scleroderma of the neck and slight arrest of development of the face, associated with chronic tooth trouble on the same side.



## Dermatological Section.

President—Dr. J. H. STOWERS.

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(January 20, 1916.)

### Three Cases of Syphilis treated by "Intramine" (Di-ortho-amino-thio-benzene).

By J. E. R. McDONAGH, F.R.C.S., and H. SPENCE, M.D.

#### CASE I.

H. P., AGED 70, contracted syphilis in 1866, and has complained of a bad tongue for twenty-five years. The patient came to the hospital first eighteen months ago, when he had two intravenous injections of "606," which caused little or no improvement in the tongue condition. The salvarsan was followed by three courses of mercurial injections and iodides, which likewise produced no improvement.

On November 6, 1915, the patient was given 1 grm. intramine intramuscularly. At this time he exhibited an ulcer, the size of a sixpence, on the dorsum of the tongue, with two fissures running laterally from it. Behind the ulcer there was a wart about the size of a small cherry. The movements of the tongue were restricted, and the patient suffered considerable pain and discomfort.

On the third day after the administration of the intramine the tongue began to improve, and when he was seen again a fortnight later the ulcer had closed, the fissures had disappeared, and the wart was considerably smaller. At the present time (two months after the injection) the tongue has become soft and perfectly mobile, the wart has nearly disappeared, and the patient suffers no pain or discomfort, as he says: "The tongue has never been so well for years."

## CASE II.

J. W., aged 42, contracted syphilis in 1898, and appears to have had intermittent treatment during the following four years. The patient came to the hospital on December 20, 1915, and gave a history of recurring inflammation of the lower and central part of the face, that first appeared six years previously, and which would disappear under treatment after longer or shorter periods.

Upon admission, the upper lip and the central half of the beard region were dull red and inflamed, where now scarred, and in places exulcerated; some bullous lesions were found on the right inner thigh, and a chronic laryngitis rendered him exceedingly hoarse.

On December 22 he was given 0.4 gm. galy, and on December 24 1.5 gm. intramine intramuscularly. Six days later a considerable improvement in the condition of the face and also of the thigh was apparent, the redness of the face being distinctly lessened and the exulcerated lesions smaller or closed. On the eighth day all lesions were closed with the exception of a nummular area on the central part of the upper lip, which has now (January 20) been healed a week. He has had two further injections of galy, each 0.4 gm., on January 4 and January 11 respectively.

## CASE III.

D. G., aged 28, entered the hospital on November 27, 1915, and is obviously a congenital syphilitic. The patient exhibits all the classical signs: the adenoid facies, flattened bridge, low brow, high-arched V-shaped palate, chronic interstitial keratitis, together with partial destruction of the tongue, which is more or less fixed and cannot be protruded. He is very deaf, speech is nasal, and there is chronic disease of the nasopharynx, and an odorous discharge, which was particularly marked upon admission to the wards. It is interesting to note that he furnishes a history of exposure in the latter part of August, 1915, followed in four or five weeks by a "white pimple," which he scratched, the ulceration then spreading around the sulcus to the extent of one-quarter the circumference of the penis. Notwithstanding this circumstantial account the penile lesion upon admission exhibited the characters of, and is doubtless properly described as, a recurrent syphilide.

On November 30, 0.35 gm. galy was given, and on December 2 2 gm. intramine. Two days later the patient developed acute lobar

pneumonia, the crisis occurring on the third day of the disease. Meanwhile the lesion in the sulcus had greatly improved, and upon discharge from hospital (December 29) had practically closed.

REMARKS BY MR. J. E. R. McDONAGH.

The cases which Dr. Spence and I have brought up to-day have been exhibited, not so much with the idea of showing that a drug, not containing arsenic, has, if properly used, a stronger antisyphilitic action than salvarsan, but mainly to show what can be done in the way of finding new remedies for syphilis, once the rationale of chemo-therapy is understood.

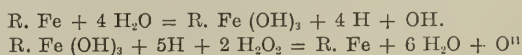
It was only by going from A to Z, or by dint of exhaustive perseverance without any logical guidance, that Ehrlich finally succeeded in discovering salvarsan. The reason why Ehrlich worked with arsenic was simply due to the fact that this drug had, as far as was known at that time, the greatest therapeutic action in sleeping sickness, for it will be remembered that it was to combat this disease that Ehrlich's work was primarily inaugurated. Ehrlich considered that salvarsan was parasitotropic and not organotropic, and that the arsenic was the toxophore group.

From my own researches it is perfectly clear that the arsenic in the salvarsan molecule does not attack the *Leucocytozoon syphilidis* directly, with the exception of the adult male phase and the female phase after impregnation, and these phases it is able to attack directly, owing to the free hydroxyl groups in their lipid-globulin molecules.

Salvarsan, when administered, becomes attached to the lipid-globulin molecules of the serum and of the plasma cells; therefore the drug is more organotropic than parasitotropic. Its influence is borne on the protozoal lipid-globulin molecules only, owing to the increased size and adsorptive capacity of these molecules.

Salvarsan is adsorbed in virtue of its ortho-amino groups, and when adsorbed, the compound breaks down and liberates the arsenic. In my opinion the arsenic becomes converted into a hydroxide and behaves in this respect like ferric hydroxide, the function of which is to form active oxygen. Active oxygen is required for the physical action of adsorption, which takes place between the lipid-globulin molecules of the serum and of the plasma cells and those of the parasites, with resulting destruction of the latter. This active oxygen is formed

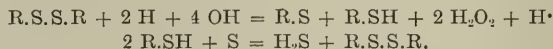
normally in the body by the action of ferric hydroxide, which is called a peroxidase, upon a peroxide, as in this equation:—



The arsenic, then, in salvarsan acts simply as a peroxidase.

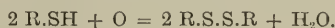
As the disease becomes more chronic the power of the host to form active oxygen directly decreases; consequently he is forced to form it indirectly, which he does by reduction.

As the result of some experimental work I found that metals acted as oxidizing agents and non-metals as reducing agents; therefore it struck me that a non-metallic compound, provided that it was powerfully adsorbed, would increase the reducing action of the lipoid-globulin molecules in recurrent and in late syphilis, as salvarsan increases the oxidizing action in early syphilis. Consequently I made several compounds of sulphur, and the compound which turned out to be the best was di-ortho-amino-thio-benzene, or, as it is commercially called, intramine. After adsorption the sulphur becomes free and acts as a disulphide, as in this equation:—



The disulphide protein produces active hydrogen and the peroxide necessary for the formation of active oxygen in the oxidase system.

From these few remarks you will see that intramine should be prescribed first in recurrent and in late syphilis, and a metallic compound afterwards, while in early syphilis a metallic compound should precede the intramine. In fact, if intramine is prescribed before a metallic compound in early syphilis, it may occasionally aggravate the symptoms. This is due, I think, to the avidity the mercaptan protein (R.SH) has for active oxygen, as seen in this equation:—



To-day I have shown you only cases treated with a non-metallic compound which contains no arsenic. At a future meeting I hope to show you some cases treated with metallic compounds which contain no arsenic. I have already prepared some strongly adsorbed compounds of aluminium and iron, which, I think, will replace the arsenical compounds altogether.

Arsenic is a poisonous metal, while the other metals just mentioned

are not; and when, in conclusion, I state that 12 grm. of intramine can be injected intramuscularly, and 8 grm. or more of aluvine and ferrivine (the new aluminium and iron compounds) can be injected intravenously without producing the slightest toxic symptoms, I think you will agree that the statement made, to the effect that arsenic is not necessary, is a correct one.

#### DISCUSSION.

Dr. G. PERNET: Are local reactions produced in the muscles by intramine? Mr. McDonagh alluded to other metallic substances; some time ago antimony was tried experimentally, and though I do not want to anticipate his work, I suppose he is trying antimony?

The PRESIDENT: I think you will all realize that this is a very important communication on the part of Mr. McDonagh on a subject in which we are deeply interested. We shall look forward with pleasure to hearing him further on the matter in the course of a few weeks. It needs a larger knowledge of bio-chemical problems than I, personally, possess at present to discuss such a subject as this. But two things are clearly needful: one is, that we should understand the principles upon which the most efficient treatment of syphilitic disease are based; and, secondly, we should, as far as possible, obtain for our patients the least toxic agents.

Mr. McDONAGH (in reply): Whether there are local reactions in the muscles or not depends on how the substance is made. The patient may or may not have pain, but if there is pain it is not nearly so acute as that following the injection of salvarsan, or, at any rate, the pain that used to occur on injection in the early days of its use. Intramine is absorbed in four or five days, and it does not form lumps. Antimony is one of the metals with which it is impossible to work, being, as it is, more toxic than salvarsan. The reason it is unsuitable is because its atomic weight is too great. Intramine is on the market, and can be obtained from the British Drug Houses, Ltd.

(January 20, 1916.)

### Case of Very Extensive Tinea Circinata of Tropical Origin

By E. G. GRAHAM LITTLE, M.D.

THE patient is a British soldier who lately arrived in London from West Africa. He has had gonorrhœa but not syphilis, and the Wassermann test is negative. The eruption began in the groin in September last, and has spread extensively until, as at the present time, the



greater part of the body has become affected. The eruption consists of (1) large ringed patches, the largest on the nape of the neck and left side of the cheek and the chin, where the ring is some 5 in. in diameter; and (2) eczematoid surfaces, resulting, no doubt, from the irritation caused by the disease. The abdomen, the scrotum, the thighs from pubes to ankle, the dorsum of the feet, the clefts between the toes, the sides of the chest, the sacral and interscapular regions on the back, the axillæ, the forearms, and backs of the hands, are involved in the eruption. The large ring on the nape spreads into the scalp, but there is no disease of the hair either here or in the pubic region. The nails are nowhere affected. The eruption has varied in severity, and under the mistaken diagnosis of psoriasis he appears to have had some treatment with a chrysarobin ointment, under which it improved, as well also as under applications of iodine. The fungus was identified in scales taken from several parts of the body, and the mycelium was plentiful and coarse. An attempt has been made to grow the fungus on a Petri dish containing Sabouraud's maltose agar medium, but with no success, a common mould alone having been grown after a couple of weeks in the incubator. The exhibitor is obliged to Professor Castellani for some hints for securing growth, and another effort will be made. The man travelled from West Africa, with the eruption fully developed, in company with four others who shared the same cabin, but have not contracted the disease. The fact that the eruption began in the groin and so quickly assumed the eczematoid aspect, and that it nowhere affects the hair, points to the probability that the organism responsible is the *Epidermophyton inguinale*, and if so this case is certainly the most extensive the exhibitor has seen or read of.<sup>1</sup>

(January 20, 1916.)

### Case of Chronic Ulceration in a Boy, shown at the October Meeting.

By E. G. GRAHAM LITTLE, M.D.

THIS patient is shown again to illustrate the remarkable result of treatment by vaccines. A report was also made on the case at the November meeting,<sup>2</sup> when the boy was too ill to be shown. On

<sup>1</sup> Specimens of the fungus in the scales were demonstrated after the meeting.

<sup>2</sup> *Proceedings*, 1915, ix (Derm. Sect.), p. 21.

November 15 the first dose was given of a vaccine derived from his own organisms, grown from the surface, which proved to be chiefly streptococcus and *Bacillus pyocyaneus*; 5 million of the streptococcus and 3 million of the *pyocyaneus* constituted the initial dose, and this was increased to 10 million of both organisms on November 21. This dose was again repeated on November 29. On December 5, 10 million streptococcus and 20 million *pyocyaneus* were given, and the respective doses repeated on December 10, and 10 million streptococcus and 25 million *pyocyaneus* given on December 16. During this time continuous dressings of compresses of sanitas and baths with 5 per cent. saline were used, with a trial for a short time of a treatment consisting of painting the lesions with pure carbolic and swabbing the painted surface immediately after with absolute alcohol. Some improvement resulted from treatment with these vaccines, but no conspicuous success was evident until after December 20. On this date a vaccine obtained from an organism which had been isolated from the blood by Dr. Colebrook on November 25 was first given, when the other excesses were stopped. Some time later films prepared from an unbroken bleb seemed to indicate the presence of the same organism, and this statement was made by the exhibitor, which he now wishes to correct, as further investigation makes it apparent that this identification was erroneous. On December 20, 5 million of the new vaccine obtained from the coliform bacillus isolated from the blood were given,  $7\frac{1}{2}$  million on December 23, and 10 million on December 27. This last dose was repeated on January 3, 10, and 17, and the patient was then discharged, completely free of any disease of the surface of the skin. Improvement was rapid and uninterrupted from the date of the first injection of 10 million of the coliform vaccine, and as there was no synchronous change in any other detail of treatment the improvement is attributable to the vaccine. There is considerable scarring of the affected parts which, it will be remembered, were deeply ulcerated, but with this exception no trace of the disease remains. The temperature from start to finish has never been above 99° F., and has usually been subnormal. The pulse-rate in the earlier stages of observation (the patient has been under the charge of the exhibitor only from November 21—up to that time he was in the Inoculation Department) was usually about 100, but is now settled around the rate of 80. The bowels have always tended to constipation, and at no time has there been any looseness or abdominal pain suggestive of colitis. The fæces, however, have not been examined.

A few days after the report of the exhibition of this case (in October) had appeared in the *Proceedings*, the exhibitor received a letter from Dr. Stacey Wilson, of Birmingham, who mentions that on reading this report he identified the case as similar to a case which had occurred in his own practice several years ago. Dr. Stacey Wilson has been good enough to contribute the following account of this interesting patient.

NOTE BY DR. T. STACEY WILSON.

I regret that the notes of the case I am about to record have been mislaid, and therefore the following description, being from memory, is not so full as could be desired, although it is substantially accurate, as its striking and unusual features were indelibly impressed upon the memory.

The patient, a married woman, aged about 30, was admitted to the Birmingham General Hospital in a septicæmic condition, with a very extensive affection of the skin. Her temperature was moderately high, and she appeared to be dangerously ill. The skin affection varied in different parts of the body. One of the most remarkable types was an inflammatory infiltration of the subcutaneous and cutaneous tissues over the front of the chest, extending from about the second rib down to near the lower end of the sternum, and embracing almost the whole width of the chest. The infiltration was so great that this part of the skin stood some  $1\frac{1}{2}$  in. or more above its proper level. This raised area was of brownish colour, very hard, not œdematous, not specially tender on pressure. There were similar asymmetrical hard thickened areas on the thighs and parts of the back and, I think, on one side of the abdomen.

There was another type of eruption on, I think, the arms and legs, which may possibly have been an earlier stage of the one just described. These showed more distinct congestion, and were purple-red patches which were apt to become vesicular and then ulcerate, as described by Dr. Douglas Heath.

As regards the changes taking place in the above-mentioned infiltrated areas. In some instances—as in the case of the large shield-like area over the front of the chest—there was no definite change (other than a slight increase in its thickness) until resolution began to take place. In other parts of the body, however, a portion of a thickened area would commence to swell more than the rest and then break down and ulcerate. These ulcerated areas were in one or two places as large as the palm of the hand. They did not seem due to a loss of substance, but rather to exuberant, fungating granulations bursting through the epidermis. Subsequently there was a certain amount of loss of substance, causing the ulcer to have a ragged, irregular surface. There was no unusual fœtor noticeable. This process of swelling and ulceration would take place in the course of a few days.

The cause of this eruption was suggested by the appearance of the early stages of an infiltrated area, which was watched carefully throughout its progress. The area affected was the back and radial side of the left wrist, and the adjacent parts of the hand and forearm. In this area congestion was

first noticed with some sense of heat and pain. Then, in a day or so, all over the area small yellow points were noticed, due to small collections of serum, of the size of a pin's head and smaller—i.e., the characteristic appearance seen in cheiropompholyx. Then day by day the swelling increased, and the little collections of serum became less evident. In the course of a few days the area was raised to the extent of 1 in. or more above its normal level, and then the surface began to break down in places, and irregular masses of granulations were formed. This process went on until, at the end of a week or ten days from its commencement, a large part of the area spoken of was covered with ragged suppurating granulations. The appearance of the characteristic "sago grain" collections of serum in the skin (as in cheiropompholyx) pointed strongly to a blockage of the lymph vessels by inflammatory changes within them and to a collection of the serum in the intercellular spaces. This appearance was suggestive of an infective origin, and after several fruitless attempts the *Bacillus coli* was obtained in, I think, pure culture from the subcutaneous tissue.

This case occurred before the days of vaccine treatment for ordinary diseases, and I therefore gave an effective general antiseptic, consisting of sodium sulphocarbolate in 20-gr. doses every two hours, night and day, and 2 gr. of quinine every two hours alternating with it. In this way something over 90 gr., I believe, of pure carbolic acid, as well as the 24 gr. of quinine, were got into the system each twenty-four hours, and could be continued for weeks or months. The only precaution necessary is to watch for the appearance of the muscular weariness on slight exertion which shows the physiological effect of carbolic acid. When this appears the dose must be lessened somewhat. A liberal ordinary diet was given with 6 oz. or 8 oz. of raw meat in addition. Under this general treatment, combined with local antiseptics, fresh areas ceased to become affected and the ulcerations commenced to heal. Later, the thickened areas began to disappear, and in the course of two or three months the patient left the hospital quite well and with but little sign of the widespread and deep skin eruption.

The subsequent history of the case is of interest. A year or two later she came to the hospital again, but suffering from an extensive consolidation of the apex of the left lung. From this she made a good recovery in the course of a year or so. It was diagnosed and treated as phthisis.

A year or two later she again presented herself at the hospital, looking extremely ill. She was in a septicæmic state, and it was found that there was chronic pelvic inflammation too extensive for operation. There were old sinuses discharging pus in the neighbourhood of the rectum, uterus, and bladder. The main organism in the pus was the colon bacillus. The patient was again admitted to the hospital but did not live many weeks. Thus the question arises whether a pelvic inflammation caused by the *Bacillus coli* may not have been the source of the first infection.

With this exception I have been unable to find any analogue to this extraordinary case of mine.



I wish to express my thanks for the courteous assistance I have received from Dr. Colebrook and Dr. Parry Morgan, of the Inoculation Department of St. Mary's Hospital, to whom all the credit of the investigation is due. Dr. Colebrook's report is as follows:—

#### BACTERIOLOGICAL REPORT BY DR. COLEBROOK.

Cultures taken from lesions (ulcerated) always showed many streptococci, usually also *Bacillus pyocyaneus* and staphylococcus. Cultures from unbroken blebs within a day or two of their appearance showed on one occasion staphylococcus and streptococcus, on another occasion sarcinæ and staphylococcus. Blood cultures: No. 1 was positive, yielding a bacillus; Nos. 2 and 3, taken a few days later, were both negative, 10 c.c. of blood being taken on each occasion.

*Re* the positive blood culture: This was performed at a time (November 25) when temperature was subnormal, but fresh lesions had recently appeared. The organism was obtained in pure culture from three of the seven tubes of broth into which the blood (10 c.c.) was divided.

Character of the organism isolated: A bacillus with rounded ends, somewhat pleomorphic, but short forms predominate; some short chains have been seen. It is Gram-negative. Staining is uniform. Growth abundant on ordinary media, colonies being raised and opaque. On potato no brown coloration. Gelatine is liquefied slowly. Inoculation of culture into guinea-pig's peritoneum has given rise to no sign of illness (six weeks).

*Note.*—This organism isolated from the blood has not been found in lesions, as was at first thought to be the case.

#### DISCUSSION.

Dr. PRINGLE: Those who saw this patient when he was previously exhibited must be astonished at the remarkable recovery which has taken place. When the case was first brought to the Section, Dr. Pernet and I thought it might be a case of some form of pemphigus, possibly a "vegetans." I should like to ask Dr. Little whether the stools were also examined bacteriologically. I raise the point because I have recently had occasion to use vaccines made from various abnormal coliform bacilli found in the fæces in somewhat similar conditions, and with a success comparable with that which has attended treatment in this case, upon which I very heartily congratulate Dr. Graham Little.



Dr. DOUGLAS HEATH: As I saw the case which has been referred to by Dr. Graham Little, which was under the care of my colleague Dr. Stacey Wilson, I may say that the skin lesions in that case when I saw it were not so severe as they were in the case under discussion. The eruption in Dr. Wilson's case resembled at first that of erythema multiforme. The affected areas became deeply cyanosed and congested on the limbs, the lesions seeming to start from underneath the skin, and a few of them were bullous and slightly ulcerated on the surface. *Bacillus coli* was obtained from the breaking down areas. I believe there was some bowel trouble as well. I know sulpho-carbolate of soda was given in big doses, but in that case when I saw it there was not the severe breaking down and the offensive odour observed in the present patient.<sup>1</sup> I have had at the hospital one other case of my own, a woman, aged 30, showing purple-red patches on the legs, and after a time bullæ appeared. The hospital bacteriologist obtained from it a coliform bacillus, such as I have not seen before, and cultivations from the bullæ yielded a coliform bacillus. In the bullæ was a creamy white pus. It disappeared readily under ordinary antiseptic treatment; I did not administer a vaccine. Careful sterilization of the skin was carried out, so there was no contamination from without.

The PRESIDENT: I hope we may look forward to a final report on this case.

(January 20, 1916.)

**Note on the Case shown for Diagnosis at the last Meeting,<sup>2</sup> of an Indian Medical Student with an Eruption which it was suggested by several Members was Tuberculous.**

By E. G. GRAHAM LITTLE, M.D.

THE largest tumour, removed from the forehead, has been divided into two parts, and one part used for inoculation into a guinea-pig and the other part submitted to Dr. Spilsbury for report. The guinea-pig has shown no symptoms of inflammation at the site of inoculation, and has now been killed and an autopsy made five weeks after inoculation. Dr. Colebrook reports that there were no signs of tuberculous infection. As the disease, if tuberculous, was clinically of the nature of acute lupus, failure to infect the guinea-pig was, as far as it went, a contra-indication

<sup>1</sup> I have since heard that more severe ulceration took place afterwards.

<sup>2</sup> *Proceedings*, p. 36.

of the diagnosis of tubercle. Dr. Spilsbury reports of the section of the tumour that this is quite unlike tuberculous tissue, and that he conceived the possibility of leprosy, but sections stained for bacilli did not reveal the presence of any organism. The diagnosis at present offered is simply that of a granuloma of unknown causation.

(January 20, 1916.)

### So-called Idiopathic Multiple Pigment Sarcoma of Kaposi (Acro-sarcoma Multiplex Cutaneum Telangiectodes of Unna).

By F. PARKES WEBER, M.D.

THIS patient, who was born in Galicia of Jewish parents, was shown at the Dermatological Society of London on February 8, 1905, and his case was described in the *British Journal of Dermatology* for April, 1905 (xvii, p. 135).<sup>1</sup> At that time he was aged 46, and presented the typical bluish nodules in the skin of the right foot, the left lower extremity, the hands, and the penis. In some of these parts, in addition to the bluish nodules, there were likewise small sessile or pedunculated outgrowths. There was also considerable chronic œdema of the left foot and ankle, such as is present in many cases of the disease. The illness was at that time of about three years' duration, and the prognosis (as was remarked in the description of the case) seemed to be not altogether unfavourable, "considering that the patient's general health is good, that the disease is probably seldom of itself fatal, and that in some cases improvement or spontaneous cure has apparently occurred after the disease has lasted twenty years or more."

The patient was for a time treated with arsenic, and for a time externally with lead lotion, but for the last four years he says he has had no treatment at all. He is now aged 59, and looks well and active. There is moderate œdema of both legs. The left lower extremity is still the more affected of the two. There are brown-red or purple patches on the feet and (much less) on the legs. On the feet and legs there are a few small dark-coloured hard cutaneous nodules, and there are a few

<sup>1</sup> Some notes on the histology of the case were contributed by Dr. J. M. H. MacLeod to the *Brit. Journ. Derm.*, 1905, xvii, p. 173.

on the thighs and glans penis, and there are one or two minute bluish nodules on the hands. There are now not any of the small pedunculated (pendulous) tumours. There is no evidence of any visceral disease, and the urine is free from albumin and sugar.

#### DISCUSSION.

Dr. J. H. SEQUEIRA : Dr. Pringle has asked me to say a word about the patient, also a Galician Jew, whom he first saw in 1888, and who died in the London Hospital from dilated heart secondary to bronchitis and emphysema two or three years ago. At one period the disease was in a grave state, and one leg was amputated, but the other leg cleared up entirely, and at the time of his death there was no sign of tumour.

Mr. T. P. BEDDOES : A case similar to this was under my observation for some years, and is of interest from its duration. It started about twenty-one years ago, under Dr. Wyndham Cottle, the patient continually having pedunculated growths, mostly below the knees. These healed up for a time and under treatment, but recurred and fresh ones formed which ulcerated, and finally the only diagnostic point about which there could be no doubt was one spot on the forearm which was only slightly raised. In the course of twenty years considerable œdema formed about the limbs, with gangrene. I did not see the patient for about two months before the end, but consider that the condition was a good deal associated with age—he was 75 when he died—and that, if it had not been for kidney trouble and heart trouble, his case would have been looked upon as favourable. At no time did the pigmentation disappear, and, during the years I saw him, the pedunculated growths, which ulcerated, were never absent. Members of the Section may remember that I showed a case both here and at the International Congress of Medicine, which was generally looked upon as of a similar nature. But the patient was younger, the growths were more vivid, grew more rapidly, and there was, as in this case, ulceration of the penis.

*(January 20, 1916.)*

### Case of Tertiary Syphilide.

By GORDON WARD, M.D.

THE patient, a male, aged 37, previously a stoker and now a soldier, had a primary sore sixteen years ago, and the present rash appeared at or about the same time. It has never caused any trouble or interfered



FIG. 1.  
Case of tertiary syphilide. Front view.



FIG. 2.  
Case of tertiary syphilide. Back view.

with his work as a stoker. It has been treated on different occasions as a syphilide, but without improvement. The patient states that the eruption is markedly better in winter and worse in summer, and that it has never given rise to irritation. There has been no itching at any time. There is no eosinophilia. The general health is not affected, and there is no history of any change in type of the disease. The patient has had no other serious illness. There is no relevant family history and he has not been abroad.

The lesions are most typical on the back (*see illustration*), and consist of papules with scaly tops at the periphery and pigmented patches more centrally. These papules are, in some cases, surmounted by flat yellow vesicles which dry quickly, leaving scales. Fluid from small bullæ only shows degenerate leucocytes and staphylococci. Scales show many clusters of yeast-like organisms but no mycelium.

This case is shown to elicit answers to the questions: (1) What is the diagnosis? (2) Is this man fit for general military service overseas?

THE PRESIDENT: I do not think any member will differ as to the diagnosis. There is no doubt the case is syphilitic. The point of interest is: what treatment has been applied, and what should now be adopted? Intravenous injections of galy, or the like, are indicated, and intramuscular injections of mercury at intervals subsequently. I have seen several cases lately which have impressed me very much as to the lenient manner in which men are let off military duty for comparatively trivial ailments. I do not think this patient should be unfit in a couple of months to do what is required of him as a soldier.

(January 20, 1916.)

### Case for Diagnosis.

By S. E. DORE, M.D.

THE patient is a male, aged 40. I brought this case at the suggestion of my surgical colleague, Mr. Rock Carling, under whose care he was admitted to Westminster Hospital three days ago. The patient has just come from France, where he was at a military hospital in Rouen. This is one of a number of cases of dermatological interest which are being sent from the Front, and one of the important points



is that, according to his own statement, eight out of seventeen patients in the same ward suffered from the same complaint. The history is that the affection began six weeks ago on the right hand with an eruption of papulo-vesicles which subsequently became bullous, and the dorsal surface is now covered with bullæ and excoriations. A week ago the left hand and elbows and knees became affected in a similar manner, the lesions also becoming bullous, and three days later the eruption began to appear in the form of erythematous patches on the feet. My view of the case is that it is a streptococcic impetigo, but some circinate erythematous patches, with dark centres, on the feet suggest the diagnosis of erythema multiforme.

#### DISCUSSION.

Dr. A. EDDOWES: I suggest that when the history is carefully taken it will show that this is a condition with which we are fairly well acquainted in the out-patient departments of hospitals. A little itching begins between two toes; the condition is neglected; toxins accumulate in company with the organisms themselves, and then extend to other parts, producing a so-called eczema rubrum. I look upon this infection as probably mainly streptococcic, with perhaps some addition of *Staphylococcus albus* infection. *Staphylococcus albus* is often a very vigorous organism, causing blistering. It may be carried from one leg to the other by socks, to the hands by washing, and to projecting parts by drawing on the pants, &c., and takes most effect on the points most subject to pressure and friction.

The PRESIDENT: Dr. Dore will probably give us a further report on this case. The possibility of septic pemphigus is not to be forgotten.

(January 20, 1916.)

#### Sclerodermia with Graves's Disease.

By J. H. SEQUEIRA, M.D.

G. F., FEMALE, aged 22, single, came to the London Hospital on December 21, 1915, on the recommendation of Dr. Smulian. She had suffered from measles and scarlet fever in childhood; she had never had rheumatism. For some years she has had a swelling in the neck, and two months ago she noticed some white patches on the front of the

chest. The thyroid gland is very large, the eyeballs are prominent, there is a fine tremor of the hand and tongue, there is no excessive sweating, but the patient complains of flushing. The pulse-rate has been as high as 128. She states that she has lately been very irritable; micturition is somewhat frequent, and there is some increase in the quantity of the urine passed. The patient sleeps well; the bowels are opened regularly without medicine.

*Sclerodermia*: There is a large oval patch extending obliquely across the left clavicle. Its surface is of ivory whiteness and remarkably smooth. It is surrounded by a lilac margin about a finger's breadth in width. Extending from it towards the middle line are numerous small spots of the same character. There is another area of smaller dimensions over the right side in the middle of the sternum, and numerous macular areas below it. On the back there are two oval patches, one situated at the level of the spine of the left scapula. This is about the size of a five-shilling piece, and there is a somewhat larger patch at a slightly lower level along the base of the right scapula. There are a few macular areas on each hip, just above the trochanters. The *tâche* is obtained very easily, and the patient is anæmic and obviously of a nervous temperament. She suffers frequently from headaches, and the pupils are dilated. The Wassermann reaction is negative. The skin of the arms shows common keratosis follicularis.

The case was shown as a contrast to the myxœdematous patient exhibited at the last meeting; in whom sclerodermia developed while taking thyroid extract.

#### DISCUSSION.

Dr. TRAVERS SMITH: The connexion between Graves's disease and sclerodermia is of great interest to myself. I had under my care a whole family which showed this relationship. The father had sclerodermia, exophthalmos, and some valvular trouble of the heart; his daughter had Raynaud's disease and had lost the ends of several fingers, and had ulcerations in various parts of the body; two children, aged 12 and 8, had enlarged thyroids, some prominence of the eyeballs, and slight tachycardia. They were exhibited at a local medical society in Kilburn. The father died of syncope. He had not ordinary valvular disease, and at times he seemed to be quite free from symptoms, the usual character of which was a rapid and tumultuous action of the heart.

Dr. F. PARKES WEBER: Cases like the present one, which prove the occasional association of sclerodermia and Graves's disease, suggest that, as

I remarked at the last meeting with regard to Dr. Sequeira's case (of sclerodermia and myxœdema), there cannot be any *direct* connexion between myxœdema and sclerodermia; if there is any connexion at all between the last two diseases, it is only an indirect one.

Dr. A. EDDOWES: This is another instance of a long list of cases of sclerodermia beginning over the left clavicle.

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(February 17, 1916.)

### Case of Sclerodermia associated with Graves's Disease, and later Myxœdema, conspicuously benefiting by Implantation of Human Thyroid into the Bone-marrow.

By E. G. GRAHAM LITTLE, M.D.

THE patient is an English lady, aged at present 52, the wife of an Army officer. Her home in Ireland is very damp. In January, 1902, she was under the care of the late Sir William Broadbent and also of Sir Thomas Barlow, and presented symptoms of tachycardia, anæmia, and dilatation of the heart, but there was then no exophthalmos or apparent thyroid affection. About this date also there were severe pains in the fingers and shoulders, suggesting a neuritis. She was seen again in 1907 by Sir Thomas Barlow, who has a note in January, 1907, that the thyroid was normal, but the other symptoms pointed to a "half-developed exophthalmic goitre." The hands are stated by the husband, who keeps a careful diary of his wife's condition, to have been "puffed, stiffened, and dead." About this time, too, Sir Thomas Barlow noted brawny swellings below the lower eyelids, variable, feeling hot, and somewhat resembling a chronic urticaria, "such as is found in people with a gouty diathesis. It was certainly not a typical myxœdema." There was a good deal of pigmentation as well. In January, 1908, she had a bad attack of "influenza." In March, 1908, she was seen by Professor Ashley Cummins, of Cork, who made a definite diagnosis of myxœdema and administered thyroid by the mouth in the form of dried

extract. A little later in the same year she was seen by Sir Thomas Barlow, whose note in October is to the effect that the thyroid was enlarged, especially on the right side, the heart was "murmurish," the pulse-rate 120, and there was much anæmia. Periods of irregular pyrexia with "urticarial" swellings, as noted before, and very severe pains in the shoulders and neck, rheumatic affection of tissues round finger-joints, elbows and shoulders (fibrositis ?) are also now recorded.

These pains and affections of the joints rendered her incapable of walking, and she was carried in a chair. The symptoms were always worse in damp weather. There was no deformity or overgrowth of bone. In January and February, 1909, she was again under Sir Thomas Barlow's care for a very curious parotitis, first of one side and then of the other. There was apparently a slight block in the duct, and the gland became swollen and tender, especially under the stimulus of savoury cooking. There was always some fever with these attacks lasting a few days, not more than a week; there was never any suppuration, but there were pain and difficulty in moving the jaw. These attacks were frequently repeated during the subsequent eighteen months. The first definite diagnosis of sclerodermia, which was probably foreshadowed in the "neuritic pains of the fingers," was recorded in June, 1910, affecting chiefly the fingers, forearms, face, and thighs. These symptoms became so pronounced that she was unable to dress herself or write, and was practically crippled. She was treated at Droitwich, with perhaps some benefit, and later with sulphur baths at Biskra. Here she made the acquaintance of Professor Kottmann, of Berne, who persuaded her to seek the advice of Professor Kocher, which she did in May, 1911. Sir Thomas Barlow, in a letter dated May, 1911, wrote to Kocher a long summary of the facts detailed above, and commenting especially on the condition of the thyroid, says: "My opinion is that on the whole it has shown some degree of atrophy, but it is not easy to say how far the wasting is proportional to her general wasting [which was very marked]. She has had very considerable anæmia with a certain amount of pigmentation. This at times has been very marked indeed, almost bad enough for a pernicious anæmia. As to treatment, I think it cannot be denied that some benefit has resulted at times from thyroid extract, but it has never been considerable, and I am of opinion that at times it has exhausted her. Various forms of hot bath applications have been beneficial, and massage has at times helped her, but the permanent results of therapeutics have, it must be confessed, been limited indeed. I have never seen any case of pure myxœdema behave



like this." And the mental accompaniments of myxœdema, the lethargy and dullness, were certainly conspicuously absent. The first implantation of thyroid was made by Kocher in Berne, in May, 1911, the thyroid being removed from a case of goitre and immediately transferred to the bone-marrow of the tibia through an incision in its anterior surface. She was seen immediately on her return in June, 1911, by Sir Thomas Barlow, who noted that the wound in the leg was unhealed, that she could walk about, that her colour was better, and the movements more free, but that she could not move her jaw owing to the immobility of the skin of the face. There was no parotitis. The sclerodermia was very much better. In November, 1911, Sir Thomas has the following note: "The isthmus of the thyroid can be felt. Sclerodermia much less; for example, she can pick up a pin, but there is still some tightening of the skin, the palmar fascia is thickened, the extremities are cold, left less than the right."

She was taking, on Kocher's orders, from the date of the operation, small doses of iodothyrene, which seem to have suited her better than thyroid extract. In February, 1912, she made a second visit to Kocher, who performed a second implantation of thyroid, also from a goitrous subject, into the marrow of the other tibia. After the operation she got a chill and developed a right lobar pneumonia and nephritis with scanty urine containing 25 per cent. albumin. On March 22, 1912, Dr. Albert Kocher, son of Professor Kocher, wrote to Sir Thomas Barlow: "As to the condition of the sclerodermia it is difficult to judge as yet whether it is any better. We think she ought to go on with the iodothyrene for the present. We gave her, three times [a day?], 0·05 gr., but we think that she may soon go down to 0·05 gr. once [a day?]." She appears to have been taking this dose almost constantly ever since. Exactly a year later, in February, 1913, she was taken for a third visit to Kocher, who, however, did not operate, but administered "a new drug," according to the husband, which made her very ill with heart attacks, and this administration was stopped after ten days' trial, when iodothyrene was resumed.

In June, 1913, the patient was "very ill," and was seen by Professor Ashley Cummins, who apparently diagnosed a pyorrhœa alveolaris, and ordered vaccine injections, of which she had twelve, with benefit. In September, 1913, she had an attack of herpes zoster, and was seen later by Sir Thomas Barlow, who, in consultation with Mr. Oddy, advised the removal of all her teeth. This, according to the husband's narrative, was followed by a notable improvement in every way in her general

health and appearance, an improvement which has been steadily maintained.

Menstruation ceased in 1908.

Present condition: The patient is anæmic and thin, but active and alert. The areas where sclerodermia was noted have improved to the degree of practical cure of the glossy skin and tightening effect, except on the hands and fingers, where the fingers are partially atrophied, and the skin over them is very shiny and tense, but without contracture, the fingers being sufficiently mobile to allow of her playing the piano. There are on the face very numerous telangiectatic patches about  $\frac{1}{4}$  in. in diameter, which apparently vary in intensity, but never quite disappear. She is also suffering from several corns on the plantar surface which impede walking, which, however, is nevertheless freely undertaken. She expresses herself as incomparably better than before the thyroid implantations, when, as has been mentioned, she had to be carried everywhere in a chair. The skin of the face is supple and otherwise normal, and this is a great relief, as she says complaints had previously been frequent when she was staying at hotels that her corpse-like, rigid face disturbed the other guests.

*Comments.*—The coincidence of sclerodermia with thyroid disorders has long been observed, but great differences of opinion have been expressed as to whether the association has been more frequent with hyperthyroidism (Graves's disease) or with hypothyroidism (myxœdema). In a recent communication to this Section, Dr. Sequeira described a case associated with myxœdema, and in the present case the symptoms of sclerodermia were certainly more pronounced in the stage of diminution of thyroid activity, and seem to have progressed *pari passu* with that.

It is obvious that I owe a special meed of thanks for his very generous help to Sir Thomas Barlow, my venerated colleague for the past ten years in the representation of medical graduates on the Senate of the University of London. He has allowed me to reproduce his notes, and has taken infinite trouble in looking up his records in innumerable case-books over the long period during which this lady has been under his observation. I feel sure the Section would wish to thank him with me for the resulting fullness and interest of this unique record.

## DISCUSSION.

The PRESIDENT: This case is exceptionally interesting, partly because of the combination it presents, and partly because of the long observation of the case by Sir Thomas Barlow. I should like to know what was the vaccine used, and whether any observations have been made on the patient's blood.

Dr. KNOWSLEY SIBLEY: I shall be glad to know what dose of thyroid she was taking before the implantation of thyroid was carried out; also whether she tolerated it well by the mouth?

Dr. F. PARKES WEBER: One of the patient's earliest symptoms was the tendency of the hands, especially the fingers, frequently to become numb, white, and cold. That is the way, I believe, in which the "sclerodactylia form" of sclerodermia most frequently commences. It is later on that the contraction and cicatricial changes occur, which still, to some extent, exist in the present patient, in spite of the great improvement which followed Professor Kocher's implantation of thyroid tissue into the medullary cavity of both tibiae. The patient's thyroid symptoms seem to have been at first of the nature of Graves's disease, and it was probably when the symptoms of Graves's disease had given way and become overshadowed by those of myxœdema that the main improvement in the sclerodactylia set in. This, however, as already stated, to some extent persists—in the stage of contraction.

Dr. GRAHAM LITTLE (in reply): I cannot say what dose of thyroid she had been having, but I will try to find out. I would correct Dr. Parkes Weber and say that the symptoms of sclerodermia became pronounced only after 1908, whereas her first symptoms of Graves's disease, when Sir Thomas Barlow saw her, occurred in 1904. The sclerodermia became more obvious with transformation of the condition into myxœdema. The association seems to be further corroborated by the fact that she improved on the thyroid. In answer to the President, the vaccines were given for the pyorrhœa from which she was suffering, as this was supposed to explain the pernicious anæmia. There is no record of any blood examination, and there is no history of rheumatism.

(February 17, 1916.)

**Case of General Carcinomatous Infiltration of the Skin of Arm  
(Cancer en cuirasse), apparently resulting from an X-ray  
Burn in Treatment of an Indolent Breast Scirrhus.**

By E. G. GRAHAM LITTLE, M.D.

THE patient is a single woman, now aged 52. There is no history of cancer or venereal disease in the family, the members of which are mostly long-lived. The earliest origin seems to have dated from an injury to the left breast by striking it against a chest of drawers, after which accident a small nodule was noted in the breast in June, 1904, and some enlarged glands in the left axilla were first noticed in February, 1905, but may have existed earlier. The tumour in the breast increased very slowly in size during the next nine years until it reached the dimensions of half an orange, but there was no suppuration in it until October, 1913. There was very little pain, and only at rare intervals, until suppuration developed. She was taken to a London hospital and given X-ray treatment, the first application on May 26, 1914, the last on August 24, 1915, forty-four treatments having been given between these dates. Ulceration of the breast had been present seven months before the first application, and during the treatment the tumour lessened in size and the ulceration decreased until, just before the last treatment, it was confined to a narrow, hard ring with a white edge. The left arm had begun to swell a month before the last treatment, but not at all notably, and there was no pain in it. On August 24 the application of the rays appears to have been made by a nurse, and a very severe burn resulted, a blister forming which covered the whole area between the base of the throat and the lower margin of the breast. The skin sloughed off this area, leaving a large raw surface which gradually healed except at the centre, which remains ulcerated over a circinate area about 4 in. in diameter. The breast tissue on the left side has entirely disappeared, and the ulcer is on the level of the chest wall, and superficially resembles a Paget's ulceration in the particular that there are islets of sound epidermis in the ulcerated area. The edge, however, is hard, and at the axillary border merges into a very



hard glandular mass which fills the axilla. The whole of the left arm from shoulder to wrist is swollen, and the skin thickened and reddened, with a sensation to palpation like the rind of a very thick-skinned orange. The arm is painful and immobilized. There is a moderate degree of pitting on deep pressure, but the œdema accounts only for a minor portion of the general swelling. The infiltration has advanced very rapidly apparently since the burn took place, though some swelling was noticed, as has been said, a month before while she was under X-ray treatment. There has, during the last two months, been acute neuralgic pain in the arm which has prevented sleeping, and the patient has lost ground in consequence. Since August, 1914, she has had no treatment beyond the application of hydrogen peroxide, zinc sulphate, and other weak antiseptics.

The case presents points of special interest in the history of a remarkably slowly growing scirrhus of the breast with only very moderate glandular enlargement in the axilla during nine years, the apparent dispersion of the tumour and diminution of the ulceration under moderate X-raying, but at the same time an extension of superficial carcinomatous infiltration, and a very rapid acceleration of this infiltration of the skin and glands after the X-ray burn. A portion of the skin from the extreme limit of the ulcer nearest to the axilla has been histologically examined (the slide has been brought to the meeting), and definite and deeply disseminated carcinomatous infiltration has been demonstrated to be present. This sufficiently disposes of the suggestion that the edge is the edge of a healing X-ray burn, a suggestion which is further negated by the carcinomatous infiltration of the arm. The exhibitor has felt obliged to take the most serious view of the case, and has recommended the removal of the breast, arm, and shoulder-blade, with evacuation of the axillary glands.

The excellent notes are largely due to the brother of the patient, who is chief dispenser to the Children's Hospital, Shadwell.

#### DISCUSSION.

The PRESIDENT: Was there any precancerous condition of the nature of Paget's disease? I fear the graver issue is likely to result, and that the left arm is implicated in the disease. Amputation of it seems to be indicated. I hope we shall have a later report on the case.



Dr. KNOWSLEY SIBLEY : Dr. Little spoke of the very rapid extension of the carcinomatous growth. I do not know whether he considers that the whole of the ulceration which we see is malignant ; I do not think it is so ; the edges are healing, and in many places there is a healthy granulating surface. Though she has had a severe X-ray burn, I am not clear that that has produced a rapid extension of the cancer, either from the history or the present appearance. The œdema of the arm is what generally results in these advanced cases of breast cancer, and the patient says the arm swelling started three weeks before she received the burn. I understand that though the rays were given under the superintendence of a doctor at first, a nurse alone did the administration in the later stages. I do not know from what part of the disease the section was taken for microscopic examination.

Dr. ALFRED EDDOWES : Many years ago I used an old-fashioned remedy for the pain in hopeless cases of malignant disease—namely, the injection of a drop or two of a solution of bromine in alcohol. It completely destroyed a large gland in the neck. So great was the relief due to sloughing and reduced pressure that the patient's friends even hoped for a cure, though I had explained previously to the operation that it could only relieve suffering.

Dr. GRAHAM LITTLE (in reply) : The section shown was taken from the outside edge, and that is where the scirrhus is definite. I regard the swelling of the arm as more than œdema ; it is undoubtedly an extension of the carcinomatous infiltration of the skin, and as the axilla is full of enlarged glands, I have suggested that amputation of the arm may be the best course.

(February 17, 1916.)

### Case for Diagnosis (Tuberculosis).

By W. KNOWSLEY SIBLEY, M.D.

THE patient, D. W., is a stout, healthy-looking, single woman, aged 23, who has five brothers and sisters, all well, and both parents are living. The father is stated to suffer from asthma and bronchitis. When a child, the patient had a large superficial ulceration over the right calf which has left a pale scar, and at about the same time she had an abscess over the region of the left hip. Six years ago the patient suffered from lupus of the left cheek, leg, and ankle, all of which healed up after two years' treatment, leaving marked scarring. She also had some ulceration on the mucous membrane of the left

upper eyelid, which was treated with radium. Three years ago an ulcer appeared between the big and second toe on the left foot, followed by some small hard papules scattered over the dorsum of the foot. About this time the left leg swelled, and has remained considerably œdematous ever since. The surface of the leg up to the knee is cold, but there is no loss of sensation nor any pain or discomfort in walking. A verrucose condition exists in the region of the former ulcer between hallux and second toe, and there are several hard cutaneous nodules, the size of peas, scattered over the dorsum of this foot. The right leg is normal, and there are no obvious visceral lesions to be detected. The von Pirquet reaction was on two occasions distinctly positive, and the Wassermann negative. A section of one of the nodules from the foot shows the structure of a granuloma, with a tendency to giant cell formation.

The case is obviously one of tuberculosis, but I show it in order to elicit opinions as to the nature of the œdema in the leg. I cannot ascertain that there are any visceral lesions, and there is no albumin in the urine.

#### DISCUSSION.

Dr. PRINGLE: The condition of the leg is typical of that described in France many years ago as "tuberculous lymphangitis" with elephantiasis; there are models of several cases in the St. Louis Museum. The nodular lesions are, I have no doubt, lymphatic varices. An excellent case was described and illustrated by Mr. John Cahill in the *British Journal of Dermatology* in 1895,<sup>1</sup> which was annotated by myself. I am not aware of any histological reports on these cases, and therefore I do not know whether the name tuberculous lymphangitis is microscopically justified. My present impression is that cases of this kind, of lesser degree, are not very uncommon; I have had a considerable number under observation. The concomitant phenomena of tuberculosis are seldom so manifest as in Dr. Sibley's case, but their existence only bears out the general tenor of my remarks. My experience is that these cases do just as badly as does the analogous syphilitic elephantiasis.

Dr. F. PARKES WEBER: As there are tuberculous lesions present, I suppose one must regard the case as one of "tuberculous lymphangitis," and commencing "tuberculous elephantiasis." But the difficulty is to be certain that, in addition to the local tuberculous lesions, there is not associated a condition of unilateral, persistent, so-called "idiopathic œdema" or "trophœdema"

<sup>1</sup> *Brit. Journ. Derm.*, 1895, vii, p. 1.

(which is an early stage of some cases of so-called "idiopathic elephantiasis"), independent of the tuberculous lesions in question. Such cases of "idiopathic œdema" in lower extremities may be unilateral or bilateral, and may extend upwards to the knee-joint or to the hip; they are more commonly met with in females than in males, and, as far as I know, cannot be permanently cured by any kind of treatment.

Dr. G. PERNET: I agree with Dr. Pringle. I do not see that this condition has any relation to trophœdema or to idiopathic elephantiasis, whatever that may mean. This patient presents what is analogous to syphilitic elephantiasis.

Dr. EDDOWES: I have had the good fortune to be able to watch a similar kind of case for twenty-five years. Originally my patient had lupus on the end of the nose. By taking away a part of the nose I improved her personal appearance and the disease has not recurred. On the foot the first lesion, in addition to the swelling, was a warty condition between the great and second toes, and I thought that might be the original site of inoculation of tubercle in that leg. The leg became enormously swollen, but by treatment with mercurial ointment a wonderful recovery followed, and the patient now considers herself well. I look upon the prognosis in such cases as decidedly favourable. The particular patient referred to does not require any treatment now, and she is in regular employment. I recommend that in the present case 10 gr. of yellow oxide of mercury to the ounce of paraffin moll. be rubbed over the leg and between the toes, in order to maintain asepsis as well as the absorption of mercury.

Dr. SIBLEY (in reply): This patient has been in bed six weeks, and the leg is now more swollen than at first. I did not apply the radium; I had not seen her until quite recently.

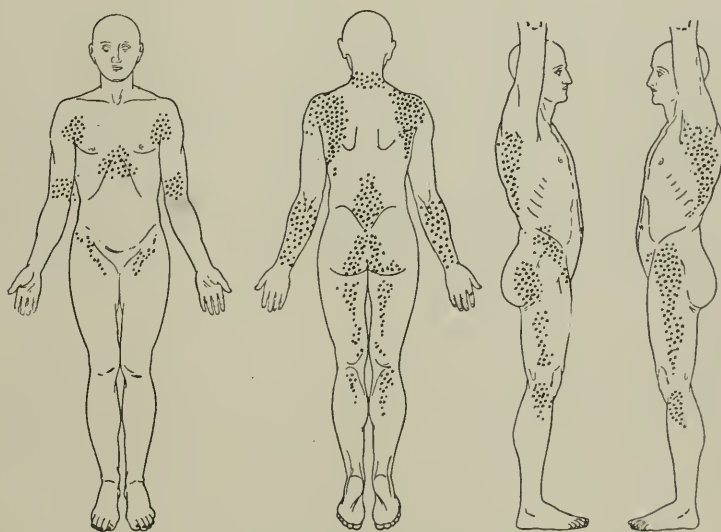
(February 17, 1916.)

### Case of Dermatitis Herpetiformis.

By S. E. DORE, M.D.

THE patient is a rather tall and thin young man, aged 26, who was sent to me by Dr. Cumming Grant. His father died at the age of 30 of inflammation of the lungs; his mother, aged 50, is alive and healthy. He has one sister, aged 29, who suffers from phthisis. His own health has been fairly good, but he has had three attacks of pleurisy, the last attack, five years ago, being followed by empyema. He is a journalist

by occupation and of a somewhat nervous temperament. The skin disease began last April, ten months ago, and he first noticed a few slightly itchy spots on the shoulders and arms, which became much more numerous and spread over large areas of his body towards the end of the year. When I last saw him he presented a characteristic eruption of dermatitis herpetiformis, with grouped papules and vesicles, but as the result of taking increasing doses of Fowler's solution since the beginning of January, the eruption has nearly disappeared, leaving only pigmentation and a few papulo-vesicles in the areas previously



Case of dermatitis herpetiformis.

affected. To me the eruption is striking by reason of its almost perfect bilateral symmetry which has not been a conspicuous feature in the cases I have previously had under my care. It affects the shoulders, the posterior scapular regions, the extensor surfaces of the forearms, the lower part of the back, centre of the chest, lateral and posterior surfaces of the thighs, popliteal spaces, lower part of the buttocks and the intergluteal fold. The glands in the groin are enlarged, and the eruption gives rise to intense itching and burning, principally at night. I shall be glad of suggestions for treatment other than that by arsenic.

## DISCUSSION.

The PRESIDENT: I presume you do not consider that you have yet obtained the fullest benefit from arsenical treatment, and that you propose to continue the administration until the lesions have disappeared? Have you been able to detect evidence of intestinal toxæmia in the patient?

Dr. MACLEOD: Salvarsan has been tried in these cases, but the results have been uncertain.

Dr. ADAMSON: Another treatment is the injection of human blood, which has been employed in cases of pemphigus and of dermatitis herpetiformis. I have not yet used it for dermatitis herpetiformis, but, with Dr. Stansfeld, I employed it in five or six cases of pemphigus, and in two of them we got good results; in a third the patient is improving, though not so clearly, as the result of the injections. In the others there was practically no result. It is worth trying in a case of dermatitis which does not yield to arsenic: 10 to 20 c.c. of blood from a healthy person are injected into the veins or the buttock of the patient, it having been first determined (for intravenous injection) that the two bloods are compatible, for it does not do to inject a blood which will hæmolyse the blood of the patient.

Dr. PRINGLE: This man represents a type of case with which I know Dr. Dore is familiar, the "old-fashioned" type of Duhring's disease, which does yield with some amount of certainty to arsenic; it was a point made by Duhring in his earliest writings on the subject. But the quantity of arsenic administered must be ample; every patient appears to have his own personal dose. Dr. Dore will remember a man who had numerous attacks, and they always yielded rapidly when we got his dosage up to 15 minims of liquor arsenicalis three times a day. Short of that quantity, the drug did not seem to influence the disease in the least. In such cases the drug has to be pushed up to or even somewhat beyond the physiological limit. It would be interesting to find out what doses of arsenic have been taken in this case. I think that the disappearance of itching is often the first manifestation of recovery either temporary or permanent.



(February 17, 1916.)

## Case of Herpes Recurrens of the Right Cheek in a Boy, aged 9.

By H. G. ADAMSON, M.D.

I HAVE brought this case as a good example of that form of herpes recurrens which attacks a large area of one cheek and which has been described particularly by Dubreuilh. It is an illustration of the fact that extensive herpes of one cheek is not necessarily herpes zoster—i.e., herpes corresponding to the second division of the fifth nerve. In this boy the eruption has appeared several times a year since he was aged  $1\frac{1}{2}$  years, so that he has already had twenty-five to thirty attacks. He feels poorly before each attack and has a rise of temperature ( $100^{\circ}$  F. to  $101^{\circ}$  F.) during an attack, but there are no other symptoms. The eruption leaves no scar. I show photographs of extensive recurrent herpes of the cheek, of the ear, of the gluteal region, of the wrist, and on one finger. The ætiology of these cases is obscure. Dr. Sequeira has suggested some local source of irritation which acts reflexly, such as adenoids in the facial cases. This boy has some adenoids which will be removed, and it will be interesting to see whether the eruption ceases as it did in one or more of Dr. Sequeira's cases. But it should be noted that recurrent herpes occurs with various febrile illnesses—pneumonia, diphtheria, scarlet fever, typhoid, &c.—and in such instances reflex irritation does not seem to explain it.

### DISCUSSION.

Dr. GRAHAM LITTLE: I agree with Dr. Adamson that this is herpes simplex, and that that is a different condition from herpes zoster. I am still an adherent of the view which Dr. Head expressed. I regard a recent case of Mr. Trotter's as similar to this present case, and as an instance of herpes simplex, in that, as Dr. Parkes Weber pointed out, there was no wasting of muscles, which would be present if it were a trophic nerve affection. There has been recently a little sporadic and not very authoritative literature as to the connexions between herpes zoster and herpes simplex. A South African writer collected forty cases from the literature of apparent association either in families or schools, but there is always a difficulty about diagnosis. Since I went through this literature I have had a curious case of severe zoster in the

husband of a woman who had suffered from herpes simplex for fifteen years ; she had an attack once in ten days, during one of which the husband came out with severe herpes zoster. Head's observation of maximal points of incidence in herpes zoster is a valuable differentiation in some cases and is never present in herpes simplex.

Dr. ADAMSON (in reply) : I think we may group herpes under three or four headings—namely : herpes zoster, which is due to a specific micro-organism which attacks the posterior root ganglia and protects against a second attack ; arsenical herpes ; and herpes (which may be recurrent) due to pressure on a nerve. All these forms are associated with a definite nerve lesion. Herpes recurrens or herpes febrilis, the real cause of which is obscure, belongs to a class apart, and is distinguished by recurrence of the attacks and by the fact that the eruption does not correspond with a unilateral nerve distribution.

(February 17, 1916.)

### Necrosis of the Skin from handling "Composition" (an Explosive).

By J. M. H. MACLEOD, M.D.

THE patient, a woman, aged 30, a munition worker, has small, painful necrotic lesions on the hands, especially the tips of fingers, which last about a fortnight. She attributes them to handling an explosive composition containing fulminate of mercury, &c. The patient has also a slight iritis from the same cause. Several of the other workers in the same factory are similarly affected.

#### DISCUSSION.

Dr. G. PERNET: I have had one or two instances in people employed in a similar way ; they have come on account of various forms of dermatitis on the fingers and forearms. I will try to bring a case next time.

Dr. F. PARKES WEBER: Have these lesions been noticed only in cold weather? One knows how cold weather may intensify the harmful results of irritating antiseptics on the skin (as used, for instance, in the ordinary way for disinfecting the hands). Perforation of the nasal septum has been reported as a result of working with chromates or chromic acid.

Dr. MACLEOD (in reply): The patient has had to handle the highly explosive powder and to work it, with her fingers, into a paste with methylated spirit and press it into a mould. This procedure is probably the cause of the necrosis.

(February 17, 1916.)

### Case of Multiple Comedones of Frontal Region in a Boy.

By GEORGE PERNET, M.D.

THE patient is a boy, aged 8, with numerous small comedones about the upper part of the forehead, arranged transversely and impinging on the margin of the scalp. They have been present about two and a half months. There is no sign of inflammation or suppuration about the comedones. The mother has lately been rubbing in brilliantine on account of a dry seborrhœa of the scalp. This, combined with rubbing of the cap, has no doubt given rise to the comedones. Although I have seen a number of children in my clinic, the condition is not a common one in my experience. Radcliffe-Crocker<sup>1</sup> was apparently the first to call attention to this class of case in infants and young children.

#### DISCUSSION.

The PRESIDENT: The point of interest is that the lesions do not undergo suppurative action. Such cases are seen here less frequently than formerly, probably for the reason that they are not uncommon.

Dr. PRINGLE: I think the idea of a brilliantine of sorts being responsible for this eruption is probably right; and he probably has used the same cap with the lining unchanged for a long time, which is a common cause of seborrhœic scalp trouble.

<sup>1</sup> See his "Atlas," plate lxxxiii, fig. 5.

(January 20, 1916.)

**Case of Dermatolysis and Molluscum Fibrosum, with  
Congenital Morbus Cordis and Kyphosis.**

By J. H. SEQUEIRA, M.D.

THE patient, an undersized youth, aged 21, who looks more like a boy aged 15, was born in Yorkshire. He presents a remarkable, soft, pendulous growth on the left side of the forehead. The tumour hangs



FIG. 1.

down over the orbit and cheek. The surface in the lower part has the appearance of the adjacent skin, while on the scalp it has a peculiar corrugated character resembling the skin of the scrotum (fig. 1). The left eye was removed some years back for the condition known as

buphthalmos. On the trunk, particularly on the back, are numerous molluscum tumours the size of small nuts. They have a bluish tint, and on pressure can be herniated under the adjacent skin. The patient has congenital kyphosis of the lumbar spine, with compensatory curvations in the dorsal region, well shown in a skiagram exhibited. He has also congenital morbus cordis, the defect being, in the opinion of Dr. Robert Hutchison, at the aortic valve. I propose to ask one of my surgical colleagues to see the lad, with a view to operative treatment of the facial deformity.



FIG. 2.

The frequent association of congenital anomalies was pointed out long ago by Dr. Francis Warner. The head tumour resembles some of the cases of dermatolysis which have been published, while the growths on the trunk are characteristic examples of molluscum fibrosum.

Dr. F. PARKES WEBER: I venture to suggest that the so-called dermatolysis tumour about the patient's left eye belongs really to the class of plexiform neuroma, although I admit that I cannot feel any distinct bands of



plexiform neuroma in it. If I am right, then, in the present case we have the typical trio of von Recklinghausen's disease — namely, plexiform neuroma, molluscum fibrosum, and spots of cutaneous pigmentation on the back and trunk.

P.S.—A third photograph has been taken to show the result of Mr. H. S. Souttar's operation. The orbit was cleared out and the



FIG. 3.

upper part of the growth removed, the dependent skin being brought up, with an admirable cosmetic result. The tumour consisted of fibrous tissue and fat; there was no plexiform neuroma.

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(*March* 16, 1916.)

**Case for Diagnosis. Provisional Diagnosis: Nodular Lupus Erythematosus followed by Atrophy of the Skin and Subcutaneous Tissue.**

By J. M. H. MACLEOD, M.D.

THE patient is a short, spare, woman, aged 48, who presents an irregular atrophic condition of the face affecting both cheeks, but most marked on the right side, and producing pronounced disfigurement. The atrophy has resulted from the involution of pinkish nodules and plaques. On the right side of the face the skin is atrophic and irregularly depressed from the right malar prominence down to the angle of the jaw, while the malar region on the left side is similarly affected.

The general health and nutrition of the patient are good. In her childhood she suffered from tuberculous glands in the neck and axilla on the right side, the scars of which are still noticeable, but she has no signs of active tuberculosis. There is a history also of tubercle in the family, a sister having died of phthisis.

She first came under the observation of the exhibitor at Charing Cross Hospital eight years ago, when over the right malar prominence she presented a reddish, slightly infiltrated plaque about the size of a penny with a well-defined irregular outline, the surface of which was smooth and covered with a network of dilated capillaries. On the left

side of the forehead there was a small reddish nodule with a telangiectatic surface. The diagnosis then made was lupus erythematosus of the nodular variety as described by the late Dr. Radcliffe-Crocker.

The lesions were treated by zinc ionization and disappeared after a few applications, being followed by atrophy involving not only the skin, but also the underlying subcutaneous tissue. The atrophic skin has not the appearances usually associated with the scarring left by lupus erythematosus; there are no follicular pits—indeed, some of the lanugo hairs have remained, and the surface of the skin is smooth and soft. It seems as if the main part of the atrophy has taken place, not in the skin, but in the subcutaneous tissue immediately underlying it.

The diagnosis is exceptionally difficult, but that of nodular lupus erythematosus seems the most probable. The question of syphilis is eliminated by all the facts of the case and by the Wassermann reaction being negative. The behaviour of the lesions and the telangiectases differentiate it from the superficial type of lupus vulgaris. It seems as if the pathological process involves the corium and upper parts of the subcutaneous tissue. Unfortunately a microscopic examination so far has not been possible owing to the difficulties of obtaining a biopsy.

#### DISCUSSION.

Dr. KNOWSLEY SIBLEY: Will not sclerodermia cover the condition? This case reminds me of one I had a few years ago, almost identical in appearance. My patient, however, had the condition well developed when I saw her. She had marked atrophy on the left side only, which gave the impression that her malar process had been removed, but she had never had an operation. She had a punched-out ulcer in the margin of it which was obviously syphilitic. It healed up with antisymphilitic treatment. There had never been ulceration over the rest of the patch. The patch was not produced by ulceration, and so far as could be determined it was preceded by a rash, which had been scraped and X-rayed, after which, as the patient expressed it, "the tissues faded away." It was an asymmetrical case. I have a photograph of it; the condition had been coming on for seven years when I first saw her.

Dr. F. PARKES WEBER: I venture to suggest that this is a case of what I should like to call "double hemiatrophy" of the face, in which the cutaneous inflammatory disease described by Dr. MacLeod acted as an *exciting cause*. Traumatism is now generally admitted as being a possible exciting cause of facial hemiatrophy, and therefore one can easily conceive that other lesions, such as inflammatory skin diseases, may occasionally in predisposed individuals also act as exciting causes. Facial hemiatrophy is doubtless in some way allied to sclerodermia, for there are several cases known

in which hemiatrophy of the face, or of the face and whole body, has been accompanied by sclerodermatous changes (not necessarily limited to the side of the hemiatrophy).

The PRESIDENT: I agree with Dr. MacLeod's view of the case.

Dr. S. E. DORE: I understand that Dr. MacLeod diagnosed the precedent condition as lupus erythematosus. I once had a somewhat similar case in a middle-aged woman in which treatment was followed by deep atrophy of the skin of the same type, and in which the diagnosis of the "lupus erythematosus nodularis" of Radcliffe-Crocker was made. I have seen cases in which a condition analogous to scleroderma has followed lupus erythematosus, but I think this case is not of that kind.

Dr. G. PERNET: I do not remember ever seeing anything quite like this case. As Dr. MacLeod has been observing the case for eight years and is able to exclude morphœa and scleroderma, I think we can clearly exclude those conditions.

Dr. MACLEOD (in reply): There has been no definite scleroderma in this patient, but simply an atrophy in which the skin remained soft and pliable, following the removal of the indurated lesions by treatment.

(March 16, 1916.)

### Case of Staphylococcia in a Xerodermatous Patient.

By GEORGE PERNET, M.D.

THE patient is a girl, aged 6, who first came under observation in October, 1915. She then presented closely aggregated pustules in multiple foci and *en nappe*, which extended at the periphery and involved the scalp, face, neck and limbs. The disease started at the age of 2. Xeroderma is also present. There is inguinal and femoral adenitis.

In addition to local remedies, the treatment has consisted of vaccine injections of stock mixed staphylococcus, gradually increased from one million to 200 million, and internally thyroid from the point of view of the xeroderma. The patient has improved considerably.

Although a case of this kind would be classed under the heading of eczema, I consider it comes into the category of staphylococcia, and

is related to the group of *Pyodermites en nappe et en foyers à progression excentrique* (of Hallopeau and others), and to the condition I have described under the name of "dermatitis pustulosa vegetans recurrens."<sup>1</sup> I have brought forward the case in order to hear the opinions of members of the Section.

#### DISCUSSION.

Dr. S. E. DORE: I do not know why Dr. Pernet objects to the term "eczema" in this case. Xerodermatous subjects frequently suffer from eczema of the extremities, and the excessive vulnerability of their skins also makes them liable to staphylococcus infection.

Dr. H. G. ADAMSON: I regard this as a characteristic case of chronic eczema in a child. Most cases of chronic eczema in children date from infancy, and begin as the well-known infantile eczema of the face particularly. The majority of infantile eczemas get well at or before 2 years of age; but some continue into childhood, and patches occur on the trunk and limbs. Such cases are most obstinate to treatment, because the eczematous patches are kept up by constant scratching, and they often get lichenified and secondarily infected. Dr. Pernet's case seems to me to be a typical eczema of this kind.

Dr. ALFRED EDDOWES: I have a case like this now under my care, and I draw a very marked distinction between it and ordinary eczema in children. I think the xerodermia plays the most important part. Such children are delicate, and need building up in every way. I labelled my case impetiginous eczema, superadded upon xerodermia. No doubt there is infection, but the best treatment is to give thyroid, phosphates, and cod-liver oil, in addition to local measures. Some patches become thick and irritable, and cause bad nights. I have found X-rays of great benefit to these patches.

Dr. PERNET (in reply): I do not agree from the clinical point of view that the case is one of advanced eczema. It began as a staphylococcia. The xerodermia is an additional factor. As a result of treatment the case is very different to what it was when originally seen.

<sup>1</sup> Pernet, *Journ. of Cutan. Dis., &c.*, New York, September, 1912.



(March 16, 1916.)

### Case of Primary Syphilitic Chancre of the Lip.

By GEORGE PERNET, M.D.

THE patient is a widow, aged 32, who was first seen on March 10, 1916, for a sore on the lower lip to the right of the middle line, of one month's duration. It has resisted ordinary treatment, and has become gradually larger and harder—the usual history, by the way. The sore is a typical primary syphilitic chancre. The submaxillary glands are



Primary chancre of the lower lip.

greatly enlarged, and there is also submental adenitis. There is post-sternomastoid and inguinal adenitis, and a typical mottled roseolar rash about the body and limbs. The patient complains of bad nocturnal headaches. Nothing has been found about the vulva. Galyl 0.20 grm. was given intravenously on March 11, 1916, and was followed up by mercury, calomel cream being applied locally.

#### DISCUSSION.

Captain C. H. MILLS: In most of the text-books on syphilis one finds it stated (as a point in the differential diagnosis) that primary sores of the lip are more common on the upper lip, whilst epithelioma is more often seen on

the lower lip. But most of the cases of labial chancre I have seen have been on the lower lip (as in this one), and therefore I consider that the situation is of no diagnostic importance.

Dr. PERNET (in reply): My experience leads me to agree with Captain Mills—the labial chancres I have seen have been more frequently on the lower lip.

(March 16, 1916.)

### Chronic Diffuse Papillomatosis of the Left Foot and Leg.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a married woman, aged 52, separated from her husband. She gives no history of syphilis, and the Wassermann reaction is negative. Her left eye was removed by my colleague, Mr. Paton, in 1907, and his report states that the eye was taken out for a "perforating hypopyon ulcer, of pneumococcal causation. There was no evidence of syphilis." The patient first came under my care in January, 1916, and she then gave a history of having had the condition of the foot for about a twelvemonth or longer. She seems to have had some septic infection of the leg prior to this, whether an abscess or a varicose ulceration it is difficult to determine. The left leg is reddened and swollen, with a brawny œdema, such as is common in varicose ulcer. The lower third of the leg and the whole dorsum of the foot are covered with a thick carapace-like warty envelope; the surface is rugose and dry, and innumerable closely juxtaposed small warts occupy the affected area. There is some blackening of the skin in these parts, probably due to external dirt. The skin is inelastic and stiff, and the sole, though free of warts, is hard and hyperkeratotic. The right foot is somewhat swollen, and the skin covering it is also hardened and hyperkeratotic, but to a much less degree. There is no papillomatosis here. The patient is sallow and thin and has lost much flesh during the last ten years. There is no pigmentation and the mucous membranes are unaffected.

I regarded the case at first as an eczematous hyperkeratosis, started by a varicose ulcer, and this view was supported by the presence of a copious discharge, which speedily dried up by the application of ung. acid. salicylic and oil of cade. A section of the skin was obtained, and on

examination of this I was somewhat struck by its similarity to sections of a case of *acanthosis nigricans* which I once reported.<sup>1</sup> The sections show a pure papillomatosis with great increase in the thickness of the stratum corneum (the section was taken from the upper margin of the carapace on the leg) and of the stratum granulosum, with a certain degree of a mononuclear cellular infiltration of the papillary zone of the corium, but there were no cellular foci suggestive of tuberculosis, and this finding seemed to negative an alternative diagnosis of which I had thought—namely, “a verrucose tuberculosis with elephantiasis.” The absence of pigmentation militates, of course, against the diagnosis of *acanthosis nigricans*, but there are some instances of this rare disease in which pigmentation developed later than in the papillomatosis. The absence of fœtor, or of any but the most moderate degree of sepsis during my observation of the case, did not dispose me to regard the case as a septic papillomatosis, nor am I familiar with this degree of involvement in any locally septic conditions. I am, however, very open to suggestions as to its causation.

#### DISCUSSION.

Dr. MACLEOD: In my opinion the condition is probably one of septic origin. For comparison with it I show a coloured drawing of a case of “*elephantiasis nostras*,” which was under my care at Charing Cross Hospital, in which the foot and ankle were similarly affected. It occurred in a stout, middle-aged woman, with varicose veins, and followed upon an ulcer of the leg. In this case there were recurrent attacks of lymphangitis.

Dr. PERNET: I am familiar with this kind of case, but I think that during the last few years there have been fewer of them seen in hospital work. I regard them as elephantiasis with staphylococcic infection, leading to the warty growth. In tropical medicine these feet have been called “mossy feet.” I do not see any connexion between this condition and *acanthosis nigricans*.

The PRESIDENT: I consider that the case is of septic origin.

Dr. S. E. DORE: It is important to remember that the patient has suffered from varicose veins for many years, and I think such a condition could follow chronic venous congestion, to which is possibly superadded a septic element.

Dr. H. G. ADAMSON: I agree with Dr. Pernet that it is a familiar form of warty growth or “mossy foot” associated with elephantiasis; you may see it with any form of elephantiasis. The diagnosis rests between tuberculous,

<sup>1</sup> *Brit. Journ. Derm.*, 1901, xiii, p. 42; 1902, xiv, p. 26.

syphilitic, or recurrent streptococcic infection. In this case I think it may be syphilitic. There are nodules on the upper part of the leg, and the scars rather suggest syphilis. Has there been any antisiphilitic treatment?

Mr. J. E. R. McDONAGH: When I first saw the case the diagnosis of syphilis entered my mind, but I was aware that septic trouble and tuberculosis might give rise to a similar condition. The microscopical section would fit in with any one of these three diseases. To establish a correct diagnosis, it will be necessary to undertake further tests.

Major GRAY: The history in this case seems to me to suggest tuberculosis or septic infection rather than syphilis, because, so far as I can make out, the condition began with definite abscesses along the lymphatic tracts. There was one abscess over the internal malleolus, and another on the inner part of the thigh, both of which burst and discharged for a time. The present lesion apparently started from the abscess in the foot.

Captain C. H. MILLS: Was the technique of the Wassermann test employed in this case the original technique, or a modification? In view of the fact that this patient has had three miscarriages, and that the scarring higher up the calf is very suggestive of previous gummatous ulceration, I think it might be well to administer a provocative injection of salvarsan, and twenty-four hours later take another Wassermann test, this time using the original method.

Dr. GRAHAM LITTLE (in reply): The Wassermann test in this case was performed by Dr. Alexander Fleming, who no doubt used his own modification of the original Wassermann test.

(*March 16, 1916.*)

### **Acute Lupus Erythematosus, with Nodular Necrotic Cutaneous Tuberculides of Arms and Hands, Feet and Legs.**

By ALFRED EDDOWES, M.D.

THE patient, a strong, well-developed woman, aged 27, with a good family history, was seized at the end of December last with great and sudden swelling of the face, and at the same time observed spots on her arms. Lately the swelling of the face has become less, but nodules have been noticed on the feet and legs.

## DISCUSSION.

Dr. G. PERNET: I consider this is a case of acute lupus erythematosus d'emblée, in which there is no albuminuria. I collected the records of nine cases in addition to my own, and only one out of the ten patients survived, notwithstanding various methods of treatment.<sup>1</sup> The small lesions on the arms and back of the hands come into the clinical picture of lupus erythematosus of the acute type. There is an acute onset, with swelling of the face, and I think these are very characteristic. Kaposi gave the name of erysipelas perstans faciei to the condition, though it had nothing to do with true erysipelas. The case which survived did well on quinine, and a recurrence again yielded to the drug. I think it should be tried in this case. Dr. Payne, of St. Thomas's Hospital, introduced quinine in the treatment of lupus erythematosus. I ask if there is albuminuria in this case. There was no albuminuria in the ten cases to which I have referred. The lupus erythematosus cases with albuminuria are in a different category.

The PRESIDENT: Does the exhibitor regard the eruption on the face as of the same nature as that on the extremities—i.e., are they both, in his opinion, of tuberculous origin?

Dr. DUDLEY CORBETT: I ask what was the state of the mouth for which Dr. Eddowes placed the patient in the dentist's hands. So far as I could see there is a considerable amount of pyorrhœa present which may be a contributory factor in the case.

Dr. H. G. ADAMSON: I think it is acute lupus erythematosus. I do not see any reason for calling the arm lesions tuberculides, nor are they necrotic. I think there is not much evidence that acute lupus erythematosus is tuberculous. The fatal cases I have seen died of pneumonia. I ask Dr. Pernet what were the causes of death in the nine cases which he collected. I think the only treatment for acute lupus erythematosus is to keep the patient strictly in bed. I have seen cases get well when so treated.

Dr. PERNET: My patient with acute lupus erythematosus d'emblée died of pneumonia, and this was also the cause in some of the other cases I collected. In my monograph on those cases I said I could not regard tuberculosis as the essential cause. I agree that an important indication in these cases is to keep the patient in bed; I assumed that that had been done.

Major GRAY: Some members may remember a case which I showed a little while ago.<sup>2</sup> It was extremely acute, many of the lesions actually having hæmorrhagic bullæ. Whilst that patient was under my observation, a period

<sup>1</sup> Pernet, "Le Lupus érythémateux aigu d'emblée," Paris, 1908.

<sup>2</sup> *Proc. Roy. Soc. Med.*, 1914, vii (Derm. Sect.), pp. 82-84.



of about eighteen months, I think she had three attacks, the condition clearing up in the intervals between the attacks. The second was the most acute, and was complicated by pneumonia, from which she recovered. A slight pleurisy occurred in the third attack, which was milder. I do not remember what medicinal treatment was given, but I remember she was kept in bed.

Dr. EDDOWES (in reply): The lesions are both atrophic and necrotic. In answer to the President, whether "tuberculide" or not, I consider these nodules are the isolated lesions of erythematous lupus. As the patient had some bad teeth, I passed her on to the dentist. I have not found any lesions due to the disease in her mouth. She is getting better without rest in bed. Twenty years ago I stated that the nodular necrotic cutaneous tuberculide—then often called "folliclis"—was in reality a well-marked isolated lesion of lupus erythematosus, but my view was rejected by several of the leading authorities, and I did not receive any articulate support. Nevertheless I have stuck to my point and reassert the statement with confidence, although the writers of textbooks do not yet see it. A question that awaits an answer is: If this eruption is due to tubercle in the system, why do we not see it oftener in cases of acute general tuberculosis? I may be asked: If I am right in considering that these nodules are the same as lupus erythematosus, why does the face often escape when the nodules appear abundantly on the limbs? My working theory is that the disease attacks the face especially and leaves scars behind because of pre- or co-existing seborrhœa and acne, or both. We have an analogy in lung abscess from tubercle—a double infection.

(March 16, 1916.)

### Recurrent Syphilide simulating Reinfection.

By H. SPENCE, M.D.

(Introduced by J. E. R. McDONAGH, F.R.C.S.)

PATIENT, a male, aged 31. He first noticed the primary lesion on the under surface of the penis about May 20, 1915, six weeks after exposure. Upon his first attendance at the hospital three weeks later he exhibited an indurated sore somewhat larger than a sixpenny piece, and associated glandular enlargement of the specific type. He received four intravenous injections of "606," and left the wards on July 10, attending the out-patient clinic ten days afterwards, when he received an intramuscular injection of mercury, and the chancre was

found to have healed. His work then necessitated his departure from London, so he underwent no further treatment, and for several months he was without symptoms. Early in January last he noticed a small spot to one side of and distinct from the original sore, which he states healed in a week's time, but was immediately succeeded by the two larger sores now in evidence, one upon the site just referred to, the other approaching the upper surface of the penis. At the same time there appeared the rounded sore on the left fronto-parietal region, nearly the size of a shilling, and similar sores on the dorsum of the tongue and in the left groin, as well as smaller lesions on the upper lip (1), scalp (2), chest (1), left leg (1), and back (6). There is also a distinct induration under the foreskin. A scar marks the site of the original sore.

#### DISCUSSION.

Captain MILLS: Is it the chief point of interest that these lesions are supposed to resemble primary chancres? [Dr. Spence: Yes.] It did not occur to me that any one of them resembled a primary sore. That on the left frontal region started as a periostitis, subsequently involving the subcutaneous tissues and skin, and then broke down. I have not probed it, but I think bare bone will be found there. A gumma is developing at the point of pressure where the man wears his truss; there are others on the shoulder corresponding with the pressure of his braces. I do not think the tongue lesion resembles a primary sore of the tongue. There is no loss of tissue on the surface, and no actual ulcer. And I also regard the non-indurated sore on the penis as gummatous.

The PRESIDENT: It is common experience in practice to see delayed results from good and appropriate treatment insufficiently administered, and it is likely that this is an instance of the kind. A continuous administration of the same remedies will undoubtedly cure the patient.

Mr. J. E. R. McDONAGH: This case is especially interesting for several reasons. The initial lesion was a large ulcerating chancre, a type of sore which is often accompanied by no enlargement of the inguinal lymphatic glands and by delayed secondary symptoms. In other words, the call for the production of resisting bodies is minimal; treatment with "606" renders them still less necessary, with the result that when symptoms reappear, they simulate the initial lesion, as they are to all intents and purposes developing on a new soil. The present type of sore on both the tongue and the penis is typical of a chancre, the characters of the chancre exactly simulating the initial lesion. Had the initial lesion left no scar, and had not other sores appeared simultaneously elsewhere than on the penis, the case might easily have been diagnosed as one of true

reinfection. In my opinion a large percentage of the reported cases of reinfection are really of the nature of the case here presented. In scanning the literature of syphilis, it is to be noticed that the reported cases of reinfection diminish year by year, and yet our treatment of syphilis is continually improving, a paradox which supports the view just enunciated.

Dr. PERNET: Although the sore on the penis might at first sight look something like a primary sore, I do not consider the appearances of the tongue are those of a primary chancre.

(March 16, 1916.)

### Diffuse Sclerodermia with Sclerodactylia.

By J. L. BUNCH, M.D.

THE patient, a woman, aged 50, has suffered from a gradually progressive sclerodermia for many years. The skin is now extensively involved and in many places has become hard, shiny, leathery to the touch, and rigid. In some places it is pigmented. In the upper limbs especially the underlying muscles have become atrophic. The sweat secretion of the involved areas is greatly diminished. The fingers are bent, fixed and practically ankylosed. There appears to be but little diminution of sensibility in the affected areas, and the occasional co-existing symptoms of cyanosis or quasi-Raynaud's disease (described by Chauffard and others) are not present in this case.

I bring the case in order to ask for suggestions as to treatment, and in relation to the case Dr. Graham Little showed at the last meeting in which thyroid was implanted into the tibia of a patient suffering from sclerodermia with apparently excellent results so far as the sclerodermia was concerned. Should the same thing be done in this case? The disease is spreading rapidly, and it only dates back fifteen months. The patient has been given thyroid by the mouth during the short time she has been under my care, but that does not appear to have led to any appreciable improvement. She has not yet had light treatment, as she has been under my care only a short time. I propose to give her vapour baths, but I do not suppose they will arrest the disease.

## DISCUSSION.

The PRESIDENT: Has there been any injury to the nervous system or nerve shock? I do not know whether any member would be willing to undertake implantation of thyroid in such a case. It is a complicated and serious method of treatment.

Dr. GRAHAM LITTLE: This patient was under my care for several months, and for some weeks was an in-patient at St. Mary's Hospital, where she was treated with administration of thyroid extract by the mouth, and by massage, warmth, and rest in bed. The condition, I was afraid, seemed stationary, and I was obliged to replace her by a more urgent case. With regard to the relative advantages of administration of thyroid by the mouth and by implantation, I notice with some interest that Professor Starling, in the last edition of his "Physiology," remarks that implantation offers no advantages over feeding by the mouth. In the case shown by me to which reference has been made, the lady was certainly greatly impressed by the effect of the two operations performed on her by Dr. Kocher, and I may have laid perhaps too much stress on these operations in presenting the case, for it must not be overlooked that she had throughout, in addition to the implantations, both before and after these, continuous feeding by thyroid, and that when she offered herself for a third operation Dr. Kocher refused to operate, and contented himself with continuing the oral administration of thyroid.

Dr. F. PARKES WEBER: I understand that treatment by implantation of thyroid gland into the medullary cavity of the long bones has never yet been carried out with the object of curing sclerodermia or sclerodactylia; for in Dr. Little's patient it was carried out against the thyroid disease, not against the sclerodactylia. If Dr. Bunch were to adopt the implantation method in this case, it would probably be the first case in which such operative treatment has been undertaken to oppose sclerodactylia; though, as is well known, thyroid feeding, iodothyrene, &c., have been tried in the various forms of sclerodermia, not always, however, with beneficial results.

Dr. PERNET: I do not consider that large doses of thyroid should be given to a woman of this age, and the fact that she has got rapidly worse may be due to the thyroid administration having been pushed. I think small doses should be given to patients of this age—large doses, it seems, have a tendency to hasten the break-up of such patients.

Major GRAY: There is one indication in favour of implantation of thyroid as against oral treatment, which Dr. Graham Little did not mention; I am speaking of thyroid treatment generally and not specially in sclerodermia. A certain number of people cannot take thyroid by the mouth without feeling

ill. I know of one case of myxœdema in which that was true, and implantation produced a distinctly good result, though only of temporary duration. A second implantation was done, with equally good but temporary result. The implantation was made in the abdominal wall.

Dr. BUNCH (in reply) : There is a story of a fall three years ago, but the present trouble does not date back as far as that. I think we cannot make the excessive dosing with thyroid responsible for the rapid decline in this case. I do not know that the patient has had any at all during the last six months until the last week or two. I was much struck with Dr. Little's recent case, and perhaps, from what is now said, I credited the implantation with a better result than I should have done, as I did not know what additional treatment, if any, the patient had had.

(March 16, 1916.)

### Sections from a Case of Syringoma (?).

By H. G. ADAMSON, M.D.

(For Dr. NORMAN PAUL, of Sydney.)

I HERE exhibit a lantern slide, photograph, and microscopical slides of a case of syringoma (?) which has been under the care of Dr. C. Norman Paul, of Sydney, N.S.W. Dr. Paul's notes are as follows: "The patient, J. S., is a man, aged 46. Situated over the left side of the sternum, 2 in. inside the left nipple and reaching to the mid-sternal line, there are about twenty-five smooth, firm, raised growths, the largest 25 cm. across and raised 15 cm. The growths are sessile, intimately associated with the skin, and move with it. The bases of the smaller nodules coalesce to form an infiltrated plaque. The two largest growths have been present for twenty-five years, three others for twelve years and a number have appeared in the past three years. In colour some show a decided dark blue, the remainder being either pink or pinkish-blue. Dilated vessels are coursing over the two larger growths, which appear cystic. The blue colour has been noticed only in the past twelve months. There are no enlarged glands. The patient has dyspepsia and states that for several months past, after an attack of





FIG. 1.

Photograph of case of syringoma (?)



FIG. 2.

Section of a small nodule, showing cystic dilatations.

dyspepsia, a new growth appeared and became painful, but otherwise there was no pain. The whole mass was excised and the blue coloration was found to be blood, the larger growths showing cysts filled with



FIG. 3.

Section of portion of growth, under higher power, showing tubule-like arrangement of cell growth.

blood. The section shows the growth to be situated in the corium, lobulated, surrounded by fibrous tissue, and appears to be derived from the sweat-glands. Diagnosis: (?) Adenoma of the sweat-glands."

I think you will agree that Dr. Paul's diagnosis is probably correct, although the clinical appearances and the microscopical appearances are somewhat different from the majority of syringomata which have been recorded. At first the microscopical sections suggest a growth of the type of tricho-epithelioma, and especially under the low power. Under a high power, and by a study of different parts of the growth, it would appear to be made up of tightly compressed tubules and not uniform solid masses of cells as in tricho-epithelioma. It is proposed to publish the case in the *British Journal of Dermatology* with references to literature and comparison with other cases of syringoma.

(March 16, 1916.)

### Case of very Unusual Tumours of the Skin.

By E. G. GRAHAM LITTLE, M.D.

THE patient is not shown as the operation for removal of the tumours is too recent to bring her from St. Mary's Hospital. She is aged 22, a single woman, a "stiffener" by trade, and was first seen by me in December, 1915, when she showed me two swellings in the thickness of the skin, one in the left labium majus and one on the right forearm. The tumours were of stony hardness, deeply situated, about the size of a shilling and of a half-crown respectively, the smaller one being on the forearm; and the history was that they had been present for about nine months. They apparently caused no pain. The Wassermann reaction was negative, and the patient was a *virgo intacta*. My clinical diagnosis was fibro-sarcoma, and at my suggestion the tumours were freely removed by my colleague, Mr. Ernest Lane, and I exhibit the pathological specimen together with a microscopical slide from one of them. Naked-eye inspection of the specimen shows a glistening white infiltration of the tissues immediately below the integument for the depth of about  $\frac{3}{4}$  in. Microscopical examination shows a slight acanthosis, and a dense and deep infiltration of the skin and subcutaneous tissue to the boundary of the subcutaneous fat, with a very extraordinary type of cell. This is a greatly swollen body with a very granular endoplasm, with numerous nuclei and an ill-defined wall.

Masses of such cells are wedged together and contained by fibrous trabeculæ. Dr. Adamson, who examined the section, remarked on the resemblance of the cell to the so-called "xanthoma cell," and the resemblance is, in fact, close, but the clinical diagnosis of xanthoma was in no way suggested. Dr. Kettle, Pathologist to St. Mary's Hospital, who has a unique experience of tumours, has not met with any exactly similar condition, and the diagnosis remains at present obscure.

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(April 13, 1916.)

### The Rationale and Practice of Chemotherapy.

By J. E. R. McDONAGH, F.R.C.S.

#### INTRODUCTION.

MY reason for introducing the subject of chemotherapy is one prompted by circumstances. The War has prevented us from relying upon Germany, and in consequence of this we have had to shift for ourselves. Some have been content to imitate German empiricism with incomplete success, while others have attempted to place the subject upon a logical basis. Although the result of these logical endeavours may not at present be all that is desired, time will no doubt improve them. Anyhow, invention is the only thing which will redound to the credit of British medicine; imitation, however successful, will never do so.

The prevailing view of chemotherapy is shortly this: arsenic is essential, and cures syphilis—a disease, by the way, for which it was not originally thought suitable, owing to its parasitotropic and to its non-organotropic properties. I will therefore ask those who have kindly come to take part in the discussion to answer the following questions :—

(1) Why was arsenic the element chosen, otherwise than for the reason that it was at the time the only drug known which was of any value in the treatment of sleeping sickness, the disease for dealing with which chemotherapy was inaugurated?



(2) Why were several hundred compounds tried before a suitable one was found, otherwise than for the reason that Ehrlich had no sound basis upon which to work?

(3) Why does salvarsan not cure syphilis when its spirillocidal action is so strong?

(4) Why should arsenic exhibit a chemical affinity for parasitic cells—and, it is to be remarked, for only certain parasitic cells—and not for the cells of the host? In other words, why is salvarsan parasitotropic and not organotropic, and why only parasitotropic for certain parasites?

My answers to the first two questions are contained therein. My answer to the third question is that syphilis is not cured by salvarsan because the *Spirochæta pallida* is not the cause of the disease. My answer to the fourth question is that salvarsan is more organotropic than parasitotropic, and that it is only parasitotropic for certain parasites owing to the chemical constitution of those parasites.

Although chemotherapy is to be the main theme of my paper, I should like to say that it is only one of the links in my chain of research upon syphilis. The first link of that chain was the discovery of the *Leucocytozoon syphilidis* in 1912. The next link was formed by a chemical study of this organism, about which a few words must be said, as the points revealed in this study laid the foundation upon which the rest of my work was built. I found that the phases of the *Leucocytozoon syphilidis* consisted mainly of lipid-globulin molecules, which proved themselves to be more resistant to reagents than the lipid-globulin molecules of the host's cells—viz., the protoplasm of the plasma cells and of the nucleoli. Further, that their reducing action was also greater, especially in the case of the spirochætal phase, owing to its containing in its molecules a fatty acid not completely saturated.

The parasitic lipid-globulin molecules were found to be combated by certain lipid-globulin molecules of the host, which are formed by the lymphocytes. Some of the host's lipid-globulin molecules are destined, in the form of plasma cells, to attack those of the parasites locally, while others circulate in the serum, in which they form the internal phase, as the resisting substance or the antibody.

The action of the lipid-globulin molecules which circulate in the serum is general, but in other respects it is the same as that exhibited by the plasma cells. The host's lipid-globulin molecules have a stereochemical molecular configuration, homologous to those of the parasite. This fact explains specificity.

The lipid-globulin molecules which circulate in the serum are colloidal particles, which are kept in "solution" by the water their molecules contain and by the ions which are attached to them. In this form they are known as emulsoids. The fluid medium in which the emulsoid particles circulate is known as the external phase, and consists mainly of water or, let us say, of hydrogen and hydroxyl ions, which constitute the substance we call water ( $\text{H}_2\text{O} = \text{H}^+ + \text{OH}^-$ ). In order to keep the particles of the internal phase in "solution" it is necessary for the external phase to maintain a balance between the hydrogen and hydroxyl ion concentration. This balance is known as the normal hydrogen ion concentration. There are normally more hydroxyl ions than hydrogen ions in the external phase, hence the reason why serum is always on the alkaline side of neutrality.

This normal hydrogen ion concentration is maintained, on the one hand, by the free ions of the external phase, and by the adsorbed ions of the internal phase. The former constitute the so-called end-piece of complement and the latter the so-called middle-piece of complement, which is better designated as the oxidase-reducase system.

The parasites are killed by the lipid-globulin molecules of the host by a chemico-physical process which is known as adsorption.

Adsorption is regulated by three factors:—

(1) The stereo-chemical molecular configuration of the adsorbed molecules. For the adsorption to be perfect, the molecules must have an homologous stereo-chemical molecular configuration, and this is mainly regulated by the amino groups ( $\text{NH}_2$ ).

(2) The permeability of the adsorbed molecules. The more permeable the molecules the easier the adsorption. Permeability is mainly regulated by the carboxyl groups ( $\text{COOH}$ ).

(3) The amount of active oxygen. Adsorption is an action in which energy is used, and similar to all such actions it requires active oxygen. Active oxygen is formed directly by ferments which are known as peroxidases. Peroxidases are hydroxides of metals, and they form active oxygen provided a peroxide is present. The peroxide is formed directly by ferments, which are known as perhydridases. Perhydridases are non-metals, and they form peroxide by their action upon hydrogen and hydroxyl ions.

A peroxidase is an oxidizing agent and a perhydridase is a reducing agent—hence the meaning of the term oxidase-reducase system.

The phenomena which follow the adsorption between the parasitic lipid-globulin molecules, on the one hand, and the lipid globulin

molecules of the host, on the other hand, *in corpore* are not the same as those which occur in a test-tube when an antigen meets its antibody. In a test-tube, either the antigen or the antibody is less emulsoïd than when in the body; consequently when to such particles perfect emulsoïd particles are added, in the shape of complement, the perfect emulsoïd particles attempt to bring the imperfect emulsoïd particles into their perfect emulsoïd state. As this action deprives the lipoid-globulin molecules, which constitute the complement, of some of their ions, which they are not able to replace as they are when *in vivo*, all the colloidal particles concerned change their colloidal state. The previously emulsoïd particles now become suspensoid particles. Suspensoids are not kept in "solution" like emulsoids, so the particles become precipitated. Such a change causes considerable alteration in the surface tension, a change which can be readily seen if red blood corpuscles form either the antigen or the antibody. Hæmolysis is the change observed.

It is now evident why agglutination, precipitation, complement-fixation, &c., are the pictures given by the immunity reactions *in vitro*.

If the tests just mentioned are specific, it is necessary for the molecules of the antigen to possess a stereo-chemical molecular configuration homologous to those of the antibody.

Since the antigen in the Wassermann reaction does not possess particles having a stereo-chemical molecular configuration, homologous to those of the reagin, how is it that fixation of complement occurs when such particles meet?

The antigen particles in the Wassermann reaction are adsorbed by the syphilitic lipoid-globulin molecules solely in virtue of the increased size, number and adsorptive capacity of such molecules. Complement is destroyed in the attempt by the syphilitic emulsoïd particles to bring the adsorbed antigen particles into "solution." If the host's lipoid-globulin molecules are not sufficiently powerful to kill all the parasites early in the disease, when their oxidizing action is greatest, they will naturally attempt to increase their adsorptive capacity and their supply of active oxygen. The particles can only do this by increasing their permeability and reducing action, and this they do by increasing their carboxyl groups. This is the reason why the Wassermann reaction is more positive in late than in early cases of syphilis. There are so many other factors which I have not time to mention here influencing the Wassermann reaction that it is certain that a positive reaction is no certain indication of active syphilis or that the patient requires treatment. The Wassermann reaction does not give us an indication

of the colloidal state of the lipid-globulin molecules or a measure of their number, the knowledge of which is necessary both for diagnostic purposes and for regulating treatment. This difficulty has fortunately been overcome by the discovery of the emulsoïd-suspensoid reaction—or the E.-S. test, as I have called it for short. The outstanding feature of my research work, as I have described it up to this point, is that the lipid-globulin molecules of protozoal diseases, especially syphilis, are larger, more numerous and have a greater adsorptive capacity than normal lipid-globulin molecules, or the lipid-globulin molecules of bacterial diseases.

#### NATURAL AND ARTIFICIAL PROTECTION.

When a man runs the risk of becoming infected with syphilis, there must always be, first of all, a struggle between the cells of that man and the spores of the leucocytozoon. If the former win, then, of course, there is no infection. Assuming that the latter win, the spores, having gained a firm foothold, will develop at their host's expense and produce a lesion which we call a chancre. A further development of these spores will lead to a generalization of them in the body of the host, with a result that lesions will occur all over the body. Instead of the development going further still, the tide turns in favour of the host, and the number of the parasites gradually becomes lessened, with the result that the lesions disappear. The turning of the tide is due to the fact that the host's lymphocytes have been able to manufacture lipid-globulin particles, the molecular configuration of which is homologous to that of the lipid-globulin molecules of the parasite.

Two questions now arise: (1) What is the nature of the primary struggle? (2) What is the *modus operandi* of the turning of the tide?

There are only two factors which come into play in the adsorption between the antigen and the antibody *in vivo*: (1) hydrogen ion concentration (oxidase-reducase system); (2) specific amino groups.

In the primary struggle the second factor cannot possibly play a part, since it takes some time for the amino groups in the lipid-globulin molecules to be arranged in a specific manner. Moreover, the primary struggle is a local affair, a struggle between spore and cell, while the turning of the tide is a general affair—a struggle between the lipid-globulin particles of the serum generally, of the plasma cells locally, and those of the parasites.

When a syphilitic spore enters the human body it must, in order to develop, enter a connective tissue cell or an endothelial cell. To enter a



cell it must alter the surface tension of that part of the cell where it is going to enter, and this will mean a local disturbance of the normal hydrogen ion concentration. It is certain that most cells require and use oxygen, and that most cells in exchange for oxygen exhale carbon dioxide. Should a spore be able to take some of the oxygen required by the cell it tries to enter, the normal ratio between the oxygen and the carbon dioxide of that cell will cease to exist, with the result that the normal hydrogen ion concentration, or the oxidase-reducase system, will be locally and temporarily deranged, so that the spore wins, and the local surface tension of the cell will be such that the spore is easily able to enter it.

How this temporary derangement of the oxidase-reducase system can be directly prevented is not at present clear, but from the analogy with experiments I have undertaken in another direction (anaphylaxis), the derangement can be indirectly prevented by increasing the stability of the permeability, and this can be done by intravenous injections of calcium chloride.

In this connexion it is interesting to note how the risk of contracting a venereal disease increases if the patient is under the influence of alcohol, owing to the fact that alcohol has exactly the opposite effect to the salt just mentioned. It is highly probable, but not so practicable, that if a patient had an injection of calcium chloride, or if he took large quantities of sodium chloride, before having connexion, that his chances of contracting a venereal disease would be considerably diminished.

The *modus operandi* of the turning of the tide is probably as follows: Both the lipid-globulin particles of the syphilitic phases and of the serum have an oxidase-reducase system. In the case of the former, the active oxygen is used by the nucleus for propagation purposes. In the case of the latter, there being no nucleus, the active oxygen is used for that purpose for which the particles exist—namely, for the purpose of adsorption. Since the amino groups have the same arrangement in the two kinds of lipid-globulin particles, the necessities for the physical action of adsorption will be at their best. As the active oxygen in the host's lipid-globulin molecules will be only required for this physical action, it will follow that the host's lipid-globulin molecules will have an advantage over those of the parasite, with the result that the changes which the spore caused the cell it was attacking to undergo in the primary struggle will now affect the parasites themselves.

If these views are correct, we should be able to forestall the turning of the tide and to hasten it, by increasing the amount of active oxygen



in the lipoid-globulin particles in the serum of the host. This can be done—(1) directly, by the administration of strongly adsorbed metallic compounds, which act as peroxidases and produce active oxygen; (2) indirectly, by the administration of a strongly adsorbed non-metallic compound, which acts as a disulphide and produces peroxide and active hydrogen.

This now brings me to discuss the rationale of chemotherapy in syphilis.

#### RATIONALE OF CHEMOTHERAPY.

It will be remembered that, in the “side-chain” theory, Ehrlich considered the union between the antibody and the antigen was due to chemical affinities which each of these bodies possesses for each other. Then Ehrlich conceived the idea that this principle could be applied to treatment, and the application of this later received the name of “chemotherapy.” The principle was shortly this: To find drugs, the chemical groups of which would combine with the chemical groups of the parasites to be attacked, without combining with the chemical groups of the host’s cells. The chemical groups both of the drugs and of the cells received the name of “chemoceptors.” The disease which Ehrlich hoped to combat was sleeping sickness, and since arsenic was the body which appeared to have the greatest influence upon the disease, experiments with this metal were undertaken.

For some time after the commencement of his experiments, Ehrlich appears to have been of opinion that the sole receptors between the chemical groups in the protoplasm of the parasites and in the drugs used were the arsenic receptors. How Ehrlich could ever think that arsenic would find its chemical affinity in the protoplasm of cells and, it is to be remarked, of only certain cells, is difficult to understand; and it is surprising that most of us should still consider that this element is of paramount importance in the synthetic antisyphilitic remedies. That other receptors existed as well was shown only when it was noted that the action of arseno-phenyl-glycine was not affected by previously subjecting the parasites to the action of the arsenic derivative of phenyl-oxy-acetic acid. Then it was assumed that acetic acid receptors also existed. Since fatty acid receptors were said to exist in the protoplasm of the parasites, it was only logical to suppose that amino-acid receptors would also be found there. Working upon this hypothesis, Ehrlich discovered salvarsan.

According to Ehrlich, salvarsan works by means of its arsenic

receptors and its ortho-amino-phenol receptors. When it was discovered later that a certain drug had a fatal action on one kind of parasite and not upon another kind, although in both instances a "combination" occurred between the drug and the bodies of the parasites, some further explanation of the action of salvarsan was required. Consequently, salvarsan was stated to act in the following way: The arsenic was considered to be the toxophore group, the benzene-ring to be the carrier, and the amino groups the haptophore groups.

Summing up Ehrlich's views as to the mode of action of salvarsan, we find that a union takes place between the drug and certain parasites, with a destructive action upon the parasites. What the nature of the union is, and why the death of only certain parasites should follow, are not explained.

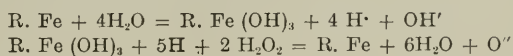
Before discussing the mode of action of the anti-syphilitic drugs, I will briefly recapitulate what I have already said concerning the way in which the body naturally protects itself against syphilis. The protective power of the body can be divided into two groups: (1) the power which acts generally—i.e., the lipid-globulin molecules of the serum; (2) the power which acts locally—i.e., the plasma cells. The plasma cell is typical of chronic inflammation, whatever be the cause. In all instances, the plasma cell is the same morphologically, but, although its gross action is similar in every instance, it is nevertheless specific in each case. To be more exact, one should call the specificity a group specificity, not an individual specificity, since the plasma cells behave in trypanosomiasis in the same manner as in syphilis, and also in some other diseases. This specificity exhibited by the plasma cells is the same as that exhibited by the lipid-globulin molecules of the serum, and, so far as it concerns chemotherapy, it is solely due to certain physical properties which these two protective powers possess in protozoal diseases.

The lipid-globulin molecules in the serum of protozoal diseases, especially in syphilis, are characterized by their large size and their increased adsorptive capacity. As the disease becomes more and more chronic, the size of the molecules and their adsorptive capacity increase. As has already been explained, a parasite is vanquished *in vivo* by a change which takes place on its surface. This change causes a local alteration of the surface tension, which so disturbs the normal permeability that the organism perishes. This change is brought about by adsorption. Like most physical and chemico-physical reactions, the phenomenon of adsorption is dependent upon active oxygen. Therefore

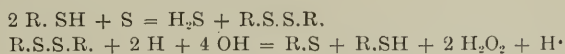
some ratio exists between the destructive power of a molecule and its supply of active oxygen. In the early stages of the disease it is naturally to be expected that the supply of active oxygen will be as great as the body can manage without extra assistance. If the supply of active oxygen is not sufficient to rid the host of its parasites, the disease becomes chronic, and, all the direct methods of producing active oxygen having failed, the host is obliged to obtain it indirectly. Active oxygen is obtained indirectly by increasing the reducing action, hence the reason why the lipid-globulin molecules are larger, and have a greater adsorptive and reducing action in late than in early cases of syphilis.

There are three actions exhibited by the lipid-globulin molecules in the serum: (1) adsorption; (2) oxidation; (3) reduction. In virtue of the size and of the increased adsorptive capacity of the anti-protozoal lipid-globulin molecules, it will follow that their powers of oxidation and reduction may be enhanced by oxidizing and reducing agents, and that a ratio must exist between the amount of active oxygen and active hydrogen produced and the degree of the adsorbed state in which are the oxidizing and reducing agents.

The active oxygen formed by the lipid-globulin molecules can be seen in the two following equations:—



The active hydrogen formed by the lipid-globulin molecules can be seen in the two following equations:—



From the above equations it will be noted that non-metals increase the production of active hydrogen. Therefore, from what has gone before, it will now be seen that extra active oxygen can be produced in the serum of the host, directly by the administration of strongly adsorbed metallic compounds, and indirectly by the administration of strongly adsorbed non-metallic compounds. Without treatment, the lesions of syphilis vanish because the lipid-globulin molecules of the serum and the plasma cells contain a stereo-chemical molecular configuration homologous to those of the parasitic lipid-globulin molecules, an homology which is necessary for perfect adsorption, and because they contain active oxygen necessary for the adsorption, which is formed directly by an iron hydroxide and indirectly by a disulphide.

Since we do not know the chemical formula of the protozoal lipid-globulin particles, it is impossible to synthesize a compound homologous with them. Therefore the only way in which we can assist the host is to increase his supply of active oxygen, and this we do directly with arsenic, aluminium, and iron, possibly also with antimony and mercury, and indirectly with iodine and sulphur.

Treatment, then, assists the host's resistance by increasing his amount of active oxygen. Therefore, treatment destroys the parasites indirectly, and not directly, as Ehrlich thought. The only phases killed directly, and these only by the metallic compounds, are the adult male phase and the female phase after impregnation. These phases are killed directly owing to the free hydroxyl groups their molecules contain. The free hydroxyl groups occur because some of the fatty acid groups are unsaturated, owing to the important function these two phases have to perform. It is owing to the presence of these free hydroxyl groups that the *Spirochæta pallida* and the female cell after impregnation and not the other phases stain with silver nitrate in Levaditi's method of staining. The silver being a positive ion readily fixes on to the negative hydroxyl ion, and as the arsenic in salvarsan, the aluminium in aluvine, and the iron in ferrivine, are trivalent positive ions, it can be understood how readily they will adsorb free negatively charged hydroxyl ions.

The question might now very naturally be asked, If the action of salvarsan is an indirect one, why should arsenic be more active than mercury, since under ordinary circumstances the latter is a more powerful antisyphilitic remedy?

In my opinion, arsenic acts as a catalyst—i.e., it accelerates a reaction going on spontaneously, but more slowly without its assistance. Mercury possibly acts in the same way, but in virtue of its high atomic weight its action is more probably felt in the influence it exerts on mechanical adsorption. X-rays and radium probably act in malignant disease as mercury does in syphilis.

A ratio exists between the intensity of action of a catalyst and the degree of the colloid state of the catalyst. In salvarsan the arsenic is in a colloidal state, hence its action would necessarily be greater than that of any mercurial compound which we are in the habit of using. The proof of what has been said can be found if we compare the action that different manganese compounds exert on plant oxidases. Manganese formate, for instance, has nothing like such a powerful accelerating action upon plant oxidases as has manganese hydroxide. The latter is colloidal but the former is not.



In many other synthetic organic arsenical compounds, the arsenic is also in a colloidal state, but the action of the drug is very inferior to that of salvarsan. This fact therefore at once suggests that salvarsan, when injected, becomes fixed to the substance which the arsenic is going to accelerate, and proves that a ratio exists between the therapeutic action of a drug and the degree of its adsorbed state.

Since the "606" molecule enters into adsorption with the lipoid-globulin molecules of a syphilitic serum and of the plasma cells in a case of syphilis, it will be seen that Ehrlich's statement that the drug is parasitotropic only and not organotropic cannot hold good. For a drug to be parasitotropic it must be organotropic; indeed, its organotropic properties are far more important than its parasitotropic properties.

Salvarsan and the other antisyphilitic remedies are not specific in the sense that a vaccine is specific, and the attachment of these compounds to the syphilis parasites, to the plasma cells, and to the lipoid-globulin molecules in a case of syphilis, is not due to an homologous stereochemical molecular configuration between the adsorbed molecules, since for one thing alone the synthetic compounds are optically inactive bodies.

In the case of syphilis and in all protozoal diseases the lipoid-globulin molecules are bigger and have a greater adsorptive capacity than the lipoid-globulin molecules in the serum of bacterial diseases. I do not wish to infer that there is a hard and fast line between protozoal and bacterial diseases, since in chronic bacterial diseases the lipoid-globulin molecules of the serum approach in their action those in protozoal diseases, since the continued use of oxidizing and especially of reducing agents has a markedly remedial effect. I need refer only to the use of iodine and intramine in tuberculosis, chronic *ulcus molle* and chronic gonorrhœa.

The bigger the size of the molecule and the greater its adsorptive capacity, the more easily will a drug like salvarsan be adsorbed, and the more readily will the adsorbed compound break down. When the lipoid-globulin molecules first break down the area of their action is widened, hence the catalytic action of the arsenic will have its fullest play. If salvarsan became attached to small molecules, the adsorbed compound would not break down; at all events, not until much later than it does in the case of syphilis. Moreover, as much of the arsenic compound would not be adsorbed, this would mean that the arsenic would get little chance of acting as a catalyst. Therefore, salvarsan seems to act especially in protozoal diseases because of the physical state of the lipoid-globulin molecules of the serum, and it fails to act



as a bactericidal agent because the lipid-globulin molecules in the serum of bacterial diseases do not possess the requisite physical properties. From the fact that Ehrlich noticed that it was only such organisms as the *Spirochæta pallida*, the spirillum of recurrent fever and the trypanosome which were killed by the action of salvarsan *in vitro*, two important deductions may be made: (1) that the spirillum of recurrent fever and the trypanosome are killed because of the free hydroxyl groups their molecules contain; (2) that the trypanosome is not the sole cause of sleeping sickness.

To sum up the points which stand out, we can briefly say that strongly adsorbed metallic and non-metallic compounds become attached to the lipid-globulin molecules of the syphilitic parasite, the plasma cells, and the serum, by virtue of their amino groups and of the peculiar physical properties of the protozoal lipid-globulin molecules. The metallic compounds attack those phases of the *Leucocytozoon syphilidis* in which reaction is most marked, especially the spirochætal phase, in virtue of its free hydroxyl groups. Both metals and non-metals act as catalysts, that is to say, they accelerate the complementary or oxidase-reducase action of the lipid-globulin molecules of the serum and of the plasma cells by increasing directly and indirectly the supply of active oxygen, which is necessary for the action of adsorption—a chemico-physical action which destroys the parasites.

#### RESEARCH IN CHEMOTHERAPY.

If salvarsan be compared with atoxyl and arsacetin, it will be seen that in salvarsan the arsenic is trivalent, while in atoxyl and arsacetin it is pentavalent. Arsenic in a pentavalent state is less active in exerting its therapeutic influence on protozoa than when it is in a trivalent state. This is due to the fact that, in a molecule in which all the valencies are satisfied, the influence desired is less likely to act than when some of the valencies are unsatisfied. In this connexion, Hardy's rule is an important one to bear in mind—viz., that the precipitating power of an electrolyte depends upon the valency of the ion. From this it is clear that no compound of a univalent ion would be likely to have a therapeutic action at all akin to that of salvarsan. Therefore, so far as future research is concerned, the element employed must at least be a di- or, better, a trivalent ion.

Apart from mere valency, two other important considerations present themselves: (1) The degree of adsorption of the compound; (2) the atomic weight of the element.

It is a well-known fact that a strongly adsorbed organic anion, such as salicylic acid, is more powerful in precipitating aluminium hydroxide than a weakly adsorbed univalent inorganic ion. Hence, to obtain the best therapeutic action with a compound, we see that it must be organic, and the more strongly adsorbed it is the better, the reason being that a ratio exists between the strength of adsorption of the compound and the power it has in diminishing the mechanical surface energy. It is now clear why salvarsan should be so much more effective than arseno-phenyl-glycine, arsacetin, or atoxyl. It is also a well-known fact that, in the precipitation of a negative colloid by kations, those of the heavy metals and of organic bases are more active than would be expected from their valency, and that this is to be accounted for by the fact of their great mechanical adsorption. This shows us, then, not only why mercury should be useful in syphilis, but also why it should be toxic. Owing to the great mechanical adsorption exerted by the heavy metals, it is at once obvious that strongly adsorbed organic compounds of them would be too toxic for injection.

Studying the rationale of chemotherapy in this way, one may be saved from doing an amount of useless experimental work by excluding all univalent and divalent elements, as well as all those elements which have high atomic weights.

Seeing that arsenic, a trivalent element in the form of a di-oxy-di-amino-benzene compound, may be toxic, the atomic weight being only 75, and that the corresponding compound of antimony, di-oxy-di-amino-stibino-benzene is more toxic still, the difference in the atomic weight being  $(120 - 75) = 45$ , it is perfectly clear that, if we wish to find a non-toxic compound, we shall have to seek for elements which are trivalent with a smaller atomic weight.

Before mentioning the elements I tried there are one or two other points to be considered.

Di-oxy-di-amino-stibino-benzene is not only more toxic than its corresponding arsenic compound, but its therapeutic action is infinitely less. Comparing the two bodies we find that the arsenic in salvarsan is both metallic and non-metallic, while in the antimony compound just mentioned the antimony is entirely metallic. It therefore struck me that a less metallic element than arsenic might prove to be its therapeutic superior; in any case the probability was great that it would be considerably less toxic.

Salvarsan is a strong oxidizing body; indeed, its arsenic acts catalytically as a peroxidase, but at the same time it is a reducing agent, although its properties in this direction are probably not made

use of in the body. This led me to think that a body similar to salvarsan, which had stronger reducing properties, would probably do just what salvarsan failed to do.

I wanted, then, an element which had the following properties: (1) of more than one valency; (2) an atomic weight of less than 75; (3) a non-metal; (4) a reducing agent.

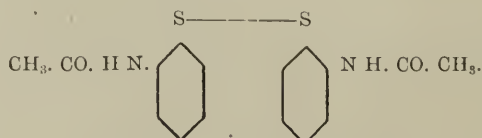
Only two elements occurred to me which would fulfil these conditions, and they were phosphorus and sulphur. I discarded phosphorus, owing to the fact that it can produce very toxic symptoms, such as fatty degeneration of the liver, so I conducted experiments with sulphur and succeeded in making a compound which has in some respects a more powerful therapeutic action than that of salvarsan. I prepared several compounds, but soon found that one was much superior to the others, and this one was di-ortho-amino-thio-benzene, or "intramine," as it is called commercially.

Further research showed me that it was highly probable that other metallic compounds could be found which would prove to be as efficacious as and less toxic than salvarsan. Seeing that the metal must be trivalent and not of high atomic weight, I began to experiment with iron, manganese, and aluminium. In a short time I was able to prepare some compounds of iron and aluminium, which turned out to be non-toxic and to have in some cases a marked therapeutic action like salvarsan, if used in sufficient and repeated doses.

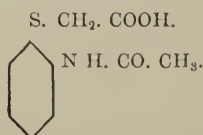
#### NON-METALLIC COMPOUNDS.

##### *Sulphur.*

I prepared two basic and two acid compounds. The first acid compound was the acetyl derivate of di-ortho-amino-thio-benzene, and the second acid compound was the condensation product of the base with mono-chloroacetic acid.

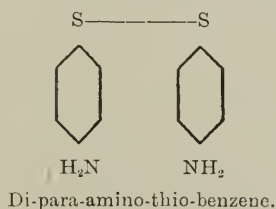
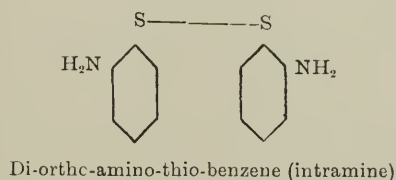


Acetyl derivative of intramine.



Condensation product with mono-chloroacetic acid.

These two products may be quickly dismissed, as both were extremely painful when injected intramuscularly, and their therapeutic action was practically *nil*, which was to be expected, since the amino-groups have been interfered with. The two basic compounds prepared were di-para-amino-thio-benzene and di-ortho-amino-thio-benzene. As the therapeutic action of the first-mentioned drug was not as good as that of the second, it was discarded.



Intramine is absolutely non-toxic; consequently, any dose may be employed, and it may be repeated as often as is thought necessary. The usual dose I employ for an adult is 1 grm., this quantity being suspended in 9 c.c. of olive oil or liquid paraffin and injected intramuscularly.

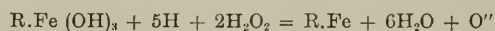
Intramine does not oxidize on exposure to air, therefore the unused contents of a phial need not be thrown away, and a wide-mouthed bottle containing several grammes may be kept and used like mercurial cream. The pain caused by the injection, when 0.5 to 1.0 grm. or more is employed, usually renders the patient *hors de combat* for two or three days. Unfortunately, this cannot be entirely avoided. A few days later an erythema may appear over the injected area, which remains localized or spreads over the body, but in all cases it disappears in a day or two. The great point in favour of intramine is that, however severe be the local reaction, it rapidly becomes absorbed and never leaves any induration behind it, provided no quinine and urea hydrochloride are mixed with it.

When compared with salvarsan, I soon found that in the primary and generalization stages salvarsan was superior to intramine, but that in the recurrent stages the opposite was the case.

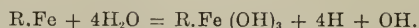


Further experiments showed me that, in the early stages of syphilis, previous administration of a metallic compound greatly increased the action of intramine, but that in the recurrent and late stages the intramine increased the action of the metallic compounds.

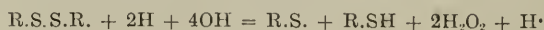
Before describing the metallic compounds I prepared, it will be as well to discuss the mode of action of intramine in further detail. Having already made some allusions to the *modus operandi* of intramine, to save recapitulation I will begin with its action in the oxidase-reducase system. The oxidase-reducase system can be expressed in short as the hydroxyl-hydrogen system of the internal phase. The metallic ion becomes attached to the hydroxyl group; normally, the metal is iron; when salvarsan is injected it is arsenic, &c. This metallic hydroxide combines with hydrogen ions and hydroxyl ions in the form of hydrogen peroxide to form active oxygen, as shown by the following equation:—



The iron-protein which is formed as the result of the reaction soon becomes reconverted into iron hydroxide-protein, as in the following equation:—



When intramine gets into the body, the compound presumably breaks down at the link between the two sulphur atoms. The sulphur then forms disulphide-protein, which reacts with the hydrogen and hydroxyl ions of the external phase to form sulphur-protein, mercaptan-protein, hydroxyl ions in the form of hydrogen peroxide, and active hydrogen, as indicated by this equation:—



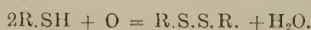
In this way two molecules of peroxide of hydrogen are formed, exactly the amount required for the metallic hydroxide to act upon in order to form active oxygen.

Metals, then, form active oxygen directly, and non-metals do so indirectly.

The mercaptan-protein is able to combine with any free sulphur to form hydrogen sulphide and disulphide-protein, as in the following equation:—



The mercaptan-protein is also able to combine with any free oxygen to form water and disulphide-protein, as in this equation:—





Adding the knowledge just obtained to that gained by clinical observation, further light is thrown upon the affinity of mercaptan-protein for oxygen. If the mercaptan group becomes oxidized by the active oxygen present, there will be less active oxygen resulting from the oxidizing action of the metallic hydroxide; hence, if intramine be given first and salvarsan afterwards in early syphilis, when the greatest quantity of active oxygen is present, one would expect that those phases of the *Leucocytozoon syphilidis* which are most easily killed by oxidation would be able to flourish, that the customary therapeutic action of salvarsan would not be witnessed, and that the symptoms might even be temporarily aggravated. This is exactly what happens clinically. The spirochætæ are not killed quickly by intramine, and, if salvarsan is given directly afterwards, it appears to have no immediate influence on the symptoms.

If the metallic compounds are allowed their full play first, as the catalysts of the oxidase system, the introduction subsequently of a strong reducing agent, like intramine, in virtue of its action, sets free sufficient peroxide to be subsequently converted into active oxygen by the metallic compounds, thereby preventing the adverse action of the mercaptan-protein. This being the case, one would expect that the phases of the *Leucocytozoon syphilidis* would be quickly vanquished, and that the symptoms of the disease would rapidly disappear. Once again this is exactly what happens clinically.

If metallic compounds be first administered in early syphilis the spirochætæ are killed at once and the symptoms improve. If intramine be now injected some of the other phases are killed immediately and the symptoms vanish at a very much quicker rate than they otherwise would have done. I need only cite one case to show the truth of this:—

*Case I.*—A patient had a typical Hunterian chancre in the corona; there was a moderate loss of surface, and the sore was markedly indurated. I gave an intravenous injection of galy (40 cgm.), and three days later an intramuscular injection of intramine (3 grm.). There was practically no change in the chancre when the intramine was given, but a few days later not only had the sore completely healed, but the induration had entirely vanished.

I have seen severe papular syphilides, scarcely influenced by galy alone, as the following case shows, disappear in a few days when intramine was injected:—

*Case II.*—The patient had a primary sore in the corona, hard infiltrative œdema of the whole of the penis, and a generalized papular syphilitic eruption.

No improvement followed three intravenous injections of galyl; instead, more papules appeared. Within ten days after one injection of intramine every lesion had vanished.

In the recurrent and late stages of syphilis, the exact opposite to what has just been said would be expected, since in these stages the reducing action of the resisting substance is more powerful than the oxidizing action. One case will suffice to prove this:—

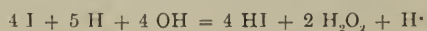
*Case III.*—A patient had a gumma which encircled the corona and spread on the under surface of the penis on to the scrotum. In less than one week after receiving only one intramuscular injection of intramine (2 grm.) the ulceration had completely healed. If this case had been treated with salvarsan alone it would have taken about three weeks for the sores to have healed up, and during this time it would have been necessary to have given two or three intravenous injections.

I have treated with immediate success by intramine late syphilitic lesions which had remained practically uninfluenced by salvarsan, with mercury and iodides subsequently. I must cite a case which bears this out to the letter:—

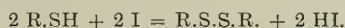
*Case IV.*—A man, aged 70, had syphilitic glossitis for twenty-five years. Eighteen months before he was given intramine he had two intravenous injections of salvarsan, and these were followed by three courses of mercury and iodides, with practically no improvement in the clinical condition. When an intramuscular injection of intramine was prescribed (2 grm.) the tongue exhibited the following features: There was an ulcer in the centre about the size of a sixpence, with a lateral fissure running from it on both sides. Just posterior to the ulcer a hard wart was situated. The movements of the tongue were impeded and very painful. In a few weeks after the injection, the ulcer had completely healed, the fissures and wart had vanished, the natural movements of the tongue and its suppleness had returned, and, in the words of the patient, "my tongue has never been so well as long as I can remember."

### *Iodine.*

While on the subject of reducing agents, I might as well explain how iodine acts. Iodine has a more marked therapeutic action in late than in early cases of syphilis for the same reason that intramine has. Like intramine, iodine adsorbs hydrogen and hydroxyl ions to form peroxide for the oxidase system, as in the following equation:—



The preparation of organic compounds of iodine, possessing a therapeutic action at all akin to intramine, is impossible, owing to the fact that iodine is a monovalent element. Iodine increases the action of intramine, presumably because it is readily adsorbed by a mercaptan group, thus checking the deleterious action of this group. The following formula shows the interaction:—

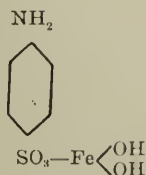


Therefore, when prescribing intramine, I usually order iodine beforehand. To obtain the maximum effect with iodine, it is best to give intravenous injections of colloidal iodine. Colloidal iodine can also be injected intramuscularly, and it can even be taken internally. Colloidal iodine has none of the disadvantages of the potassium salt, it is not depressing, and patients do not show an idiosyncrasy towards it. Almost any quantity can be injected intravenously and as often as is considered necessary. I usually inject 100 c.c. to 200 c.c. of collosol iodine, which contains 1 in 500 of Crookes's colloidal iodine.

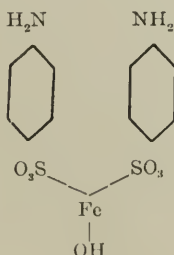
#### METALLIC COMPOUNDS.

##### *Iron.*

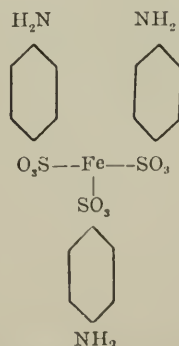
As iron seemed to me to be the natural peroxidase, and having obtained such good results with intramine, a drug which increases the natural perhydridase, I anticipated good results from the use of an organic iron compound, and my anticipations have fortunately in part been realized. The iron bodies prepared were the para-amino-di-hydroxy-ferric-benzene sulphonate, the di-para-amino-hydroxy-ferric-benzene sulphonate, and the tri-para-amino-ferric-benzene sulphonate, the formulæ of which are as follow:—



Para-amino-di-hydroxy-ferric-benzene sulphonate.



Di-para-amino-hydroxy-ferric-benzene sulphonate.



Tri-para-amino-ferric-benzene sulphonate.

Of the three compounds just mentioned, the tri-para body appears to be the most suitable. It is a crystalline body, soluble in water, to which it imparts a deep orange colour. Ferrivine, as this compound is commercially called, is non-toxic and it is supplied as a 1 per cent. solution—i.e., 100 c.c., or 3.5 oz., contains 1 gm. of the salt. The solution is stable, it does not oxidize in the air, and if desired it can be diluted with distilled water. The dose suggested may be repeated without risk at frequent intervals. The dose I usually employ is 1 gm., but I have injected 2 gm. or 3 gm. The only disadvantage which I have so far experienced with big doses of ferrivine is that the patient sometimes feels faint after the injection. Recovery sets in in a few minutes, when the patient can do as he likes. Occasionally typical shock or collapse immediately follows the injection, due to a temporary upset of the normal hydrogen ion concentration of the serum. I am in the midst of experiments in the hope of overcoming this disadvantage, the results of which up to the present point to success.<sup>1</sup>

#### PRACTICE OF CHEMOTHERAPY.

It would be as well now to give a sketch of the clinical value of these new drugs, and to state how they should be used with the already known remedies in the treatment of syphilis.

Ferrivine acts like salvarsan, and, owing to its non-toxicity, larger doses can be given, which may be repeated at shorter intervals, even daily. Generally speaking, the symptoms of primary and generalized syphilis disappear within four days after the second or third intravenous injection. Induration in chancres and those dense, infiltrated, brownish papules are not so resistant to ferrivine as they are to salvarsan; and if intramine be prescribed after the second or third injection, the hardest induration and the most resistant papule will vanish within a fortnight. Owing to the iron not being in a sufficiently adsorbed condition, its therapeutic action is not regular. Experiments are in hand to prepare a more strongly adsorbed iron compound.

In the recurrent and late stages of syphilis, intramine should always be prescribed first, when the symptoms vanish in a few days. In cases of cerebrospinal meningitis the pains disappear within twenty-four hours; in degenerative myelitis the subjective symptoms are often improved, and I have also seen considerable benefit follow intramine in cases of degenerative encephalitis.

<sup>1</sup> While correcting these proofs I am able to say that the disadvantage has been entirely overcome.



I will describe a case of degenerative encephalitis treated with intramine, as it shows so well what can be done by combining reducing and oxidizing remedies:—

*Case V.*—The patient was suffering from the depressive type of degenerative encephalitis; he showed no change of facial expression; would not speak; he had no control over his sphincters; and was at times violent. I prescribed two intramuscular injections of intramine with the most extraordinary improvement—the facial expression returned within a few days and the patient entered into conversation. Hoping that the condition might be still further improved, a third injection was given, with the result that the patient became worse than he had ever been. I had suggested that after the second injection of intramine a metallic compound should be administered, but as the medical man in attendance wished to see what intramine alone would do it was not prescribed. Seeing that the patient had become worse after the third injection, which was what I expected, I hazarded the opinion that if a metallic compound were given now his improvement would be sudden and greater than on the first occasion. An intravenous injection of galyl was administered, with the result that in forty-eight hours' time the patient became a rational individual. For how long the improvement is going to be maintained I cannot say, but this case is especially interesting to me as a proof of my theory of oxidation and reduction. More than two months have elapsed since the patient had his last injection, and a steady improvement has been maintained. The patient has now returned to his business. Before he came under treatment he could not write.

I have had two other cases in which as marked an improvement has been obtained. With salvarsan I have seldom seen a case of degenerative encephalitis improve, and I have seen many get worse.

Just as the maximum therapeutic action is obtained in early syphilis if intramine follows the administration of a metallic compound, so is it the case if in late syphilis one or other of the metallic compounds follow intramine. I have treated some cases of cerebrospinal meningitis first with an intramuscular injection of intramine, then with two intravenous injections of a metallic compound, and, finally, with mercury and iodides, with the very greatest success; so much so that I think such patients are better advised to have the treatment just outlined in preference to intraspinal injections of salvarsanized serum. Owing to the non-toxicity of intramine and ferrivine, serum withdrawn after their administration and injected intraspinally has a most beneficial influence upon central nervous lesions, superior to that of salvarsanized serum, and, moreover, no toxic symptoms occur, as are too often witnessed when galyl and the other arsenical substitutes are used. The following case is one in point:—



*Case VI.*—A man, aged 34, came to me complaining of double vision. He had contracted syphilis five years previously, and had been treated thoroughly with mercurial inunctions and injections before the symptoms of the generalization stage manifested themselves. The patient also had nine intravenous injections of salvarsan at various periods, but had never had another symptom of syphilis. On examination, I found that there was a unilateral partial third nerve paralysis, the pupils were unequal, and the reflexes were sluggish. The patient had no knee-jerks, and there were areas of diminished tactile sensation. I elicited from the patient that he had lost much weight and that all sexual desire had vanished in the last few months. I gave him three intramuscular injections of intramine, alternating them with three intravenous injections of aluvine. After each injection I gave an intraspinal injection of the prepared serum. The cerebrospinal fluid when first examined exhibited the typical features of degenerative myelitis; after the fifth injection it was practically normal, and by this time all the objective and subjective symptoms had vanished. The double vision disappeared after the first injection of intramine, showing clearly that the cranial nerve lesions in degenerative myelitis must be of meningeal origin, commencing in the meninges covering the nuclei in the base of the brain.

I have recently had a case of degenerative myelitis, the chief symptom being that the patient could not walk without the aid of a stick. After one intramuscular injection of intramine and two intravenous injections of ferrivine, the gait so improved that the patient was able to walk without a stick.

Interstitial keratitis, a lesion uninfluenced by salvarsan, has cleared up in some cases in a most marvellous way with intramine. In those not immediately improved mercury has acted beneficially afterwards.

In outlining the treatment I now adopt in the various stages of syphilis, there are two very important questions which arise, the correct answers to which it is necessary to find, in order that we may treat our patients in the best way possible:—

(1) Assuming that in the doses given the oxidizing effect of salvarsan and ferrivine is equal, is that effect going to be enhanced by giving an injection of each, or is the effect going to be the same if three injections of either one are prescribed? Theoretically, I should doubt whether any difference in effect would be produced, but whether this is the case practically, remains to be seen—a point by no means easy to determine.

(2) If we fail in curing a case of syphilis at once, are we right in continuing treatment after the disappearance of the symptoms, or might it not be better for the patient to be treated only symptomatically?

The syphilitic parasites are attacked first and foremost by the

resisting substance—i.e., the lipoid-globulin molecules in the serum. The resisting substance has an oxidizing and reducing action, actions which we increase by salvarsan and ferrivine, on the one hand, and by intramine and iodine on the other.

An increase in size of the lipoid-globulin molecules, produced by the administration of the synthetic remedies just mentioned, results in a breaking down of the molecules, ultimately with a diminution in the amount of the resisting substance. Therefore, the continued administration of powerful oxidizing and reducing remedies might possibly be expected to result in so little of the compound being adsorbed that their oxidizing and reducing actions would be reduced perhaps to *nil*. Seeing that both oxidation and reduction are catalytic reactions, it might be possible theoretically that only the minutest traces of the metallic hydroxide and the disulphide are necessary to continue the good work. Since mercury acts mechanically and probably not as a peroxidase at all, and considering that its action is entirely dependent upon the size and adsorptive capacity of the lipoid-globulin molecules, it might be assumed theoretically again that the administration of mercury, once the properties of the resisting substance have been reduced, would be entirely superfluous.

Ever since the advent of salvarsan, I have been struck by the large increase of cases of meningeal syphilis, and the very frequent occurrence of lesions, in spite of the fact that the patient was well under the influence of mercury. I have also seen many cases in the latent stage precipitated into a condition of degenerative myelitis and degenerative encephalitis by the administration of our powerful remedies. So far as dealing with patients in the latent stage is concerned, I have for some time made up my mind never to prescribe treatment if after a thorough clinical examination I found no symptom. I have also long since made up my mind to treat symptomatically only those patients who have once entered the recurrent stage; but as to how a case in the generalization stage should be best treated, a little further discussion is necessary.

To make the subject clear, I will picture the body as consisting of two parts: (1) the systemic part; (2) the nervous. The nervous part is largely dependent upon the systemic part for its resisting substance.

If the resisting substance in the systemic part has been reduced by treatment, spores which are resistant to treatment may possibly be prevented from developing if small doses of the remedies are continually being prescribed. The rule is far from being invariable. Against this,

one must consider the frequency of recurrences when mercury and iodides are not prescribed after a course of several injections of salvarsan, and how the protective action of mercury and iodides compares with that of the resisting substance when working alone.

Systemic recurrences are far more common when salvarsan is not followed by mercury and iodides, but the frequency of the same and as to when they appear depend upon the number of injections given. If three or less injections of salvarsan are given, the recurrences are more frequent than when six or more injections are given in one course of treatment, but they appear much later. Usually, twelve or eighteen months have elapsed before they make their appearance, while, after a course of several injections, a recurrence may appear in two or three months. Unless the individual has the opportunity of observing many cases, this somewhat paradoxical result may easily lead to false conclusions being made.

The explanation of this disparity as to the time at which the recurrences appear is as follows: When three injections of salvarsan are prescribed, the resisting substance is scarcely reduced, with the result that it continues exerting its power until the parasites ultimately get the upper hand, which is hardly ever under twelve months. When several injections have been administered, the resisting substance is so reduced that the spores which have not been killed by the treatment will be able to develop on a soil which is the same to them as a new soil; consequently, symptoms will appear after the usual incubation period; therefore, two or three months later.

The early syphilitic lesions are far less dangerous, and cause much less local damage, than late syphilitic lesions; hence the systemic recurrences, after a course of several injections, are preferable to those which occur when only three injections have been made, since the former produce fewer degenerative changes, especially in the vessels, which in most lesions are primarily attacked.

Therefore, a course of several injections produces better results than a course of a few injections, and since the recurrences after both are fewer in number when the injections are followed by mercury and iodides, these two drugs should not be omitted in the treatment of syphilis.

As mercury and iodides can have such an infinitesimal action when the resisting substance is reduced, might it not be better for the patient after a course of several injections to wait eight to twelve weeks before taking mercury? Eight to twelve weeks is about the time the resist-

ing substance takes to get back to its normal state. Now mercury and iodides have nothing like the influence in altering the state of the resisting substance that the strongly adsorbed compounds possess, and since both may have some power in preventing the spore from developing, it would seem wiser not to allow any interval to elapse between stopping the synthetic metallic compounds and commencing the mercury.

A question which is impossible to answer is how long the mercurial treatment should be maintained. A correct answer could only be given if we knew that a spore would not develop if it had been dormant for a certain number of years. A spore may remain dormant for fifty years, and then redevelop and cause symptoms. Therefore a definite answer will never be forthcoming. Furthermore, we do not know what influence treatment has upon dormant spores, nor do we know whether treatment postpones their recrudescence. In this uncertainty, it is best to let our clinical experience guide us, and to give one year's treatment when the case of syphilis is in the primary stage, and two years' treatment in the generalization stage.

Owing to the fact that the nervous part is dependent for its protective substance upon the systemic part, it will be readily understood that if the organisms have reached the nervous part before treatment has commenced, they will be able to develop therein the more readily the quicker the resisting substance in the systemic part has been reduced, provided at the same time the organisms in the nervous part are not vanquished by the treatment.

It will follow, therefore, that the incidence of cerebrospinal meningitis will be greater when many injections of salvarsan have been prescribed than when a few, since it is difficult in every case to make injections sufficient to kill the organisms in the nervous part. Cerebrospinal meningitis is a curable condition, and, moreover, its occurrence practically precludes the patient from getting a degenerative lesion later.

Now let us consider what happens to the nervous part if only a few injections of salvarsan are administered. The resisting substance is not strong enough to kill the parasites, but strong enough to prevent them developing sufficiently to produce symptoms. In this state, the spores can and do migrate into nerve tissue proper, where they meet with still less resistance, with the result that now any symptoms which appear will be degenerative in character, and, therefore, more or less incurable. Once again we see that a course of several injections is preferable to a course of only a few.



The onset of nervous lesions is naturally dependent upon the number of organisms that reach the meninges before treatment is begun. Cerebrospinal meningitis is much more common in patients who have severe generalized symptoms than in those in whom the so-called "secondaries" are slight. For all practical purposes, it may be said that a ratio exists between the severity of the early cutaneous eruptions and the occurrence of early cerebrospinal meningitis. As the symptoms of cerebrospinal meningitis almost invariably occur between eight and twelve weeks after the last injection of salvarsan, it is wise to give every patient in the generalization stage an extra course of powerful treatment round about this time.

What effect the introduction of intramine is going to have upon the points just raised I am not in a position yet to say, but one thing is clear—namely, that the administration of intramine reduces the number of the long course of the metallic compounds by about half, and increases the result produced.

In the primary stage I prescribe first of all collosol iodine and inject intravenously a large dose of a metallic compound, which I repeat one or two days later. Three days after the second injection I give an intramuscular injection of intramine (1 gm. to 2 gm.), and between the fourth and seventh day after, I inject intravenously another maximum dose of a metallic compound, and continue mercury, intramine and iodides for one year, as follows:—

- (1) Eight fortnightly intramuscular injections of grey oil, 2 gr. to 3 gr. of mercury.

- (2) Eight fortnightly intramuscular injections of intramine, 1 c.c. to 3 c.c.; mercury and intramine alternately. By injecting mercury and intramine alternately, the former is prevented from producing salivation, tender gums, and local accumulations in the muscles.

- (3) One month in every three, collosol iodine should be prescribed. The first month of iodine should be commenced after four doses each of mercury and intramine have been injected.

- (4) One month's rest should follow every eight injections each of mercury and intramine.

In the generalization stage, I prescribe collosol iodine at once, and two or three intravenous injections of a metallic compound both before and after one or two intramuscular injections of intramine, allowing two days to intervene between each intravenous injection. I then give four courses of the mixed mercury, intramine and iodine treatment, and, two months after the last intravenous injection, I inject intravenously a



maximum dose of a metallic compound before and after a large intramuscular injection of intramine.

In all recurrent and late cases, I give first of all one or two intramuscular injections of intramine, then one or two intravenous injections of a metallic compound, and follow up the same with one course of the mixed treatment. This treatment will also do for cases of non-degenerative meningo-encephalitis and myelitis. In cases of degenerative encephalitis and myelitis, in which more drastic treatment might be expected to give more satisfactory results, I make the following :—

(1) An intramuscular injection of intramine.

(2) Four days later, an intraspinal injection of prepared serum which has been drawn off on the third day.

(3) Three or four days later, an intravenous injection of a metallic compound. The metallic compound should not be galy, kharsivan, neo-kharsivan, or any of the other French substitution products for salvarsan, as they are too toxic.

(4) Three days later, an intraspinal injection of prepared serum, which has been collected the day before.

The injections of the non-metallic and metallic compounds should be continued alternately, until the cerebrospinal fluid is normal or closely approaching the normal.

Women who are pregnant should be treated as first described under the heading of the generalization stage, and two courses of the mixed treatment should be given without any pause between. Extra injections at the second month are unnecessary.

Congenital syphilitics should have the mixed treatment for two years, with the exception that the mercury should be administered *per os* and continuously. All recurrent symptoms should be treated symptomatically and intravenous injections prescribed if practicable, as suggested in recurrent and late cases of acquired syphilis.

Although I have not yet used ferrivine on quite such a large scale as intramine, I have made sufficient injections to state the fact that the need for the use of a toxic metal like arsenic will, it is to be hoped, soon no longer remain. Ferrivine is in some cases superior to salvarsan: I had under treatment a man with three chancres on the penis; ferrivine was injected on a Tuesday, and by the following Saturday the sores had healed; in other cases it is certainly inferior. The drug is non-toxic, consequently, bigger doses may be given, and they may be repeated at shorter intervals.

Ferrivine can be improved upon, and I am now undertaking

experiments with that end in view. I am confident that in time an iron compound will be prepared which will be in every way superior to salvarsan.

So far as the oxidizing agents are concerned, there is nothing further to be said, but as regards the reducing agent, intramine, I must say a few more words, as its introduction into our therapeutic armamentarium is going to have not only a very marked effect in combating protozoal diseases, but also all chronic infections.

No drug so far known has had much effect upon lupus. Dr. Adamson has treated five cases with intramine with the most striking results, even after only one injection. All ulcerative areas heal up in a few days, and the large granulomatous masses become flush with the surrounding surface. As to whether intramine only temporarily benefits lupus or cures it, the future will show. Chronic ulcers of the leg, which have been treated for years without improvement, immediately begin to mend when intramine is injected. Intramine is useful in chronic gonorrhœa, and it possesses very strong antiseptic properties. It is just possible that intramine may have a place in the treatment of malignant disease, and it is practically certain that in leprosy it will prove invaluable.

Before closing, I must refer to one or two other points which are not only of practical interest but which also strongly support my oxidation-reduction theory.

Chilblains probably arise owing to insufficient oxygenation of the tissues in the extremities. If this be true, metallic compounds applied locally to the particles should relieve the condition. The metallic compounds must be in the colloidal form, because with friction and the Brownian movement of the particles it is possible to get some of the particles into the tissues. If colloidal silver (*collosol argentum*) be rubbed into the chilblains, one application is sufficient to get rid of them. I have treated two bad cases in this way with marked success.

Colloidal silver in the form of suppositories has also a beneficial action on piles, because the Brownian movement of the silver particles still continues, even in the depth of the gelatine with which the suppositories are made.

If fresh or acute ulcers are examined microscopically and stained with pyronin and methyl green, it will be noticed that nuclei predominate, and that the whole section looks as if it has only taken the methyl green. Methyl green is avid for active oxygen; hence it is clear that the cells in acute ulcers are rich in active oxygen. Applications

of colloidal metallic compounds to such ulcers greatly accelerate their healing. If chronic ulcers are treated in the same way, the avidity of the cells for pyronin is as marked as was the avidity for methyl green in the previous case. Pyronin stains reducing foci, as it exhibits a strong affinity for the peroxide. Applications of colloidal non-metallic compounds cause such ulcers to heal immediately. One application of intramine to a chronic soft sore which had persisted for two months healed it in forty-eight hours.

I have treated other chronic ulcers with intramine with the most extraordinary results. The action of intramine is much more rapid than that of scarlet red, and I should imagine it will prove of much greater value than the Simpson light. Another great advantage possessed by intramine when applied locally is that it causes a great accumulation of lymph around the area affected. Owing to its lymphagogue action, intramine might possibly prove useful in the treatment of old bullet and shrapnel wounds.

#### CONCLUSION.

The theory that diseases are combated by drugs which simply increase the amount of oxygen directly or indirectly is, I think, fully proved, with the result that treatment is now put upon a logical basis and empiricism is removed. The paths opened up for future research are infinite, the treading of which must always lead to some substantial progress being made, and not to a further penetration into darkness, the direction taken by so much of the research work done in medicine in recent years.

The theory of oxidation and reduction has not only explained directly the *modus operandi* of chemotherapy, but also indirectly that of immunity, with the result that the rationale of dyeing and staining, of the immunity reactions, and of anaphylaxis is rendered clear. We should now be able to discard the hundreds of coined words which have never served a useful purpose, and to replace irrational by rational tests.

Further details of the subjects referred to will be found in my Hunterian Lectures.<sup>1</sup>

<sup>1</sup> "Links in a Chain of Research on Syphilis (Oxidation and Reduction)," Harrison.

## DISCUSSION.

The PRESIDENT: I express the thanks of the meeting to Mr. McDonagh for his paper upon a subject which is of the greatest importance, and is especially so at this time. I regard the detailed description of the investigations made, with illustrative cases, as evidence of the author's desire that the whole subject should be fully discussed by those present who are competent to do so in order that the most efficient treatment of syphilis, and possibly other constitutional diseases, may be secured.

Professor W. M. BAYLISS, F.R.S.: This paper is one that contains much important and suggestive matter, but is very difficult to criticize. I find in the physico-chemical hypothesis much that conveys no very definite meaning to me, while the statements appear to be contrary to the knowledge which we possess. To begin with, I think it unfortunate that the phenomena of oxidation and reduction should happen to form the basis of the main thesis. Although these phenomena are undoubtedly of the greatest importance, their mechanism in the living cell is, as yet, one of the most obscure in the whole of physiology. There are, indeed, enzymes which result in the production of active oxygen and active hydrogen respectively, but we do not yet know precisely what these agents are. Again, while, of course, any oxidation of one substance is accompanied by the reduction of another, the same substance cannot be oxidized and reduced at the same time, as seems to be implied in the author's conception of a lipid globulin as an oxidase-reducase system. But I admit that I have possibly misunderstood the statement. There is no evidence, so far as I am aware, that oxygen has any necessary connexion with adsorption, which is purely a result of molecular forces at boundary surfaces. The possibility of a lipid globulin must be admitted, but I fail to find any experimental evidence brought forward to show that it plays any part in the phenomena of chemotherapy. The phrase "permeability of molecules" conveys no meaning to me. When a membrane is permeable to a particular substance, this substance is not supposed to pass through the molecules of the membrane, but between them. Again, I fail to grasp how the valency of an element can play the part assigned to it. The peculiarity of an organo-metallic compound is that it is not electrolytically dissociated. Hence electrically charged ions are not concerned in its activity. It seems rather more probable that the properties of the metal itself are required, and that the function of the organic part of the molecule is to bring it into close relation with some constituent of the cell, but not necessarily by means of special receptive molecular groupings. Why arsenic should have so powerful a toxic action is almost as impossible to say at present as why its chemical properties differ from those of lead or zinc. I should like to say in conclusion that any doubts expressed as to the validity of the theoretical basis of the author's treatment of syphilis do not necessarily detract from its possible practical value, the importance of which requires the most careful testing.



Dr. C. H. BROWNING : I desire to associate myself with the views expressed by Professor Bayliss in regard to the highly speculative discussions in Mr. McDonagh's communication. Professor Bayliss is universally recognized as one of the leading authorities on the chemistry of those processes which Mr. McDonagh invokes in order to form a basis for his chemotherapy ; accordingly one must attach the greatest weight to Professor Bayliss's expression of opinion on the theories advanced by the author of to-night's communication. However useful such lines of thought may have been to Mr. McDonagh himself, I doubt whether, in their present phase and without rigid experimental proof, they will contribute materially to the understanding of the subject by others. These considerations, however, do not detract from the therapeutic results, which are of outstanding interest. To be able to clear up, with certainty and rapidity, the lesion of interstitial keratitis in congenital syphilis is an achievement of the first magnitude. It is highly interesting that Mr. McDonagh should have attained this success with an organic sulphur compound which has been known for a very long time—upwards of thirty years. As regards the question of the relationship of chemical constitution to therapeutic action, it is a striking confirmation, to my mind, of the general soundness of Ehrlich's work that this sulphur compound which, in its molecular configuration, bears so close a resemblance to salvarsan, with sulphur substituted for arsenic atoms, should likewise exert a marked action on syphilis. With regard to Mr. McDonagh's opening remarks on the rationale of chemotherapy, I desire to draw attention to a grave misapprehension which might arise in the mind of readers. Ehrlich, to whom all subsequent workers in this domain must remain for ever indebted, did not, to begin with, devote his attention exclusively, or even principally, to the investigation of arsenical compounds. The curative action of the non-arsenical, sulphur-containing dye, methylene blue, in malaria was his first observation, and the discovery of trypan-red, also a non-arsenical substance, marked the commencement of the systematic investigation into synthetic antiparasitic drugs. It was only after the therapeutic action of the organic arsenical compound, atoxyl, had been demonstrated by Thomas and Breinl, of the Liverpool School of Tropical Medicine, that Ehrlich concentrated his attention on the investigation of this group of arsenical derivatives. Salvarsan was the logical outcome, but the logic was that of genius. That salvarsan does not by any means mark the culminating point of this work is proved, *inter alia*, by to-day's communication. That arsenical compounds did not, even for Ehrlich, constitute the acme of chemotherapy is shown by the potent non-arsenical, acridin derivative "trypaflavin," which was synthetized under his guidance, and also by one of his latest discoveries, the complex copper-salvarsan compound, the work on which was interrupted by his death. His experiments showed this compound to be many times more active than salvarsan itself in trypanosome infections.

Dr. F. H. TEALE : I ask, what evidence has Mr. McDonagh that his so-called phases of the "leucocytozoon" of syphilis consists mainly of lipoid



globulin "molecules"? How has he demonstrated these "molecules," even in the definitely established spirochæte, and still more so in the highly problematic "spore stages" of the leucocytozoon? I would draw attention to the fact that the lipoids only form loose combinations with proteins, and therefore only loose chemical compounds and not molecules result from their combination. On what evidence does Mr. McDonagh conclude that the lipid globulin molecules in syphilitic infection are larger, more numerous, and have a greater absorptive capacity than normal "lipid globulin molecules"? There is no evidence that lipoids play any essential part in the production of antibodies. Specificity cannot be explained by homologous stereochemical molecular configuration of the lipid globulins, since, experimentally, an antigen may be completely deprived of its lipid constituents and the resulting antibody production be in every way the same as if the native antigen had been used, and, vice versa, an antibody, by careful treatment, can be deprived of its lipoids without its properties being affected by such treatment. Mr. McDonagh also states that the reducing action of the spirochætal lipid globulin is greater than that of the lipid globulin of the host, owing to the presence in the former of a non-completely saturated fatty acid. How has he arrived at this conclusion, and has he compared the saturation of the fatty acids of the lipoids of the spirochæte with those of the host, which are known to have a high iodine number?

Mr. JOHN WARD: A very great point in favour of Mr. McDonagh's synthetic compounds is his selection of those containing iron and sulphur. In 1911 the late Mr. Henry Crookes exhibited a number of photographs before the Royal Society showing the action of various metals and non-metals on bacteria. By these photographs it was shown that arsenic was the most powerful bactericide of all the elements, with the possible exception of antimony. It follows that arsenic is also the most powerfully organotropic of the elements. With regard to the action of salvarsan, galyol, intramine, &c., whether the parasites are killed by adsorption of the particular compound or not, it is certain that the element employed is eventually deposited in the tissues, probably in the colloidal condition. Ullmann showed by X-rays that after the injection of salvarsan intramuscularly arsenic persisted in the tissues from a few weeks to many months, and even when injected intravenously it took a long time to eliminate. Is it not probable that the increase in cases of degenerative encephalitis and myelitis may be due to the catalytic action of arsenic in the nerve tissue? It will be seen from the photograph shown that iron is practically non-bactericidal, and therefore probably non-organotropic. Sulphur, far from being bactericidal, exerts a stimulating influence, and therefore may reasonably exert a stimulating influence on the organic cells and tissues. Indeed, it is well known that the arteries and veins contain nearly 1 per cent. of sulphur. With regard to the point raised of oxygen being necessary for adsorption, it is certain that the cells must be in a healthy condition for this to take place, and for this oxygen is, of course, necessary.

Comparison should not be made with a rigid structure like charcoal, the specific surface of which cannot vary. Mr. McDonagh's theory of oxidation and reduction seems to offer an explanation of many phenomena which have hitherto seemed enigmas. It is quite reasonable to suppose that the chemical bodies contained in the body of the spirochæte vary in constitution—compare the constitution and amount of alkaloid contained in plants at various times of the year and it is very highly probable that a different chemical reaction should be necessary for their death. The ultimate end of either a reducing or oxidizing agent is of course oxygen, and before closing I should like to mention the effect of colloidal iodine in recovery from alcoholism. The alcohol probably forms a film over the cell tissues, causing considerable atrophy with consequent depression. The iodine combines with the alcohol to form ethyl iodide and oxygen, thus restoring the cell to its normal condition. The action of iodine in rheumatism is probably similarly explained.

Sir MALCOLM MORRIS: I am of the opinion that it must be definitely decided as soon as possible whether the drugs referred to possess advantages which render their general use necessary or not. This is not the moment when there should be a difference of opinion in the profession as to the best drugs to be employed for combating a disease which is rapidly increasing.

Lieut.-Colonel L. W. HARRISON, R.A.M.C.: Since most of my remarks will relate to the clinical side of the question, to be discussed at an adjourned meeting, they had better be deferred till then. What I intended to say on the chemical and biological section of the paper has already been said much better by other, more competent, speakers. I shall therefore on this occasion only touch on one point. Mr. McDonagh puts it mildly when he states that the pain from an injection of intramine renders the patient *hors de combat* for two or three days. I have had an opportunity of seeing some patients in my hospital who have received injections of intramine, and the pain was described as horrible. I can liken it only to that which follows an intramuscular injection of "606." As to the induration disappearing, this is far from being the case in these patients, who still, three to four weeks after the injection, have very marked induration at its site. Whether intramine is of any use or not I cannot say, but I am quite certain that its effects will have to be rendered much less painful before I can recommend its general use in the Army.

Captain C. H. MILLS: In support of Colonel Harrison's remarks concerning the local reaction following upon the intra-muscular injection of intramine, I cannot do better than describe a case in which, only this morning, I opened an abscess resulting from the administration of this drug four weeks ago. The pain during the week following the injection was very severe, the patient, who was confined to bed, was unable to lie upon the side on which the injection had been made. Definite necrosis of muscle-fibres had ensued—a well-defined cavity resulting—from which a yellow "mucoid" fluid was evacuated. There

was no evidence of sepsis. A tag of partially necrosed fascia protruded from the incision. In three other cases in which treatment with intramine was commenced at the same time as the case just described, absorption of the drug is not yet complete, as can be evidenced on palpating the site of injection. In each case two injections of 1 grm. were administered at an interval of a week. In every instance a definite "gap" can be felt in the body of the muscle where necrosis has ensued. I cannot attribute such unsatisfactory absorption in each of the eight injections to any error of technique. Each injection was made slowly and strictly intramuscularly, tension being avoided. Massage was employed for the three succeeding days. The pain following injection was comparable with that produced in the early days by the old intramuscular injection of salvarsan.

Mr. McDONAGH (in reply): None of the views expressed either disproves my theories or upholds those of Ehrlich. I am surprised that, in view of the scientific knowledge possessed by the various speakers, so much of it has been used for destructive criticism and none for constructive purposes, and it is particularly to be observed that any reference to the questions asked at the beginning of the paper has been avoided—proof that the general opinion of Ehrlich's theories is that they are illogical. It is impossible to supply in a few minutes the various proofs asked for, especially by Professor Bayliss, as to do so would entail a recapitulation of the research work done during the last four years. Moreover, many of the doubts raised would not have found expression had that research work been studied. The main object of the paper has been to show that arsenic, a toxic metal, is not essential, and that its place can be taken by iron and sulphur. Ferrivine and intramine were not discovered empirically, but as the result of an ordered sequence of experiments which showed not only that the action of iron and sulphur is mutually complementary, but also that their introduction into the body simulates and accelerates processes which are normally taking place. The clinical cases reported show that the drugs mentioned possess a powerful therapeutic action, but that, being new, there attach to them disadvantages which, in time, can be easily overcome. It is doubtful whether those who decry the value of intramine owing to the pain it causes are familiar with its use. Even in the worst cases the pain cannot be compared with that produced by salvarsan when it first came in. I have administered over 200 injections of intramine without any ill-effects, and have obtained results which, in my opinion, prove that it should be used in every case of syphilis.

[The Discussion of Mr. McDonagh's paper from the clinical standpoint was postponed to June 1.]

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(May 18, 1916.)

Case of Parakeratosis Variegata of the Type described by Juliusberg as "Pityriasis Lichenoides Chronica."

By E. G. GRAHAM LITTLE, M.D.

THE patient was a young girl, aged 18, one of three children, and the sole member of the family with any skin disease. She had shown no signs of this until August, 1908, when she was spending a holiday at Felixstowe. During one night a vivid red rash appeared quite acutely on the legs and arms, which was ascribed to the patient having bathed in the sea when she was hot. A medical man summoned immediately to see the patient seems to have regarded it as possibly measles, although she had had this disease four years previously. From this beginning the eruption spread continuously but slowly, until within about two years the entire surface was affected, with the exception of the nails. The scalp was very scaly but the hair has not fallen out. Two years later, at the age of 13, she began to have epileptic fits, which have persisted up to now, and for these she is being treated with bromide. She began to menstruate when aged 11, three months before the rash appeared. There was, and has been, singularly little interference with the general health, and she has a good appetite, is well nourished and active, and sleeps well. She has no itching and has never complained of this symptom at any time; she is fond of, and has some skill in, painting and designing. The epileptic fits seem to occur about the times of menstruation, which is normal and regular in frequency and duration. The fits are fairly controlled by bromide treatment, and she is enjoying



longer intervals of freedom, intervals of four months being not uncommon.

In 1911 she was seen by my friend, Dr. Leslie Roberts, in Liverpool, who has been kind enough to send me this note of her condition when he saw her: "The patient, aged 14, was seen by me on February 24, 1912. She was a very nervous person with a history of epilepsy. She was suffering from universal pityriasis. From my notes taken at the time I must have been under the impression that the disease was not the pityriasis rubra of Devergie, but that it sprang from a seborrhœic base. The post-aural spaces were affected and in the region of the forearms the disease was tending to pass into dermatitis. She also improved considerably under treatment between February 24 and May 15, 1912. From these data I conclude that the disease in 1912 was not parapsoriasis."

Present condition: The eruption is very nearly universal, in the form at present of a fine branny lichenoid desquamation, with points of accentuated redness as compared with the bluish-red general tone, these points constituting a very indefinite maculo-papule. There is a shadowy effect of meshwork. The face is desquamating and red in blotches, not entirely all over. The palms and soles are hardened, the nails unaffected. Over the extensor surfaces of the joints, elbow, wrist, dorsum of hand, knee and ankle, there is much thickening, rather like that of pityriasis rubra pilaris, but there is nowhere any resemblance to a follicular papule, so that this diagnosis can be excluded. The forearms and arms, from wrist to axilla, are especially bluish, almost cyanosed, but there are nowhere telangiectases. The mucous membranes are normal. The colour of the eruption seems to vary, and at times assumes a vivid blotchy redness like measles, for which, in fact, as has been stated, it was mistaken at its inception. Silvery fine scales are freely shed, and in some positions, such as in the meatus of the ears, form heaped-up masses which block the orifice. The thyroid is probably small and is not palpable. There are some enlarged glands in the neck. The teeth are very decayed and faulty. The pulse at the time of examination was 80, somewhat small and compressible. For the past two years she has used very persistently and freely an ointment containing 10 gr. of ammoniate of mercury to the ounce. Since the full development of the eruption—that is, since about the age of 13—there has been no change in the rash except, as has been said, that a certain vividness of colour may be added to the general prevailing bluish-red tint. No treatment, and she has had many forms of it, has



made the smallest impression on the disease. The chest and abdomen appear normal and there is nothing to remark as to the urine.

The general facts in this case seem to approximate it rather closely to the group of seven cases collected by Fox and MacLeod in their admirable paper on "Parakeratosis Variegata," and classed by them with the "pityriasis lichenoides chronica" of Juliusberg. The youth of the patient, the character of the eruption, with its resemblance to a receding lichen planus, the wide distribution, the absence of constitutional symptoms, and the resistance to treatment, which are such noteworthy features of this case, are all reproduced in that series of seven cases. In one of these series, that reported by Jadassohn, there was a history of an almost equally sudden inception. With this exception I have not found any other instance in which an acute onset—so acute, in fact, as to suggest an exanthem to an experienced general practitioner—has been recorded in any of the somewhat dissimilar cases which are provisionally included in the category of parakeratosis variegata.

*Histological Note on the Case.*—By the kindness of Dr. Staddon, of Ipswich, the patient's private practitioner, who performed a biopsy, I am enabled to add a note on the histology. The sections proved extremely hard and blunted several razors. There was a much thickened stratum corneum, the cells of which showed no deficient cornification and stained uniformly, no nuclei being seen. In several parts of the section there was free separation of thick stratified lamellæ of the stratum corneum, which was everywhere unusually dehiscent. There were especially numerous and abundant plugs of horn cells at the mouths of the follicles, almost recalling the appearances of pityriasis rubra pilaris. The stratum lucidum was not detected, but as the sections were, as has been stated, very hard and difficult to cut, finer points of histology could not be very definitely made out. The stratum granulosum was deficient or more often entirely absent, being at the most represented by a single layer of cells. The rete was thinned and the interpapillary processes practically non-existent, the papillæ being flattened out. There was some separation of epithelial cells (inter-epithelial œdema), and the inconspicuous demarcation between the epidermis and corium (the "blurring" of the basal layer) was a very notable feature. The localized papular character of the eruption was much more noticeable in the sections than would have been supposed from clinical inspection. In such localized areas there was a moderate cellular infiltration, probably mainly leucocytic, of the pars papillaris of the corium and perhaps some dilatation of the superficial vessels, but

certainly not a conspicuous dilatation. The bundles of collagen seemed on the whole somewhat swollen, but there was no cellular infiltration, and below the level of the pars papillaris one could not detect any abnormalities.

#### DISCUSSION.

Sir MALCOLM MORRIS: Is there anybody present who saw the four cases shown at the annual meeting of the British Medical Association in Edinburgh, one of which was exactly similar to this? If so, does any member know the subsequent history? From my knowledge of a limited number of these cases I would say that they go on for years, and then one loses sight of them, without learning the later history. They certainly do not get well from drug treatment—in fact, I do not know that this even improves them. One would like to know whether the disease wears itself out. One of the four cases of which I speak turned out to be mycosis fungoides. All four were Scotch cases.

Dr. PRINGLE: Previously to that there were one or two cases shown at the old Dermatological Society of London by Dr. Payne among others. My impression is that these cases never get well, that the skin becomes atrophic and the patients die ultimately of marasmus. In one case I have certainly seen that happen. No treatment seems to be of any avail.

The PRESIDENT: I have recently had, privately, a case very similar to this, which Dr. Pringle was good enough to see with me, the patient being the sister of a medical man. The itching in my case was very continuous and severe, but in the patient now exhibited it is almost absent. It is remarkable how subjective symptoms vary in different people suffering from this rare disease. I agree with the remarks made concerning treatment, but in my case a course of X-ray exposures lessened the cutaneous thickening where it was most severe and reduced the itching to such a degree that a life of distress became comparatively comfortable. The nails in my patient were slightly affected.

(May 18, 1916.)

#### Case of Lichen Scrofulosorum.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a girl, aged 13, who had been under the care of the Inoculation Department at St. Mary's Hospital for some two years for a scrofuloderma of the neck of five years' duration, resulting from a tuberculous glandular abscess. She had been treated for most

of the time with small doses of tuberculin in the form of bacillary emulsion. She was sent to my department on February 17, when I saw her for the first time, my opinion being desired on a new eruption which had come out suddenly. This consisted of a large number of patches (twenty-three were counted) of grouped follicular papules, the patches forming oblong or circinate shapes from 1 in. to 4 in. in diameter, and distributed on the back and front of the trunk at a level between the navel and the nipples. There was also a small single circinate patch in the right groin and another over the left shoulder. The papules were of a vivid red colour, not spiny, and at the periphery the patches were somewhat more salient and more vivid in colour. No complaint was made of subjective sensations. There was no rise of temperature. The patches persisted with little change except that there was a slow but progressive fading of colour, so that the aspect at the time of showing the case was much the same as that described at its inception. At my suggestion the injections of tuberculin which she was having have been omitted until the date of the meeting, and this fact may partially explain the retrogression of the lesions.

The case is especially interesting in view of the recent papers contributed by Sequeira and Adamson to the *Journal of Dermatology* in comment on the work of Rist and Rolland. The eruption is the most perfect example of the disease which I have seen since I showed a case almost equally typical at the Dermatological Society of London fifteen years ago,<sup>1</sup> a coloured reproduction of which appears in the *Medical Annual* for 1908, p. 363.

#### DISCUSSION.

Dr. J. H. SEQUEIRA: I remember an interesting paper of Lesselliers,<sup>2</sup> in which he showed that the histology of the lesions of lichen scrofulosorum produced by the injection of tuberculin is identical with that of the lesions in a tuberculous person who has had no injections. I look upon the present as a typical case following tuberculin injections. I have also seen cases from time to time in which tuberculin injections have produced other types of reaction in the skin: sometimes of the nature of Bazin's disease, sometimes of the nodular tuberculide type.

Dr. S. E. DORE: I do not think it necessarily follows that this eruption is due to tuberculin; it might possibly arise from the tuberculous lesion itself.

<sup>1</sup> *Brit. Journ. Derm.*, 1901, xiii, p. 167.

<sup>2</sup> Lesselliers, *Ann. de Derm. et de Syph.*, 1906, p. 897.

Last week I saw a child with a large patch of scrofuloderma on the arm and a typical eruption of lichen scrofulosorum on the trunk, and no injections of tuberculin had been given in that case.

Dr. PERNET: Although one has to think of the tuberculin as the possible cause of this lichen scrofulosorum, I may state that a week or two ago I had a case at the West London Hospital, that of a child with lichen scrofulosorum, and it had had no injections. All that could be found were enlarged glands in the neck.

Sir MALCOLM MORRIS: These cases occurred long before tuberculin injections became the vogue.

Dr. GRAHAM LITTLE (in reply): The Inoculation Department at the Hospital have been giving tuberculin injections over a very long time in many types of disease, and it is very rare for such eruptions to occur during these injections.

(May 18, 1916.)

### Case of Acute Disseminated Tuberculosis Cutis.

By E. G. GRAHAM LITTLE, M.D.

THE patient, a male infant, aged 7 months, was brought to me on May 4 for my opinion on an eruption which had come out quite acutely about a fortnight before. When I saw it the whole of the skin from the buttocks to the feet was closely covered with an eruption of small deep bluish nodular lesions varying in size from that of  $\frac{1}{16}$  in. to  $\frac{1}{4}$  in. The eruption was so closely set that one could not have put a threepenny-piece on the affected parts without obscuring several lesions. Above the level of the pubes the skin was unaffected. The child was under the care of my colleague, Mr. Kenneth Lees, who was about to remove some tuberculous glands in the neck. There was a sinus from one of these glands and another from a necrosed area in the mastoid. The mother stated that the glands had become enlarged at the age of 2 months, and that the mastoid had been operated upon for tuberculous disease of the bone at the age of 3 months. The eruption was not itchy at the time of the visit to me, and there was no scratching. The child looked desperately ill. It had been proposed to postpone the operation on the glands on account of the rash, but by my advice this was performed the following day, and about twelve glands, some already caseous, were removed.



As seen now, a fortnight later, at the meeting, a remarkable change has taken place in the aspect of the eruption, which has faded almost beyond recognition; the blue nodules have disappeared or diminished to pale papules not unlike those of a papular urticaria, and there is now some itching, which was certainly absent earlier. The child looks much healthier. A portion of the affected skin was excised at the same time as the operation on the glands, and half of the specimen was injected into a guinea-pig and half retained for histological examination. It is too early at present to make any statement as to the effect on the guinea-pig, but the section of skin has been examined. No bacilli were found in sections suitably stained. There was a deep-seated cellular infiltration in the corium in circumscribed areas constituting foci, but there were no giant cells. The cellular infiltrate consisted chiefly of mononuclear cells, with some large epithelioid cells; the histology is thus totally unlike that of an urticaria. The dramatic disappearance of the nodular eruption after removal of the infected glands is remarkable, for it had not shown any clinical resemblance to the papulo-necrotic tuberculide, as there had been no necrotic summits to the papules. Bacilli were easily demonstrated in the sections of the glands removed, so that the tuberculous nature of these is undoubted.

The clinical and histological appearances of this case present a remarkable resemblance to a case reported at great length by Pelagatti.<sup>1</sup> In this instance the patient was a male child, aged 2, and the eruption had covered nearly the same areas—that is, the lower limbs and lower part of the trunk. The papules were small hemispherical lesions with a scale or crust on the surface. The eruption had come out acutely, there was a widespread tuberculous adenitis and the patient died about three months after the appearance of the eruption, and post-mortem examination showed the cause of death to be acute disseminated tuberculosis. Histologically there were the same focal infiltrations of cells, with, however, some giant cells, which were not found in my case.

<sup>1</sup> Pelagatti, *Giorn. Ital. delle Malattie e della Pelle*, 1898, p. 704 (with coloured plate of histology).



(May 18, 1916.)

**Case of Tertiary Syphilis treated by Intramine.**

By GEORGE PERNET, M.D.

THE patient is a woman, aged 30, who was first seen on March 3, 1916, for broken-down gummata about the lower extremities of three months' duration, and beginning below the left ligamentum patellæ. In front of the left patella there was a punched-out circular ulceration, with a larger and somewhat reniform ulceration below it. On the outer side of the right knee-joint, and a little above it, a smaller circular ulcerated area of the same type was observed. Lot. nigra only was ordered. On March 7, 8 c.c. of intramine were injected into the left buttock. The 10-c.c. syringe used was that devised by Dr. Chatin, and made by Gentil, of Paris. This has an ingenious device for fixing the needle to the nozzle, and is supplied with a rubber piston worked on the Archimedean screw principle. On March 10 the patient stated she felt better, though there had been a good deal of pain for the first night after the injection. The buttock was tender and swollen. The lesions themselves were unchanged in appearance, but they were less painful, a symptom about which she had previously complained. On March 14, the patient considered the ulcerated areas were better, and she felt better in herself. Ung. acidi borici was ordered locally. On March 21 the ulcerated lesions were distinctly healing up; they were smaller and flatter. The left buttock had quite recovered from the injection. Seven cubic centimetres of intramine were injected into the right buttock. On March 24 there was some hard swelling and some redness about the seat of the injection and beyond as a result. Healing of the lesions is still going on. March 31: Healing up well; pain from injection occurred as before. April 14: Buttocks quite recovered. The greater part of the lesions on the left leg had healed up, but there were still some ulcerated foci about. The lesion on the right side was almost completely healed up.

As there was no clinical meeting of the Section in April, the patient was put on iodide of potassium 5 gr. ad. 1 oz. t.d., p.c., and she has been taking this up to present date. There were still one or two small, superficially ulcerated foci on the lesions on the left leg.

This case shows that intramine will lead to healing of syphilitic

ulcerated lesions, but compared with arseno-benzol and galyl intravenous treatment it is slower in action. There is also the question of pain with intramine. This patient was exceptionally good about it, and readily agreed to the second injection. But that would not be the case probably with most patients, especially those who have to earn their living by laborious work, as is usual with hospital patients.

A point of some interest in this case is that four years previously she had attended the Gynæcological Department of the West London Hospital for soreness of the genitalia. On looking up the case it was found she had been under Dr. Simson, who noted she had what looked like a primary sore, which he had confirmed later by a positive blood test. She had one intravenous injection of salvarsan at the time (that is, four years ago) and no other antisyphilitic treatment since. She was no doubt "whitewashed," and she never turned up again—as occurs so frequently among out-patients—until she came under my care last March. The fact is that the very efficiency of salvarsan and other similar bodies in clearing up early symptoms rapidly is a disadvantage in dealing thoroughly with syphilitic cases. It need not be added that this case will be followed up with mercury.

*Addendum.*—Since the patient was shown the following notes have been made. May 26: The scar of the intramine injection (plus some iodide of potassium) is not firm. It is thin and has broken down in several points with an eroded appearance. An intravenous injection of 0·25 galyl was ordered and injected on May 27. June 2: Scar still eroded. June 9: Ulcerated eroded areas are healing up, but with psoriasiform scaling.

(May 18, 1916.)

### Case of Mycosis Fungoides in a Woman.

By J. J. PRINGLE, M.B.

THE patient, S. S., aged 43, is the wife of a railway platelayer. She has lived at Hatfield (Herts) for the last seventeen years, and all her previous life in Leicestershire. She is of more than average intelligence. She has had four healthy children, and subsequently one miscarriage (at the sixth month), six years ago. About the same time she observed a "brown patch" on the inner side of the right thigh, which has persisted ever since. Her attention was drawn to it by itching.

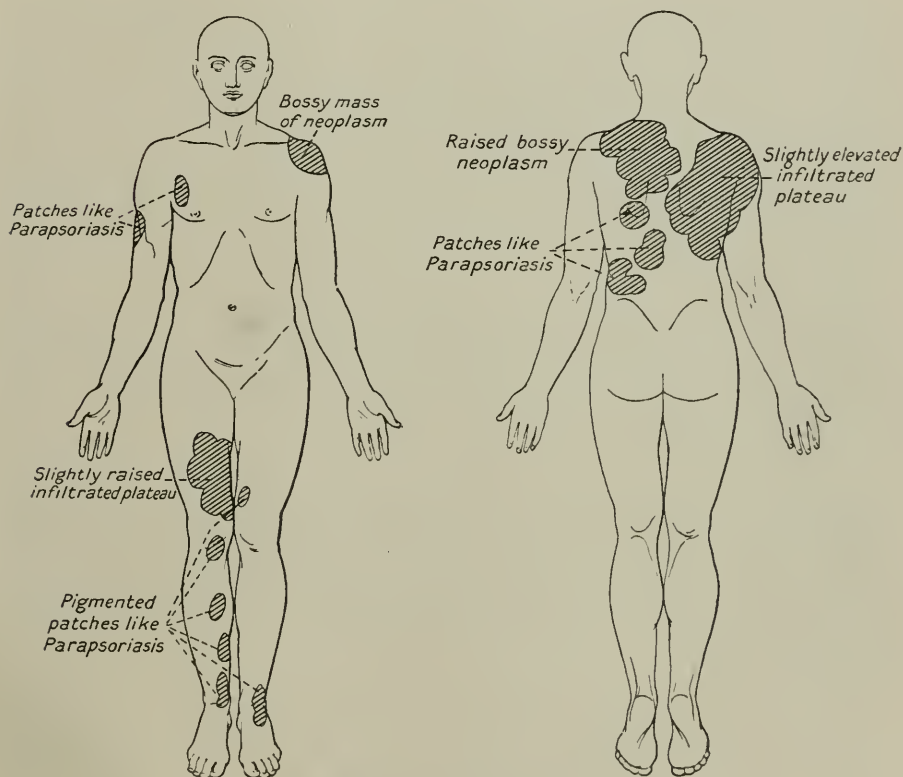
The patch gradually spread, but the condition remained limited to the right thigh for two years. Four years ago large brown patches appeared over both scapular regions, and several others over the rest of the back, but no definite dates can be assigned, and apparently little significance has been attached to them. The first "lump" appeared in the left supraspinous fossa about two years ago. The accompanying very rough diagram, made on admission to the Middlesex Hospital on April 14, indicates the distribution of the lesions. The only one of these exhibiting emphatic tumour formation is situate in the left upper scapular and supraspinous regions, which are occupied by soft, bossy, pale pink, almost gelatinous masses which project more than  $\frac{1}{2}$  in. above the general skin level. The surface of these masses is superficially eroded, and from it a thin, clear, sticky fluid exudes. The dimensions of this tumefied patch are 7 in. by 9 in. A rather larger composite patch is situate over the left shoulder and scapular regions; and although it is very superficially eroded and moist it is only slightly pro-eminent and shows no actual tumour formation. The original patch on the inner side of the right thigh presents identical characters. All the smaller isolated patches present to a very remarkable degree the characteristics of "parapsoriasis en plaques"; many of them are accurately oval or circular; their colour is deep brownish-yellow; their surface is distinctly lichenified and slightly scaly; their outlines are regularly convex and festooned where they have become confluent and the borderline between healthy and diseased skin is remarkably clearly defined. At no point is any induration palpable to touch. The spleen is not enlarged, and there is no augmentation in size of any of the superficial lymphatic glands. The blood count is as follows: Hæmoglobin, 90 per cent.; red corpuscles, 3,250,000 per cubic millimetre; leucocytes, 4,400 per cubic millimetre. Differential count: Neutral polymorphonuclears, 46 per cent.; lymphocytes, 33 per cent.; transitional and hyaline cells, 10 per cent.; eosinophile polymorphonuclears, 1 per cent. No abnormal forms seen. The chromatocytis shows no notable abnormality, either as regards morphology or staining reactions. The Wassermann reaction, tested on two occasions (Dr. C. H. Browning), was strongly positive. There are no other signs or symptoms suggestive of syphilis, except the history of a miscarriage at the sixth month (previously referred to).

The largest granulomatous patch was exposed to a full unscreened pastille dose of X-rays on April 16, and its characters have been totally changed for the better; its surface is seen to be flush with the surrounding skin and is almost uninterruptedly epidermized. All the other

patches have been subsequently exposed to the same dose of X-rays or to the mercury vapour lamp, but the results are not yet equally manifest. It has not been possible to make any histological examination.

The points in the case to which I specially draw attention are:—

(1) The pure “parapsoriasis en plaques” type of the primary lesions without erythrodermia.



*Mycosis fungoides in a woman.*

(2) The unusual “gelatinous” character of the only mycotic growths which have so far developed. (This could be demonstrated at the meeting.)

(3) The positive Wassermann reaction.

I hesitate to make the almost inevitable deduction from this fact in view of the absence of other confirmatory signs of syphilis; but I think (illogically, perhaps) that it gives me some justification for testing the



value of galyl, which I propose to use, or one of the other arseno-benzol series. I am aware of the disappointing results reported by a few who have given these substances a trial, but no great or convincing amount of evidence has been adduced one way or the other.

## DISCUSSION.

Dr. J. H. SEQUEIRA: I was asking Dr. Pringle whether he lays particular stress on the fact that the Wassermann reaction is positive: whether in that statement there is a suggestion that mycosis fungoides in a non-syphilitic subject may give a positive Wassermann reaction just as may cases of lepra. In a collection of cases which I made for a paper I read here some time ago, there was a definite history of syphilis in a small proportion of them, and in some the Wassermann reaction was positive. I did not find sufficient evidence for considering syphilis an ætiological factor as most cases give a negative Wassermann reaction.

Dr. GRAHAM LITTLE: Members may be interested to hear that the acute case of this disease which I showed and reported last year died a few weeks after exhibition. On the recommendation of a number of speakers at that meeting treatment with X-rays was continued, but with no diminution in the activity of the disease.

Dr. PERNET: I do not think this case is syphilitic although the Wassermann reaction is positive.<sup>1</sup> I have seen a good many cases of mycosis fungoides, but only one or two in which salvarsan has been used. I do not think 0.3 grm. of galyl would do any harm, and it might do good. I consider the case is undoubtedly one of mycosis fungoides. In any case of mycosis fungoides *à tumeurs d'emblée*, I was inclined to think the salvarsan did more harm than good.<sup>2</sup>

Dr. PRINGLE (in reply): I laid stress on the fact that the Wassermann reaction is positive in this case in order to elicit, if possible, the experience of others. I have no opinion to offer on the point, but I believe certain conclusions have been drawn by others. I propose to treat this patient with salvarsan or galyl. I am aware that the effect of arseno-benzol compounds in such cases has been stated to be unsatisfactory, but what Dr. Sequeira has said seems to give tangible grounds for such treatment.

<sup>1</sup> Pernet in "Discussion on Mycosis Fungoides" (thirteen cases), *Brit. Journ. Derm.*, 1914, xxvi, pp. 260 *et seq.*

<sup>2</sup> Pernet, "A Case of Mycosis Fungoides d'Emblée treated Unsuccessfully by Salvarsan and X-rays" (International Congress, London, 1913).



(May 18, 1916.)

### Case for Diagnosis.

By DUDLEY CORBETT, M.D.

THE patient is a clerk, aged 47. Ten months ago he was admitted to St. Thomas's Hospital suffering from a fungating carcinoma of the scrotum. Most of the latter, together with the inguinal glands on each side, were removed; suppuration inevitably followed, but he was discharged to a convalescent home with all the wounds soundly healed. Shortly after his arrival there a red patch similar in character to those now visible appeared on the anterior aspect of the left thigh. It rapidly faded, but left behind a discoloration, which has persisted to the present time. On examination one finds a large oval patch, purplish in colour. On pressure this fades completely, leaving no staining, and it returns when pressure is removed. Just below and to the inner side of this are some definite nævoid vascular tufts, not in any way resembling varicose veins. Three days later a similar red patch appeared on the left buttock. It was slightly raised, diffuse, with no definite margin, and was hot but not tender to the touch. There was no œdema. In two days it has now faded, leaving behind the same kind of purplish patch already described. Further red patches have appeared in this interval on the right buttock and on the upper part of the right thigh, and the patient feels ill. There is no external septic focus visible. I regret that owing to the short time available I have been unable to investigate the case further. On the evidence at hand I cannot account for the condition, and would be glad of suggestions as to the diagnosis.

### DISCUSSION.

Dr. G. PERNET: I suggest that possibly these manifestations are connected with the fact that he has had many glands removed from the upper part of the thigh and inguinal region in the operation; indeed, that is the *fons et origo* of the condition; there is an interruption of the lymph circulation. I do not know whether Dr. Pringle remembers seeing at the last Berlin Congress a man who had had his inguinal glands removed on both sides, and who had a severe lymphangiectatic condition about the scrotum in consequence. In cases of elephantiasis due to filarial disease there is a similar blocking. Such patients are liable to erythemato-inflammatory attacks in the skin of the affected leg, with febrile manifestations.

Dr. J. H. SEQUEIRA: Is the temperature raised? This kind of lymphangitis is not uncommon after extensive breast operations. Mr. Sampson Handley has called attention to the subject. In some cases it goes on to *cancer en cuirasse*, a blocking of the lymphatics by recurrent attacks. I think the veins are also affected.

Major GRAY: I think this condition has a very direct connexion with the removal of glands at the operation. The case presents a most remarkable picture of the anatomical distribution of the lymphatics which drain into the glands in the groin. Speaking from memory, the glands in the groin receive lymphatics from three areas: the inner group from the front of the thigh, the middle from the lower abdomen up to the level of the umbilicus, and the outer from the buttock. In this patient the lymphatics of the inner and outer groups—namely on the thigh and buttock—are involved, but those of the abdominal or middle group are not affected. I consider the condition is a lymphangitis, but I cannot say why the abdominal area is not involved, except possibly that there is a freer anastomosis of the lymphatics there, so that they have not been blocked in the same way. With regard to Dr. Sequeira's suggestion that this may be an early stage of *cancer en cuirasse*, I think that is scarcely tenable here, because the patient has had the condition on the left side and this has cleared up, which I do not think ever happens in cases where the lymphatics are blocked with cancer cells. I regard this as a lymphatic obstruction of inflammatory origin. What the organism causing the trouble is, or how it got there, is another problem. I believe that the dilated venules are the result of a vascular obstruction which one often sees associated with a chronic œdema.

Dr. J. H. SEQUEIRA (in reply to Major Gray): I do not suggest this is *cancer en cuirasse*; I simply say that it has been shown that lymphatic obstruction is a cause of *cancer en cuirasse* where there is malignant disease of glands.

(May 18, 1916.)

## Case of Asphyxia Reticularis (Unna).

By DUDLEY CORBETT, M.D.

THE patient is a woman, aged 31. Two years ago she was engaged in laundry work, but has done no similar work since that time. Though naturally she worked in a hot atmosphere she was never exposed to the direct heat of a furnace, nor does she now make a habit of sitting unduly close to the fire at home. Twelve months ago the present condition

appeared on the outer aspect of the left leg, and nine months ago on the right leg. On the former it consists of a strongly marked venous network, the meshes of which have a diameter of about  $\frac{3}{4}$  in., it is of



Case of asphyxia reticularis. The bullæ appear on the darker patches where the network has coalesced.

a dark purplish colour, and in places there are the remains of small bullæ, which appear from time to time and break down. On the right leg the process is less advanced, and there are no bullæ. There is a certain amount of irritation and a good deal of aching pain constantly

present. I have called the condition asphyxia reticularis, but I might perhaps have used the term "livedo annularis." I should be glad to know something of the pathology of the condition and the nature of the inflammatory or degenerative condition of the veins. Does it end in fibrosis with visible scar formation or does it ultimately disappear entirely?

#### DISCUSSION.

Dr. GRAHAM LITTLE: I think this must fall into the erythema ab igne class. The distribution on the aspect of the leg which would be most exposed to a source of heat with the legs crossed is very characteristic. The causation is often ignored by the patient and may be quite trivial. The ridges marking out the meshes in this case are certainly unusually prominent, but we have all seen degrees of this, and Sir E. C. Perry<sup>1</sup> showed a case in which this feature was exaggerated. I had recently under observation a girl with a very similar appearance of the legs, and here the ridges slowly disappeared after some months. I think the same result will follow in this instance. I certainly do not think it can be called an asphyxia reticularis of Unna, in which, if I remember right, ulceration was present. I showed a case with this title at the Dermatological Society of London in 1903. The members apparently accepted this diagnosis, and I have a water-colour drawing of the case. Ulceration was a prominent symptom.

Mr. SAMUEL: I think this is possibly a case of lichen planus taking on a peculiar pattern. Some time ago I showed here two sisters with lichen planus. One of them presented a condition very similar to this. It was agreed by members present that it was lichen planus as she had typical lichen planus elsewhere. On the legs there was the same reticular formation. Some of the papules in this case are shiny and of a violaceous tint.

Dr. J. M. H. MACLEOD: The case may be one of lichen planus reticularis, as there are certain shiny, violaceous, obtuse papules towards the lower part of the lesion which somewhat suggest that diagnosis.

Dr. H. G. ADAMSON: In spite of the fact that the patient denies exposure of the legs to heat, I regard this as an example of "melanoderma reticularis," the result of erythema ab igne. The long hours of standing at her work would be a contributory factor. The fact that the inner side of the left leg is more affected than the right suggests that when she does sit by the fire she sits on the right-hand side, and this she admits. I do not think that Unna's name of "asphyxia reticularis," which was applied to a unique case, should be used to describe this more ordinary affection. I agree with Dr. MacLeod that

<sup>1</sup> Perry, *Brit. Journ. Derm.*, 1900, xii, p. 94.

the eruption of lichen planus often follows the network of livedo, but I do not consider Dr. Corbett's case an example of reticular lichen planus. Very interesting reticulate eruptions are those in which syphilides and tuberculides follow this pattern, as in the livedo racemosa syphilitica of Ehrmann, and in certain examples of Bazin's disease; but this case does not belong to these groups.

Dr. PERNET: I consider the case is one of erythema ab igne. With regard to the lichen planus suggestion, I would point out that there is mild reticulation on the right leg too, but there are no signs of lichen planus-like lesions in that situation.

Dr. F. PARKES WEBER: The important point in this case of reticulate erythema and pigmentation of the legs, I think, is the one alluded to by Dr. Adamson, namely, that as the exciting cause we have a combination of venous congestion owing to the dependent position of the lower extremities and a certain amount of hyperæmia due to heat, probably to sitting before a fire, as in most cases. The facility with which erythema ab igne can be artificially produced varies perhaps in different persons, but amongst my hospital patients the frequent local application of heat (hot fomentations, hot india-rubber bottles, &c.) to the back or abdomen, for the relief of pains, has often sufficed to produce considerable and typical reticulate erythema and pigmentation exactly resembling ordinary erythema ab igne. Ætiologically, this kind of reticulate erythema should be distinguished from the various forms of reticulate livedo, which it somewhat resembles.

Major GRAY: There seems to be some tendency in certain individuals to get this particular kind of pattern. Some years ago I examined a good many school children, and if one put a row of boys, stripped, in front of a hot fire awaiting examination, about one in ten would get redness in this reticulated pattern instead of a diffuse redness.

Dr. CORBETT (in reply): I think that if there is anything in the history to account for the condition it is the prolonged standing at her work, for there are some varicose veins. I cannot agree that heat has much to do with it, as I have particularly asked her whether she was near the fire at the laundry and she has assured me that she was not, nor have her legs ever been unduly exposed to heat.



(May 18, 1916.)

**Case of Extensive Carcinoma of the Face occurring in the  
Course of a Xerodermia Pigmentosa treated by a Massive  
Dose of Radium.**

By J. J. PRINGLE, M.B.

THIS patient, a male, was previously exhibited on March 19, 1914, and reported, with photographs.<sup>1</sup> He then had a huge ulcerating carcinomatous mass of the left cheek, involving the maxilla and extending as far back as the zygoma and ear, into which mass three tubes, containing in all 152 mg. of pure radium bromide, had been introduced and maintained in position for twenty-one hours. The remarkable improvement reported in 1914 persisted, but at the end of 1915 some recurrence had taken place at the inner portion of the growth, and on admission to the Middlesex Hospital three weeks before present exhibition a large, soft, ulcerating mass completely blocked the left nostril, and projected from it for a distance of more than  $\frac{1}{2}$  in. It has been treated with a tube containing 50 mg. of pure radium bromide plunged deeply into it and kept *in situ* for twenty-four hours. The result has been immediately and greatly beneficial, and the growth has shrunk well inside the aperture of the nostril.

(May 18, 1916.)

**A Severe Case of Acne.**

By J. H. SEQUEIRA, M.D.

E. P., AGED 20, a dressmaker, was admitted to the London Hospital on account of severe acne on March 7, 1916. The skin was said to have been quite clear until three years ago, when the face became gradually covered with "blackheads and pimples." Large pustules formed later, but the inflammatory condition gradually subsided during

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1914, vii (Derm. Sect.), pp. 130 *et seq.*

the next year and a half. In December, 1915, the face again became very inflamed. On admission, the condition depicted in the photograph was present; the abscesses were of an unusually large size, and there were numerous comedones. It is remarkable that the eruption was limited to certain areas, and that the adjacent parts were absolutely free from any lesions. The patient had suffered from indigestion for many years, and four years ago was in a provincial hospital for goitre. The large abscesses have been evacuated and treated on general principles. The pus has been examined several times, and has been found to be sterile. The patient had not been taking either iodides or bromides.



A severe case of acne.

(May 18, 1916.)

### Case of Congenital Urticaria Pigmentosa.

By GEORGE PERNET, M.D.

THE patient is a male infant, aged 7 months, first seen at the West London Hospital on April 14, 1916. The case is a typical one, the body generally (head, body, and limbs) exhibiting the characteristic pigmented spots of the complaint. On stretching the skin over them

the yellow tint of the lesions is well brought out. The child is otherwise a healthy-looking baby, the only trouble one can find being that the motions are very dark. The lesions are extremely irritating, and keep the child and its parents awake at night. The mother is not suckling the infant, which is therefore being reared on an artificial diet. This included groats, which have now been knocked off. Carbonate of magnesia and soda, and a lotion of sanitas (1 in 8) have been ordered, to be applied by local pressure to irritating lesions. Since the applications have been made the infant has greatly improved all round. The lesions reacted in the usual way to pressure with the nail.

The congenital nature of this case has been purposely emphasized in the title. It was the grandmother who brought the baby to my clinic. She was present at the confinement, and noticed the spots on the infant at the time of its birth, so there is no doubt about it. The parents, who have not been seen by me, are said to be quite healthy. No history of a maternal impression, such as seeing a leopard or dreaming of a leopard's skin, is forthcoming.

#### DISCUSSION.

The PRESIDENT: I have a case under observation of very much the same kind. The probability is that with care and dietary such complications will pass away.

Dr. MACLEOD: One of the most interesting features of the case is that the condition was noticed at birth, as in all the cases which have been under my observation it has not appeared till some months after birth.

(May 18, 1916.)

### Case of Keratodermia Blenorrhagica.

By E. G. GRAHAM LITTLE, M.D., and P. A. HAYNE, M.R.C.S.

THIS case, which appeared on the agenda paper for exhibition, proved at the last moment too ill to be moved from St. George's Hospital, where he is under the care of Dr. Cyril Ogle, who has very kindly given us permission to report it.

The patient, F. W., is a married man, aged 56, a leather-worker by trade, but his occupation for the past fifteen years has entailed no manual labour, as he has been a directing foreman.

Previous history: He took venereal risks somewhat freely from about the year 1886, but had no apparent symptoms of infection until 1889, when he developed urethritis, followed by an arthritis of the left knee, and had an eruption of warty brown excrescences which he recognizes as the same lesion as is now present, but with a much scantier distribution, for they were restricted to two lesions on the left foot and three lesions on the right foot. They were "as big as buttons," and "dropped off" in the same way as the present limpet-like growths are wont to do. He seems to have slowly developed a stricture, and to have had a severe urethritis in 1896, when there was a free discharge. In 1913 he again had severe urethral symptoms, including discharge, scalding and pain, with scanty water, but these symptoms, with the exception of the discharge, had been more or less present for many years. At the end of November, 1915, he again noticed some discharge from the penis, in the form of "white beads" after micturition, and then for the first time began to feel seriously ill, and gradually "wasted to a skeleton." He had arthritis of several joints, the left knee, left foot, right knee and right foot becoming affected in that order. He was confined to bed, for the first time during his adult life, in the first week of January, 1916. About four weeks later, while still keeping his bed, the eruption made its appearance in the form of hard, waxy, brownish-yellow "scabs," which he had not seen since 1889, although he had had several attacks of urethritis, some attended by arthritis.

Present condition: The man has a profoundly cachectic aspect,

with the peculiar "fade" odour of chronic septic conditions, and is much wasted and very feeble. His voice is weak and soon gets tired. He lies motionless in his bed, on his back for the most part, and his muscular system is generally impoverished. He has difficulty in turning on his side or front and has to be helped in making these movements.



FIG. 1.

Keratoderma blenorrhagica. Showing keratotic area on plantar and inner surface of right foot.

The skin is universally dry, thinned, and of an "old ivory" colour. In addition to the arthritis of the knees and feet the middle joint of the right medius finger became acutely swollen and red on May 9, but



subsided somewhat rapidly after an injection of half a million gonococcal vaccine, given on May 10 for the first time, and repeated on May 15. Condition of the skin: The chief incidence of the eruption is on the soles of both feet, which have been occupied with a very heaped-up



FIG. 2.

*Keratoderma blenorrhagica*. Showing horny masses in front of knee, and swollen joints.

mass of hard, horn-like material, as is well shown in the photographs. This has, however, now been largely shed, leaving a reddened dry scaly

surface with a sharp line of demarcation from the healthy skin. The affected area encroaches on the dorsum of the foot as well as occupying the whole sole, and at the upper line of the diseased skin nodules of waxy material are again forming on the reddened margin. There is a small brown mass on the front of the leg, about mid-way up the anterior aspect, and a larger but similar mass in front of the right knee, and also in front of the left knee. There are some small reddened areas the size of a sixpence on the anterior surface of the right thigh, which have been covered with brown waxy masses, but these have been shed. At the summit of the internatal sulcus there is a large suppurative lesion, perhaps caused by the weight of the body lying for so long in one position. There are three small brown heaped-up masses at the back of the right elbow, and along the outer and interior surface of the left forearm there are some small brown nodules. In both palms, but especially in the left, there is a coarse brown desquamation, and there is a similar small patch in the hollow of the umbilicus. Dotted freely over the scalp, which is quite bald except at the sides, there are several small limpet-like brown masses, with a base of the size of a threepenny bit, and a conical waxy mass protruding from it to the height of about  $\frac{1}{4}$  in. Nails: The patient has never shed his finger- or toe-nails. Some of the toe-nails are thickened, but not more than is usual with people who do not tend them particularly carefully; the thumb-nail of the left thumb is thickened at the distal end. The remainder of the finger-nails are normal and in good condition. Urine: Shows flocculent precipitate and threads. Weight: 5 st. 10 lb. Height: 5 ft. 6 $\frac{1}{2}$  in. Material from the brown waxy masses was teased out and examined for bacteria. No gonococci, but some staphylococci were found.

The following notes have been contributed by Dr. Hayne, District Medical Officer of the Burma Railways Company, now on leave, who has been in charge of the case under Dr. Ogle since admission. We have to thank Mr. Hamilton Beattie, of St. George's Hospital, for the excellent photographs.

Condition on arrival at St. George's Hospital (March 17, 1916):—An extremely ill, pale, emaciated-looking man; lips dry, tongue dry and coated, breath very foul, teeth dirty, gums of both upper and lower jaw puffy and swollen, with pus exuding from them. The whole trunk very emaciated; skin dry and rather yellowish. Heart and lungs: Nothing abnormal detected. Abdomen: Nothing abnormal detected. Joints: Both knees swollen; in colour waxy-white with dilated veins coursing over them; hot to the touch; a small quantity of fluid can be

demonstrated in both knee-joints without causing much pain, and the greater part of the swelling seems to be due to a general thickening of the tissues immediately surrounding the joint; they are extremely painful on passive movement; no creaking felt. Both ankles are swollen; the skin over the external malleoli is dusky red, but over the rest of the joint white; there is some fluid in both these joints, extremely painful on passive movement, but not on examination without movement. Hip-joints: There is no swelling or redness over either hip-joint; there is pain on passive movement; no creaking. No other joints affected. Pulse 120, temperature 99° F., respirations 17. Urine: Specific gravity 1020, reaction acid; no flakes floating about; no deposit; albumin and sugar *nil*.

Family history: Nothing important; wife has had four children (youngest aged 10) and two miscarriages. Alcohol: Moderate during the last ten years, but has, as a younger man, had bouts of fairly heavy drinking (spirits and beer).

During the seven and a half weeks patient has spent in this hospital the chief points of importance are that—

(1) He still looks extremely ill, but that he eats and sleeps well, and his general condition now is much the same as on admission.

(2) The condition of the gums responded very rapidly to treatment, and are now no longer swollen and no pus can be pressed from them.

(3) The condition of the joints is much the same, except that the fluid has disappeared from the knee-joints and there is less pain on passive movement and movement is less restricted. Ankles and hips as on admission. On May 9 the middle joint of the left fourth finger became red and swollen, but was quite painless to touch or passive movement.

(4) A specimen of blood was taken, and on examination gave a negative Wassermann reaction. A specimen was taken from the serum under the crusts of one of the cutaneous lesions on the scalp, and showed staphylococci microscopically, and on culture media *Staphylococcus albus* was isolated.

(5) Urine on weekly examination has not contained albumin or pus, except on May 20, when it was noticed to contain some shreds and flakes of (?) mucus; there was a trace of albumin, and on microscopical examination there were a few polymorphonuclear cells, epithelial cells, and structureless-looking matter, probably mucus. No discharge could ever be obtained from the urethra. No bacteriological examination of the urine was made.

(6) Pulse-rate varies between 120 and 130 or higher per minute, sometimes regular, sometimes irregular. Temperature variable, usually ranges between 99° F. and 100° F.

(7) At the beginning of May a small subcutaneous abscess developed in the region of the tip of the coccyx, now almost dried up.

Treatment: Local treatment to gums and cutaneous lesions. Tonics (quinine, arsenic, and iron) internally. April 28: Massage was commenced to knee-joints and continued until May 10. May 10: Gonococcal stock vaccine,  $\frac{1}{2}$  million units (slight reaction). May 15: Gonococcal stock vaccine,  $\frac{1}{2}$  million units (no reaction). May 20: Gonococcal stock vaccine, 1 million units (no reaction). May 25: Gonococcal stock vaccine, 2 million units.

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(June 1, 1916.)

### The Theory and Practice of Chemotherapy.<sup>1</sup>

(SECOND COMMUNICATION.)

By J. E. R. McDONAGH, F.R.C.S.

THE main object of my last paper was to point out that once the rationale of chemotherapy was put upon a logical basis, empiricism would vanish and the paths would be open for the preparation of non-toxic drugs. My theory of oxidation and reduction satisfactorily explains the *modus operandi* of chemotherapy. As iron and sulphur are the two elements which play an important rôle in these two processes in the body, it stands to reason that suitable compounds of them will be non-toxic; also that if such compounds are able to exercise a therapeutic effect in syphilis, it will prove the correctness of the theory, since neither of these elements has a marked parasitocidal action—indeed, sulphur even stimulates the growth of bacteria. Iron and sulphur, in their respective compounds ferrivine and intramine, do exercise, if properly employed, a therapeutic action not only in syphilis, but also in many other diseases, which is corroborative evidence of the soundness of the views enunciated. This piece of work is only one link in my chain of research on syphilis, and is based, like the other links, upon the proofs brought forward to show that the *Spirochæta pallida* is not the only cause of syphilis. Until those who are equally interested in the subject of syphilis with myself have proved my work on the life-cycle of the *Leucocytozoon syphilidis* to be either correct or incorrect, they will be

<sup>1</sup> Mr. McDonagh's first communication was discussed at the meeting of April 13 (see *Proceedings*, pp. 105-138).



unable to appreciate my motive for introducing the drugs, the therapeutic action of which this paper is designed to describe. Intramine and ferrivine have been introduced, first, to form a link in the chain referred to, and secondly to show that a toxic metal like arsenic is not an essential ingredient of the chemotherapeutic preparations. As to whether intramine and ferrivine, as they undergo improvement, are destined to replace salvarsan and its substitutes in the treatment of syphilis only the future will show.

So far as other diseases are concerned, I may say that in their acute stages ferrivine and in their chronic stages intramine exercise a more powerful action than salvarsan. Intravenous injections of ferrivine act beneficially in cases of acute ulceration—ulcus molle, for instance. Intramine has a therapeutic action in cases of chronic ulceration, tuberculosis, gonorrhœa, meningococcic infections, &c. The local application of intramine to chronic wounds, sinuses, &c., will bring about in some cases an improvement unobtainable with any other drug. Furthermore, intramine can be taken internally, but with what effect has yet to be determined.

All elements which are parasitotropic are in the same degree organotropic, hence the use of arsenic, antimony and silver will always be fraught with a certain amount of danger. Iron and sulphur are not parasitotropic, therefore they are not organotropic, hence their use can never do the host any harm. As both sulphur and iron stimulate processes that naturally occur in the body—processes which are employed to combat all diseases—and as the one is complementary to the other, it becomes us to perfect the compounds which contain these elements. The best sulphur compound so far prepared is intramine, and the best iron compound is ferrivine. Intramine has the misfortune to be painful, a disadvantage which co-operation should soon enable us to overcome. By its upsetting the hydrogen ion concentration of the serum, ferrivine at first did produce shock, a disadvantage I have now completely remedied. It is also clear to me how its action could be materially increased, but here again co-operation is needed to achieve this result.

The pain produced by intramine varies in different individuals, and in my own cases it never remains severe for more than three days. Provided there is no urea and quinine hydrochloride present, an abscess never forms, and no sloughing nor induration ensues. I have given 287 injections of intramine without experiencing, with the exception of two cases in which a sterile abscess occurred, any ill-effects other than pain. In the two cases referred to, some urea and quinine hydrochloride

had been added to the drug. The use of intramine, complementary as it is to all the metallic compounds now employed, is in my opinion indicated in every case of syphilis. Its effect in removing the induration of chancres, the resistant papules and the fibrous tissue in the recurrent and late lesions, far surpasses that produced by salvarsan. In syphilis of the nervous system the administration of intramine has wrought results which I have never experienced with other remedies. Furthermore, intramine increases the action of the drugs which succeed it. I had a case of congenital interstitial keratitis which responded to neither mercury nor intramine, but when mercury was prescribed after the intramine had been given, the condition immediately began to mend. I particularly mention this case because it shows so well how the action of one drug helps that of another, and because cases may be met with which do not respond to the treatment which usually benefits them. One of the tongue cases exhibited to-day well exemplifies this—a case of gummatous glossitis which, though resistant to salvarsan, healed up immediately with intramine.

In the primary and the generalization stages of syphilis, an adsorbed metallic compound must be prescribed before intramine, or the contrary action to that desired may be experienced if the non-metallic compound precedes the metallic. In the recurrent and late stages of syphilis, the best results are obtained if intramine be given before an adsorbed metallic compound. Intramine not only increases the action of ferrivine, but also that of the arsenical compounds. Salvarsan and its substitutes have little effect upon the induration of chancres, and some of the cutaneous papular lesions are extremely resistant to these drugs; if in such cases intramine is prescribed after two injections of the metallic compound have been administered, the hardest induration and the most resistant papule will have vanished within ten days.

Ferrivine, as it is now prepared, has no disadvantages; there is practically no limit to the dose that may be employed, and the injections can be repeated as often as is desired—even daily. In some cases of early syphilis its action is quicker and more pronounced than that of salvarsan, while in others it is not so good, for very obvious reasons. The changes produced in the lesions by ferrivine are different to those which follow salvarsan.

Under salvarsan a chancre becomes drier, the surface heals, but the induration takes some time to disappear; the dorsal lymphangitis likewise vanishes slowly and the hardness in the lymphatic glands persists, often many months.

Under ferrivine the chancre swells, the surface becomes much moister, and does not heal very quickly, but when repair is set up and the swelling has vanished it will be found that every trace of induration has gone, and no scar is left to mark the position of the sore. The dorsal lymphangitis quickly disappears, and the hardness of the inguinal lymphatic glands gives place to their normal consistency. These changes are increased and accelerated by the addition of intramine. The ordinary early syphilitic rash does not disappear so quickly with ferrivine as it does with salvarsan, the infiltration rapidly subsides, but an erythema marks the site of the macules and papules for some time. The same may be said of the mucous papules in the throat. After ferrivine reactionary inflammation in the lesions is more frequently met with than after salvarsan.

Why the changes above mentioned should be so different I cannot at present say, as I have not examined the lesions after ferrivine to see if those phases of the *Leucocytozoon syphilidis* which are not killed quickly by salvarsan are killed by ferrivine, and vice versa. It is possible that the spore stage, which is so resistant to arsenic, may be susceptible to iron, and the opposite the case with the adult sexual phases. That some explanation of this kind is highly probable is suggested by the fact that ferrivine does clinically just what salvarsan does not do, and vice versa. All that can be said at present is, that the *Spirochæta pallida* is not killed quickly by ferrivine as it is by salvarsan, a fact which proves nothing, since the adult male phase is not the phase that is responsible for the spread of the disease.

I have given to present date 126 intravenous injections of ferrivine, and have never experienced any toxic symptoms other than shock, which is now avoidable. For the preparation of serum for intraspinal injections, ferrivine and intramine are extremely suitable, and there is no fear of encountering those toxic symptoms which are so liable to follow the use of the substitution products for salvarsan.

No one realizes more than I do that intramine and ferrivine are by no means perfect; of their therapeutic action I am fully convinced, and in view of the non-toxicity of the elements iron and sulphur I feel confident that in time they will replace toxic elements like arsenic, antimony, and silver. As matters stand at present, intramine is called for in every case of syphilis, and the benefit wrought by its inclusion among the antisiphilitic remedies far outweighs the temporary inconvenience caused by the pain. I for one would never hesitate to take intramine if I were so unfortunate as to contract syphilis. I have

administered intramine to several medical men, and they have been struck by the rapidity with which the induration of the primary lesions and the late lesions have disappeared. I have also received several reports from medical men as to the efficacy of intramine in cases which did not respond to salvarsan. In view of the different effects produced upon the lesions by ferrivine and salvarsan it is possible that the employment of both at the same time is indicated. Cases which have not responded well to ferrivine have certainly cleared up more rapidly under salvarsan afterwards than they would have done if salvarsan alone had been employed. It is this fact which makes me think that certain phases are killed more quickly by one drug than the other, and vice versa. I have a suspicion that Ehrlich was trying hard just before he died to replace the arsenic in his compounds by other and non-toxic metals, since there have been many more cases of arsenical poisoning than have ever got into print. The following are the cases which I have either seen myself or about which I have had letters since the War began, therefore they are cases which have developed toxic symptoms after the employment of the various substitutes for salvarsan: Three cases of sudden death; twelve cases of generalized arsenical dermatitis, two of which died; nine cases of mild or localized arsenical dermatitis; a fatal case of cerebral degeneration after an intraspinal injection of serum drawn off after galy; three cases in which the symptoms of degenerative myelitis were very much aggravated, and the degeneration increased by the use of serum injected intraspinally after the administration of kharsivan and galy; five cases of very severe neurasthenia; four cases of arsenical neuritis; six cases of jaundice; an uncertain number of cases of shock. The symptoms varied from loss of pulse to difficulty in breathing, with asphyxia and swelling of the lips and tongue.

I now give the following brief notes of the cases exhibited, which you have all had the opportunity of seeing:—

*Case I.*—J. C. Ulcus molle on penis with twenty-seven small ulcera mollia in right inguino-scrotal region. The sore on the penis was deeply ulcerated and about the size of a shilling. Before treatment was commenced the patient had had the sore for six weeks. May 20, 1916: Intravenous injection of ferrivine (100 c.c.). May 22: Most of the small ulcers had healed. May 23: Intravenous injection of ferrivine (100 c.c.). June 1: All the small ulcers had healed and the large one was very much better.



*Case II.*—P. B. Syphilis four years ago, for which patient was treated irregularly. Advice was sought for a recurrent serpiginous syphilide of the scrotum. Within a few days after an intramuscular injection of intramine (1 gm.) the lesion completely disappeared.

*Case III.*—J. B. Patient, on examination, was found to have a typical hard, papulo-erosive chancre on the left side of the corona, indurative lymphangitis on the dorsum of the penis, and indurative lymphadenitis. April 1, 1916: Intravenous injection of ferrivine (300 c.c.). As a result of the injection the sore swelled and became very inflamed. As the swelling disappeared it could be noticed that the induration was much less. The reactionary inflammation was equally marked in the lymphatic cord and glands. April 8: Intramuscular injection of intramine (1 gm.). April 29: Sore had completely vanished, leaving no scar or induration. The lymphangitis had entirely disappeared, and the lymphatic glands had returned to their normal state.

*Case IV.*—W. A. Patient had a well-marked intra-urethral sore, general indurative lymphangitis and lymphadenitis, and a maculo-papular rash. March 28, 1916: Intravenous injection of ferrivine (300 c.c.). April 15: Intramuscular injection of intramine (1 gm.). April 29: The sore had completely vanished, leaving no induration nor narrowing of urethra behind. The rash had disappeared and the patient felt extremely well.

*Case V.*—H. S. On examination patient was discovered to have a hard, papulo-erosive chancre on the left side of the corona, a diffuse papular syphilitic eruption of the penis and scrotum, indurative lymphadenitis and gonorrhœa. The foreskin could not be retracted. May 2, 1916: Intravenous injection of ferrivine (100 c.c.). Swelling of foreskin disappeared forty-eight hours later. May 9: Intravenous injection of ferrivine (100 c.c.). Foreskin could be retracted. May 13: Intramuscular injection of intramine (1 gm.). Induration completely disappeared, sore healed, and the other symptoms vanished except the gonorrhœa, which had only slightly improved.

*Case VI.*—T. W. When seen patient had a swelling of the penis which, from the feel, allowed of the diagnosis of a hidden primary sore being made. Foreskin could not be retracted. There were the usual lymphangitis and lymphadenitis. May 16, 1916: Intravenous injection of ferrivine (100 c.c.). Forty-eight hours later swelling had disappeared. May 20: Intravenous injection of ferrivine (100 c.c.). Forty-eight hours later foreskin could be retracted. May 23: Intravenous injection of ferrivine (100 c.c.). When examined, three days later, it was seen that there had been a gangrenous chancre of the glans penis. The ulceration had practically cleared up, and the lymphangitis and lymphadenitis had vanished.



*Case VII.*—G. P. Patient presented himself with a huge swollen and indurated penis. There was a large chancre on one side of the prepuce and another on the other side, as well as an ulcerative chancre of the glans penis. Lymphangitis and lymphadenitis were present, there was a diffuse maculopapular rash, and mucous papules in the mouth and around the anus. March 28, 1916: Intravenous injection of ferrivine (300 c.c.). Seven days later the two preputial sores had healed, the lymphangitis and lymphadenitis had disappeared, and the rash was very much fainter, but the mucous papules remained about the same. April 15: Intramuscular injection of intramine (1 gm.). May 6: Intravenous injection of ferrivine (100 c.c.). As the result of the last two injections the chancre on the glans penis entirely disappeared, but one or two of the mucous papules remained.

*Case VIII.*—J. J. Syphilis seven years ago, for which patient was irregularly and inadequately treated. About a year ago the tongue became swollen, painful, and ulcerative, and its movements were restricted. April 29, 1916: Intramuscular injection of intramine (1 gm.). Some days later the tongue, with the exception of some patches of leukoplakia, returned to its normal condition. The improvement commenced forty-eight hours after the injection.

These cases, I think, prove conclusively that intramine and ferrivine have an antisymphilitic action. Such being the case, and since it is obvious to me that the therapeutic action of these drugs can be enhanced, and since sulphur and iron never produce such ill-effects as those I have just related as occurring under arsenic, I feel that not a stone should be left unturned to promote the spirit of mutual co-operation, and to make ourselves pioneers in a field which has not yet been exploited by Germany. When the improvements which suggest themselves have been forthcoming, I feel certain that our treatment of syphilis will be better than it has been with salvarsan, because I am perfectly convinced in my own mind that, however rapidly salvarsan may get rid of symptoms, it most certainly does not cure syphilis.

## DISCUSSION.

Dr. J. J. PRINGLE: My remarks will be directed almost exclusively towards the clinical aspect of Mr. McDonagh's paper. In order to qualify myself for making these remarks with any degree of pertinence, propriety, or assurance, I have for many weeks past pursued the perfectly obvious course of attending Mr. McDonagh's clinic at the Lock Hospital, where every facility has been afforded me of observing and following out his cases as far as that is possible in out-patient practice. According to my knowledge it is only at the Lock Hospital, apart from military hospitals, that sufficiently abundant material can be found, even in London, for drawing conclusions of decisive value as regards the efficacy of treatment of the primary lesion of syphilis. And surely the success, or otherwise, of treatment of the initial chancre is the best criterion at our disposal for estimating the value of any remedial measure? In other words, the cutting short of syphilitic disease before its systemic generalization is, whenever attainable, the main and paramount object of our therapeutic attack. The value of salvarsan and other bodies of the arsenobenzol series in aborting the primary chancre is manifest and indubitable; but I say without hesitation that the results obtained in similar circumstances by ferrivine—as I have witnessed them—are even more striking and immediate. The local reaction is generally speaking less severe, and, although the shock and collapse which follow an overdose are certainly sometimes alarming, they are almost always of short duration. The general reaction which ensues upon the injection of 1 gm. in 100 c.c. of sterilized distilled water (the dose now established as the normal), if present, is usually quite negligible. The injection may with safety and advantage be repeated on two or three occasions, at intervals of only a few days, owing to the non-toxicity of the drug. I may sum up my views on this all-important branch of the subject by saying that the results of treatment by ferrivine as regards the complete and rapid healing of the primary sore, with subsidence of its induration, must be seen to be believed. Coming well within the sphere of influence of ferrivine are the implication of the lymphatics of the penis and of the immediately adjacent lymphatic inguinal glands. These yield in a manner which I believe to be unprecedented. When an early roseolous eruption is present, a well-marked local aggravation, apparently on all fours with the Herxheimer reaction, may occur immediately after or even towards the end of the injection of ferrivine; this indicates, I cannot but think, that the *modus operandi* of ferrivine is essentially parallel to, or identical with, that of arsenical preparations. I have not seen aluvine used.

The clinical evidence of the therapeutic value of intravine appears to me much less positive, less tangible, less capable of definite appreciation, more elusive, and it is in the nature of things that it should be so. My opportunities for estimating its value have not been confined to what I have seen at the Lock Hospital, as in the case of ferrivine, but have been derived also, and

chiefly, from the study of its employment in a number of cases of so-called secondary and tertiary syphilis in my own hospital practice, where good opportunities are afforded for close and continuous observation. My experience leads me to endorse, on the whole, Mr. McDonagh's view that intramine is a most valuable adjunct, or rather adjuvant, to treatment by arsenical or iron salts in cases of early cutaneous syphilis. I customarily use galyl as my principal instrument of attack in such cases, and I have convinced myself that the additional employment of intramine has accelerated the disappearance of hard papular or nodular syphilides, as well as those of follicular, racemose, and corymbose type (which are notoriously and universally acknowledged to be obstinately resistant to ordinary methods of treatment); and in one case of a deeply pigmented rash its use was followed by a remarkable and unexpected recovery. A word of caution may, perhaps, be pronounced against the risk of administering intramine at too short a period after an arsenical preparation. On one occasion, in which I was constrained by circumstances to give an injection of intramine to a woman who had had 0.4 gm. of galyl less than twenty-four hours previously, the characteristic symptoms of an acute galyl poisoning (so well known to all who have wide experience of that drug) were faithfully reproduced. I content myself with recording the fact as a warning, without entering into any attempt to explain the rationale of its occurrence. There can also be no doubt in my mind of the value of intramine in late superficial recurrent cutaneous syphilides, one injection often sufficing to cause their rapid disappearance, and this observation of Mr. McDonagh's is fully borne out by several cases in my own practice. I speak with less confidence as to the efficacy of intramine employed alone in late gummatous manifestations. The cases of this type in which I have been able to witness its efficacy have all been treated either previously or synchronously with arsenical preparations, the relative value of which it seems to me impossible to estimate or discount. I have personally seen nothing of the results obtained by ferrivine or intramine, either in combination or employed alone in cerebral or spinal meningitis or in degenerative encephalitis or myelitis; but I may, perhaps, state in passing that I agree to the full with Mr. McDonagh, that all of these complications have been of much more frequent occurrence since the introduction of salvarsan and other arsenical preparations. I have also no experience to record as to the value of these two new drugs in interstitial keratitis or other manifestations of congenital syphilis.

It would be a poor compliment to him, and my contribution to this debate would be of very dubious utility, if I did not submit Mr. McDonagh's originally conceived and almost revolutionary work to some unbiased criticism. My first and most obvious observation of this nature is that the time is yet far too short and the amount of observation far too small to permit of any definite conclusions being drawn as to the permanence of the results obtained. We must all remember the numerous and sad disillusionments which too frequently followed the bright early promise of salvarsan. It appears to me, however, that the undoubted superiority of action of ferrivine, which I believe to have

witnessed, and probably of other corresponding metallic salts in the primary lesions of syphilis over arsenical preparations, gives some *prima facie* probability of their ultimate action being also more complete and more permanent. All the cases of primary syphilis I have seen treated with ferrivine have been in men, and I question gravely in what proportion of women (with abundant subcutaneous fat and small flaccid veins) such bulky intravenous injections would be tolerated. The great pain resulting from the intramuscular injection of intramine has been loudly proclaimed—almost acclaimed—from many sides. The cry is, I think, to some extent justified. In my own experience the pain, which does not immediately succeed the injection, and which is accompanied by rather high temperature, renders the patient unfit for work for more than a week. In only two instances have I been able to persuade a patient to have two injections of intramine, and both were stalwart men particularly keen to be passed into the Service. This is a widely different experience from that of Mr. McDonagh, who goes so far as to recommend "eight fortnightly intramuscular injections of intramine" alternating with mercurial intramuscular injections. I can scarcely conceive of intramine being applicable for congenital syphilis, but I am bound to add that I have seen cases in which it apparently caused no pain nor pyrexia. And I am aware that the preparation of an intramine derivative is already on the way—if not actually on the market—which is much less painful.

I have observed with a feeling almost akin to sadness that Mr. McDonagh has thrown overboard our old friend the Wassermann reaction. I am well aware that the Wassermann reaction is an excellent servant and a bad master. I realize fully that complete reliance upon it to the exclusion of other clinical methods of observation has led to much bad practice on the part of the medical man, and to much disappointment and even suicidal syphilophobia on the part of the patient. But I am still of opinion that the Wassermann reaction, intelligently employed, is a valuable aid to diagnosis in many cases where diagnosis is doubtful, and is a precious indication of the success or non-success of treatment in carefully appreciated cases. I admit that I do not understand the nature of the substitute for the Wassermann reaction which Mr. McDonagh proposes.

I pass over, for the sake of time, many points which might perhaps be raised and discussed with advantage. There is no doubt that with the medical "man in the street" the opinion is still prevalent that the object to be held in view in the treatment of syphilis is a destructive frontal attack of the element arsenic upon the spirochæte. This theory has, I venture to think, received its death-blow from the researches of Mr. McDonagh and the success of his treatment founded upon them. We, as dermatologists, probably appreciate more fully than the majority of syphilographers how grave the results of the prolonged administration of arsenic may be; and there cannot, I believe, be any doubt that the employment and substitution of the remedies, which are in the ordinary sense of the word non-toxic, constitute an immense advance upon our procedures up to date.



May I be permitted, as a past President of the Section, to offer to Mr. McDonagh my congratulations upon his work, which has been carried out in troublous times and, to my knowledge, in circumstances of great difficulty; work which is eminently original in conception, and which opens up a great and, I believe, fruitful vista of therapeutic utility? And may I likewise congratulate the Section upon the honour of having had this admirable piece of all-British work brought forward and discussed at its meetings?

Lieutenant-Colonel L. W. HARRISON, R.A.M.C.: At the last meeting, very little was said on the biological side of the question. Possibly because those who had any views on the matter felt that if they did not agree with Mr. McDonagh's microscopical interpretations it would be a waste of your time to discuss here matters which could only be settled with the help of a battery of microscopes and an interminable argument. Still, it may not be out of place to discuss the matter on general lines. Mr. McDonagh published his first description of the life-cycle of the micro-organism of syphilis, unless I am mistaken, in 1912. It appeared in English and German periodicals, and Mr. McDonagh read a paper on the subject at the last International Congress, so that I think we can safely say that it has been widely read. He has shown his specimens here and at other societies, including the Pathological Society of Great Britain, so that many expert microscopists have seen them. Further, it is probably true that most biologists who are interested in the subject are favourable to the idea of the micro-organism of syphilis having a life-cycle. It is an idea which has been in vogue almost since the *Spirochæta pallida* was discovered, and it is quite certain that biologists all the world over would be eager to take up any work on the subject. Yet, does it not seem strange that not a single word of confirmation of Mr. McDonagh's interpretations has appeared in print from any microscopist of standing? Actually, to-day the *Leucocytozoon syphilidis* has not a single friend outside its birthplace. Is this the fate of sound discoveries in these days? Are we to assume that those who confirmed in less than three months the discovery of the *Spirochæta pallida*, the Wassermann test, and Noguchi's discovery of spirochætes in the brains of general paralytics—that is to say, biologists all over the world—are so unworthy as to withhold confirmation of this discovery of Mr. McDonagh's. As I have said, the biological world is ready for, and would welcome, the discovery of a life-cycle of the micro-organism of syphilis. But it does not accept the *Leucocytozoon syphilidis*. Mr. McDonagh has built up on this biological foundation a chemistry of syphilis which is so intricate and its terminology so strange in many respects that, as you heard at the last meeting, expert chemists cannot understand it. However, let us grant for a moment that the other biologists and chemists are all wrong; or, if you like, that even if Mr. McDonagh is wrong, he may still have produced remedies for syphilis which are an advance on present treatments. If he had we would gladly leave the chemists and biologists to their quarrel and accept the remedies. Which brings me to the clinical side of the



question. Are ferrivine and intramine specific remedies for syphilis? The firm which is responsible for the manufacture of these compounds very kindly gave me some samples of them, and nothing would please me more than to be able to say they are all they are claimed to be. For the test we took at first three untreated cases of ordinary secondary syphilis. They were cases with lesions from which it was easy to obtain the *Spirochæta pallida* and which we could watch easily from the point of view of clinical progress. Two of them were injected with two doses of ferrivine, then a dose of intramine, and lastly a dose of ferrivine. One received two injections of ferrivine only. All of them were examined daily for spirochætes and for changes in clinical lesions. I should like to say here that the cases were under the immediate care of Captain Mills, whose detailed notes would, I am sure, interest you. I can confirm them from my own daily observation of the cases and agree entirely with Captain Mills's descriptions. I will leave the details to him and confine myself to a summary of the results of our observations.

First, as to the reactions which ferrivine and intramine cause. The reaction from ferrivine is distinctly alarming. Briefly, it amounts to acute respiratory distress, signs of collapse, epigastric pain and most severe vomiting. The temperature may rise to  $103^{\circ}$  F., or higher, and the patient is acutely ill for some hours after the injection. The injection of intramine is followed by a local reaction which is, if anything, worse than that which follows the intramuscular injection of salvarsan. Judging by the patient's description of it one would say it was undiluted torture. I should mention here that four other cases had been treated with intramine some weeks before we started this series and we have since injected another case, so our experience of the local reaction caused by intramine is based on eight cases. It was practically the same in all of them. The local swelling and tenderness last for weeks and abscesses have been opened in two of our cases. One of these is still discharging after twelve weeks and will take many more weeks to heal up. In those cases where no abscess has yet burst outwardly it is easy to feel by the gap in the muscles that there has been considerable necrosis there. Altogether, we have good grounds for disagreeing with Mr. McDonagh when he says that intramine does no permanent damage. I cannot accept, either, any imputation as to the technique of these injections. They were most carefully carried out, with the strictest attention to aseptic details, and the greatest care was taken to make them strictly intramuscularly. Nor were quinine and urea used. I lay stress on the reactions which follow intramine and ferrivine because they are severe enough in themselves to make these compounds quite impracticable for general use, even if they had any therapeutic value. As to the therapeutical effects of intramine and ferrivine, I will omit the four cases which we treated earlier with intramine because they are not a fair test. They were all secondary cases, and the fact that intramine had no effect on their lesions proves nothing against it. Mr. McDonagh has said that ferrivine is like salvarsan—a metallic compound which acts only on the *Spirochæta pallida* and the impregnated female. We may consider whether ferrivine acts on the *Spirochæta pallida*.

It certainly did not do so in our cases. *Spirochæta pallida* was found just as abundantly for ten days or longer after the first injection of ferrivine as on the day before it was first given. After one case had been injected with two doses of ferrivine, one of intramine, and then another of ferrivine, a mucous patch made its first appearance on one tonsil and *Spirochæta pallida* was found in the new lesion. Another case was very instructive. He received two injections of ferrivine, but a mucous patch inside the upper lip remained absolutely unchanged and *Spirochæta pallida* was recovered from it for a week—in fact, right up to the day on which we at last gave the patient an injection of 0.3 grm. salvarsan. On the morning after the salvarsan injection we were unable for the first time to discover spirochætes in the patch, and on the next day the lesion had healed: surely a striking example of the comparative merits of salvarsan and ferrivine, which Mr. McDonagh has told us are alike in their therapeutic effects? As I have said, the addition of intramine to the treatment of the cases failed to produce any better effect. We have since treated with intramine a case of multiple skin gummata in all stages of development; such a case, as we know, does particularly well on salvarsan treatment. The only effect of the intramine has been to give him a buttock which at one time was three times the size of its fellow. So far from any beneficial effect having followed the injection, at least one of the gummata broke down, and all the lesions behaved as if no treatment whatever had been applied. Yesterday I reluctantly consented to this patient receiving an injection of ferrivine. I witnessed the injection and the reaction which resulted from it. In the course of a few minutes I saw a ruddy, healthy-looking man rendered pulseless, gasping for breath, restless, and as nearly dead as I ever want to see any patient. I shall not forget the scene, and if I can ever save a patient from being injected with this remedy I will do so.

On a biological foundation, which has not been confirmed by anyone of standing and a chemistry which nobody seems to understand, Mr. McDonagh has invited—I might almost say commanded—us to forsake salvarsan for ferrivine and intramine. We have found his substitutes not only extremely unpleasant in their effects, to put it mildly, but they have failed to affect three perfectly straightforward cases of early syphilis and one of tertiary—three of them cases which we know would have healed in a week or ten days under salvarsan. Would anyone consider me justified in subjecting any more patients to the injection of these compounds? For myself, I am satisfied, and shall be content to let somebody else's patients bear the brunt of any further experiments with them. I must admit that I had very great compunction about submitting our patients to the intramine torture after ferrivine had failed so miserably to affect their lesions. I have doubts now as to whether I was justified in giving these injections, but if our experience serves to save any further victims, then our patients have not suffered in vain. As to Mr. McDonagh's cases, they carry no conviction to my mind. He says nothing of any standard laboratory test which supports his diagnosis, and in an inquiry of this kind it is unwise to trust any diagnosis which is not

supported in this way. As to the effect of a remedy on tertiary lesions, even a good effect does not prove the remedy to be specific for syphilis. Everyone has seen tertiary lesions disappear under the most various non-specific treatments. I lately saw a woman who for years had successfully treated her recurring gummata with the application of nothing more specific than hot water.

I am afraid I have been rather severe in my criticism. I have favoured a German compound at the expense of a British. I would ask you to believe me when I say that it is not because the one is German and the other British, but because it is vital that we should choose the right remedy, whatever the country of its origin. Mr. McDonagh has exhorted us not to imitate German methods but to support British science and discovery. I echo the sentiment. But is the method which he has adopted of introducing intramine and ferrivine likely to enhance the reputation of British science? I can find no parallel for its dogmatism and other features except in German literature, and if there is one thing more than another which has bred in me a healthy scepticism of remedies which are introduced with a flourish of trumpets it is my experience of the many useless German compounds which I have tested after reading the perfectly convincing articles of their German authors. Ferrivine and intramine have been introduced to the medical public on the strength of a few trifling preliminary experiments. In contrast to salvarsan, which was not placed on the market until 20,000 doses of it had been tried in accredited clinics, these new compounds have been thrust upon private practitioners before any report on them has been received from any independent clinic. I feel very strongly that the course pursued by Mr. McDonagh in introducing these remedies is likely to result sooner or later in a disaster which will not only be terrible in itself, but will have a bad effect on the progress of therapy.

Dr. J. H. SEQUEIRA: In the first place, Sir, I beg to tender to Mr. McDonagh my congratulations on the interest which his work has aroused. I regret that I could not be present at the first meeting, but as the Editorial Representative of the Section I have had the privilege of carefully reading both the opening paper and the discussion. I feel we owe some recognition to the distinguished bio-chemists who, at the invitation of the officers of the Section, came to give us the benefit of their criticism on this intricate subject. We must also regret that time prevented Mr. McDonagh from answering in detail some of the important points raised by Professor Bayliss and others. A survey of the paper shows that it is divisible into two parts: one which is highly speculative and theoretical, and the second which offers us new drugs—"intramine," which is said to have an almost universal application, for it is claimed to be of value, not only in syphilis but also in tuberculosis, lepra, and even in old shrapnel wounds; and "ferrivine," which is specially useful in the treatment of syphilis. On the theoretical side I confess my inability to follow much that has been put forward, and after a careful search in the writings of Mr. McDonagh I have failed to find detailed accounts of the experimental

work upon which it is based. I may, however, be permitted to refer to two points: Mr. McDonagh admits that we have no knowledge of the chemical composition of the lipoid-globulin bodies of which the parasite of syphilis is said to be composed, yet we learn that the "adult male spirochæte and the impregnated female spirochæte are affected by certain remedies in virtue of their free hydroxyl groups." The virgin female spirochæte and the spores lack these free hydroxyl groups, and are therefore unaffected. What proof is offered in favour of this remarkable statement? In passing, I should like to thank Mr. McDonagh for using the word "spore" in this paper. It was so troublesome to be obliged to have a Liddell and Scott by one, and the simpler term suggests a change towards the views of Balfour, Leishman, Noguchi, Fantham, and others. My second question on this side is suggested by the statement that certain lipoid-globulin molecules increase in size during the evolution of syphilis and also in response to certain drugs. Upon this growth of molecules some important deductions are made. In all humility I ask, How does one recognize the growth of a molecule? To come now to the practical points: We are asked to discard the remedies whose inception we owe to Ehrlich and Hata, remedies which, we may remember, were given a prolonged trial in a number of clinics before they were put on the market. I think I am correct in stating that the author of the paper was one of the gentlemen in this country to whom Ehrlich sent material for experiment. We have now had some years' experience of the salvarsan group of drugs, and before we discard them let us ask ourselves for what reasons a drug or drugs should be discarded. In my opinion there are four reasons: (1) If we can replace the remedy by one of greater therapeutic value; (2) if a drug causes toxic symptoms and can be replaced by one that is non-toxic; (3) if it can be replaced by a remedy equally efficacious which causes no pain or discomfort; (4) if, while equally efficacious, the substitute is less expensive. Let us examine these points in order.

(1) In my own limited experience of ferrivine and intramine, I have seen nothing approaching the therapeutic results I have been accustomed to see after injecting salvarsan, neo-salvarsan, galyl, and nov-arseno-benzol. Mr. McDonagh himself says: "*In some cases ferrivine is superior to salvarsan*" (the italics are mine). So that I presume it is usually not superior. Further, has this treatment had a year's trial? I think I am right in saying that intramine was put on the market (that is to say, it was available for persons not working at the Lock Hospital) three months ago, and ferrivine came out a month later. I, like Colonel Harrison, have only used these remedies as they have been supplied to me from the drug house which makes them. Mr. McDonagh states that he is accustomed to pursue a certain course for a year, and the course follows a definite ritual for different types of case. Is it not a little early to be dogmatic when the ultimate results are hardly yet observable? We do not yet know the ultimate value of the salvarsan treatment and shall not for some years.

(2) The degree of toxicity of the arsenical compounds is now known. We



are informed that ferrivine is absolutely non-toxic, and that doses of 2 grm. and upwards may be given with impunity. I have not given more than 1 grm. for a dose, but I have seen obviously toxic symptoms (and I need hardly insist that a drug may be toxic yet not lethal) in every case. Air-hunger and shock are common, for instance. In a perfectly healthy man with a primary chancre, intense collapse immediately followed the injection. He required energetic stimulation before he recovered. Ferrivine is the most rapidly acting emetic I know; severe vomiting may occur even before the needle is withdrawn. Violent diarrhoea with abdominal pain is also a frequent phenomenon. The effects are almost identical with those which may follow the intravenous injection of an antimony compound, and antimony has a much higher specific gravity than iron. So far I have seen no toxic effects from intramine. There may be a slight degree of pyrexia, doubtless due to trauma.

(3) Intramine, however, causes intense pain and discomfort when injected into the muscles, and my experience entirely confirms that of Colonel Harrison and Captain Mills. Even five and six weeks after an injection massive, tender, painful swellings may be left in the site of the injection, comparable with those seen after the intramuscular injection of salvarsan.<sup>1</sup> I find it almost impossible to get a patient to submit to a second injection, and until I have more evidence of its therapeutic value I am not inclined to urge it.

(4) As to expense, Mr. McDonagh, in a recent article in the *Practitioner*, informed us that there was an enormous difference between the cost of manufacture of the salvarsan compounds and their selling price, but the new remedies, even if they are as efficacious, are little, if any, less expensive.

In conclusion, I am satisfied that, at this present time, there would be no advantage—and, indeed, certain grave disadvantages—in discarding the remedies which have developed from the work of Ehrlich. We know that these remedies have a definite therapeutic value, though they do not fulfil all the hopes which were once entertained. Why should we replace them by drugs which cause serious, if transitory, symptoms, or intense pain and discomfort, without benefiting our patients to an equal extent?

Mr. A. SHILLITOE: I have here the reports of nine cases kept for me by Dr. Bonard, Resident Medical Officer to the Female Lock Hospital. I was particularly interested when first I learned that Mr. McDonagh was working on sulphur. We all know that from quite early times the benefit derived from the use of sulphur-containing waters, in many skin conditions, was considerable, though exactly how the sulphur acted was not generally understood. One hundred years ago it was the custom of certain medical men to treat cases of malignant syphilis or cases cachectic from the abuse of mercury, not with sulphur water but with sulphur fume baths, and with very great benefit. For some years past it has been my custom in dealing with a severe case of syphilitic cachexia, or a case going rapidly downhill from

<sup>1</sup> Urea and quinine were used in none of my cases.—J. H. S.



mercurial poisoning, to use no other treatment at first save sulphur fume baths, and in a very short time the improvement is so great that ordinary treatment can be employed. I cannot explain how the sulphur acts. I only know it does so most markedly. Of the nine cases, in Cases I, IV, V, and IX, the improvement was marked; in Cases VI, VII, and VIII not so pronounced; Case VIII developed a fresh lesion after the conclusion of the course; in Cases II and III, little or no improvement; in Cases II, III, V, VI, and VII, all the lesions became at first more pronounced after the injection of ferrivine; in Case IX the reaction to ferrivine was much greater than that to intramine. The pain following the injection of intramine was obviously severe in six of the cases; in the remaining three there were no complaints. The pain generally started forty-eight hours after the injection, and lasted for two or three days, and was sufficiently severe to cause the patients not to look forward with any pleasure to the next injection. In none of the cases did an abscess develop. Some of us may remember how very painful were certain of the mercurial injections—e.g., sal alembroth, cyanide, and biniodide, used years ago. Calomel was formerly so exquisitely painful that certain French authorities said we were not justified in using it. However, acting on improvements suggested by the late Colonel Lambkin, we used it in the Lock Hospital frequently and painlessly in a very large number of cases. Salvarsan, as originally used, was often most painful, but nevertheless it did not debar us from employing it. I think we may honestly claim that these few cases show that ferrivine and intramine have an antisiphilitic action, in some cases as rapid as those exhibited by salvarsan, in others perhaps not so. As iron and sulphur are less toxic elements than arsenic, it certainly appears to me that a united effort should be made to produce compounds of them possessing a better action than those at present obtainable. The present War has brought home to us all in how many ways we were absolutely dependent on our enemies for necessities in daily use. I do think Mr. McDonagh has done or is doing a great work in arousing us from the state of lethargy into which we had sunk. \* Undoubtedly the one objection to the use of intramine is the pain resulting from its injection. But I think we may look forward with confidence to the time when, perhaps with the aid of friendly suggestions from members of this Section, we shall be enabled to overcome this difficulty.

*Case I.*—Patient, aged 31, married, has three children living and healthy; one died last November, aged 5 weeks. A year ago a chancre appeared; no eruption, no throat trouble. For the last two months she has had "bad ulcers" on the body and extremities. Admitted on May 5 with many small ulcers on the labia; vaginitis and general adenitis, also large tubercular syphilides on arms, buttocks and extremities, and pustular eruption on face and scalp. Wassermann reaction positive. May 11: 1 grm. intramine. The following evening the temperature was 102.4° F.; much pain. May 16: Intramine repeated. May 18: The face is better and clearing up, and the large place on the buttock is healing quickly. May 22: Ferrivine; temperature 103° F. May 25: Face still improving; the large sore place on the buttock is cleaner and more healthy, a considerable area in the centre is becoming skinned over, and patient says she feels generally much better. May 26: Ferrivine repeated; temperature again rising to 103° F.

*Case II.*—Patient, aged 21, married; no children, no miscarriage. Three months ago chancre on vulva, and a few weeks later general eruption and sore throat. Admitted on May 2 with oedema of left labium majus, vaginitis, and numerous large mucous tubercles about the vulva and anus; general adenitis, leucoderma cervicis, a general and fading eruption; tonsils inflamed and ulcerated. Wassermann reaction positive. May 3: Ferrivine, repeated on May 7; no untoward symptoms. May 4: Mucous tubercles look more healthy, but are larger and redder. May 16: Intramine, 1 grm. May 21: Rash at site of injection red and punctiform, and very painful. May 23: The rash is fading and assuming a blue colour. May 25: Rash still fading; no pain; the mucous tubercles are about the same; no marked improvement. May 26: Intramine repeated.

*Case III.*—Patient, aged 20, married; no children, no miscarriage; is six months pregnant. Three weeks ago, vaginitis, swelling of labia, and chancre. Admitted on April 29 with oedema of the labia, a large indurated chancre at the fourchette, slight vaginitis, double inguinal adenitis, and a macular eruption on the abdomen. Wassermann reaction positive. May 3: Ferrivine, repeated on May 9; hardly any reaction. May 5: Oedema more pronounced; chancre dark in colour and discharging freely; the spots on the neck are fading rapidly. May 11: Intramine, repeated on May 16. May 18: Very painful, lesions not clearing. May 22 and 26: Ferrivine. May 25: The injections of intramine were very painful for two days; practically no change in the symptoms.

*Case IV.*—Patient, aged 30, married; three children, no miscarriage. A week ago first noticed vaginitis and a "pimple" on the vulva. Admitted on April 27 with a large dry sore on the left labium, and a second one at the upper part of the right thigh; marked left inguinal adenitis. Wassermann reaction positive. April 28: Ferrivine; no reaction. April 30: Lesions clearing up. May 2: Intramine; not much immediate pain; the places are very nearly healed. May 4: Local pain very great; temperature 102° F. May 9: Intramine. May 11: Great pain; vomiting; temperature 102° F.; the sores are healed, but surface not quite level with the skin. May 16: Ferrivine; no particular reaction.

*Case V.*—Patient, aged 21, single; no children. Wassermann reaction positive. Since October, 1915, has suffered from a yellow discharge. Noticed "sores" on vulva two weeks ago, but has had an eruption for two months. Admitted on April 29 with mucous tubercles on vulva, marked inguinal adenitis, leucoderma cervicis, and a few fading spots on the skin. May 3: Ferrivine, repeated on May 9; stood the injection well, but during the night had pain in right side, with vomiting and feeling of giddiness. May 4: Tubercles look clean and dry, but more swollen than yesterday. May 11: Intramine; temperature rose two days later to 103° F. May 15: Tubercles on vulva are healed. May 16: Intramine; no particular reaction. May 22 and 26: Ferrivine. May 25: The tubercles remain quite healed; the leucoderma has faded but is still evident.

*Case VI.*—Patient, aged 25, single; no children, no miscarriage. Three months ago had a sore on the vulva, and eruption on body one month. Admitted on April 12 with sores and mucous tubercles on vulva and anus, vaginal discharge, sore throat, glands enlarged and covering the body in a maculo-papular, very pigmented eruption. April 26 and 28: Ferrivine; no reaction, temperature normal. April 28: The sores on vulva are larger, redder, and more swollen. April 30: Ulcerations and tubercles are much better, smaller, and healing. May 2: Intramine; in forty-eight hours the temperature ran up from 97° F. to 103° F.; the vulva is healed, not much change in the body eruption, but patient says she feels better generally. May 9: Intramine; great pain; the eruption is fading. May 16 and 19: Ferrivine. May 25: The eruption is flattening.

*Case VII.*—Patient, aged 20, single; no children, no miscarriage. Wassermann reaction positive. Six weeks ago she noticed eruption on body, and four weeks ago "lumps" on the vulva. Admitted on April 18 with cedematous labia, covered with excoriations, adenitis, and general roseola. She is four months pregnant. April 26 and 28: Ferrivine; no reaction, save slight vomiting. April 28: The ulcerations are tender and redder, and the oedema has

increased. April 30: The ulcerations are healing, the edema has disappeared, and the sores are dry and healthy. May 2: Intramine; pain slight. May 4: Very great pain. May 9: Intramine. May 11: Great pain; sores not much better. May 16 and 19: Ferrivine; no reaction. May 18: Sores practically healed; the labia are still swollen, and the glands still enlarged.

*Case VIII.*—Patient, aged 30, married; no children, no miscarriage. Admitted on April 6 with several sores on labia and mucous tubercles, adenitis, leucoderma cervicis, sore throat and laryngitis. April 26 and 28: Ferrivine; no reaction, but slightly giddy. April 28: Ulcerations about the same. April 30: Throat much better. May 3 and 9: Intramine. May 11: Sores healed; considerable pain. May 16 and 19: Ferrivine. May 25: This patient never complained about anything after the injections, which she stood very well, but there was pain; the sores are all healed; the glands are about the same; a fresh sore with an infiltrated base is appearing on the right labium majus. (This patient was in the hospital from August 5, 1915, to September 11, with early syphilis, during which time she received, with other treatment, one injection of Billon, 0.3 grm., and one of neo-kharsivan. She was deaf. Left facial paralysis and fits when a child. These conditions dated from the age of 5, when she had measles.)

*Case IX.*—Patient, aged 38, married; no children, no miscarriage. Wassermann reaction positive. Three months ago she had a general eruption, and five weeks ago first noticed sores on vulva. Admitted on March 29 with mucous tubercles on vulva; general roseola. April 26 and 28: Ferrivine; temperature rose to 101° F. and 103° F.; patient much collapsed, no pulse, cyanosed, difficulty in speaking and breathing; this occurred about three hours after the injection. April 30: Patient feels better. May 2: Intramine; mucous tubercles have disappeared and the roseola is fading. May 4: Site of injections very painful. May 9: Intramine. May 11: Feels very stiff and sore; no temperature. May 16 and 18: Ferrivine; was very bad after these. May 25: Sores quite healed; vulva very well; nothing to be seen.

With regard to the alarming sequelæ mentioned by Colonel Harrison and Dr. Sequeira, it was only in two cases out of the nine that we had a really serious reaction following ferrivine. Certainly in Case IX the reaction was rather alarming for the time being. Dr. Bonard informs me that we had one, but only one, case of aseptic ulcer. There were no other complications whatever in any of the cases.

Dr. H. G. ADAMSON: I have only given injections of intramine in cases of lupus. I have given sixteen injections of intramine in eight cases, and at first I thought I was going to get most excellent results. In the first two cases injected there was remarkable improvement, and those were cases in which there were ulceration and crusting. The ulceration healed up at once and the infiltration seemed less. The difficulty consists in getting the patients to submit themselves to further injections, though I think that difficulty is not so serious as some speakers would have us believe. One of my patients has had four injections, and is ready to have another, on account of the improvement which has taken place in the lupus. But I feel it is as yet much too early to say whether intramine is going to do lasting good in lupus. There can be no doubt that the injection is painful, for it keeps the patients in bed for three days at least. It is therefore necessary to take the patient into hospital, and as there are not many beds available this is rather a slow process.



Captain C. H. MILLS, R.A.M.C.: The following are my clinical notes upon all four of the cases which Colonel Harrison has mentioned; they are mostly details as to the microscopical findings in the daily examinations of lesions, with observations as to progress:—

*Case I.*—R., aged 27. Healed indurated frenal chancre of two months' duration; anal mucous patch (dry); pigmentation of macular rash still present on trunk; mucous patch on upper lip, fauces ulcerated; *Spirocheta pallida* present. May 3: First injection of ferrivine, 1 grm., diluted to 200 c.c. with distilled water; vein washed through afterwards with normal saline; patient became extremely collapsed on table with respiratory distress; pain in epigastrium followed by repeated vomiting; vomited during the night in ward, slight rigor; temperature 100° F. May 4: *Spirocheta pallida* still present in mucous patches; patient recovered from reaction. May 5: Second injection of ferrivine, 1 grm. (technique as above); patient again collapsed on table and vomited; profuse perspiration; temperature 99.4° F. May 7: Veins of both arms thrombosed; *Spirocheta pallida* still abundant on unaltered mucous patch on lip. May 9: *Spirocheta pallida* still present in same. May 10: *Spirocheta pallida* still abundant in the morning; it was here decided to discontinue the ferrivine course, and 0.3 grm. salvarsan was administered without reaction. May 11: *Spirocheta pallida* disappeared from mucous patch (which has markedly altered in appearance) eighteen hours after 0.3 grm. salvarsan injection. May 12: Mucous patch healed; no *Spirocheta pallida* found after a thorough search by different observers.

*Case II.*—F., aged 25. May 1, 1915: Multiple indurated ulcers in coronal sulcus of glans penis; papular lichenoid syphilide, nape of neck, around elbows, and over both hips (symmetrical), also faint erythema; *Spirocheta pallida* present in chancres; no albumin in urine. May 1: First injection of ferrivine, 1 grm., solution diluted to 200 c.c. with distilled water; vein washed through afterwards with normal saline; severe reaction whilst on table; respiratory distress; pain in epigastrium; pulse very weak, profuse perspiration, vomited; slight rigor, 6 p.m.; temperature 101.2° F. May 2 to 5: *Spirocheta pallida* in chancres on daily examination; vein used for injection thrombosed; rash unaltered. May 5: Second injection of ferrivine, 1 grm. (technique as above); similar reaction and vomiting; temperature 101.6° F. May 7: 1 grm. intramine injected intramuscularly. May 8: Bad night; severe pain from intramuscular injection; patient vomited during night; temperature 101.6° F.; *Spirocheta pallida* still present in chancres, which are not yet healed. May 10: Third injection ferrivine, 1 grm.; less reaction; patient vomited in ward; temperature 101.6° F. May 12: Veins of both arms thrombosed; cannot bear pressure at site of injection of intramine; *Spirocheta pallida* still present. May 14: Fresh papules appeared on both flanks; snail-track ulcers developed over right tonsil; *Spirocheta pallida* present in same; both new lesions; intramine injection very painful and swollen. May 16: Both fauces ulcerated; *Spirocheta pallida* present; penis healing; intramine injection painful, infiltrated, and fluctuating in centre. May 17: Treatment with salvarsan and mercury commenced.

*Case III.*—D. M., aged 38. Condition on admission (May 2, 1916): Large indurated mass at frenum under phimosed prepuce; duration approximately six weeks; profuse papulo-pustular rash general, general adenitis, anal mucous patch, moist papules on scrotum and inner surface of thighs; fauces ulcerated; no albumin in urine. *Spirocheta pallida* present in moist papules under dark ground examination. May 3: 3.30 p.m.—First intravenous injection of ferrivine solution, 1 grm., diluted to 200 c.c. with distilled water, and vein washed through with normal saline; whilst on table patient exhibited sudden respiratory distress, pain in epigastrium; turned ashy-grey colour; pulse became imperceptible; broke out into profuse perspiration and was then violently sick. 6 p.m.—Rigor, temperature 102° F., vomited again, severe headache; diarrhoea ensued during the night, with persistent vomiting; symptoms subsided the following day; temperature 99° F. May 4: *Spirocheta pallida* still present in moist papules. May 5: Second injection of ferrivine, 1 grm. (same technique);

similar reaction to above; severe rigor at 6 p.m.; temperature 103.4° F.; *Spirochæta pallida* still present; vein used at previous injection now thrombosed. May 7: Injection of intramine, 1 grm., into gluteal muscles; "rough night," pain excruciating; *Spirochæta pallida* still present in moist papules. May 8: Site of intramine injection very swollen and tender; marked œdema of lumbar pad; patient unable to lie on side of injection; *Spirochæta pallida* still present in moist papules. May 9: *Spirochæta pallida* still present; patient somewhat more comfortable. May 10: Third injection of ferrivine, 1 grm.; reaction not so severe, but patient again vomited; temperature 99° F. May 12: Rash beginning to fade (nine days after first injection); papules on scrotum still moist; *Spirochæta pallida* present; site of intramine injection swollen and tender; superficial veins of both arms thrombosed from insertion of deltoid to 6 in. above wrist. May 14: *Spirochæta pallida* still present. May 16: *Spirochæta pallida* abundant in chancre. May 17: Site of intramine injections infiltrated and painful; much albumin present in urine, with epithelial casts; ulceration of fauces more marked; treatment with salvarsan and mercury commenced.

*Case IV.*—P., aged 39. History: Contracted syphilis four years ago. Commenced treatment during the secondary stage consisting of mercury pills and mixture, and continued same for two years; five weeks ago a gumma developed over the head of the left fibula. Condition on admission: He now has multiple gummata on each leg; some can be felt as nodules in the muscles, some subcutaneous, and others in the various stages of necrosis with resultant ulcerations; there is one large ulcer situated over the left popliteal space; the upper extremities, head and trunk are unaffected excepting for one small gumma over left lower ribs; pupils react equally well to light and accommodation; reflexes normal; Wassermann reaction positive. May 24, 1916: 1 grm. intramine injected into the muscles of left gluteal region. May 25: Patient passed a sleepless night from exquisite pain at site of injection; hot fomentations applied. May 26: Left buttock very tender, painful and swollen (see photograph, p. 186); morphia administered. May 30: Much œdema of lumbar cushion; no albumin in urine; pain from injection less; swelling more localized, and can be made to fluctuate. Gentle massage followed by hot fomentations; two gummata, which had not previously involved the skin, have now broken down and are discharging through same. May 31: Two fresh gummata have developed over the head of the right fibula. June 1: To this patient I administered yesterday 1 grm. of ferrivine (solution diluted to 200 c.c. with distilled water) intravenously. It was not without a feeling of grave responsibility that Colonel Harrison finally gave his sanction to this injection, taking into consideration the reactions previously witnessed during and after the introduction of ferrivine. I must state emphatically that the reaction which resulted in this instance was the most severe I have ever experienced after an intravenous injection of any of the antisyphilitic remedies with which I have worked up to the present—that is to say, in an experience of upwards of 9,000 intravenous injections. The alarming symptoms came on towards the end of the injection, commencing with severe epigastric pains, followed by marked dyspnœa. The pulse then became imperceptible, and the patient turned an ashy-grey colour, when strychnine was immediately administered hypodermically, and for a period of five minutes there was grave doubt as to the patient's survival. He was finally resuscitated and passed a disturbed night; he was still having rigors when last seen this morning. [June 9: The above reaction gradually subsided, with occasional attacks of vomiting, by the morning of June 2. At the date of this note the gummata have shown no signs of repair whatsoever. There is a large fluctuating and very tender swelling at the site of the intramine injection.]

*Conclusions.*—The first three cases above described were chosen simply by virtue of their exhibiting abundant clinical manifestations, in order that the therapeutic effect of these drugs could be watched by several observers from day to day. There were present accessible lesions from which the *Spirochæta pallida* could readily be obtained, and so a daily search for the same was rendered both simple and accurate by the dark ground illumination. Now it might be



urged, of course, that it is absurd to attempt to form any opinion upon but four cases which have been subjected to this combination of drugs. But I would point out that, having at the present day upwards of five years' clinical experience with salvarsan, we are justified in drawing a comparison, and in stating as a result that one injection of salvarsan would have done more towards clearing up the lesions in these cases in twenty-four hours than did the whole four injections of the drugs under discussion. Again, there is no getting away from the fact that the mucous membrane lesions—the most



The above photograph was taken forty-eight hours after the intramuscular injection of intramine 1 grm. into the left buttock in Case IV. The X of strapping indicates the point of insertion of the needle, the injection being made slowly, to minimize tissue tension, into an area approximately 2 in. external to this. That the deposit was strictly intramuscular was verified on palpation by several medical officers present at the time. The local reaction is well illustrated, and can be gauged by a comparison with the normal buttock.

infectious, but happily the most amenable to treatment of all lesions—were as rich in *Spirochæta pallida* after the ferrivine injections as before. One injection of salvarsan, even 0.3 grm., is sufficient to render the finding of the

*Spirochæta pallida* in a mucous patch an extremely rare occurrence within as little as twenty-four hours after the injection. This was emphasized in the case in which two injections of ferrivine failed in any way to affect the mucous patch on the upper lip, either with respect to the healing of the lesion or in bringing about the destruction of the spirochætes. Yet, as recorded, within eighteen hours of an injection of only 0·3 grm. of salvarsan not a single *Spirochæta pallida* could be discovered in repeated examinations; and, moreover, the patch had healed by the following day. What is of even greater importance to my mind is the fact that not only did these drugs fail to influence the clinical manifestations in any way, but fresh lesions developed whilst the patients were undergoing treatment—namely, the ulceration of the fauces in one case, and the appearance of fresh papules in the other, in each of which *Spirochæta pallida* could be demonstrated. One of the greatest assets, from a sociological point of view, of salvarsan (and indeed of most of the other organic arsenical compounds now being substituted) is the rapidity with which the infectious lesions are rendered non-infectious. I have not been able to prove that ferrivine and intramine have any influence in this direction. Though I had been able to prove that intramine and ferrivine were even superior in effect to salvarsan, neo-salvarsan, kharsivan, galyl, nov-arseno-benzol or luargol, I would yet hesitate to substitute them for any of these until they had been subjected to far more exhaustive trials, controlled by microscopical and serological findings, in several disinterested laboratories, such is the severity of the reactions, local and general, produced. I have nothing to add to the description I gave last month before this Section of the local effect produced by an intramuscular injection of intramine, beyond the fact that necrosis has ensued in every instance, though fortunately the subcutaneous tissues and skin have only broken down in two instances. A discharging sinus still exists in the one, and has now remained obdurate for twelve weeks. I consider that apart from any toxicity that ferrivine possesses, the solution is far too acid to be put upon the market for indiscriminate administration to the public. I would like to mention, now, the cases which Mr. McDonagh has brought up for us to see this afternoon. In two of those cases one could definitely feel that the injection of intramine was encysted, and on puncturing some “mucoid” material would escape, as in our own experience with these residual abscesses. In another case, in which instance the patient has been under these drugs for over two months, and I presume is shown because he is doing well, he has definite lesions on his fauces and an anal mucous patch. I feel positive that I could demonstrate the *Spirochæta pallida* from either of these lesions to-day. It was in this very room—less than four weeks ago—at the meeting of the British Ophthalmological Congress that I was privileged to hear Mr. Ernest Lane’s remarks upon his experience with these drugs in his wards at the London Lock Hospital. I need only state that his description of the reaction resultant upon the administration of intramine and ferrivine, local, general, and therapeutic, tallies with our own findings. If anything, he painted a darker picture.

Dr. C. H. BROWNING : On attempting to weigh the evidence which has been advanced this evening regarding the therapeutic effect of ferrivine, one is struck by the absolutely conflicting character of the statements. On the one hand, independent observation has confirmed the striking influence of the drug on the primary sore in cases treated by Mr. McDonagh himself ; from the chemical standpoint it does seem remarkable that ferrivine, which from the formula given appears to be the iron salt of the well-known substance sulphanilic acid, should exert such a powerful specific action in syphilis ; but still one must always be prepared for surprises in a field as yet so little investigated. On the other hand, several observers of great experience have shown that when administered by themselves in similar cases little or no therapeutic effect could be detected. As regards intramine, extended experience seems to have led Mr. McDonagh himself to modify his previous statement ; at the last meeting we heard from him that interstitial keratitis yielded to intramine, and I think I expressed the general opinion of those present when I commented on the remarkable advance which this achievement signified. To-night, however, it appears that Mr. McDonagh has met with interstitial keratitis which was resistant to intramine. It is evident that judgment on the therapeutic action of these drugs must remain in suspense until there has been a much more extended trial in the hands of a number of independent observers.

Sir MALCOLM MORRIS, K.C.V.O. : I have come here to listen rather than to speak. As I said at the meeting a month or so ago, I am under a grave responsibility at the present time, having been a member of the Royal Commission on Venereal Diseases, and having had to advise a Government Department as to the remedies that should be supplied throughout the country free of cost. I confess that to give up the remedy that has stood the test in tens of thousands of cases and to switch off to another which so far has been tried, as Mr. McDonagh says, in less than 300 cases, is a course which does not commend itself to me as rational or justifiable. He himself exhorted us, in the early days of salvarsan, not to accept that remedy until a sufficiently long experience should enable us to form a competent judgment of its merits. At that time he was very enthusiastic about salvarsan, and gave addresses in various parts of the country on its advantages ; but he warned us not to be in a hurry to accept the results claimed for it. I would remind him of that now in connexion with his own compounds. I have a perfectly open mind in the matter. If a new remedy were discovered to-morrow which would destroy or suppress the spirochæte without producing toxic effects in the patient I should be among the first to welcome it. But I know of no sufficient evidence that such a new remedy has been discovered. I wish Mr. McDonagh success in the future. I admire his energy and his abundant labours ; but, as one much older than himself, I may venture, perhaps, to suggest to him the propriety of submitting his remedies to further tests, and not to press them upon the profession until he has obtained a much more considerable body of evidence of their value.

Mr. JOHN WARD : I should like to add that I was asked by one of the most eminent bacteriologists in London to go and see the life-cycle of the *Spirochæta pallida* prepared by Mr. McDonagh, and, on behalf of the Crookes Laboratories, I went. I expected to find bits of stained protoplasm which might be found under conditions frequently associated with degenerative tissue. I do not know that I am particularly qualified, but we have over 25,000 stained, indexed, and numbered slides, and I was convinced that Mr. McDonagh's statements are correct and that he has the life-cycle of the *Spirochæta pallida*, as is represented by his slides. I do not see how anyone, looking at them from an unbiased point of view, could possibly mistake them, nor could their appearance be likened to that of degenerative tissue. The stains employed, especially pyronin and methyl green, are not easily or generally used, but when obtained the result is excellent and a brilliant example of the theory of staining. I am convinced that any bacteriologist who saw these slides and carefully followed the *modus operandi* of each stain employed could not come away without being convinced of the proof of this life-cycle. I am only too pleased to think its discovery was due to an Englishman.

The PRESIDENT : As the time at our disposal has expired I will not now detain you by any observations of my own on the subject which is before us except to say that, so far, my opportunity of forming a practical opinion on the use of the drugs that have been so well advocated by Mr. McDonagh is practically *nil*. For the treatment of syphilis I employed neo-salvarsan and neo-kharsivan, until the introduction of galyl. Since then all my cases requiring intravenous injections have been treated with galyl, and the results have been very satisfactory. It will necessarily require a long observation of the relative value of these new agents before it is possible to arrive at anything of the nature of definite conclusions. And I feel confident that Mr. McDonagh, by the energy he has shown, the careful and detailed studies he has made, and the knowledge of chemistry he possesses, together with his desire permanently to advance the treatment of syphilis, will not fail to give the fullest consideration to the views and criticisms of other practical observers before he comes to his own final conclusions. In these important matters—so important, as you have heard, that Government action will probably be taken upon them—it is our bounden duty in the interests of the community to secure not merely what is good but that which is certainly the best. I wish to thank all the speakers for their attendance to-day, and the members of our Section who have contributed to the success of our meeting. I will now call upon Mr. McDonagh to reply.

Mr. McDONAGH (in reply) : Before I brought up the subject it was universally thought that the action of salvarsan was a direct one upon the parasites, and that arsenic was an essential ingredient of the chemotherapeutic preparations. I venture to think I have conclusively proved that neither of these accepted opinions is correct. My object in introducing intramine and



ferrivine was first to substantiate my theory of oxidation and reduction, and secondly to show that non-toxic elements like sulphur and iron have a therapeutic action in syphilis. Intramine and ferrivine were not introduced immediately to replace salvarsan and its substitutes, as disadvantages which required to be overcome attached to both. Intramine is painful, but in my experience the pain it causes is nothing like so severe as that described by several of the speakers. From the accounts given of the pain and from the photograph shown it is perfectly clear that the best site for intramuscular administration of drugs is not so widely known as would be expected. No speaker has offered any suggestion for ridding intramine of this disadvantage. Ferrivine did at first cause shock in some cases, a drawback now overcome, but not with help derived from discussions. No suggestions have been offered how to carry out the improvements with ferrivine, the need for which must be clear to those who understand the foundation upon which chemotherapy is built. Unless I am able to get the necessary assistance in this country, I shall seek it abroad, as I am convinced that an improved iron compound will be invaluable in the treatment of syphilis. Concerning Colonel Harrison's remarks on the life-cycle, I should like to ask him if he has repeated any of the work. Two observers have done so on the Continent and they have confirmed my results. Because English biologists have not repeated the work is no argument against the correctness of mine; indeed, it is a good argument in favour of it, since medical history has shown that the longer it has taken work to be accepted the sounder it is, and vice versa.



## Dermatological Section.

President—Dr. J. H. STOWERS.

(Dr. J. J. PRINGLE, Vice-President, in the Chair.)

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(June 15, 1916.)

### A New Test for Syphilis (the Emulsoid-gelation or Gel Test).

By J. E. R. McDONAGH, F.R.C.S.

MY research work has led me to look upon the serum as a colloidal system in which the protein particles form the "solid" or internal phase. The particles are emulsoid—i.e., they are partly in solution—therefore they will contain water. The particles in a syphilitic serum differ from those in a normal serum. The size and number of the particles in a syphilitic serum are greater than those in a normal serum—a fact that can be easily proved by examination under the ultra-microscope; but as this method is not always available, I have proved the point in other ways. A syphilitic serum contains more protein nitrogen than a normal serum, more adsorbed amino-groups, and more adsorbed electrolytes; note the readiness with which syphilitic sera clot. None of these methods can be used as a routine for examining sera, so I undertook further experiments, and to-day I have the pleasure of exhibiting before you my new test for syphilis.

So long as the particles are in "solution" it is naturally impossible with the naked eye to distinguish one kind of serum from another, but if the colloidal form of the particles could be changed and an electrolyte used for their precipitation, to distinguish sera which contained varying quantities of protein would be a simple matter. In order to bring this about I employ glacial acetic acid, which throws out the protein as a gel. The presence of the gel can be made still more apparent by the addition of the sulphate anion, since the gel as it exists in an acid medium will have a positive charge. Thinking that that sulphate anion, which was the

salt of a heavy metal, would have the greatest precipitating action, I chose lanthanum sulphate. At first I thought the changes which resulted from adding glacial acetic acid to lanthanum sulphate was a transformation of the protein particles from the emulsoid into the suspensoid state, hence I called the reaction the emulsoid-suspensoid reaction, or, briefly, the E.-S. test. As the precipitate formed by the glacial acetic acid lanthanum sulphate is soluble in water and reversible, I presume the changes which occur are those of gelation.

Whatever be the more correct explanation of the changes which take place, there is no doubt that, by adding glacial acetic acid to very small quantities of serum, a syphilitic may readily be differentiated from a normal serum by the degree of the opacity caused and the rapidity with which a precipitate forms. The differences can be made more apparent and be produced more rapidly by the addition of a suitable electrolyte, such as lanthanum sulphate. These are the tests exhibited:—

## NORMAL SERUM.

	Tube—	1	2	3
Glacial acetic acid	...	1.0 c.c.	1.0 c.c.	1.0 c.c.
Serum	...	2 drops	3 drops	4 drops
Saturated solution of lanthanum sulphate in glacial acetic acid	...	0.2 c.c.	0.2 c.c.	0.2 c.c.
Result read twenty-four hours later		f. p. <sup>1</sup>	+	—

<sup>1</sup> f. p. = complete flocculent precipitate; (f. p.) = partial flocculent precipitate; + + + +, + + +, + +, +, —, = varying degrees of opacity, water being — = perfectly clear.

## SYPHILITIC SERUM.

Result read twenty-four hours later	f. p.	...	(f. p.)	...	+
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In the above tests the differences are most marked the next day. In order to read the results quickly, the following procedure is the best to adopt: To a tube containing 2.0 c.c. of glacial acetic acid put 0.5 c.c. of serum. Then obtain four test tubes and add the following quantities of the acid serum:—

## NORMAL SERUM.

	Tube—	1	2	3	4
Glacial acetic acid	...	1.0 c.c.	1.0 c.c.	1.0 c.c.	1.0 c.c.
Acid serum	...	2 drops	4 drops	6 drops	8 drops
Saturated solution of lanthanum sulphate in glacial acetic acid	...	0.2 c.c.	0.2 c.c.	0.2 c.c.	0.2 c.c.
Result in a few minutes	...	+	+	+	+
Result in one hour	...	+	+	(f. p.)	(f. p.)
Result in four hours	...	(f. p.)	(f. p.)	(f. p.)	f. p.

## SYPHILITIC SERUM.

	Tube—	1	2	3	4	
Result in a few minutes	...	(f. p.)	...	(f. p.)	...	(f. p.)
Result in one hour	...	(f. p.)	...	(f. p.)	...	f. p.
Result in four hours	...	(f. p.)	...	f. p.	...	f. p.

The CHAIRMAN: I shall be glad if anyone who has positive knowledge on this subject will help us towards its elucidation. I have ascertained the fact that these sera have been taken from patients who have not been treated in any way. As no one seems to wish to discuss the matter, I will simply say I hope Mr. McDonagh will go on and will continue to show us the results which he obtains, because I am sure we are all enormously interested in the subject which he has opened up so energetically.

(June 15, 1916.)

## Case of Pityriasis Rubra Pilaris.

By W. KNOWSLEY SIBLEY, M.D.

THIS boy, aged 17, has had the skin condition of which he complains for the last nine years. He states that he had a bad attack of measles and on recovering from that this rash has persisted on his upper lip and the scalp, and ever since that day he had had an eruption on many parts of the body. The question arises as to whether he really had measles. A year or two later he was told he had ringworm of the scalp, but there again I doubt the diagnosis. X-rays have never been applied to his scalp, but he presents a typical extensive condition of pityriasis rubra pilaris, and he has considerable alopecia cicatrizata, with superficial scarring on top of the scalp. There is a marked infiltration of the hair follicles on the eyebrows and the backs of the fingers, as well as on other parts of his body, especially over the arms, forearms, back, lower abdomen, and thighs. Why he should have this seborrhœic condition round the mouth I do not know.

In June, 1913, I showed a little boy, aged 9, who, in the opinion of most of the members who were present, had pityriasis rubra pilaris and psoriasis. I hoped to have had him up to-day for comparison. At the time I showed him he was distinctly ill, with an elevated temperature. I gave him radiant heat baths, and thyroid gland internally. A few weeks afterwards he had become quite well. The only remaining evidence that he ever had pityriasis rubra pilaris was that he still had

marked infiltration of the hair follicles on the fingers and over the coccygeal region. From that date he has kept comparatively well. Recently he has had signs, but only slight, of another psoriasis attack,

With regard to treatment, on the supposition that this condition is allied to psoriasis, I have commenced to treat this case with X-rays. Three weeks ago he had one-third of a pastille of X-rays applied to the right hand and forearm, repeated at the end of a week. He has now had three treatments over that region, and if we compare that region with the opposite side we see considerable improvement: individual follicles no longer stand out on that hand, and after a few more such applications I think the condition will clear up.

In a photograph of the boy whom I showed in 1913, you see an extraordinary hyperkeratosis of the skin of the face. The thickening subsided, but marked scars were left by the sides of both eyebrows. I do not know whether scarring following this condition has been described previously.

#### DISCUSSION.

The CHAIRMAN: It has always struck me as an extraordinary fact that a disease so characteristic in its features as pityriasis rubra pilaris should not have been recognized by the dermatological public of this country until the year of the first Paris Congress, 1889, when several cases of it were shown. Ever since then we have all been perfectly familiar with it—that is to say, with its gross appearances. But I remember that before 1889—about 1885—Dr. Liveing showed a case of this sort at the Dermatological Society, which he called “a peculiar psoriasis.” That passed, and was supposed to be a satisfactory description. But perhaps the diagnosis was not so far amiss as one might think, because the curious relationship of this disease to psoriasis has been fairly well established by Dr. Sibley in his previous case which we remember, by Dr. Adamson, Dr. Graham Little, and by others in this country. The two diseases sometimes coincide, sometimes they alternate; and although the lesions of the two conditions are different in every way, there appears to be some sort of correlation between them. In this case the seborrhœic distribution of the disease is very marked. I suppose that ultimately this boy will get quite well, as the immense majority of such cases do.

Dr. H. G. ADAMSON: I do not think it is by any means the general opinion that pityriasis rubra pilaris is related to psoriasis; my own view, which I have expressed before, is that pityriasis rubra pilaris is in some way related to psoriasis, since attacks of psoriasis sometimes alternate with pityriasis rubra pilaris in the same patient, and Dr. Little and others have recorded cases of pityriasis rubra pilaris with psoriasis in other members of the family.

Dr. GRAHAM LITTLE : Dr. Adamson has referred to my case of a girl who had pityriasis rubra pilaris for at least nine years. I saw the sister, and she had a perfectly typical psoriasis, which had existed upon her for many years. I rather deprecate associating the two : pityriasis rubra pilaris is an extremely rare disease. If there were any causal connexion with psoriasis I do not see why it should not be met with more frequently than is the case. I also think the histology is different as well as its behaviour to treatment, for I do not feel sanguine about the treatment of these cases by any of the means at my disposal. In some cases, however, the condition has disappeared spontaneously, as in one instance under my observation.

Dr. MACLEOD : I have observed the association of psoriasis and pityriasis rubra pilaris, but I do not consider that they are phases of one disease, for remedies which influence psoriasis have no apparent effect on pityriasis rubra pilaris. With regard to Dr. Sibley's reference to treatment of the pityriasis rubra pilaris by X-rays I am not so sanguine as to the X-rays causing the pityriasis rubra to disappear. In my experience the X-ray treatment of psoriasis is not always effective, for although certain patches melt away under the influence of the rays, occasionally old-standing patches do not appear to respond to them unless the X-rays are given in dangerous doses.

The CHAIRMAN : It occurs to me that a case so extensive as this might yield to light baths, as lichen planus sometimes does.

Dr. S. E. DORE : I should like to ask the exhibitor if he regards the patch of alopecia on the scalp as due to the same process which affects the skin of the body. It is well known that this disease affects the scalp, but, as far as my experience goes, it does not cause cicatricial patches. The boy says he has had much discharge from the scalp, and the baldness might be due to some other process ; it resembles folliculitis decalvans very closely.

Dr. SIBLEY (in reply) : I regard the condition of alopecia cicatrizata as the result of the pityriasis rubra pilaris, which has affected the hair follicles of the scalp. He is supposed to have had ringworm, but I doubt it. With regard to light baths, I treated the other patient whom I brought forward three years ago with electric light baths and he got well. As this boy has only had X-ray treatment three weeks, it is early yet to know what the result is likely to be. I think it will clear up the condition in that area, and I shall be pleased to bring the case forward again if it does so.



(June 15, 1916.)

## Case of Darier's Disease.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a woman, aged 28, sent to me by Dr. Grosart Wells, of St. Albans, who has had the case under observation for the past twenty years, and to him I am indebted for permission to bring the patient here to-day and for some excellent notes. Dr. Wells writes: "June 14, 1916: I have known this girl for nearly twenty years, and a skin affection of the upper lip and around the mouth dates back nearly the whole of the time. I looked upon it as an eczema, and upon her general condition as strumous, knowing she had some still earlier hip-joint disease. Her mother at the time (twenty years ago) had been in receipt of parish relief for a previous thirteen years, having fibroid phthisis. The mother is still alive. For years I did not see this girl, and until just before the date you very kindly saw her. In the interval she had attended a sea-bathing institution, and been under treatment, returning with a certificate that she was suffering from favus, and this diagnosis I passed on to you with the girl. Ointments and medicines never gave any satisfactory result in the case."

The eruption seems to have become troublesome to the patient more especially some twelve years ago, being notably present on the forehead, the scalp, the chin and neck. From these parts it spread slowly but persistently to occupy the areas in which it is now found: the chest, where it is very copious and forms a fairly continuous sheet with the eruption on the neck; over the mammaræ and below these, and again around the navel, and on the side of the thorax and over the suprapubic region. There are more scanty and discrete lesions scattered over the back of the trunk and on the upper part of the thighs. These lesions are very uniform in appearance, consisting of a yellowish keratotic excrescence about  $\frac{1}{8}$  in. in diameter and about  $\frac{1}{8}$  in. in height. Less characteristic and less frequently met with, and then mostly in parts where the eruption is scanty—as for example on the shoulders—are some flatter wartlike elements, resembling the "juvenile flat wart." On the dorsum of the hands there are several

of these, as well as some quite typical common "cauliflower" warts, these probably being of later and accidental development. The nails are split vertically on several digits of the fingers but not on the toes. They have never been shed. The skin of the palm is thickened and hyperkeratotic. The scalp is extensively affected, probably more so than any part, and the small elevations can be readily felt, on passing the hand over it, as an almost continuous sheet of eruption; but notwithstanding this involvement and long continuance, for the eruption appeared here very early, there has been no loss of hair. There is a particularly thick distribution of the characteristic papules behind the ears. The face—the chin and the temples especially—are conspicuously affected. On the chin there is a crusted seborrhœic-looking eruption disguising the underlying papular sheet, and it is probable that the affected skin readily becomes eczematous, for when she first came to my department the case was for the moment mistaken for a seborrhœic eczema, until examination of the covered parts showed the real nature of the disease.

The patient gives a history of having had an abscess of the hip-joint at the age of 12 months, which was followed by ankylosis of the left hip-joint. Probably as a result of the lack of freedom of movement in the joint there is a chronic and rather foetid vegetative growth in the sulcus separating the thigh from the abdomen and the vulva. But there are no vegetations elsewhere, and it may be mentioned in particular that the axillæ, where these vegetations are frequently met with, are quite free, though there are numerous keratotic papules here. There is no family history of similar eruptions.

*Comments.*—The case reproduces with extraordinary fidelity the chief characteristics in the magistral description by Darier of his original cases. Thus the initiation of the eruption on the face and temples and scalp, the absence of loss of hair, the development of the disease at an early age, the changes in the nails, the warty lesions on the hands, which, as Darier remarked, "can in no way be distinguished from flat warts," the extraordinary resistance to treatment, the seborrhœic distribution, are all present. The colour of the lesions is perhaps unusually yellow, so much so as to allow of a diagnosis of "favus" by well-qualified medical men. The diagnosis is established by the absolutely typical appearance of the sections.

*Histology:* A patch showing very characteristic lesions was excised from the abdomen. Sections were stained with Gram's stain and examined for micro-organisms, but with a negative result. Sections

stained with hæmatoxylin and eosin showed a characteristic follicular hyperkeratosis, the stratum corneum being very thickened, especially about the follicles, which were plugged with scales in the affected area. The whole rete seems rather more loosely attached to the corium than usual, and dehiscence is frequent. The changes in the epithelial cells, which led to the earlier and erroneous identification with psorosperms, are very pronounced, especially in the uppermost layers of the rete. Here there are very numerous double-contoured "corps ronds," some



Case of Darier's disease. **A A**, "corps ronds" of Darier; **B**, Fissure in deeper layers of rete; **C**, plug of stratified horn cells and debris.

of enormous size, as is well shown in the accompanying sketch, which was drawn from a case under Dr. Winkelried Williams, who asked me to cut sections of it. There is no resemblance to a papilloma visible in the section, although the clinical simulation of this effect was so close.

## DISCUSSION.

Dr. H. G. ADAMSON: On first seeing this patient I did not think it was Darier's disease. I thought the lesions were not so soft or greasy as they are in Darier's disease. I regarded them as merely multiple plain warts. She certainly has quite ordinary warts on the hands, and those on the shoulders when viewed under the glass closely resemble plain warts. But the microscopical section makes it quite clear that the diagnosis of Darier's disease is the correct one.

The CHAIRMAN: I think the microscopic evidence in favour of Darier's disease is unquestionable and decisive. The likeness of the lesions on the shoulders and hands to warts is, however, very striking. The condition appears to me to have progressed unusually slowly if she has had it for twelve years. A few cases of the disease which I have seen, especially in France, have, I think, been much more rapid in their course. The "keratosis follicularis" cases of Dr. J. C. White, of Boston, were surely much acuter? We are very much indebted to Dr. Little for having brought the case; I have learned very much by seeing it.

Dr. MACLEOD: I regard Dr. Little's case as typical of Darier's disease. I had his patient under my care at Charing Cross Hospital for three years, from 1909 to 1912, and had ample time for studying her eruption. The condition of her skin does not seem very different from what it was then. At that time all manner of local treatments were tried, but without marked benefit. In 1904 I had the opportunity of studying a case which was under Dr. Ormerod at St. Bartholomew's Hospital, a report of which was published by Dr. Ormerod and myself in the *British Journal of Dermatology*, September, 1904. That case was very much like the one exhibited to-day, except that the lesions were slightly pigmented and the face not so markedly involved. The nature and ætiology of this rare affection remain unknown. The "corps ronds" seem to me to be due to an imperfect process of cornification of the prickly cells, which, to a slight extent, resembles that which occurs in molluscum contagiosum, and I have wondered if this affection could not be an infective condition due to some micro-organism which finds a suitable soil in certain individuals in the situations usually affected.

Dr. SIBLEY: This is a very interesting discussion. You say, sir, you have seen other cases, all of which have been of more rapid progress than this. Therefore one wonders whether there is anything special in this patient's history. The patient had a hip accident followed by abscess, which may possibly have been tubercular, as it generally is in such cases; that may account for the retardation of the process. If the condition is warty, why not try ionization with magnesium sulphate or zinc? It could be tried on the hands first of all, and if successful its use could be extended to other parts of the body.



Dr. GRAHAM LITTLE (in reply): When this patient first came I was puzzled: it was a very busy out-patient morning. I saw on her a mass of what seemed to be ordinary eczema of the face, which showed an extensive weeping condition. But I was puzzled by the duration. She appeared to have had it for a very long time, and further examination revealed the condition on the body. This is the first case of Darier's disease I have had under my own care, though I have been associated with other cases, and I have no experience of treatment. If it is infective, as suggested, one can scarcely understand a disease so long-lasting giving rise to no histories of infectivity. Hereditary transmission, but not actual infection, has been frequently noted. There is no history of inheritance in this case.

(June 15, 1916.)

### Case of Lichen Planus Hypertrophicus.

By H. C. SAMUEL.

THE patient is a married woman, aged 65, with two children. The condition for which she attended St. Bartholomew's Hospital began eighteen years ago. Eleven years ago she was under the late Dr. Colcott Fox and Dr. Adamson. The present state is fairly typical of lichen planus hypertrophicus, but I am not so familiar with this appearance of what looks like centrifugal spread with scarring in the centre, a condition one associates with granulomata. The mucous membrane of her mouth is affected, and she confesses to the presence of another patch on her back. As usual, the disease is extremely resistant to treatment. A large variety of applications, X-rays amongst them, have been made, as usual without result. The scarring in the centre is not the result of X-rays, as the appearance was the same before the X-rays were applied. I attempted to treat some of the spots with CO<sub>2</sub> snow, but the patient would not submit to this. In one case of lichen planus hypertrophicus recently under my care I practically got rid of the lesions by firm and long exposures to CO<sub>2</sub> snow; this was a case which had failed to be influenced by X-rays and local chemical applications. The patient in the case I am showing says that "water comes away at times" from some of the spots, but I have never seen a vesicle or bulla when she has been under examination.

I shall be grateful for suggestions as to treatment. Do the members regard the central areas as scar tissue? If so, do they agree that it is



spontaneous and not the result of treatment? Ought one, in that case, to label it "atrophicus" as well, or has lichen planus atrophicus (so-called) a different clinical meaning?

#### DISCUSSION.

Dr. GRAHAM LITTLE: I showed a case a year or two ago of very pronounced lichen hypertrophicus in which I got extremely good results by shaving with a razor until the parts became sensitive, then freezing the shaved portions with snow. I think X-rays are useless, and I have given up using them for it; I much prefer the method I have mentioned. If you only do a little at a time you can get patients to submit to it fairly readily.

Dr. S. E. DORE: Some time ago I showed here a case of hypertrophic lichen planus as extensive as this, though the lesions were much smaller. It was suggested by more than one member who was present that it was a factitious dermatitis and not lichen planus at all. The patient is still under my care, and I have never been able to influence the condition by treatment. There is intense itching, and she has set up deep scarring of the lesions by scratching. In this case, too, I think the scarring may be due, in part at least, to scratching and not entirely to spontaneous atrophy.

Dr. KNOWSLEY SIBLEY: This patient has had X-rays applied to some of those lesions. I am not applying X-rays in cases of lichen planus hypertrophicus any longer; I am reluctant to do so. A few years ago I applied the rays to a patient, and as a result she developed a typical patch of hypertrophic lichen planus on the back of the hand, the lesion being of the same diameter as the diaphragm used. The condition then appeared on various parts of her body. On one occasion she scalded the back of her hand with hot water, and the seat of the bleb was followed by lichen hypertrophicus. At one time she had extensive ulceration of the arm, and it was eighteen months before it healed up, which it eventually did under treatment with scarlet red ointment.

The CHAIRMAN: I think the treatment which Dr. Graham Little has suggested was introduced into this country by Mr. Morratt Baker more than thirty years ago. He used it with success, and it is the best measure at our disposal for many of these cases. I am glad to hear a general confirmation of my own experience as to the failure of X-rays. I have never seen benefit follow X-rays and I have seen harm from them. Carbon dioxide snow has also proved disappointing in my hands.

(June 15, 1916.)

### Case of Dermatitis Herpetiformis.

By ALFRED EDDOWES, M.D.

THE patient, a boy, aged 5, for three months has had a vesicular eruption breaking out symmetrically just above the buttocks. The vesicles are always clear when they first appear in little groups, but are soon broken by scratching, after which they sometimes suppurate. There are pitted scars to be seen. The mother tells me that the child had a similar outbreak on the leg further down two years ago. During the last three months the eruption has been entirely confined to the buttock region. It is bilateral, and has very much the distribution of herpes. There is a family history of tubercle on the mother's side. The boy has a barrel-shaped chest, and I am told that the abdomen has been more prominent than it is now. He has shown great liability to catch cold, and rhonchi can be heard, though as yet there are not any signs of consolidation. I shall be glad to hear any criticisms of my diagnosis.

### DISCUSSION.

Dr. S. E. DORE: I suggest that this child's eruption is due to the irritation of the urine on the skin. I showed, about a year ago, a case in which a boy had a similar eruption as the result of nocturnal enuresis, and he is still under my care. On giving him thyroid both the eruption and the nocturnal incontinence improve, but relapse when the drug is stopped. This patient suffers from incontinence of urine at night, and I do not think there is sufficient evidence on which to make the diagnosis of dermatitis herpetiformis: the eruption is localized, it has only been three months under observation, and has only been treated quite recently.

Dr. G. W. SEQUEIRA: I suggest that this case is one of bullous impetigo. The child has one or two disk-like lesions about the mouth, covered with an adherent scale. Sabouraud considers this condition due to staphylococci. I think the bullous impetigo in this case is due to such infection.

Dr. MACLEOD: I am not quite convinced of the correctness of the diagnosis of dermatitis herpetiformis, nor am I certain that dermatitis herpetiformis does occur in childhood. The chronic pemphigus of children, cases of which have

from time to time been recorded as cases of dermatitis herpetiformis, does not itch, nor are the lesions, as a rule, grouped in a herpetiform manner.

The CHAIRMAN: I also have some doubts as to the diagnosis of this case, as I have of any case diagnosed as dermatitis herpetiformis in a patient aged below 12. There are so many possible causes of local infection that one cannot eliminate them, and certainly the lesion here does not seem to be the characteristic one of dermatitis herpetiformis as it is seen in adults. There is a general consensus of opinion that many cases put down as dermatitis herpetiformis in young children are merely infected urticarial lesions, and such I believe to be the nature of the case exhibited.

Dr. GRAHAM LITTLE: I am disappointed that the Chairman should lend his authority to the statement that dermatitis herpetiformis is not to be diagnosed in a patient aged under 12. I think it is very much a dispute about names, whether we call it pemphigus or dermatitis herpetiformis. It does not really matter if we mean the same disease. It is my experience that there is a disease which to me is definitely dermatitis herpetiformis, and which is encountered not infrequently under the age of 12. Only this morning I saw in my out-patient department two cases in children under the age of 10, who have been attending there for some months, two perfect specimens of what I should call dermatitis herpetiformis—i.e., extremely itchy grouped bullous eruptions, persisting for long periods, controllable by arsenic, relapsing immediately when the drug is withheld.

The CHAIRMAN: I draw a firm clinical distinction between dermatitis herpetiformis and pemphigus. Pemphigus in children is quite frequent, but dermatitis herpetiformis is extremely rare.

Dr. EDDOWES (in reply): I have at present in hospital a perfectly typical case of extensive dermatitis herpetiformis, and I think the patches in that case are extraordinarily like the patches in this. This child marks easily with scratching, and there is an urticarial element in the dermatitis. In fact, both in this case and in the other I have mentioned, before the vesicles come out raised patches are formed upon which the vesicles develop. There is no sign of pus at the beginning. One patch on the front of the abdomen in my adult case is of special interest. It is now becoming pigmented, just as are the affected areas in the present case. As this boy has a wheezy chest, I shall be glad to give him small doses of arsenic. May not the nocturnal enuresis go with this? Are not these patches in an area in which bladder reflexes would be manifest?

(June 15, 1916.)

## Sections from a Papilliferous Sudoriparous Cyst of the Cheek in a Man.

By E. G. GRAHAM LITTLE, M.D.

THE patient is a master baker, aged 54, under the care of Dr. S. A. Clarke, of Horley, who sent him to me on May 8. He was a tall thin sallow man, much overworked, and his occupation, which entails long hours in a hot and stuffy atmosphere, probably had some connexion with the development of the tumour. This had been first noted some sixteen years ago, so that it appeared when he was aged about 38, as a single, circumscribed, colourless, painless elevation which had slowly grown larger. It was situated on the left side of the chin about halfway between the outer angle of the mouth and the ramus of the lower jaw. During the past few months it had grown somewhat faster, and a deep bluish colour on the surface had replaced the previous colourless aspect which it had maintained for many years. The surface of the skin over the tumour was and had always been intact, no discharge or scab having been noted at any time. On palpation the swelling was found to be larger than inspection of it seemed to indicate, and its base occupied an area of about  $\frac{3}{4}$  in. It was deep-seated and freely movable with the skin, to which it was obviously firmly attached. It was raised about  $\frac{1}{2}$  in. above the general level of the surrounding skin, and was about the size of a medium Barcelona nut, but gave the sensation of being considerably more voluminous below that level. It was moderately hard and tense to the touch except for a perceptible softening on the surface, especially on the blue part, which did not cover the whole extent of the prominence. No enlarged glands could be found in connexion with it, and there was no tenderness on manipulation. The disfigurement was considerable and the patient was readily persuaded to allow its removal, which was effected by my colleague, Mr. Aubrey Pannett, to whom and to Dr. Kettle, Pathologist to St. Mary's Hospital, I owe my thanks for the specimen.

Sections were cut in paraffin and stained by various methods. On naked-eye inspection of the sections it was obvious that the greater part

of the swelling was formed by a large cyst of very irregular shape, with numerous papilliferous projections into its lumen from its walls. The general bearings were best revealed by examination with a low magnification, ocular 3, objective of a focal distance of 3 in. being used. The drawing appended was made with this magnification, by the help of a camera lucida, and the proportions and topography may thus



Sections from a papilliferous sudoriparous cyst of the cheek in a man. A, large cyst; B, dilated sweat glands in papilliform projection; C, smaller cyst; D, hair shaft; E, papilla of hair; F, sebaceous gland; G, blood-vessels.

be taken as strictly accurate. The epidermis is seen to be intact throughout; numerous sections were examined at different levels of the block, and nowhere was there any opening from the cyst communicating



with the surface, from which it was in fact separated by a very considerable zone of felted fibrous tissue, which formed a thick capsule over it. The epidermis immediately above the cyst was thinned, the interpapillary processes flattened out, and the hair follicles and sebaceous glands mostly displaced by fibrous tissue. Beyond the margin of pressure exerted by the cyst these appendages were quite normally present. Further inspection revealed, in addition to the main large central cyst, a number of smaller cysts, one especially, situated more deeply, being about one-fifth of the size of the chief distension and having like this numerous papilliferous projections from the walls. The smaller cysts were of much less size, tailing down to merely dilated sweat glands, which were very numerous, indicating a distinct hyperplasia of gland tissue. In several cases it was possible to see coil glands apparently opening on to the main cyst, but with this exception there was no break in the continuity of the containing wall of the cyst. In parts this was thinned to the dimensions of almost a single row of cells, in other parts there was a considerable thickening of the wall, so that it was several layers thick. The epithelium lining the cyst was mostly columnar in shape; in the very thin parts the cells were more of a pavement shape, and in the thickened areas the cells forming the wall were in many places in actual continuity with the walls of sweat glands, some of which, as has been said, apparently discharge into the lumen of the main distension. The lumen of the two larger cysts and of some of the smaller cysts was filled with blood corpuscles, and the presence of blood doubtless explains the blue colour of the tumour in its later growth. The vessels throughout the section seem rather thin-walled but not otherwise altered. There is no resemblance to a hæmangioma.

*Comments.*—This type of growth would seem to be infinitely rare, for the only instance quite certainly comparable in my opinion with my case is the single case reported by Rolleston.<sup>1</sup> This tumour also occurred upon the face, was not congenital, appearing for the first time when the patient was aged 17, and the same curious papilliform projections from its wall into its lumen were a conspicuous feature. Rolleston regarded his case as of the same nature as two reported earlier, one by Robinson and one by Elliott. Robinson's case was apparently not congenital, for it is stated that the growth had been noticed only a short time, the patient being a girl, aged 13. There is no

<sup>1</sup> *Brit. Journ. Derm.*, 1904, xiv, p. 83.

mention made of sweat glands being in any way abnormal; and beyond the statement that the contents of the cyst were a clear non-sebaceous fluid, which was not analysed, there is no very convincing evidence of its connexion with the sweat apparatus. The tumour was in the axilla, and the argument was used that in that situation sweat glands are especially active. Histologically the papilliform projections into the lumen from the lining wall of the cyst were especially remarked. There is a remarkable case reported by Thimm, in which very numerous tumours (as many as 150 being ultimately counted) made their appearance on the skin of a youth at the age of 17, chiefly over the sternum, but also on the neck, axillæ, and elbows. This perhaps may be classed with Robinson's, Rolleston's, and my cases respectively. Sections from one of these nodules showed a large cyst in the corium at the level of the sweat coils, which were greatly hypertrophied and distended. The case reported by Dr. Adamson on behalf of Dr. Norman Paul, of Sydney, in the last issue but one of the *Proceedings*<sup>1</sup> of this Section, is apparently also of this nature, and the lesions similar in position. The patient was a man in whom the first development of tumours had taken place at the age of 19, over the lower end of the sternum, and in whom, as in my case, the growths later assumed a blue colour, which was demonstrated histologically to be due to extravasation of blood into the dilated cysts. Some other cases, which are regarded by some authorities as of the same nature, are sharply differentiated by the fact that the cysts which have been described (on doubtful data, as it seems to me) as connected with sweat structures occurred in tumours which in other respects, and especially in their congenital origin, were undoubtedly to be classed with the nævi. Petersen's case, Elliott's case, and four cases described by Walters as nævi syringo-adenomatosi were of this type; in all these the histological features include more or less solid downgrowths of epithelium from the epidermis into the corium, after the manner of nævi, of the type of moles. To my mind this fact offers a very convincing differentiation from the case reported above, in which the epithelial hyperplasia was deeper in situation and found chiefly at the level of the sweat coils, being, indeed, conspicuously absent in the zone immediately below the epidermis.

<sup>1</sup> *Proceedings*, 1916, ix, p. 100.

Dr. H. G. ADAMSON: At a recent meeting of this Section, as stated by Dr. Graham Little, I showed sections of a case for Dr. Norman Paul, of Sydney, in which the lesions were clinically very like those of Dr. Little's case. The microscopical appearances were, however, not so frankly those of sweat-gland tubules, but those of a more embryonic, less differentiated "syringoma," and such as are found in cases of multiple syringomata or "lymphangioma tuberosum multiplex." Notes of Dr. Paul's case were also published in the *British Journal of Dermatology* for June, 1916.

## Dermatological Section.

President—Dr. J. H. STOWERS.

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(July 20, 1916.)

### Case of Xanthoma Tuberosum.

By E. G. GRAHAM LITTLE, M.D.

THE patient is an Irish lady, aged 50, living near Dublin. Ten years ago a swelling began to appear in the fleshy part of the right upper arm above the elbow. It was painless and gave no discomfort, but increased slowly in size, and there is now a tumour, about  $\frac{3}{4}$  in. in diameter, freely movable with the skin to which it is firmly attached, and in which it appears to sink to the depth of about  $\frac{1}{2}$  in. This is probably also xanthomatous, but the colour is a much fainter yellow than the other lesions about to be described. Two years ago small lemon-yellow nodules began to appear on the backs of both elbows, where there is now an eruption of closely aggregated typical xanthomatous tumours, from  $\frac{1}{4}$  in. to  $\frac{1}{8}$  in. in diameter, raised to about the same degree above the level of the surrounding skin, quite painless and indeed giving rise to no subjective sensations, but spreading in extent so that the backs of the elbows are quite covered with eruption. More recently similar nodules have broken out on the front of the knees, where they are still very few in number, and on the skin over the lower sacral region. There is nothing in the mouth. The furrows of the palm of the hand are slightly yellower than the normal skin and probably there is an early xanthomatous change here. With this exception the skin is everywhere else normal. The urine has been examined for sugar and acetone, neither of which is present. There is

no history of diabetic inheritance and the patient enjoys excellent health. She has never been pregnant and has been married twenty years. She still menstruates normally. Whilst in Ireland she had two applications of X-rays to the elbows, without any effect on the nodules.

In its distribution and general characters the case reminds me strongly of a patient, a London surgeon, shown by Mr. Willmott Evans to the Dermatological Society of London in 1902.<sup>1</sup> In that patient the eruption disappeared after some applications of X-rays, but I understand reappeared in the same positions shortly after, and although at the time of showing there was no evidence of diabetes he died of that disease some seven years later. In a case of very remarkable xanthomatous tumours on the hands and feet in a girl aged 6, brought by me before the International Congress in 1913, in whom the diagnosis was confirmed by histological data, there was a family history of diabetes, but the patient herself showed no symptoms of weakened sugar tolerance after several tests, and there was no glycosuria nor acetonuria. In this case freezing with carbon dioxide effected considerable, but I understand only temporary, improvement. X-rays, which were given a prolonged trial had no effect. A coloured photograph and description of this case was contributed by me to the *Medical Annual* for 1915, p. 679.

I believe there is good reason to regard xanthoma planum as of different nature to xanthoma tuberosum, but I consider this disease and the so-called xanthoma diabeticorum as essentially the same affection. The development of xanthoma tuberosum should thus be considered as a possible forerunner of diabetes, and frequent examinations of the urine should be made throughout such a case, and the diet should be regulated so as to throw as little strain as possible on the patient with regard to the elimination of sugar. I confess I am sceptical of any advantage to be derived from local treatment short of excision of the developed nodule. As the condition is usually troublesome only because of the disfigurement, treatment is relatively unimportant as compared with the attempts at prevention of the development of diabetes.

#### DISCUSSION.

Dr. G. PERNET: I remember the condition of the surgeon referred to and that it cleared up wonderfully under the X-rays. Recently I had, in private, a very extensive case with plaques of xanthoma on the knees, elbows, and regular tumours round the Achilleian tendons.

<sup>1</sup> *Brit. Journ. Derm.*, 1902, xiv, p. 465.



Dr. MACLEOD: Members will perhaps recollect a most remarkable case of this disease which I showed several times at the Dermatological Society of London and also at the last International Congress of Medicine. The patient is a man with a more extensive xanthoma than I have ever seen or read of in any other case. I have had him under observation during the last six years and have had ample opportunity of trying all manner of local treatments. So far the results have been most disappointing; I thought there was some improvement after X-ray treatment, but relapse followed. Carbon dioxide snow gave equally unsatisfactory results, and finally I have abandoned local treatment. What has interested me most in the case is that, in spite of the ever increasing deposition of fat in the skin and other tissues, I have been able to detect nothing abnormal either in the blood or urine. Dr. Topley, of Charing Cross Hospital, kindly investigated the blood for me from every point of view of which he could think, but was unable to detect any definite abnormality.

Dr. PRINGLE: The lesions sometimes involve the arteries; I remember seeing an aorta studded with them.

The PRESIDENT: We shall be glad to hear what treatment you intend to adopt. It is fortunate that the lesions are not on more exposed parts of the body.

(July 20, 1916.)

### **Tuberculous Disease of the Face of Five Years' Duration in a Girl aged 18½.**

By J. H. STOWERS, M.D. (President).

THIS is an unusual case of so-called "lupus vulgaris." The disease commenced in the mouth about the left angle, and developed into a superficial irregular erosion, with ulceration of the mucous membrane of the lip and gums. Typical lupus nodules appeared on the skin below the left angle of the mouth, and a well-defined patch upon the chin. There was well marked enlargement of the glands on both sides of the neck below the rami of the jaw and around the carotid vessels on both sides. Much of the disease disappeared under treatment both general and local. The nose and throat are unaffected.

In 1914 fresh nodules on the gums appeared, and still exist in spite of remedies.

I am exhibiting her here to-day on account of the large mass of

tuberculous tissue which now involves the right cheek. The infiltration, which is  $\frac{1}{2}$  in. in thickness and is of rounded form and  $1\frac{1}{2}$  in. in diameter, is gradually increasing. In spite of very numerous applications both of X-ray and Finsen light, extending over a period of eighteen months, no satisfactory result has been obtained.

I shall be glad to hear any suggestions from members as to appropriate treatment. I may add that the general health of the patient is satisfactory.

#### DISCUSSION.

Dr. PERNET: Have iodide of potassium and chlorine water been tried in the case?

Dr. A. EDDOWES: May I suggest that the infiltrated tissue should be injected with a non-irritating preparation of mercury? I have already mentioned to the Section a case of mine, a man, in whom a large mass of tuberculous tissue occupied the whole of one side of the buttock. I injected the mass, in three different sections, with various preparations of mercury. Eighteen months afterwards I was astonished to find the infiltration gone, but I do not know which mercurial preparation did most good. He had several patches of lupus about him, and I amputated a piece of one of his ears for the condition. The buttock patch, however, was too large for excision. I also think this girl's case is one *par excellence* for the internal administration of arsenic.

Dr. KNOWSLEY SIBLEY: I presume iodide of potassium and chlorine water have been administered? [The PRESIDENT: Yes.] I suggest, Sir, that you try dusting or blowing on to the areas of ulceration in the mouth a powder consisting of equal parts iodide of potassium and chalk, and afterwards get her to hold her mouth full of chlorine water as long as she can. I have had some cases which have done extremely well on that treatment. And I would continue the internal use of iodide of potassium and chlorine water.

Dr. R. PRICHARD: Has an ointment of biniodide of mercury, 4 gr. to the ounce, been used? It has a very good, penetrating effect.

The PRESIDENT (in reply): Pfannenstiel's treatment was adopted in the earlier stage without success, and the only other local applications have been carbolic acid, chromic acid, the acid nitrate of mercury, oleate of mercury, and a pyrogallic acid ointment. General tonics, including iodide of iron, and cod-liver oil, with malt, have been administered.

(July 20, 1916.)

## Dermatitis Herpetiformis in a Boy.

By GEORGE PERNET, M.D.

FOR the opportunity of showing this case I am indebted to Dr. H. P. Potter, of the Kensington Infirmary, where the patient has been under treatment for some time. The lad, aged 9, was first seen by me at the West London Hospital on February 18, 1916, when the following notes were made: Duration, sixteen months. Began with redness followed by blisters. The lower part of the neck and clavicular regions are here and there reddened with vesicles. In the region of the umbilicus, the lower part of the abdomen, and upper part of the thighs, there are erythematous areas with a number of vesicles scattered about on them, also bullæ  $\frac{1}{4}$  in. in diameter; on the back of the neck a group of subsidiary vesicles; on centre of back and about buttocks subsiding erythema. Patient's skin gets quite clear at times but fresh attacks occur. The patient had had a variety of treatment, *intra et extra*. I recommended the trial of salicin internally, and lotio calaminæ. I saw the patient two or three times, but owing to a rigor, and a temperature of 104° F., he was prevented attending as arranged, and I have not seen him since until the present moment. Now he has a very severe attack, which started a month ago after an interval of freedom of three weeks, so I am informed. At the present moment the upper part of the chest and abdominal region in front are covered by erythema, vesicles, and large bullæ, in various stages of development and involution; at the back the eruption is less in degree. Itching is not complained of, but pain. This brings the case into the category of Brocq's *Dermatites polymorphes douloureuses aiguës récidivantes*. The difficulties in the way of treatment in these cases are well known.

## DISCUSSION.

Mr. SAMUEL: I suggest that this is not a case of dermatitis herpetiformis, but one of pemphigus, and for the following reasons: The lesions start as blisters pure and simple on apparently normal skin. I have inquired most carefully into that point, and the present amount of erythema is all subsequent and not antecedent to the bullæ, which is strong evidence against the diagnosis of dermatitis herpetiformis and in favour of pemphigus. Again, the boy does not complain of itching, but only of soreness, which is what one

expects in pemphigus but not in dermatitis herpetiformis. Dermatitis herpetiformis must be extremely rare in children. Another point against the diagnosis of dermatitis herpetiformis is the fact that the child has been definitely ill, which is not a feature in that disease except as a result of insomnia due to intolerable itching (which is absent in the present case). Therefore I think we should entitle this case "pemphigus in a boy."

Dr. GRAHAM LITTLE : We had a similar discussion on terminology on the same case when I showed it here, eight or nine months ago, as dermatitis herpetiformis, which is my preference of the two names in a condition where there is considerable itching, as there then was, and very scanty eruption. At that time he did exceedingly well on arsenic; the benefit was almost instantaneous. I do not know whether he has had any lately.

Dr. PERNET (in reply) : The lad is not under my care; he is in the Kensington Infirmary under Dr. Potter. When the boy came to the West London Hospital he had distinct erythematous patches, and on some of them were vesicles and bullæ; some of these were grouped. That attack was not such a severe one as the present outbreak. It is well known that Kohn-Kaposi never admitted there was such a condition as dermatitis herpetiformis; he said that what went under that name was pemphigus. But there is no doubt that there is a dermatitis herpetiformis type of vesiculo-bullous eruption associated with erythematous patches, and this case is of that kind. These lesions are very painful; and that brings it, as stated, into the Brocq category of *Dermatites polymorphes douloureuses récidivantes*. Itching may be absent in children, as in a typical case which followed varicella that I saw with Radcliffe-Crocker. Certain vesiculo-bullous eruptions are related to one another, but, for purposes of diagnosis, we put forms like the present into the dermatitis herpetiformis group. I agree that arsenic may be of service here. That he has already had. I recommended the trial of salicin.

(July 20, 1916.)

## Extensive Exuberant Lupus Vulgaris of the Leg originating from a Tuberculous Abscess.

By GEORGE PERNET, M.D.

THE patient is a girl, aged 15. The disease dates from the age of 3, and followed an abscess of the left thigh which was, no doubt, tuberculous. The depressed scar of this abscess is in the middle of an irregularly-rounded patch of lupus some 6 in. across, which is well raised above the level of the skin, especially at the lower border. On

the outer and posterior part of the left leg there is a large elongated oval patch of lupus, the long axis being vertical. Although the disease has been slowly extending for twelve years there has been no treatment apart from ointments. The upper patch was the first, and this was followed four months later by the lower focus of disease. The patient is of the fair, ethereal type so prone to tuberculous infections.

(July 20, 1916.)

### Multiform Lupus Vulgaris following Measles, with Spontaneous Involution of some of the Lesions.

By GEORGE PERNET, M.D.

THE patient is a girl, aged 8. The lupus vulgaris foci followed measles and came out together. Now there are three typical small lupus vulgaris areas on the left cheek, the lowest one of which discharged up to six or seven months ago. On the same cheek and above the foregoing lesions there is a small scar. On the right cheek there are several small depressed scars of similar lesions, according to the mother's statement. There is also a small scar on the left calf  $\frac{1}{4}$  in. in diameter, and another, rather oval,  $\frac{1}{2}$  in. by  $\frac{1}{3}$  in., low down on the right calf, the long axis being vertical. The child suffers from chilblains. No history of phthisis can be gathered from the mother, but the fair, blue-eyed type of the patient is one that is prone to tuberculous disorders.

(July 20, 1916.)

### Case of (?) Urticaria Pigmentosa in a Soldier, aged 19.

By W. KNOWSLEY SIBLEY, M.D.

I SAW this man for the first time this afternoon: he was sent to me by Captain Picton Phillips, R.A.M.C., for a diagnosis. He has an eruption over his trunk, arms and thighs. It came out suddenly two months ago, and is in the same condition now as then. He had not been taking medicine. The skin is slightly urticarial, but not markedly so. It does not itch, and there are no lesions in the mouth.

[Discussed with the next case.]



(July 20, 1916.)

### Case of Urticaria Pigmentosa in an Adult.

By E. G. GRAHAM LITTLE, M.D.

THIS patient, a man, aged 35, was sent to me by a Medical Board to ascertain whether he was fit for enlistment in the Army. He is otherwise a robust fellow, suffering little discomfort from his eruption. This appeared, apparently acutely, at the age of 17, and has persisted unchanged ever since. It consists of small red and reddish brown stains thickly distributed over the wrists, forearms and arms, and much less extensively on the trunk, both back and front. The face is quite free. The macules are about  $\frac{1}{4}$  in. in diameter, and become turgid and vividly red on rubbing; there is considerable factitious urticaria, but very little spontaneous itching.

I have advised the Medical Board that the man should not be accepted for military service. I think my experience is that of most of us—namely, that these cases are invariably mistaken for syphilis by the general practitioner and still more so by the layman, and as the man is very averse to military service he would almost certainly find means to aggravate his eruption, and perhaps still more certainly meet with a medical officer who would regard the rash as syphilitic, not to speak of the disquiet which his eruption would cause his room mates.

*Histological Examination.*—A portion of skin was removed from the upper arm. The section, stained in the ordinary manner with hæmatoxylin and eosin, shows little abnormality. Stained, however, with polychrome methylene blue, and washed with glycerine ether until almost all the stain is removed, the section is typical of urticaria pigmentosa. The deposit of pigment in the basal layers of the rete is pronounced, but irregular in degree; in some parts of the diseased area the deposit is several layers thick, in others almost absent. The other characteristic feature of the histology of this disease, the enormous increase in mast cells, is much more noticeable and regular, the vessels of the papillary body and of the subpapillary plexus being abundantly surrounded with mast cells.

## DISCUSSION.

Dr. GRAHAM LITTLE: I regard Dr. Sibley's case as undoubtedly one of urticaria pigmentosa. The absence or moderate degree of itching is in my opinion no objection to making that diagnosis. In 1905 I collected cases and wrote a paper on this subject, in which after an analysis of the then published cases, I wrote: "It will be seen that in a very large proportion of cases the itching was either very moderate in degree or entirely absent. I am satisfied that *severe* itching must be regarded as a very rare symptom." It is certainly a remarkable circumstance that the Section should have two cases presented on the same afternoon of adult urticaria pigmentosa, for in the series of 142 cases of the disease collected by me in the paper referred to, only fourteen generally accepted cases were found occurring in adults.

Dr. PRINGLE: I think there can be little, if any, doubt as to the nature of Dr. Little's case. But in regard to Dr. Sibley's, I admit there are many links of evidence wanting. There has been no antecedent urticarial condition, the eruption does not itch, and the patient has not got an appreciably urticarial skin. My first impression was, and remains, that it is probably an adult case of urticaria pigmentosa, but I am not certain that it may not be an example of the still rarer condition described by Jacquet and Darier, about thirty years ago,<sup>1</sup> under the name "*Hidradénomes éruptifs*," a condition which appears suddenly, unattended by subjective symptoms of any sort. None of Jacquet's cases were anything like so extensive as this, but I do not think the mere question of extent ought to decide the diagnosis. I have seen one or two cases of Jacquet's disease in this country, in which it was confined to the chest. I think a microscopical examination would settle the diagnosis one way or the other.

Dr. ADAMSON: I regard Dr. Little's case and Dr. Sibley's case as examples of a not very uncommon type of urticaria pigmentosa of adults. I showed a case of this sort at a meeting of this Section in February, 1914, and a photograph was published in the Proceedings of this Section.<sup>2</sup> I then mentioned five other cases which had come under my care, and recalled a similar case which had been shown by Dr. Douglas Heath.<sup>3</sup> It is characteristic of these cases that there are numerous generalized freckle-like pigmentary macules, which are often mistaken for a secondary syphilitic eruption. Urticaria and itching may be pronounced, or it may be but little marked, or even absent. It may be easy or difficult to produce swelling of the patches by friction. Cases vary much in respect of the urticarial element. Dr. Whitfield has expressed the opinion that this eruption is an independent disease, but Dubreuilh, who has recorded a few examples, regards them as an adult form of urticaria pigmentosa.

<sup>1</sup> *Annales de Derm.*, 1887, p. 317.

<sup>2</sup> *Proceedings*, 1914, vii, p. 104.

<sup>3</sup> *Brit. Jour. Derm.*, 1913, p. 410.

THE PRESIDENT: There are minor differences between the two cases, but I regard them as instances of the same disease.

Dr. A. EDDOWES: I confirm what Dr. Adamson says about the behaviour of these cases. Sometimes the urticarial symptoms are hardly perceptible. A fortnight ago I saw a girl with the condition, who exhibited marked graphodermia. I rubbed one of the pigmented patches on her back, and in a few seconds afterwards urticarial lesions formed in a group. A week after treatment there was scarcely any urticarial tendency left and the pigmented lesions were obviously fading.

Dr. SIBLEY (in reply): There is a slight urticaria in my case, and scratching brings out the lesions more plainly. I have seen on and off for some years a marked case of urticaria pigmentosa in a man aged 26, which has been present for twelve years, and whose skin has never shown any signs of urticaria. The eruption has not changed for years, notwithstanding numerous treatments, and never gives rise to any symptoms.

(July 20, 1916.)

### Lichen Obtusus Corneus.

By W. KNOWSLEY SIBLEY, M.D.

THE patient, A. T., is a fairly healthy-looking man, a tailor, aged 32. The patient's father was a Russian who was brought to England as a child. His mother was English and he was born in London. Both parents are stated to have died from diabetes at about the age of 57. Patient was in bed with rheumatic fever for one week ten years ago, the knees and ankles being affected. He has rheumatic pains in these joints from time to time. The lesions of which he complains, first appeared on the dorsum of the right hand some twelve years ago, and have gradually increased in number ever since, next appearing on the right forearm, then on the corresponding side of the left hand and forearm. Later on they appeared about the ankles and then over the knees.

There are numerous discrete, disseminated, whitish, dry, hard, round, dome-shaped nodules, almost cartilaginous in consistence, some fifty in number, scattered on the dorsum of the hands and extensor surfaces of the forearms, knees, feet and ankles, completely absent from the trunk. They are sharply circumscribed and discretely arranged.

They are of fairly uniform size, about that of a large pea. The intervening skin appears to be normal. In no place is there any tendency for a coalescence of the lesions. The lesions are always extremely irritable, and the tops of all have been torn off by scratching and exhibit crateriform depression, sometimes filled with hæmorrhagic plugs. Otherwise the skin over the tumours appears more or less normal, both in colour and consistence. There is some increase in hair



FIG. 1.

Showing the isolated papules with crateriform tops, the result of scratching.

growth over and immediately around many of the older lesions. This may be due to the stimulation from local applications. A few small lichen planus papules are to be seen on the extensor surface of both forearms just below the elbow. No obvious lesions are present in the mouth. There are also some curious circular and circinate, slightly



FIG. 2.

Showing papules on dorsum of foot.

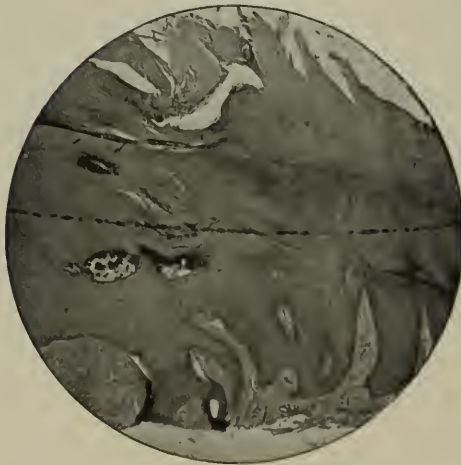


FIG. 3.

Section taken from the back of the left hand, showing a marked hyperkeratosis of the stratum corneum, which is excessively increased and is transformed into a hard, horny mass. The cells of the stratum granulosum and mucosum are increased. The cells of the latter appear to be undergoing hyaline degeneration. The blood-vessels are dilated. N.B.: The beaded line across the section is due to scratch on cover-glass. (Magnification  $\times 100$ .)



raised, beaded, lichenoid-like lesions on the penis and scrotum, stated to be scars left from boils. These have never been irritable.

The patient states that sometimes the lesions have disappeared spontaneously, and that he is sure there is some improvement since I gave him a carbolic acid ointment, which relieved the violent irritation; also, that one or two small lesions which at the time, now some six weeks ago, were just appearing, have now practically disappeared.

A section taken from a small tumour on the left hand showed the following histological features: The chief changes seen in the section consist in the marked hyperkeratosis of the stratum corneum, which is excessively increased by the number of its cells, and is transformed into a hard, horny mass. At the base of this layer there are two small cavities which contain polymorphonuclear leucocytes. The stratum granulosum, in places where it is visible, shows a marked cellular increase. There is also a marked increase of the stratum mucosum and the papillæ are increased in length; some of the cells in this layer appear to be undergoing hyaline degeneration. In the small portion of the dermis which is visible, there appears to be an increase of the connective tissue and of the spindle-shaped cells. There is a dilatation of the papillary and other blood-vessels.

#### BIBLIOGRAPHY.

- BROcq. *La Pratique Derm.*, 1902, iii, pp. 201, 213, 214.  
CORLETT. *Journ. Cutan. Dis.*, 1896, xiv, p. 301.  
HARDAWAY. *Arch. of Derm.*, 1880, vi, p. 129.  
HARTMANN. *Archiv f. Derm.*, 1903, lxiv, p. 381.  
HÜBNER. *Archiv f. Derm.*, 1906, lxxxi, p. 209.  
JOHNSTON. *Journ. Cutan. Dis.*, 1899, xvii, p. 49.  
KREIBICH. *Archiv f. Derm.*, 1899, xlviii, p. 163.  
SCHAMBERG and HIRSCHLER. *Journ. Cutan. Dis.*, 1906, xxiv, p. 151.  
WHITE. *Journ. Cutan. Dis.*, 1907, xxv, p. 385.  
ZEISLER. *Journ. Cutan. Dis.*, 1912, xxx, p. 654.

#### DISCUSSION.

Dr. GRAHAM LITTLE: I think this is a clinical variety of hypertrophic lichen planus. It has a certain resemblance to granuloma annulare, and in fact strongly resembles an instance which I included in my paper on granuloma annulare as a possible example of that disease, but more probably of a lichen planus. The patient was under the care of Dr. MacLeod, who kindly lent me a photograph which was reproduced in that paper.

Dr. MACLEOD: I do not think there is any doubt about the lichen planus part of this case. I do not think it is the same condition as in the man referred to by Dr. Little, the photograph of whose case did not give a very good idea of it.

Dr. ADAMSON: The case shown by Dr. Sibley is undoubtedly an example of the eruption to which Unna drew attention after studying a model in the St. Louis Museum, labelled lichen obtusus, which Brocq afterwards named lichen obtusus corneus. In this volume of the *Journal of Cutaneous Diseases*,<sup>1</sup> which I have just brought down from the library there is a reproduction of a photograph of a case published by C. J. White, and you will see at once that Dr. Sibley's case presents the same eruption. Lichen obtusus corneus has been usually regarded as a variety of lichen planus and the presence of typical lichen planus lesions in the case now exhibited confirms this view. I believe this to be the first example of lichen obtusus corneus shown at a meeting of this Section, so that it is evidently a rare form of eruption.

The PRESIDENT: We are much indebted to Dr. Adamson for his observations and for exhibiting the published illustrations of this disease. There can be no doubt that his diagnosis is the correct one.

(July 20, 1916.)

### Case of Kaposi's Multiple Pigmentary Sarcoma.

By W. KNOWSLEY SIBLEY, M.D.

S. S., RUSSIAN recruit, aged 19, single, barber. Duration of disease: Eleven months' ulceration of leg. Clinical features: Deeply pigmented and indurated condition of the right leg with superficial ulceration in places. The whole limb is larger and longer than the limb on the left side. The following are the measurements of the right and left legs:—

		Right		Left
Anterior superior spine to adductor tubercle	...	20 in.	...	19 in.
Tubercle of tibia to interior malleolus	...	15 "	...	14 "
Level of tubercle of tibia	...	13 $\frac{3}{4}$ "	...	11 $\frac{1}{2}$ "
Knee-joint circumference	...	15 "	...	13 $\frac{1}{2}$ "
Whole anterior superior spine to internal malleolus		38 "	...	35 $\frac{1}{2}$ "

The left leg is normal. There is an extensive superficial nævus of thigh and upper right leg.

<sup>1</sup> "Lichen Obtusus Corneus—An Unusual Type of Lichenification," Charles J. White, *Journ. Cutan. Dis.*, 1907, xxv, p. 383; see also "Two Cases of Multiple Tumours of the Skin in Negroes, associated with Itching," T. F. Schamberg and R. Hirschler, *Journ. Cutan. Dis.*, 1906, xxiv, p. 151.

Histological report of a section taken from a small pigmented patch on the right leg: There is a slight increase in all the layers of the epidermis, and in some parts of the basal layer pigment can be seen. Cellular infiltration is chiefly confined to the upper part of the corium, and here the pigment is seen to be extracellular. There appears to be



Kaposi's multiple pigmentary sarcoma, showing the general enlargement of the right leg, from the hip to the foot, the nævoid condition about the knee, together with some superficial ulceration in this region.

a considerable increase in the number of blood-vessels, which are moreover decidedly dilated. The Wassermann reaction was negative. The von Pirquet reaction was slightly positive.

## DISCUSSION.

Dr. F. PARKES WEBER: In this case some of the dark cutaneous patch on the right lower limb apparently dates from birth. Although I consider it a typical case of the so-called multiple hæmorrhagic (pigment) sarcoma of Kaposi, that is to say, typical from the macroscopic point of view, I think the latter disease is not yet acknowledged to occur congenitally. As, however, I have recently had the opportunity of seeing an absolutely typical case, in which some of the lesions had, according to the patient's mother, been there from 1 year of age, I think that I am in a position to assert that (contrary to the accepted opinion) Kaposi's so-called multiple hæmorrhagic (pigment) sarcoma may in rare instances commence early in life, or may be (partly) even congenital. That disease is a non-symmetrical disease, and therefore may sometimes probably be at first unilateral, as in the present patient. As in the present patient, moreover, the disease I refer to is usually accompanied by more or less swelling of the affected part.

Dr. PERNET: Surely Kaposi's disease, so-called, is not unilateral? This condition is unilateral.

Dr. S. E. DORE: I do not think this is a case of Kaposi's sarcoma. It is probably a lymphangioma, and there is no reason why the ulceration and the pigmentation should not be due to the same cause.

The PRESIDENT: I do not think the nature of this case can be determined without a biopsy. I shall be glad if a further report with the result of a microscopic examination is made at a future meeting.

Dr. PARKES WEBER: A biopsy will perhaps help in settling the question of diagnosis. If the case is really one of Kaposi's so-called multiple hæmorrhagic (pigment) sarcoma, the patient will have a chance of living to old age, since the disease is probably not really a sarcoma, and other patients with the same disease have, I believe, attained old age.<sup>1</sup>

<sup>1</sup> Cf. the case of a patient, aged 59, shown by Dr. Weber at the Dermatological Section on January, 20, 1916 (*Proceedings*, 1916, ix, p. 62).

I

# PROCEEDINGS

OF THE

# ROYAL SOCIETY OF MEDICINE

EDITED BY  
J. Y. W. MACALISTER  
UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

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**VOLUME THE NINTH**

SESSION 1915-16

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**ELECTRO-THERAPEUTICAL SECTION**



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LONDON  
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1916



## Electro-Therapeutical Section.

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## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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*(October 15, 1915.)*

### Discussion on the Interpretation of Certain X-ray Signs of Intestinal Stasis.

Opened by ARTHUR KEITH, M.D.

At the present time radiologists are divided into two groups—those who regard intestinal stasis as a result of mechanical obstruction and those who regard the condition as resulting from a functional disturbance of the bowel. So far, as concerns our own country, this difference of opinion is to be traced to researches carried out by Sir William Arbuthnot Lane and Professor Starling at Guy's Hospital, in the closing decade of last century. Those two men approached the problems of the alimentary tract from different directions—the one through the dissecting room and hospital wards, the other through the physiological laboratory. The first applied to the alimentary tract the mechanical laws which he found to govern the growth and shape of the human skeleton. He regarded abdominal viscera as suspended by their mesenteries; when any segment of the bowel became overloaded, it dropped—its suspending mesentery reacted, as bony tissue does, by throwing out strengthening bands of fibrous tissue to meet the increased strain. Either by the constricting action of those newly formed bands, or by kinks resulting from the sagging of the viscera, a series of obstructions are initiated, the starting point being at the lower end of the pelvic colon; then obstruction follows at the hepatic flexure, at the ileocaecal junction, at the duodeno-jejunal flexure, and at the pylorus. These are the points at which obstruction is frequently observed by radiologists to occur in cases of stasis, and the Arbuthnot

Lane theory offers a rational explanation, why obstruction at the lower end of the alimentary tract should be followed in course of time by obstruction at the other sites, higher up in the bowel.

Professor Starling, working with Dr. Bayliss, set out to elucidate the conditions which regulated the passage of food along the intestine. The musculature of the intestine was found to beat, like the heart muscle, with a definite rhythm—a double rhythm; they found that the propulsion of food depended on a complex series of reflexes resident in the intestinal wall, but subject to external influences, manifested through the vagal and splanchnic nerve paths. Two observations made then are of particular interest now: (1) That the excitability and rhythm are higher at the commencement than at the end of the small bowel, and (2) that a disturbance in one part of the tract will upset the nerve mechanism of another and distant part; distension of the duodenum, for example, inhibits contractions at the lower end of the ileum.

Early in the present century Dr. T. R. Elliott, using the same methods as Bayliss and Starling, made a most important addition to our knowledge of the mechanism of the bowel. He demonstrated that the circular musculature at the terminal part of the ileum was sphincteric in its action. He made us realize, contrary to our former beliefs, that a tract of circular muscle may act as a sphincter and yet have none of the sharply defined anatomical characters which we associate with a typical sphincter, such as that at the pylorus. He also made us realize, more clearly than we had done before, that a substance circulating in the blood, such as adrenalin, can produce on a localized part of the bowel the same effects as is produced by stimulation of the nerves supplying that part.

It is not necessary to remind you that Dr. W. B. Cannon, of Harvard, was carrying out his classical researches on the mechanism of the bowel when Arbuthnot Lane and Starling were at work at Guy's Hospital. He, too, found that the propulsion of food along the alimentary tract was controlled by an elaborate nerve and muscle mechanism.

About the same time as Arbuthnot Lane and Starling were commencing their investigations on the bowel at Guy's Hospital, my attention was drawn to similar problems in the dissecting room attached to the London Hospital. I was struck by the number of dissecting-room subjects in which the abdominal viscera were displaced towards the pelvis. It was only when my observations had been carried on for some time that I discovered that Glenard had investigated the condition



as a clinical entity, and named it "enteroptosis."<sup>1</sup> The final result of my inquiries relating to the support of the viscera of the abdomen may be summarized thus: (1) The muscles in the abdominal wall are the main agents in maintaining the viscera in position when we stand up; (2) that these muscles are called into action by reflexes which arise in the viscera of the abdomen at the moment the erect posture is assumed; (3) that mesenteries do act as ligaments, but that, as is the case with ligaments generally, mesenteries only come into action when the muscular support or resistance has been overcome or has given way. It is a law which has no exception, and which is also of the utmost practical importance, that muscles are the primary agents for the support of movable organs—whether they be soft viscera or rigid bones; ligaments are merely secondary agents which are called into use when the primary agents have given way. In opposition to Arbuthnot Lane, the conclusion is drawn that sagging of the viscera cannot be due to any primary defect or change in the ligamentous supports of the viscera, but must be traced to a defect of the muscular supports, represented by the musculature of the abdominal wall, and also by the musculature of the alimentary tract itself.

Intestinal stasis and ptosis of the viscera are closely related conditions: the one state usually accompanies the other. We have seen how Arbuthnot Lane accounts for their association. There is, however, another explanation. Recently I examined a series of colons which had been excised for the relief of intestinal stasis. A study of these led me to see how stasis and ptosis may well be manifestations of the same lesion. The basis for the explanation is provided by Professor Sherrington's observation that stimulation of the splanchnic nerves—the afferent fibres of the alimentary tract—throws the musculature of the abdominal wall into a state of tonic contraction. That the abdominal musculature is thrown into a tonic condition when we stand up is a fact which may be verified by anyone. We are justified in presuming that the impulses which bring about this tonus arise in the viscera of the abdomen, particularly in their peritoneal coverings, mesenteries, and musculature. If the splanchnic terminals in which these impulses arise are damaged or thrown out of action, then the musculature of the abdominal wall will no longer spring into action when the erect posture is assumed, and the viscera will thus lose the support which the upright posture renders necessary. Now, in the

<sup>1</sup> Dr. Frantz Glenard, "Les Ptoses viscérales," Paris, 1899.

majority of the excised bowels examined, both peritoneal covering and the intramuscular nerve plexus have been visibly altered. It is true that those peritoneal adhesions which are known as Lane's band and Jackson's membrane may be seen in a large proportion of newly born children; but in all cases of severe stasis there is to be seen a much greater abundance of bands and adhesions than is ever observed to be present at birth. We must suppose that peritoneal adhesions are freely produced in the course of the condition known as intestinal stasis. These are the result of a low irritative form of peritonitis. Mr. Sampson Handley observed that the coils of the ileum—in cases of pelvic peritonitis—are partially or completely paralysed. Mr. James Sherren has lately given me an opportunity of examining a segment of the transverse colon which, as shown by radiographic examination, was dilated and out of action. The peritoneal covering and the mesenteric plexus showed those changes which are to be seen in most cases of intestinal stasis. My conclusion, therefore, is that the sagging of the abdominal viscera is due, not to an overloading of the various segments of the bowel, but to a derangement of, or injury to, the visceral terminals of the afferent splanchnic nerves, which are essential to the maintenance of the supporting reflexes of the abdominal wall. It is very possible that the elongation of the stomach or of the transverse colon may be accounted for on a similar pathological basis.

In the Cavendish Lecture, given in June last, I proposed the recognition of certain definite neuro-muscular sections in the alimentary tract, each section being apparently demarcated from its neighbour by the possession of certain anatomical and functional characters. A concrete and diagrammatic representation of these sections is shown in fig. 1. Each section is cut off from its neighbour by a sphincter or sphincteric tract, which effectively blocks the passage of contraction waves and prevents them spreading from one section to the next. I presume that each section is provided with a special centre, or pace-maker, where the chief contraction impulses arise, but I have only succeeded in demonstrating anatomical centres in four of them. I presume that each section has its own rhythm. Dr. Alvarez's investigations have made it possible for me to make such an assumption.

Since the delivery of the Cavendish Lecture I have received an extremely kind and interesting communication from Dr. Alvarez.<sup>1</sup> He

<sup>1</sup> Dr. Alvarez has published a summary of his results in the *Journ. Amer. Med. Assoc.*, 1915, lxx, pp. 388 to 394. The reader will find there references to the reflexes existing between various segments of the bowel. Many others will be found in Professor Wingate Todd's "Clinical Anatomy of the Gastro-intestinal Tract," Manchester, 1915.

went to work with Professor Cannon at Harvard, with the definite object of translating cardiac physiology to the musculature of the alimentary tract. His observations on the intrinsic rhythms of the duodenal section and the jejuno-ileal section of the bowel confirmed his belief that our modern conception of the working of the heart might be transferred to the bowel. Indeed, he had already approached

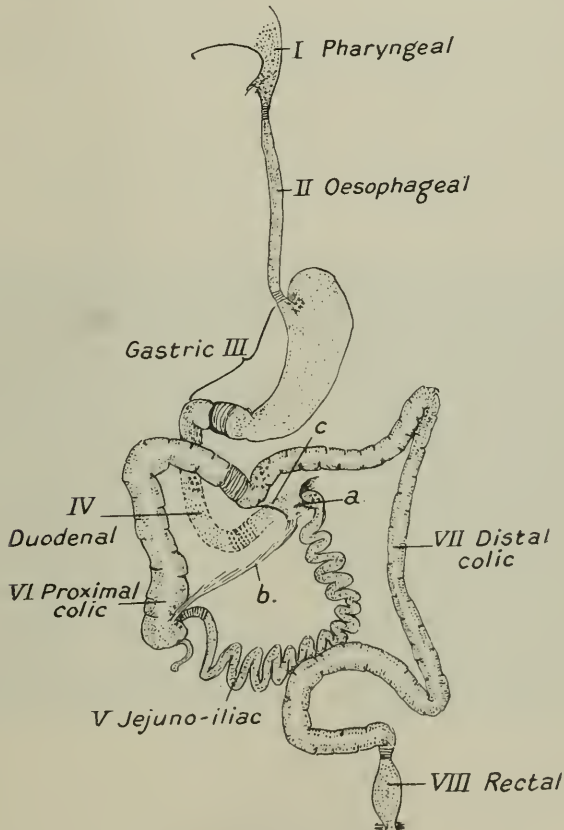


FIG. 1.

A diagram of the alimentary tract showing the neuro-muscular sections, which may be recognized in its whole extent.

some of his colleagues to investigate the matter on the anatomical side. It was from the anatomical side that I have approached the problem. Thus the conception of the neuro-muscular system of the bowel, put forward in the Cavendish Lecture, represents a combined idea; what is good in it is as much Dr. Alvarez's discovery as it is my own. How closely

these independent investigations have led Dr. Alvarez and myself to a common result may be judged from the fact that both of us have adopted the same simile to express our final conception of the bowel mechanism. We have both likened the alimentary tract to a railroad divided into sections, each provided with its signalman and telephonic apparatus. The signalman of one section refuses to accept any further traffic until his section is clear; all the sections are closely correlated; if one is blocked, the others, too, become automatically blocked. Disturbance in one section upsets the traffic in all. We begin to realize more clearly how complex is the mechanism that controls the propulsion of food along the alimentary canal. The vital points—those most liable to derangements—are at the junctions of the various sections.

Professor Karl Pearson, in one of his frank moods, cited my brain capacity as an instance of the mediocre; repeated experience has confirmed the truth of this mathematical expression. Nearly every observation which I have made, and published because I have thought it to be new, has turned out to be merely a confirmation of some former discovery. I may cite the pharyngeal sphincter, which I have mistakenly described as an upper œsophageal sphincter. When I published an account of it in 1911, Dr. Scanes Spicer drew my attention to the fact that Dr. Killian had described and figured it in 1908. No one who has passed an œsophageal bougie can have failed to notice the existence of such a sphincter, and it is possible that neither Dr. Killian nor myself was the first to notice it. The matter is recalled because the pharyngeal sphincter is the first of the series on the alimentary tract (*see* fig. 1). It marks the termination of the pharyngeal section. Each section of the alimentary tract ends at a sphincter. The cardiac sphincter marks the end of the œsophageal section and commencement of the gastric. Beyond the sphincter lies the “nodal” tissue, or the pace-maker of the section (*see* fig. 1). The nodal tissue of the stomach lies just beyond (aboral to) the cardia. The gastric section ends at the pylorus—but the duodenal section, in a neuro-muscular sense, does not commence until the second stage of the duodenum is reached. The nodal centre lies above the entrance of the bile-duct. The sphincteric tract of the duodenal section lies in the third or pre-vertebral stage of that part of the bowel. Dr. Ochsner, of Chicago, has already recognized the sphincteric nature of the musculature in the third stage of the duodenum. Further evidence can be seen in the excellent skiagram of duodenal stasis published by Dr. Jordan, and in skiagrams of the normal colon by Dr. Gregory Cole (*see* figs. 2 and 4).



Before proceeding to describe the limits of the other sections, may I draw attention to three peritoneal bands which have been recently investigated by Dr. W. Colin Mackenzie? They lie to the right of the duodeno-jejunal flexure (fig. 1, *a*, *b*, *c*). Dr. Mackenzie finds that representations of these folds or bands are constant in all mammals; we must presume that they are of functional importance. Into each of them he has traced branches of the vagus. By these folds the vagal and splanchnic nerve fibres pass to three important points in the alimentary tract: (1) To the terminal ileal sphincteric tract which marks the block system at the end of the jejuno-ileal tract, and to the "nodal" ileocæcal ring which apparently serves as a nodal centre (pace-maker) for the cæcum and proximal colon (see fig. 1, *b*). (2) To the commencement of the jejunum, where a nodal



FIG. 2.

Fig. 2.—A skiagram by Dr. Jordan (*Practitioner*, February, 1913, Pl. xxv, fig. 1), showing duodenal stasis due to kinking at the duodeno-jejunal junction. The "block" occurs at *c*, at the lower end of the second stage of the duodenum, not at the duodeno-jejunal flexure, which is situated above the point marked *C* and in front of the left side of the spinal column. (See also Dr. Jordan's skiagrams, figs. 6 and 6A, in *Proceedings*, 1912, Electro-Therapeutical Section, p. 15).

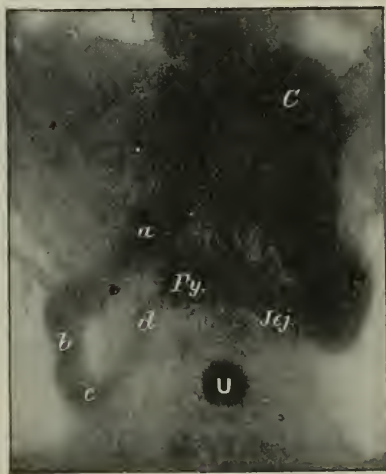


FIG. 3.

Fig. 3.—A skiagram by Dr. Jordan (*Practitioner*, February, 1913, Pl. XXV, fig. 5), showing stasis in the duodenum due to kinking at the duodeno-jejunal junction. The third stage of the duodenum (*d*) is indicated; it passes up behind the shadow of the pylorus. The junction is thus hid, but from the fact that the third stage of the duodenum is not distended one infers there can be no mechanical obstruction at the duodeno-jejunal junction.



centre is presumed to exist, although its existence has not yet been demonstrated as an anatomical fact (*see fig. 1, a*). (3) To that part of the transverse colon which, in all mammals, lies immediately under or adjacent to the pyloric segment of the stomach (*see fig. 1, c*). The part of the transverse colon thus indicated by the fold marked *c* is presumed to be the area at which antiperistaltic waves may arise in the proximal colon. But I admit that I have failed to find satisfactory and convincing evidence of a special collection of "nodal" tissue in this part of the transverse colon or of the existence of a functional sphincter. But there is evidence of a physiological change



FIG. 4.

Skiagram showing (1) that in a healthy subject the duodeno-jejunal junction lies in front of the spinal column and behind the shadow of the pyloric part of the stomach; (2) that normally there is a delay or partial block at the end of the second stage of the duodenum. (Gregory Cole.)

in the neuro-muscular mechanism of the colon at the point marked by the fold. I also regard the junction of the pelvic colon and rectum—pelvi-rectal junction—as marking the passage from the distal colic section to the last or rectal section.

These are the fundamental conceptions I would bring to bear on the interpretation of the X-ray signs of intestinal stasis. I regard a functional or pathological derangement of the neuro-muscular mechanism of the bowel as the primary lesion of intestinal stasis.

That lesion is the result of direct infection of the bowel wall or of absorption of the products of bacterial action. Blockage occurs at those junctional points where the passage of the intestinal contents is regulated under normal conditions. In stasis the signal system breaks down—it is because there is an obstruction on the line. Sagging of the viscera, intestinal kinks and adhesive bands are secondary consequences which, in the vast majority of cases, give no skiagraphic evidence of causing obstruction.

I now submit a series of skiagrams, selected from the publications of Dr. Jordan, Dr. A. E. Barclay, and Dr. Gregory Cole, to demonstrate that the appearances presented by these skiagrams are entirely in support of the conception of stasis being the result of a functional



FIG. 5.



FIG. 6.

Fig. 5.—Skiagram by Dr. Jordan (*Practitioner*, February, 1913, Pl. XXV, fig. 2B), showing ileal stasis due to kinking of the terminal part of the ileum. If there were an effective obstruction one would expect a break in the shadow of the ileum, such as is shown in fig. 5, some distance from the termination of the ileum. At the point indicated by Dr. Jordan ("kink," fig. 5), there is no break in the shadow. The ileum is distended and the appearance is such as we expect when the ileo-cæcal sphincter is incompetent, or out of action. The break shown in the ileal shadow in fig. 5 is due to a temporary spastic contraction.

Fig. 6.—Skiagram of ileal stasis by Dr. Jordan (*Archives of the Roentgen Ray*, 1914, Pl. cccxxv, fig. 14) showing the terminal sphincteric tract of the ileum strongly contracted.

disturbance, and that the evidence of these skiagrams, when rightly interpreted, is entirely in opposition to the mechanical theory of stasis. I first exhibit skiagrams of duodenal stasis, pointing out that not a single print has yet been published showing a block at the duodeno-jejunal junction (figs. 2 and 3); fig. 2 shows the block near the commencement of the third stage of the duodenum. In the series showing ileal stasis it is easy to demonstrate a tonically contracted segment at the end of the ileum (figs. 6 and 7); there are numerous skiagrams which support the contention which Dr. James Case maintains, that incompetence of the ileocæcal sphincter is a condition



FIG. 7.

Fig. 7.—Skiagram by Dr. A. E. Barclay, showing the terminal sphincteric tract of the ileum strongly contracted and the terminal coils of the ileum distended.

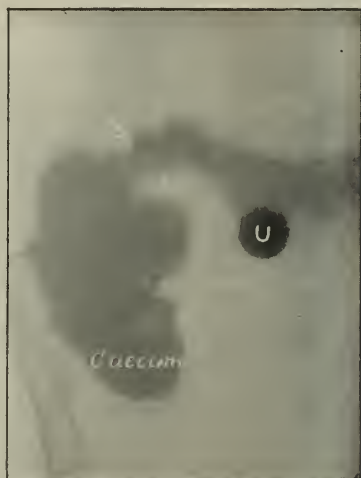


FIG. 8.

Fig. 8.—Skiagram by Dr. Jordan (*Proceedings*, 1911, *Electro-Therapeutical Section*, fig. 20), to show obstruction at the hepatic flexure caused by a fibrous band. It is to be observed that there is no break in the intestinal shadow at the hepatic flexure, such as we should expect if the band produced a physical obstruction.

frequently observed in cases of intestinal stasis (fig. 5), but there is not a single record of a sharp blank or hiatus in the kinked shadow of the terminal ileum, such a blank as we expect to appear if a kink really causes an obstruction (*see* fig. 5). Skiagrams of the appendix show that its musculature has the properties of a sphincter,

and it is clear, on clinical evidence, that it is a station from which many reflexes arise. Skiagrams of cases of kinking or of bands at the hepatic flexure show no break in the colic shadow at the site of the kink or adhesions (fig. 8), such as we expect to see if either a band or flexure causes obstruction to the passage of contents. Skiagrams of normal and pathological conditions of the transverse colon usually show in or near the subpyloric segment a change in the condition of contraction, agreeing with the conception of a physiological change at that point. The tonically contracted condition of the distal colon, the production of diverticula, so often seen in cases of stasis, are in keeping with the theory of a functional inco-ordination of the neuro-muscular mechanism, and against a mechanical interpretation. Finally, in some cases of megacolon I have found a most remarkable hypertrophy or overgrowth of the myenteric (Auerbach's) plexus.

Dr. ARTHUR F. HERTZ said he found it somewhat difficult to discuss Professor Keith's paper because he was in such entire agreement with him. He (the speaker) started in the physiological laboratory to work at the subject, and later from the point of view of the physician, and in almost every detail he agreed with Professor Keith that intestinal stasis depended upon physiological as opposed to mechanical factors.

Professor Keith seemed to think there was a very essential relationship between ptosis and stasis; but his own view was that he had rather exaggerated that relationship, as one often saw extreme degrees of ptosis without stasis. In the severest case of ptosis he had ever seen—in a child with congenital absence of abdominal muscles, in whom one could take hold of the liver, the spleen or other abdominal organ in one's hand and move them about the abdomen at will—there was no trace of stasis, and the child's bowels had always acted regularly. He had often come across patients accidentally, who had entered the hospital for some other cause, who had never had intestinal stasis, but they had very weak abdominal walls and extreme ptosis. On the other hand, he had seen cases of very severe intestinal stasis and no ptosis at all. He was also inclined to think that Professor Keith laid too much stress on the anatomical changes in the cases of stasis which he had been able to examine after the colon had been removed, because, after all, these were end-cases. The majority of the cases seen by the physician were cases in which no one would dream of



advising a colectomy, and from the complete recovery which generally occurred one would hardly imagine that there could be any anatomical changes present which would affect Auerbach's plexus.

With regard to the sphincter in the transverse colon, to which Professor Keith referred, the only evidence he had was the impression he had derived from a large number of cases in which he had seen a mass movement of the colon take place, that in many cases it had begun a little beyond the hepatic flexure. He had never seen it begin at the tip of the cæcum. He had never seen an antiperistaltic wave under natural conditions, and he would be interested to hear whether others had. He had never known it to occur in a patient who had not organic obstruction, or in whom the colon had not been artificially distended with a barium enema. He was still doubtful whether antiperistalsis occurred under perfectly physiological conditions.

He knew Professor Keith fully agreed with him in regard to the importance of what he (the speaker) had ventured to call achalasia—the absence of the normal relaxation of a sphincter—but the opener did not speak of it in connexion with the ileocæcal sphincter. There were doubtless cases in which a spasm of the sphincter occurred, and others in which the sphincter was relaxed and incompetent. But the largest number of cases of ileal stasis were due to a third physiological process, achalasia; as each peristaltic wave moved along the end of the ileum, the sphincter did not relax as it normally should, so as to allow fæces to enter the cæcum. Those three causes, he considered, accounted for practically all the cases of ileal stasis, except when gross obstruction, due to such conditions as adhesions following appendicitis, a tumour or tubercular disease, was present.

He wished to express his thanks to Professor Keith for his extremely interesting address, which showed that anatomy could be made an extremely living and practical subject.

Dr. ALFRED C. JORDAN said: I have listened with keen interest to Professor Keith's masterly exposition. He has given us much to think about. I thank him for his kind references to my own work. Although he regards chronic intestinal stasis from a different point of view to my own, I can safely say there are only a few points wherein my observations are in conflict with Professor Keith's; I ask leave to show a few slides to illustrate the points at issue.



## THE DUODENUM.

As Professor Keith is aware, all my observations are made with the fluorescent screen, and the skiagrams are taken during the "screening"; they are not instantaneous photographs, and in the case of the duodenum the photographs taken during its active movements are composite pictures. In most cases I am able to show the *whole of the duodenum* on my photographs (figs. 7 and 8); it is not safe, however, to rely on photographs, useful though they be as illustrations. It is absolutely necessary to observe the actual movements in the duodenum with the fluorescent screen while the patient lies on the couch. I can state positively, after innumerable direct X-ray observations of the normal and the "static" duodenum, that the block in the duodenum is not at its lowest point (as Professor Keith believes), but is at the end of the duodenum. I have confirmed this by numerous visits to the operating theatres, where I have seen the actual block very often. In severe cases, and when the patient has not been in bed more than two or three days before the operation, the kink at the duodeno-jejunal junction is quite acute, and its effect is accentuated by the state of torsion of the first bit of the jejunum.

## THE TERMINAL ILEUM.

In the case of the *terminal ileum* the sole criterion, again, is the fluorescent screen observation, with simultaneous manipulation and inspection in the upright posture as well as on the couch. Skiagrams alone are not to be taken as sufficient basis for general deductions. Nevertheless, I can show, in skiagrams, a block due to an *obstructive ileal kink*, one due to the upright posture with an ileal kink (figs. 5 and 6), and one due to the appendix. Three good examples of obstructive ileal kinks (figs. 1, 2, and 3) appeared in the illustrations to one of my earliest publications on stasis.<sup>1</sup> In stasis the *terminal coil of the ileum* is always thick-walled, so that it feels like a cord when rolled under the fingers. I have no histological data as to the cause of the thickening; I have assumed it to be due largely to muscular hypertrophy from the fact that this coil is seen, with bismuth, to exhibit abnormally powerful peristalsis; moreover, its appearance at operation suggests that it is unduly muscular.

In other respects my observations lead me to agree with Professor

<sup>1</sup> *Proceedings*, 1911, v (Electro-Therap. Sect.), pp. 9-48.

Keith's deductions regarding the terminal ileum. *Spasm* of the last half inch (or so) of the ileum is always present in stasis (fig. 9). Dr. Case (Battle Creek) has shown that in stasis the ileocæcal valve becomes incompetent, allowing enema fluid to enter the ileum from the cæcum under low pressure. This has been confirmed by myself and others. In my opinion the tonic spasm at the end of the ileum in stasis has arisen to counteract the tendency to regurgitation from the cæcum to the ileum.



FIG. 1.

Ileal kink in a man, aged 71. Taken twenty-four hours after a bismuth meal. Showing a long terminal coil of the ileum, hitched up for the upper three inches and obstructed in the last inch (confirmed by operation, and the patient cured). A duodenal ulcer was found at the operation. U, umbilicus.

#### THE STOMACH.

Professor Keith's remarks on the "*pace-maker*" of the stomach are most suggestive and interesting. It is true, as he said, that the peristaltic waves seem to start, in the healthy stomach, at the cardio-pyloric junction. In cases of exaggerated gastric peristalsis the waves start farther back, and the more powerful the peristalsis the farther back do the waves become visible. With the strong, deep

peristalsis seen in some cases of pyloric obstruction the waves are seen to take their origin at the fundus, and to involve both curvatures from the start. In such cases we often see three or four strong waves simultaneously, following one another from the fundus to the pylorus.

#### THE LARGE INTESTINE.

The teaching of stasis does not compel us to regard the *HEALTHY large intestine* as a "cesspool." In stasis the diseased big bowel does eventually become a cesspool, and then requires the services of a skilled human plumber.



FIG. 2.

Taken twenty-four hours after a bismuth meal, showing extreme obstruction by adhesions at the end of the ileum, with enormous dilatation of the ileum, in a man, aged 26. Cured by division of the adhesions. U, umbilicus.

The fact that the *big bowel is an elaborately developed structure* cannot be taken as proof that it is of importance to man. While the big bowel exists, it must be elaborately equipped to carry on its function of propelling solid matter through its length; if it were to become rudimentary, its presence would be incompatible with life.

## THE INITIAL STAGE OF STASIS.

With the exceptions mentioned Professor Keith's views are not in conflict with the teaching of Sir Arbuthnot Lane; on the contrary, they afford valuable confirmation. It is hardly profitable to labour the discussion as to *what constitutes the initial change in stasis*. There is much evidence to show that the beginnings of stasis may be traced back, in many cases, to the first weeks of life when artificial feeding or some other cause leads to overfilling of the infant's stomach and a concomitant dropping of the transverse colon.



FIG. 3.

Fig. 3.—Ileal kink in a man, aged 36. A sharp kink, causing obstruction, is shown at a point 4 in. from the ileocaecal valve. At this point the ileum was firmly fixed to the iliac fossa. Above and below this point the gut was freely movable (confirmed by operation). U, umbilicus.

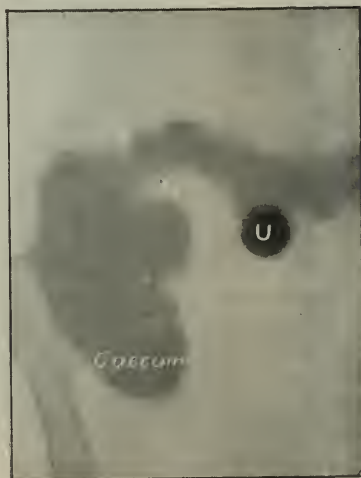


FIG. 4.

Fig. 4.—Taken forty-seven hours after a bismuth meal in a subject, aged 57. Fibrous bands run from the under surface of the liver down to the transverse colon, obstructing it just beyond the hepatic flexure (confirmed by operation). U, umbilicus.

## MECHANICAL CHANGES.

It is not claimed that the mechanical factors supply a complete explanation of all the phenomena of stasis. Even this early mechanical change in the stomach and transverse colon is determined by the nerve mechanism of the pylorus. The *mechanical changes* are capable of actual demonstration, both by the X-ray method and by inspection at operations; moreover, many of the symptoms of stasis can be relieved by the adoption of mechanical means. *Other factors* are important — chemical, bacterial, histological — all are being

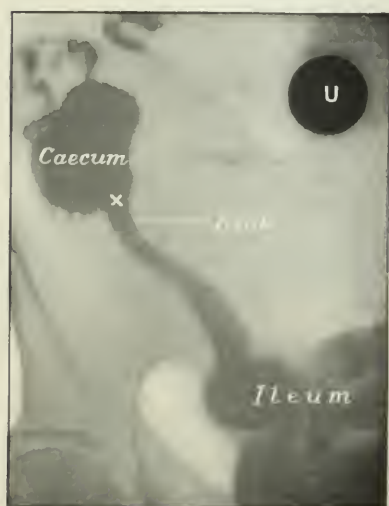


FIG. 5.



FIG. 6.

Figs. 5 and 6. —Ileal kink in the case of a naval surgeon, aged 46, taken five hours after a bismuth meal, showing that the kink only causes obstruction in the upright posture. The operation of gastro-enterostomy had been performed in Australia, nine years previously, for pyloric obstruction. A large chronic ulcer was then found at the back of the pylorus. He improved steadily on wearing a "belt," and taking liquid paraffin. X, ileocecal entrance; U, umbilicus. Fig. 5, taken on the couch; fig. 6, taken vertically.

studied, and are leading to additional knowledge of the problems of stasis. In the *large intestine* abnormal delay leads to decomposition, and then to *chronic irritation*. No wonder the delicate nerve-cells deteriorate, for the toxæmia of stasis causes a general depreciation of every tissue of the body; the cells of Auerbach's plexus cannot escape



this general adverse influence, and, in addition, these cells are subject to direct injury from their position in the wall of the inflamed big gut. Once this nerve change has occurred, it upsets the peristalsis of the big bowel, and thus aggravates the stasis. In fact, we are faced with a "vicious circle."

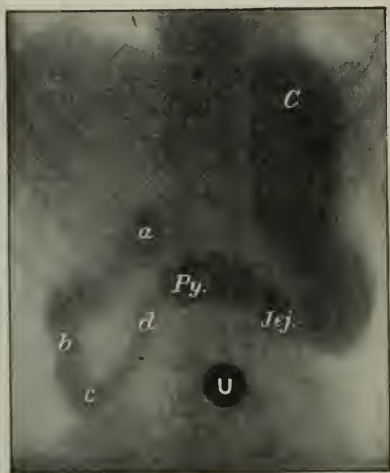


FIG. 7.

The stomach and duodenum in a woman, aged 30, who had suffered from constipation since a little girl. For the last four years she had complained of pain after food and abdominal distension. She felt cold and depressed. The duodenum is shown to be greatly elongated; its first part is dilated. There was great delay in the passage of the bismuth emulsion through the duodenum, due to kinking at the duodenal-jejunal junction. The subsequent X-ray examinations showed ileal stasis; the cæcum occupied the pelvis. *a*, *b*, *c*, *d*, first, second, third and fourth parts of the duodenum; *Py.*, pylorus; *C*, cardiac portion of stomach; *Jej.*, jejunum; *U*, umbilicus.

#### KINKS.

With regard to the *ileal kink*, it is only necessary to state that Lane has never described it as a primary cause of stasis, but always as a result of stasis. In the most severe cases of stasis in feeble women there is no ileal kink; the kink occurs only in the more robust subjects. When present, it increases the stasis in the upright posture. Here we have another "vicious circle."

*Experiments by making artificial kinks* in the healthy bowel cannot be used to provide an argument against the occurrence of obstruction

from kinks in stasis. A healthy bowel wall can overcome a great deal of obstruction; not so the diseased bowel of stasis, with its deteriorated muscles and nerves; such a bowel may resist the obstruction for a time, but the limit of its endurance is soon reached; it becomes fatigued far too readily, and then obstruction occurs (fig. 4).



FIG. 8.

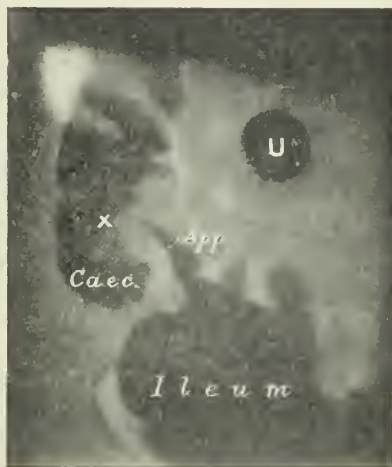


FIG. 9.

Fig. 8.—A marked case of intestinal stasis showing the duodenum greatly elongated and dilated, its vertical portion measuring  $1\frac{1}{2}$  in. orthodiagraphically (normal,  $2\frac{3}{4}$  in. to  $3\frac{1}{4}$  in.). The duodenum showed strong peristalsis, the bismuth advancing repeatedly through the third and fourth parts of the duodenum, but returning again to the lowest part as each wave passed off. After twenty-five minutes only traces of bismuth had entered the jejunum in spite of the strong duodenal contractions. *Py.*, pylorus; *a, b, c, d*, the four parts of the duodenum; *U*, umbilicus.

Fig. 9.—Six hours after the same bismuth meal, showing ileal stasis, a very large amount of bismuth having accumulated in the lower coils of the ileum. The terminal coil of the ileum was somewhat tender and felt thick—i.e., its walls were hypertrophied. The terminal coil showed active contractions, the coil being bulged in front of the waves. There was spasm of the last two inches. The appendix is shown passing inward from the cecum; it appeared normal. *Cæc.*, cecum; *App.*, appendix; *U*, umbilicus; *X*, ileocecal entrance.

#### DROPPING OF THE VISCERA.

Professor Keith states his belief that the *intestines are held up solely by the pressure of the muscles of the abdominal wall*. Undoubtedly this is the case in health, but Professor Keith admits that the

muscles often fail to perform this function. Notably is this the case when the muscles are fatigued from prolonged effort. Even in strong young men with powerful abdominal muscles, after the exhausting ordeals to which athletes love to subject themselves, the viscera drop. When these athletes reach the age of 40 or 45 they generally begin to suffer severely from symptoms due to stasis. We know now that great dropping of the viscera may occur in subjects (both male and female) whose abdominal walls are firm and flat. Once the bowel has dropped, the mesenteries begin to act as ligaments, and being unfitted for this function they suffer in the various ways described by Sir Arbuthnot Lane.

#### STASIS.

Regarding the *stasis question as a whole*, we may justly claim that it has emerged from its hypothetical stage, and now provides a firm foundation upon which we are building. Many keen workers are taking part, thrashing out details, and everything is falling into place in an orderly manner.

In studying a subject as far-reaching as stasis we must take care to avoid the danger of losing sight of the whole while pursuing some side issue.

A large number of *diseases*, both *local* and *general*, have now their allotted places in the scheme of stasis; many cancers, many diseases of the appendix, duodenum, stomach, gall-bladder, pancreas and thyroid are readily explained; included in this list are some of the most common abdominal diseases such as the dyspepsias, gastric and duodenal ulcer, gall-stones, appendicitis, colitis, &c. Another list includes a number of *general* diseases—e.g., some forms of rheumatism, tuberculosis, diabetes.

Not only does the teaching of stasis explain the essential causes of these and other disorders, it gives the clue to their rational treatment. Eventually, when universally understood, it will obviate a large number of unnecessary operations.

Professor Keith must not ask us to give up all this; rather he must let us claim him as a most valuable co-operator, whose brilliant work is throwing a flood of light on some of the obscure and difficult places in the path of chronic intestinal stasis.

Dr. A. E. BARCLAY (Manchester) said that he had recently made an observation in regard to the movement of the food in the large intestine that was of special interest in view of Professor Keith's theory. On several occasions recently he had observed a movement of the shadows in the colon which was preparatory to the occurrence of a mass movement, although the mass movement did not actually take place in every case. A temporary sphincter was found at some point, and the shadows seemed to back up to this before the onward rush occurred—i.e., before the big contraction took place. He had seen this temporary sphincter, or *point d'appui*, formed three times in the region of the hepatic flexure, and on several other occasions at various points between this region and the splenic flexure. He believed that it was the efficiency of this sphincter that determined whether the whole mass passed on in the right direction or whether some of it passed backwards towards the cæcum. If this valvular action was incompetent it was easy to see how back pressure on the cæcum would occur, and he believed that this was the cause of the sloppy cæca that were so frequently seen in the cæcal type of constipation. He believed that these sloppy cæca were the *result* and not the *cause* of constipation, the transient sphincteric action not being sufficiently strong to withstand the strain of the mass movement: the *point d'appui* giving way, some of the contents would be forced back into the cæcum. The more incompetent this sphincter the less efficient would be the mass movement when it did occur, and when there was no sphincteric action at all, the mass movement would make the cæcal accumulation itself the *point d'appui*.

He wished to thank Professor Keith for his paper and to say what a pleasure it was to hear an anatomist approach a subject such as this in the way he had done. Professor Keith was perhaps the foremost of those who had made anatomy a living subject, who attempted to "make the dry bones live." The anatomy of the living was of far greater importance than the anatomy of the dead, and, for the solution of the intricate problems with which they were faced, it was essential that radiologists should join hands with those anatomists, physiologists, physicians and surgeons who could look upon man as a *living* organism. The radiologist saw only the gross shadows—he did not see the viscera that caused the shadows to move. What did he know of Auerbach's plexus? Each man, whether surgeon, physician, anatomist, radiologist, whatever special line of study he followed, made his observations from one point of view only, and it needed a great general

to marshal the whole of the divergent sources of information until a complete solution of the problem could be found. Perhaps Professor Keith might be that general. In the meantime it was not for one man in any particular branch, certainly not in radiology, to dogmatize and think that he could find a complete solution of all these problems.

Professor KEITH, in reply, said his hearers were all much too modest. Both Dr. Hertz and Dr. Barclay had spoken kindly about some of his proposals. He did not want again to enter into them; his object on this occasion was to feel the pulse of the Section, to ascertain how members would be prepared to look at his theories and see how they fitted in with the facts they knew. He could not expect his hearers to stand up in a minute and declare themselves, but he hoped they might be led by what had been said to see what was true and what was untrue. It was important to go to work with an idea, and get data to prove or disprove it. He had not now the same paternal feelings as formerly towards ideas which he promulgated; he was now able to take these matters quite impersonally. Dr. Jordan had expressed the belief that he (the speaker) was going over to his side on this matter; but his own belief was that Dr. Jordan would yet come over to his side; he looked for him soon to interpret X-ray signs of stasis now under discussion as he himself did.



## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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(November 19, 1915.)

### A Criticism on the Reaction Testing of Muscles, and the Interpretation of the Results, with Reference to Electrical Examination of Nerve Injuries in War.

By E. P. CUMBERBATCH, M.B.

THE present War and the large number of cases of paralysis from nerve injury make this a fitting occasion for an inquiry into the subject of the electrical testing of muscle and nerve, a subject which is much in need of further investigation. In this communication I wish to point out the sources of error and inaccuracy in the methods at present in use for testing the reactions; to suggest a line of procedure in carrying out the electrical test, and to show the need for revision of the expression "reaction of degeneration."

#### (I) SOURCES OF ERROR AND INACCURACY IN THE METHODS OF ELECTRICAL TESTING.

When the induction coil is used a source of error arises from our inability to measure the strength of the faradic current. If a muscle does not respond to a faradic current that is strong enough to cause vigorous contraction of a healthy muscle, and if it responds sluggishly to a galvanic current, it has a complete R.D. Now on many occasions I have repeated the test and found that if a *stronger* faradic current were used, the muscles would then in many cases contract. The

reaction would then be a partial R.D., and the lesion in the motor nerve would be regarded as less severe. Some would regard the survival of excitability to the faradic current as a sign indicating a good prognosis, and others would go further and say that further testing by the galvanic current was unnecessary. The question therefore requires answer, With what strength is the faradic current to be used?

There is no clinically accurate method of *measuring* the strength. The only clinical method of *estimating* it is to note the contraction that it evokes from a normal muscle. But this contraction will depend on a number of widely varying factors: the resistance of the skin, the exact position of the testing electrode, the pressure with which the electrode is applied and the consequent proximity to the motor point. Besides, different coils give different currents, and the same coil may vary its current according to its adjustment. The question of the estimation of the strength of the faradic current becomes still more pertinent when we wish to be sure whether such response as the faradic current produces is of the normal strength or weak. We cannot speak accurately of quantitative alterations of the response if we cannot accurately measure the strength of the stimulus.

If the muscle responds feebly to the faradic current, the reaction will depend on the response which it gives to the galvanic current. If it contracts quickly, the reaction is of the normal type but weakened; if it contracts slowly, it has a partial R.D. The decision therefore depends upon the character of the response to the galvanic current, whether quick or slow. In some cases the decision is easy; in others it is very difficult and is a matter of personal opinion. Before we can say whether the contraction of any muscle is quick or slow, it is necessary to know the behaviour of that muscle when in a normal condition. In the normal subject different muscles contract with very different degrees of briskness. The facial muscles contract very quickly, and the word "twitch" rightly describes the response. The leg muscles contract less quickly, and the intrinsic muscles of the foot contract slowly, even sluggishly. The temperature of a limb will also alter the duration of the contraction of its muscles: cold will make the contraction sluggish, and warmth will make it brisk. The influence of temperature must be borne in mind as it may lead to error when carrying out the test. The following case shows that such error is possible:—

*Case I.*—D. D. This patient had a cold, livid, swollen hand and wrist, with a “chilblain circulation.” A faradic current strong enough to cause contraction of the corresponding muscles of the opposite hand could not make the affected muscles contract. The galvanic current made them contract very sluggishly. The reaction was evidently a complete R.D. On making the faradic current much stronger the muscles responded. The reaction was then a partial R.D. After warming the hand by diathermy, the muscles contracted briskly. The reaction was then a weak normal reaction.

The method of testing with faradic and galvanic currents will therefore not tell us, in many cases, whether a reaction is weak, normal, or partial R.D., or whether it is partial R.D. or complete R.D. If a muscle shows, without doubt, a reaction of degeneration, the lesion is located in the lower motor neuron. The question now arises, Can we, from knowledge of the reactions, obtain some idea of the severity of the lesion? If a muscle shows R.D., the nerve lesion may be said to have a certain degree of severity, greater than if the reaction was partial R.D. or weak normal, but the presence of R.D. does not give us any more information as to the degree of severity of the lesion. A muscle with a lesion of its nerve so severe that the latter will completely degenerate and the former gradually lose its excitability till it becomes extinct, will show R.D. On the other hand, R.D. may be present in a paralysed muscle, the nerve of which has received only a slight lesion, so that voluntary power will soon return. Further, R.D. may be present in a muscle in which there is good voluntary power. The presence of R.D. in muscles in which there is voluntary power is quite a common occurrence. The following cases will illustrate this:—

*Case II.*—T. W., boy, aged 14. During convalescence after operation (for abscess in upper part of thigh) he developed hallux-drop. When the electrical test was made it was found that he had complete R.D. of his extensor longus hallucis. There was no voluntary power in this muscle. It was found that the other extensor muscles and the peronei were less strong than the corresponding muscles on the opposite side. These were then tested, and it was found that they, too, showed a complete R.D. Seven months later I saw the patient again, and found that the extensor longus hallucis had regained good voluntary power, but there was still a complete R.D. The other extensors seemed as strong as those on the opposite side. Of these, the tibialis anticus showed a partial R.D., the extensor communis digitorum a complete R.D. The peroneus longus showed a weak normal reaction. This patient was able to walk about without difficulty or fatigue.

*Case III.*—I. W., aged 17. The patient had foot-drop ; she had no voluntary power in the anterior tibial or peroneal group of muscles ; these muscles all showed complete R.D. During treatment some power returned, but the R.D. persisted. This patient was kept under observation and tested periodically. The R.D. remained for a long time, although the voluntary power of the muscles progressively increased. The reactions ultimately became normal in one muscle after the other, but R.D. persisted in the extensor communis digitorum for ten months after the first test, or twelve months after the stated onset of the paralysis.

*Case IV.*—J. W., aged 18. A case of fracture of elbow. Loss of sensation in the region supplied by the ulnar nerve occurred, and claw-hand gradually developed. The muscles were tested and found to show complete R.D. The nerve was freed from callus in the region of the fracture. Nine weeks after the first test voluntary power returned in the first dorsal interosseous muscle. Both this and the other muscles supplied by the ulnar nerve showed complete R.D. Thirty-two weeks after the first test the muscles still showed complete R.D., but voluntary power was present in all. Thirty-nine weeks after the first test there was still no change.

Not only will R.D. persist and voluntary power return, but R.D. may *develop* while voluntary power is returning. Such an event occurred in Case V.

*Case V.*—B. S. A case of facial palsy. The reactions were tested on the second day ; they were normal and equal to those on the unaffected side ; there was no voluntary power in them. On the eighth day some voluntary power returned in the levator labii superioris. On the ninth day it returned in the orbicularis palpebrarum, the frontalis and the chin muscles. The reactions were still normal, but weaker than those of the opposite side. The power gradually increased. On the eighteenth day the response to the galvanic current was distinctly sluggish, and to the faradic current it was feeble. The voluntary power had then still further improved.

The presence of R.D. by itself, therefore, does not indicate the degree of severity of the lesion, and so is insufficient evidence on which to form a prognosis or, in the case of nerve injury, justify surgical operation. The electrical test may be extended so as to obtain further information, and the investigation may be pursued along three lines of inquiry.

In the first place, an attempt may be made to ascertain the degree of the degeneration—if one may use this expression—to which the muscle has proceeded. This may be done by the method of testing by condenser discharges. The degree of degeneration is measured in terms



of the duration of the shortest stimulus required to excite the muscle to contract, and it is assumed (I am not sure whether the assumption is correct) that the longer the impulse required the more advanced will be the degeneration. A healthy muscle will respond to a stimulus as short as  $\frac{1}{2400}$  sec. When it has passed into the condition in which it shows R.D. it requires a stimulus of  $\frac{1}{600}$  sec. to  $\frac{1}{400}$  sec. or longer. Some muscles that show a complete R.D. will respond to a stimulus of this length, others will require longer stimuli, even as long as  $\frac{1}{20}$  sec. or possibly longer. The condenser device which is now being used provides us with a range of stimuli, the duration of the shortest being  $\frac{1}{2400}$  sec.; that of the longest may be brought down to  $\frac{1}{20}$  sec. If we test a number of muscles by this method we soon find that those which show R.D. require lengths of stimuli that differ widely from each other; some require  $\frac{1}{600}$  sec., others require  $\frac{1}{20}$  sec., others require stimuli of intermediate duration.

In the method of testing with faradic and galvanic currents, we have two stimuli: one which is a succession of impulses of *short* duration (the faradic current), the other being a single impulse lasting as long as the current flows. The impulses of the faradic current vary in different coils, and even in the same coil from time to time, and are not short enough. In most medical coils they are  $\frac{1}{800}$  sec. to  $\frac{1}{400}$  sec. A normal muscle will respond to an impulse very much shorter than this, and a muscle cannot be regarded as normal if it will not respond to a stimulus shorter than the times given. The faradic stimulus is therefore unmeasured and not short enough. The galvanic stimulus is, on the other hand, too long, and it, also, is unmeasured. These are defects of the method of testing by faradic and galvanic currents other than those mentioned earlier. The condenser method classifies muscles showing R.D. into groups according to the duration of the shortest impulse to which they can respond. The question may now be asked, Has a muscle, showing R.D. and requiring a long impulse (as provided by a large capacity condenser), a severe lesion of its motor nerve that ought to receive surgical exploration? I hope that members of the Section who have been using condensers for testing will give their experience on this point. Personally, I do not think that a muscle showing R.D., even R.D. requiring a long impulse from a large capacity condenser, has necessarily a severe lesion of its motor nerve. The cases previously mentioned (II, III and IV) were tested also by the condenser method. Many of the muscles that had recovered voluntary power and retained R.D. still required the largest



capacity condensers, even with resistances inserted for the purpose of still further lengthening the stimulus.

It must be remembered that large capacity condensers give not only *longer* stimuli than small capacity condensers, but also *stronger* stimuli. A large capacity condenser holds a larger charge of electricity, so that when it is discharging it is giving a *stronger* current as well as a *longer* current. A muscle of which the excitability has been very much lowered will require a large capacity condenser, not necessarily because it requires a *long* impulse, but because it requires a *strong* impulse. I have seen muscles which required the largest capacity condenser (3·00 microfarads) before they would contract, yet they would respond to a strong faradic current and also (not sluggishly) to a strong galvanic current.

The degree of excitability of the muscle to the galvanic current may yield some information on the state of the motor nerve. An increased excitability *that persists* indicates, I think, a favourable prognosis. The following case will illustrate this :—

*Case VI.*—Captain G. Bullet wound to upper arm. Tested eight weeks later, muscles supplied by musculo-cutaneous and musculo-spiral showed complete R.D., with increased excitability to the galvanic current. Eight weeks after the test (sixteen weeks after injury) the voluntary power of the biceps began to return; seven weeks later (twenty-three weeks after injury) there was fairly good voluntary power in the biceps. The extensor muscles of the forearm showed no diminution of excitability, but there was no voluntary power. The latter, however, returned thirty-five weeks after the injury and the patient was able to resume light work.

I think, however, that the most important information will be derived from a test of the motor nerve. It is the loss or retention of the function of the nerve that we wish to test. If the nerve trunk is stimulated well above the injury and the muscle contracts, we have proof that the nerve trunk contains at any rate *some* conducting fibres. The late Dr. Lewis Jones devoted most attention to the reactions of the *muscles*, and he hoped that the condenser method of testing would throw more light on the condition of the nerves. If a nerve is excitable by the faradic or galvanic current *above* the injury and *retains* the excitability, I think the case need not be sent for surgical exploration. If the nerve has lost its excitability to both currents and does not regain it, the case should, I think, be sent for surgical exploration. The test of a nerve trunk by condenser discharges would throw light on its degree of excitability and would show, if the test were repeated at a later date, any gain or loss of excitability.

The realization of the frequency of the occurrence of cases like those described (II, III, and IV) led me to examine the reactions of the motor nerves as well as those of the muscle. Most of the cases showing R.D. with voluntary power showed retention of excitability by the nerve trunk, but it does not seem to be the invariable rule. The following cases were tested (both muscles and nerves) and the condition of the nerves noted at operation :—

*Case VII.*—R. C., aged 20. June 28, 1915: Bullet wound to thigh; loss of power below knee. September 3, 1915: R.D. (complete) of all muscles below knee. Internal and external popliteal nerves excitable (only by galvanic current). The main sciatic nerve trunk, above wound, was inexcitable. September 27, 1915: No alteration in response to muscles or nerves. At the operation the sciatic nerve was found to be strangled in scar tissue.

*Case VIII.*—Private E., aged 23. August 6, 1915: Bullet wound to arm. September 3, 1915: Tested. Muscles supplied by median and ulnar nerves gave no response. Nerve trunks inexcitable above or below lesion. November 6, 1915: Test repeated. Same result. At operation, both nerves were found to be tightly bound in scar tissue.

*Case IX.*—J. L., aged 20. August 19, 1915: Bullet wound to elbow. November 6, 1915: Tested. R.D. (complete) of muscles supplied by median and ulnar nerves. The median nerve was inexcitable: the ulnar nerve was not excitable above the injury, but was feebly excitable below. At operation, the ulnar nerve was found to be divided, the median nerve was not divided but swollen in the region of the elbow.

*Case X.*—F. T., aged 26. June 30, 1915: Shrapnel wound of arm. October 1, 1915: Tested. Muscles supplied by musculo-spiral nerve and posterior interosseous nerve showed R.D. (complete). The musculo-spiral nerve was inexcitable above or below injury. At operation a piece of shrapnel was found pressing on the nerve trunk, with a small abscess in the same region.

Surgical operation was justified in Cases VII and VIII, also in Case IX, at any rate so far as the ulnar nerve was concerned. Certain points call for comment. The median nerve (Case IX) was not divided and was apparently uninjured. The condition of the nerve, so far as its function was concerned, could not be revealed by inspection, and light would have been thrown on this if the nerve had been electrically stimulated when exposed. So also in Case X. In Case IX it would have been expected that the ulnar nerve would have completely degenerated below the division and lost all excitability. An electrical

test, during the operation, on the exposed nerve would have shown whether there were any undivided nerve-fibres.

## (II) SUGGESTED METHOD FOR ELECTRICAL TESTING.

The method suggested for making an electrical examination of cases of nerve injury is to test the muscles first and note whether there is R.D. Then test the nerve trunk above and below the lesion. If the nerve is inexcitable above the lesion, and remains so for, say, three or four weeks, it should be explored. When the nerve is exposed it should be tested, with the electrodes on the bare trunk. The trunk may appear to be intact (*see* Case IX, median nerve, and Case X), yet may be functionless; on the other hand, it may appear to be completely divided, yet some fibres may have regenerated along the surrounding tissues. A test on the exposed nerve will reveal its excitability and conductivity; mere inspection shows only gross anatomical alterations.

## (III) THE EXPRESSION "REACTION OF DEGENERATION."

In conclusion, it may be said that the expression "reaction of *degeneration*" is not always a correct description. When a nerve has been completely divided and does not regenerate, the muscle is ultimately replaced by fibrous tissue. Such a change can be termed "degeneration," but not so the change that takes place when the muscle loses its power of contracting to very brief impulses and contracts sluggishly to long impulses, *and, at the same time, possesses good voluntary power and does not waste.*

At the present time, a muscle is regarded as showing the reaction of "degeneration" if it has lost its excitability to the faradic current but retains it to the galvanic current, contracting sluggishly instead of briskly. Erb, who was evidently the originator of the expression "reaction of degeneration," included the loss of excitability of the *nerve* to both currents in his definition. A report on the electrical reactions should include those of the *nerve* as well as those of the muscle. The muscle which is inexcitable by impulses of *brief* duration (small capacity condensers or faradic current), but responds sluggishly to impulses of *long* duration (large capacity condensers or galvanic current), has a motor nerve which is either—

- (1) Excitable by both brief and long stimuli;
- (2) Excitable only by long stimuli;
- (3) Inexcitable by stimuli of any length.

The polar changes in excitability (the anodic closure contraction larger than the cathodic closure contraction) were thought by Erb to be the most constant phenomenon in medicine. They are, however, not constant, and they usually depend on the chance position of the testing electrode. They are no longer included in the definition of the term R.D.

## DISCUSSION.

Captain HERNAMAN-JOHNSON: During the last fifteen months any nerve testing required in the Aldershot Command has been done by myself: one has had to act as both neurologist and electro-therapeutist. In that time some 200 cases have passed through my hands; and in many of them I have had to say whether they should be operated upon or left to see if they would recover of themselves. With much of what Dr. Cumberbatch said I am in entire agreement. In muscle testing I have abandoned the use of faradism and galvanism, because it is impossible to standardize them; I have also ceased using the term "R.D." and "partial R.D." I make use of the condenser set which was introduced by the late Dr. Lewis Jones, to which Dr. Cumberbatch made reference. There are twelve degrees on the instrument, giving impulses ranging in length from  $\frac{1}{34000}$  to  $\frac{1}{200}$  of a second; and by repeating the last three studs through a resistance of 5,000 ohms it is possible to get three more degrees, counting them in units of time. So that we can get fifteen degrees in all, ranging from  $\frac{1}{34000}$  to  $\frac{1}{30}$  or thereabouts. Dr. Lewis Jones used the first twelve of his grades at a voltage of 100; the last three are used at 200 volts. You can do this on the original instrument by taking out the lamps of your resistance and changing your terminals. In the new instrument which Messrs. Watson have made for me, this is done automatically, instead of lamps having to be taken out and electrodes put in another place. The original instrument had no voltmeter. The new one is so constructed that, on any main supply of 200 volts or more, it can be adjusted to secure that the condensers shall always be charged at 100 volts for the first twelve, and 200 volts for the last three. Voltage is a matter of great importance, because I found that muscles which will not react to No. 12 stud at 100 volts can sometimes be got to react visibly to No. 6 at 200 volts. What is needed is to adopt a standard for prognostic purposes. The particular standard does not matter much, so long as we adhere to it. The original standard introduced by Dr. Lewis Jones works very well indeed; and when one speaks of a certain length of impulse being required, it means at the voltage he introduced—that is to say, 100 for the first twelve studs, and 200 volts for the last three. Our reports at Aldershot are made without reference to R.D. or partial R.D. By using the Lewis Jones condenser one does not trouble about rapidity or sluggishness of reaction, but about minimal contraction. You note the last stud which will give any visible reaction at all. In some cases there may be



a doubt, and then one notes down the two studs with a dash between, such as 7—8, 8—9.

Such records are valueless unless *all* the conditions of the test are uniform. You may have a case of nerve lesion which, on a warm day in September, reacts to No. 9; you may not see that man again until a cold day in November. You test him again, and find him, perhaps, reacting still to No. 9. Unless you take care previously to give him a warm arm-bath you might conclude, from the electrical test alone, that he has made no progress; but as he has meantime developed voluntary movement you know he must have improved. One also takes care not to test when the patient is tired.

The way in which the electrodes are laid on is also important. Lewis Jones's original description spoke of putting the indifferent electrode on some part of the body and using a single testing electrode; but I find it best always to put one electrode on a motor point and the other somewhere else on the muscle. Sometimes the trouble is that there is spreading, so you might test the extensors on the back of the arm and find a violent twitching of the flexors, which will confuse your result. But if you have the two electrodes together, and pinch up the muscle somewhat, you can eliminate that spreading. The solution used also makes some difference. I always use tap-water, the electrical conductivity of which is fairly constant everywhere. I think Dr. Lewis Jones believed the skin exerted practically no resistance to these condenser discharges, but that is not my experience; it takes time to break down that resistance—not so long as with the galvanic, but longer than with the faradic. One gets over that by rapidly putting up the numbers until one gets a definite good contraction—up to 15 if necessary, letting the instrument run until the muscle is well “gingered up.” Then, having given the muscle half a minute's rest, we proceed backwards, watching the minimal point—that at which the last contraction is seen. This is more reliable than proceeding upwards, because you have broken down the resistance before you start your observations. These points must be borne in mind if you are going to put one result against another, and particularly one man's testing against another's.

I find myself in even further agreement with Dr. Cumberbatch, in that I admit that muscles which show good voluntary movement will not, sometimes, react to any condenser stud at all, though that is unusual. Frequently high numbers, such as 12 or 13, are required in order to get a response, but obviously the muscles are recovering, because they can be used voluntarily. But I believe that all these muscles have been, at some time, either anatomically or physiologically severed from their spinal cells, and in such a case it frequently takes the muscle a long time to recover its proper electrical reactions. Still, that does not affect the prognostic results when one gets the cases fresh. For example, a man comes in from abroad; he may have been kept a month on the other side. He has a dropped wrist, and his injury may have involved the musculo-spiral. If you find that he will react to stud 9 or 10 on the condenser, is it safe to say he will recover without operation?



Experience has proved that it is not safe, the reason being that three or four weeks is not a long enough time for maximum changes in the electrical reactions to take place. Not long ago I saw a man some time after his operation, and read the surgeon's report. I found that he had had a severed nerve. My electrical report was attached, in which I said the reactions were 9—10, and that probably the nerve would be found involved in fibrous tissue, not severed. I investigated the matter, and found that the case had been sent to me as one of three months' standing, whereas the testing was, in reality, done only three weeks after his wound was inflicted. Of course, the full electrical changes had not developed in three weeks; I had been basing my opinion on the statement as to three months' duration. Not until two months after the injury can you be sure that the electrical changes will have developed to their full extent. If, after that time, testing under favourable conditions of warmth, &c., you find it takes No. 12 or more, my experience is that operation is necessary: either the nerve is severed or it is seriously involved in fibrous tissue. If after two months it gives reactions to No. 9 or No. 10, there is always a considerable possibility that that patient will recover. On the other hand, the nerve may be involved in fibrous tissue whose contraction is increasing, and in a month you may find the patient going backwards instead of improving; and in such a case one advises operation. If the response is to Nos. 7, 8, or 9, you keep the patient under observation for a month or two to see which way the reactions go. None of the cases I am speaking of will react to faradism, and being beyond the faradic range you could not, in the old days, say anything definite about them from the electrical point of view, whereas the condenser gives us eight degrees of inquiry in muscles which no longer react to faradism.

I do not wish it to be thought that I advocate condenser testing as a royal road to prognosis, because you must take other factors, such as the sensory condition, into consideration; and in a doubtful case there must be a careful balancing of probabilities. If the sensation has gone, probably the best thing is to advise operative interference; but if sensation is only slightly impaired, it is better to wait. The condenser reactions are of very valuable assistance in prognosis, as is shown by the fact that only in one case that I can remember has operation disclosed nothing abnormal to the naked eye. I had not the opportunity of testing the nerve conduction while it was exposed, as suggested by Dr. Cumberbatch; otherwise some very useful information might have been elicited.

You may find the reaction only 12 or 13 a week or two after suturing has been done, and the patient has a very anxious time, even though he has been told it will be six, eight or twelve months before definite improvement sets in. These patients are despondent, and if you can give them some visible proof of even slight improvement, they are very grateful. Even the absence of condenser improvement is not necessarily a bad sign, for the patient may still regain full voluntary power. Speaking generally, however, an improvement of at least three or four studs can be demonstrated before there is any voluntary control or response to faradism.

## 34 Cumberbatch: *A Criticism on Reaction Testing of Muscles*

Major BAILEY (Brighton): The cases at the Second Eastern General Hospital at Brighton, and from auxiliary hospitals in connexion, together with those sent from the Indian Hospital there, tested by me by the condenser method during the past fifteen months, number altogether close on 400. Among the Indians an estimate of their epicritic and protopathic loss was difficult to obtain correctly, and the condenser tests consequently gained in proportionate value; about one-fourth of these Indians reacted normally to the condensers. I agree that sensory and trophic changes, as well as loss of voluntary movement or not, must all be taken into consideration in forming a correct estimate of the nerve condition as well as the condenser reactions. I quite admit that there are, as Dr. Cumberbatch points out, many points of difficulty in condenser testing, but I am absolutely in accord with Captain Hernaman-Johnson as to the eminent practicability of the condenser method and the value of its results. The various points of procedure he advises I have arrived at independently, such as the value of warmth, using *both* electrodes on the same muscle, working backwards until one obtains the lowest response instead of Lewis Jones's original method of working forwards, testing under similar conditions each time, &c. The resistance of the skin, especially among Indians, is often very high; I recollect one case where at first the flexor sublimis would only respond to No. 12 condenser, yet finally contracted on No. 1. I use saturated warm salt solution, which I find breaks down skin resistance quickly. The practical point is—and I have the opinion of all my surgeons in agreement with me—that in the very large majority of cases where there is a contraction only to very high condenser values, the nerve has been found at operation either totally divided or constricted to such an extent that the axis cylinders are destroyed or functionless. Surgeons have told me the condition of such constricted but undivided nerves: the part above the constriction was swollen, sometimes very considerably, and the part constricted was a thread-like or tape-like, apparently fibrous band. It has been considered advisable in most cases of this kind to relieve the constriction, but *not* to resect the nerve, on the supposition that the new axis cylinders have, in this fibrous "scaffolding," the best opportunity of growing correctly down to their former terminations. The results have quite justified this procedure. The condensers give such accurate information that I have found it superfluous to trouble about galvanism and faradism. For routine work I am content to use the Lewis Jones method with Captain Hernaman-Johnson's modifications; and although I name the conditions of various muscles as normal, partial, or total R.D., I add the condenser stud number, 1 to 15, as the case may be, and I find that surgeons quite grasp now what that really means. I think for the sake of uniformity we should continue to work on Lewis Jones's lines. However, for experimental purposes, I have had an apparatus made locally; this gives a charging voltage varying, in 25 volts, from 25 to 225 volts; the condensers vary from 0.016 to 4 microfarads; and the added discharging resistance varies, by 1,000 ohms, from 1,000 ohms to 10,000 ohms. This has proved valuable

in testing cases of facial paralysis, and I hope to obtain some practical results in the future from it. Everything has been gone over so thoroughly by Dr. Cumberbatch in his most interesting paper, and all the practical points have further been set forth in such an illuminating manner by Captain Hernaman-Johnson, that there is little for me to add, except my agreement in general with them both.

Dr. TURRELL: There is one point on which, though it has been mentioned, sufficient stress has not, I think, been laid; that is, the amount of resistance from the electrodes used. There are three factors to deal with: the capacity of the condenser, the voltage at which it is charged, and the resistance through which it discharges. Nothing has been said to-night about standardizing the size of the electrodes which are applied for testing. Cluzet, to whom with Dr. Cumberbatch and the late Dr. Lewis Jones we are indebted for this method of testing in a practical form, rightly attaches considerable importance to this point. The size of the indifferent electrode which he recommends as a standard is 100 sq. cm., and the active electrode is only 1 sq. cm. One difficulty which is encountered in testing severely damaged nerves by the condenser method is the over-contraction of the healthy muscles, which masks the feeble contractions of the affected muscles when the strong condenser discharges are used. In order to overcome this difficulty, the use of two small electrodes on the muscle to be tested has been recommended. In that case the electrical resistance would be much increased and the wave length of the stimulus much shortened, rendering the results so obtained useless for comparison with those obtained in the ordinary manner. Cluzet has pointed out that this difficulty can be largely overcome by the insertion of higher resistances and the use of higher voltage; by that procedure the relative contractions of the affected muscles is much increased. It is of importance in testing to pay attention not only to the temperature of the limb but also to the dryness of the skin. Where a limb is either very cold or the skin very dry, it should be wrapped in a warm wet towel for some time before the reactions are taken. It is very important to consider the sensations of the limb in conjunction with its electrical reactions, and a record should be kept of the results obtained for future reference. This is best done on the card index system, using a card on the front of which the names of the muscles and their nerve supply are printed, and on the back of the card there should be printed diagrams giving an anterior and posterior view of the limb, with the cutaneous nerve supply outlined. On these cards the muscles should be marked according to their condenser reaction, and the defective area of sensation should be shaded or otherwise marked.

Dr. GRAINGER STEWART: I would like to express my thanks to Dr. Cumberbatch for his most interesting paper. I have for some time past been doing a considerable amount of muscle and nerve testing at the King George Hospital. I look upon nerve and muscle testing not only from the point of

view of electrical testing, but also from the neurological standpoint. I think that neurologists in general have not, for many years past, attached the importance to nerve testing that is generally ascribed to it in the text-books and by the medical profession as a whole. I have not had experience of testing with condenser methods; my work has been carried out in the old-fashioned way with faradic and galvanic currents. My experience is that electrical testing only helps one to form an approximate idea as to the condition of the nerve when it is repeated and associated with continued clinical observation. Cases often arrive at hospital without a very definite history. In one you find that the nerve was directly injured at the time the man was struck; in another the nerve was injured by concussion or contusion; or yet again there are cases in which the nerves have become involved at a later stage either by callus or scar tissue. The reactions elicited will depend largely on two factors: the severity of the injuries and the time at which the test is made. Though very inadequate, I think faradism is by far the most accurate method we have of estimating the condition of a nerve. With regard to the galvanic reaction, I think that depends very much on the state of the muscle. I have seen a case of dropped wrist from musculo-spiral paralysis in which the muscles were over-stretched, and in which there was no response at all, respond normally three days later, after the arm had been put into the hyper-extended posture. I have also been able to obtain a response in muscles which, despite the skin having been well moistened and the limb warmed, gave no response by applying massage to the paralysed muscles for a few minutes, and then repeating the test. I think galvanism is of use in giving some idea of the condition of the muscle, but is not of much help as an indication of the condition of the nerve. One of the most important suggestions that Dr. Cumberbatch has made is the advisability of testing injured nerves at the time of the operation. By that means we may hope to advance our knowledge as to the condition of a particular nerve, and, by observing the subsequent history of such cases, we shall be in a better position to give a prognosis in future cases.

Mr. N. H. M. BURKE: In general, I am more and more coming to the conclusion that not only is electrical testing necessary in attempting to arrive at a true diagnosis of the condition of a nerve suspected of injury, but that electrical testing by itself is of very little use. I am convinced that every source of information is necessary—voluntary power, skin sensation, measure of wasting of muscles, electrical reactions by all methods (faradic, galvanic and condenser), and result of stimulation over the nerve trunks themselves; and that this thorough examination must be repeated after an interval of, say, three weeks, and any change in the results noted, before anything like a good idea can be obtained of the actual condition of the nerve. With regard to the electrical methods in detail, I have been using the condenser method for some time and do not feel at all satisfied that it has yet assumed its proper position or that its meaning is at all clear or certain. One constantly comes across cases in which there is so great a variance between the findings by the old



faradic and galvanic method and the new-fashioned condenser method that the true relation between the two is, I think, still to be found. To my mind no case should be considered as thoroughly and scientifically examined until all three methods have been employed. I agree with Dr. Hernaman-Johnson that it is essential to the uniformity of results that all observers should use a constant charging current of 100 volts. Another source of error in estimation of the results obtained by use of the large condenser, say, 3 microfarads, was suggested to me by Dr. Cumberbatch a few days ago, namely, that the discharge given to the muscle is not simply one of long duration but also one of large amount, and this point is not usually taken into consideration. Further, it may be that one ought to exercise as much care in noticing the type of response, whether quick or sluggish, when using the condenser discharge as when using the galvanic current. The next point is the advisability of following every case that goes to the operating theatre, and when there not only of inspecting the condition which has induced the set of symptoms found previously, but also of going further and testing electrically the exposed nerve trunk. In this respect it has been my fortune at the Epsom War Hospital to work with a most charming set of surgeons, who have given me every sympathy and assistance, and have, I believe, found that the presence of an electrical instrument not only adds to the interest of the operation but also helps in the work that has to be performed. We are now in the middle of a whole series of nerve operations, and certain points seem to be emerging which, I think, are new and worth mentioning. I now state them quite tentatively, because the number of cases upon which to base any definite conclusions is as yet too small. I have used a sterilizable bipolar electrode and faradic current, which has been applied by the surgeon to the exposed nerve. Most of the cases have been found to be those in which a portion of nerve has been compressed by dense scar tissue, and we have several times noticed two things: (1) Stimulation above the scar produces a response which is definitely bigger than that to stimulation below the scar; (2) immediately the nerve is freed from the strangulation the responses above and below are equal and both much improved. I have hopes that, if this type of reaction is, on further evidence, found to be constant, it may prove of assistance to the surgeon who, having found his injured nerve and freed it from strangling scar, is faced with an inch or so of hard, unsatisfactory-looking material, and has to decide whether to trust it to recover or to resect and suture the divided ends. The suggestion in my mind is that a strangled portion of nerve that is capable of recovering complete function should show this immediate improvement, and that when the improvement is not found, the portion may be considered to contain only scar tissue and should be resected. Of course, the idea is not yet fully worked out or substantiated, but the following cases are of interest, and they also serve to compare the condition found at operation with the excitability of the nerve as found in the ordinary way before operation by stimulation at various points over the course of its trunk. The first case of this series was a soldier operated upon by Captain Owen, of the Australian A.M.S., three and a half months after he was wounded.



The ulnar nerve, which gave no response before operation, gave a poor one on the table before the constricting scar was dissected away, and a fairly good one when quite freed. The musculo-spiral gave a weak response after being freed and none before or in the preliminary examination. In the same case the median, which was not affected by the injury, responded strongly to all three tests. In the next case—a median nerve four months after injury—weak response was present before operation; this improved immediately before and after liberation. A musculo-spiral and a sciatic nerve gave the same type of reaction, while the remainder—ulnar, facial, and median nerves—gave no response at any time, and resection and suture were therefore undertaken.

Dr. G. B. BATTEN: May I ask Dr. Cumberbatch one question? He did not say how he tested a nerve above and below the lesion. Which of the three forms of current does he use?

Dr. CUMBERBATCH (in reply): In answer to Dr. Batten, I use the faradic and galvanic above and below; but I forgot to mention that I often go to the highest point and test the nerves at their point of origin from the spine and use big capacity condensers. You can get impulses sufficiently strong to pass down to the nerves at their point of emergence, and in that way cause a contraction of the muscles. The first three speakers seemed to think cases that required large capacity condensers should be sent for operation, and at the operation they would find injuries which justified operation. I cannot speak with so much experience of military cases behind me as the first three speakers, but I know that in one case, at any rate, the nerve after injury and subsequent removal of callus which was pressing on it still required the largest condenser to elicit response, even after the muscle had regained voluntary power in it. There is another point which may be a source of fallacy with the condenser method, and that is that if the muscle has considerably diminished its excitability, it will require a stronger stimulus to make it contract. If you have a muscle which is itself the seat of disease, it may require, on account of its diminished excitability, the biggest capacity condenser in the box; and one might regard it as showing the reaction of degeneration. That is a possible source of error in the condenser method; and, at the risk of being called hypercritical and bringing "not peace but a sword," when speaking of the reactions of muscle as shown by the condenser method, I protest against the method of referring to studs 1, 2, 3, &c., because it does not give an idea of what is meant. It is an imitation of the unscientific method of calibrating catheters by means of holes in plates which have numbers attached to them, but mean nothing at all.

(November 19, 1915.)

## The Organization of Electro-therapy in Military Hospitals.

By ETTIE SAYER, M.B.

I HAVE recently returned from a visit to French electrical clinics. The French War Office considers that the wounds of this great War call for electrical treatment on a wholesale scale. Hence the organization of medical electricity in French military hospitals has become more advanced than it is in our own. As the working of British electro-therapy depends ultimately on British electro-therapists, that is to say, on the members of this Section of the Royal Society of Medicine, I thought I would try to bring to your notice a few special points which struck me as being worthy of your consideration.

On starting, I received a letter from Professor Leduc, saying he had no clinic where he was treating patients himself, as he had been appointed to superintend all the radiographic and electro-therapeutic departments in the Loire district, which meant those of 100 hospitals; but he invited me to visit his laboratory at Nantes. This laboratory, from the point of view of electrical research work, is teeming with interest. Doctors come in and out to watch ionic and other experiments, and one felt they were indeed fortunate in constantly having access to such a teacher. On returning home one of my first visitors was a V.A.D., whom I asked whether any medical electricity was being done in her hospital. She replied, "Oh yes, we were very fortunate, as one of the masseuses they sent us knew all about ionization and so she was able to teach the doctor, and now we do lots of cases!"

The first point I would like to put is, whether our Section considers that, given a man of Leduc's attainments, anything could be better than that he should be supervising the work in a large district of numerous hospitals, with a laboratory at his headquarters for carrying on research work? The appreciation of his ability is delicately shown by placing no restrictions upon him with regard to any private practice he wishes to keep up. He has perfect freedom. I trust our Section will agree that it possesses quite distinguished and competent members in nearly every part of the country, suitable for similar posts. A serious proportion are at present unfortunately swallowed up in X-ray work alone, to the exclusion of clinical physio-therapy, but the daily life of

many Frenchmen—for example, of, say, Professor Bergonié, at the Hôpital Grand Le Brun—shows that it is quite possible for one man to direct both departments, and also do as he does, and find time for private practice as well, provided his organization is good. Professor Bergonié, with the help of a well-thought-out staff of assistants, runs an admirable X-ray department for this hospital of 500 beds. He also invents and supervises treatments in electricity, heat, light, massage, and mechano-therapy, which, when totalled up, amounts to between 700 and 800 hours of it every day. The Hôpital Grand Le Brun has a great reputation for troublesome adhesions, &c., and so a large number of these cases are sent there, which means cases needing lengthy courses of treatment.

The art of being able to get through much work seems to lie in (1) a sufficiency of assistants, (2) an adequate and convenient equipment, and (3) an abundance of space to work in. No doubt governments and war offices are difficult obstacles to overcome, but if the French can manage theirs, I cannot see why the British doctors should not do likewise.

But if the French are ahead of us in electro-therapeutics, we have our counter-balancing redeeming features. For one thing our transport, for another our nursing. Not only do we ourselves consider that British nursing is the best in the whole world, but the French War Office are of exactly the same opinion. In travelling from Paris to Bordeaux, I met in the train Mrs. Bedford Fenwick and Miss Grace Ellison, who were on their way to Talence, at the request of the French Government, to organize the placing of fully trained and specially picked British nurses in various French military hospitals. I visited Talence, not because it was an electrical clinique, of which I had previously heard, but in order to pay a friendly call on the matron. In order to give some idea of the organization of an electrical department in just an ordinary French hospital of 500 beds, I would like to describe that at Talence. The greater part of the hospital was built by German prisoners. Four large, bright, well-ventilated wards are given up to physio-therapeutic treatment, a small adjacent room serving as a consulting room for Doctor Cigelas, who directs the department. Systematic and well-organized charts are kept, recording every treatment given, with its result, &c. These reports are sent to headquarters in Paris once a month.

Physio-therapy in France is always broken up into these four branches: (1) Electro-therapy, (2) Thermo-therapy, (3) Massage,

(4) Mechano-therapy, each of which usually has a large separate room. At Talence 120 cases were having two treatments a day each: electro-therapy and mechano-therapy alternating three days a week, with thermo-therapy and massage on the other three. The whole procedure for the 120 took up only three hours a day. Sixty cases were treated in the first hour and a half, and after an interval in which the appliances are sterilized, another batch of sixty comes in for the second hour and a half. Each group of sixty, on arriving at the department, breaks up into four divisions, fifteen wounded going into each room. At the end of three-quarters of an hour they change round; each man thus visits two rooms for three-quarters of an hour on three days a week, and the other two rooms on the alternate days of the week.

#### ELECTRO-THERAPY.

The equipment of the electrical department of a civilian hospital is not convenient for treating large numbers of wounded. Unless important alterations are made, a great deal of everybody's time will be wasted. For example, in ordinary private practice I personally find I order high frequency much more often than either static electricity or ionic medication. In treating the wounded, on the other hand, I find both static electricity and ionic medication are wanted about a hundred times as often as high frequency.

*Galvanism* and *faradism* are needed in such huge numbers of cases in military hospitals that it is necessary to be able to treat a considerable number of them at the same time. In all the French electrical departments which I have seen, similar systems are installed for galvanism and faradism. A large apparatus of the Bergonié obesity type is installed at the end of a room. It has two metronomes. One interrupts a continuous galvanic and the other a faradic current. From either end emerge six to ten leads according to the size of the hospital. Down the wall, on either side of the room, is a row of small resistance boards, about 3 or 4 ft. apart. One of the leads goes to each of these boards. Under four or five of the boards is generally a long shelf to support four or five arm-baths, against which chairs are placed so that the men have only to walk in and sit down to find that their arms rest comfortably in the bath at the right level. Other leads go to the leg-baths, and the rest to lounge chairs, upholstered in mackintosh, for wounds in situations where they have to be treated with pads. Twelve to twenty patients come in together. The faradics go to one side, the galvanics to the other; they sort themselves to their particular



electrodes; the clinical assistants apply the pads; the main current is turned on; the resistance adjusted to the particular needs of each patient. Treatment is continued for an hour or some shorter time. The assistants then clear up and sterilize ready for the next batch to take their turn.

With regard to ionic medication, I would like to mention that Professor Leduc is now employing water instead of wire resistance throughout his district. This has the advantage of making the continuous current direct from the main perfectly safe, and so obviates the necessity for the installation of cell batteries.

Professor Moutier, in Paris, was treating low blood-pressure wound cases in a way which was new to me, but from which the patients were unanimous in hearty praise of the great benefit they were deriving from it. Small high frequency cages were suspended from the ceiling. They were built like the ordinary large cage, to which we are accustomed for high blood-pressure cases, but with only either three or four, or six or eight rungs. The cage is let down and suspended round the body of the patient at the level of the wound, and a strong current turned on for ten to fifteen minutes. Stagnant wounds receive a healing stimulus from the local magnetic field which is thus created, and the general blood-pressure rises. Personally, I have always during the past ten years treated all cases of low blood-pressure with static electricity, and the results have been so satisfactory, especially for shock and for its after-effects, that I prefer to remain true to the old friend.

#### THERMO-THERAPY.

I suppose one may take it for granted that all the members of this enlightened Section prefer radiant heat to wet heat? that they consider constant fomentations and continual irrigations entirely unphysiological? that they admit that, although hypotonic solutions are a step nearer physio-therapy than the crude and simple soaking in antiseptics, collections, nevertheless, of leaking bandages through which hypotonics dribble, dribble, dribble, the whole day long constitute a wet, chilly, cheerless method of treatment? How much more grateful do we electricians find hypotonic ionizations with static and radiant heat! Therefore for us the question of thermo-therapy resolves itself into the choice of the best apparatus. For hospitals I like those simple little electric radiators, which are in almost flat sections about a foot square, each carrying on its under surface a pair of small cylindrical lamps about half the size of the ordinary household heating lamp. Suitable



numbers of sections are hinged together to form an arch over the various parts of the body. A thick woollen coverlet is thrown over the arrangement and the treatment continued for an hour.

#### MASSAGE.

First-rate knowledge of this subject seems best obtained from Swedish sources, either by going to Sweden or by lessons from someone who has been through the schools there. It is a compulsory subject for the Swedish medical student, who consequently after qualification is able to collaborate more satisfactorily with the masseuse than is possible in countries where massage is not included in the curriculum. A Swedish surgeon considers it necessary to have his masseuse present at the actual operation and explain explicitly at the time what is required by way of treatment afterwards. This is an advantage to the patient. It enables the massage to be more intelligent than when the wound is a mystery to the masseuse. In the case of wounds, healthy tissue is going to encroach upon the injury and heal it, or inflammation is going to spread over it with the reverse effect. Hence, after the infliction of an injury, every day—in fact every hour—is of importance and constitutes a precious opportunity which once lost can never be regained. The more skilled and gentle the masseuse the more quickly can treatment be begun after operation. Even where the wound itself cannot at first be touched it is an advantage to maintain function from the earliest moment by electricity and massage of the surrounding parts, especially of those tissues on the proximal side, through which Nature drains and heals the wound. From an electric specialist's point of view an unnecessary adhesion is always a disgrace, signifying neglect of early treatment. None such should ever be allowed to form. Unfortunately, hundreds of thousands of men in Europe are now suffering from wounds to whom electricity and skilled massage were unobtainable at the time of operation. Loss of function has spread into adjacent tissues and structures; the surrounding parts have got matted together and immobilized in fibrous adhesions. As the number of competent masseuses available is limited, it seems to me desirable that those who are really skilled should be reserved for quite recent cases, partly because one week's treatment at the time of injury will obviate the necessity for perhaps six or eight weeks' treatment later on, but chiefly because the treatment of early cases necessitates skilled manual massage, whilst for later cases mechanical substitutes can be found. One of these substitutes is an ingenious apparatus invented by Professor Bergonié and

worked with multiple leads. The leads consist of gas-pipes, each going to a pair of india-rubber bags. The pairs of bags are of various sizes and shapes, and for treatment of uncommon or awkward wounds some may need to be specially constructed. The apparatus consists of an engine and a metronome arranged to give interrupted blasts of air down the gas-pipes into the bags, which are alternately inflated and released. They are used for old wounds which have healed but which are stiff and need much massage. A pair of bags are strapped on round the wound, which is thus subjected to intermittent compression of a very comforting kind, and which can go on for an hour without causing fatigue to any masseuse.

#### MECHANO-THERAPY.

There still remains the large mass of crippled convalescents in whom prolonged immobilization of inflamed parts has resulted in awkward adhesions and stiff joints and tendons, and for whom passive movement as well as massage are essential. Normal movements of the parts may be so impossible that none is attempted at all. Ionic medication will dissolve adhesions, but it is also necessary to institute a gradually increasing range of passive movements to work up the muscles and restore natural functioning. It is not the question of dealing with the individual case that presents the difficulty, but that of organizing treatment of the large number of cases. Zander's set of apparatus is that best known for mechano-therapy. A study of his principles (which by means of counterweights under nice control produce a gradually increasing range of movement in the various joints) is advisable; but his installation has certain disadvantages: (1) It is German, and therefore at present unprocurable among the Allies. (2) Although some of the pieces in a Zander set are good (i.e., for the hip), there are others (i.e., for the upper limb) which are very defective. The best surgical instrument-maker's apparatus for the upper limb which I have seen is that of Messrs. Gaiffe. For the fingers especially, it is much superior to Zander's. (3) There is too much stuffy green plush in the composition of Zander's pieces for them to be suitable for a hospital. (4) Although extremely complicated and extremely costly, the results to be obtained with a Zander's installation are no way better than those to be got with quite simple home-made apparatus. Personally I strongly favour simple home-made apparatus. By some curious chance the most magnificent Zander Institute which I have seen in any military hospital was achieving but poor results. The

physician in charge happened to leave the whole running to assistants, none of whom seemed particularly interested in the work. An enormous number of patients were being hustled through with such inadequate doses that it was difficult to see what good could be done to them. The contrast to the work being done by Professor Bergonié, or Dr. Besson, of Nantes, was very striking. Bergonié's devices are so numerous and varied that it is difficult to start a description. Where the inventive genius of the doctor-in-charge is responsible for each piece of apparatus, the working always seems extra keen and lively. Professor Besson has invented seven principal apparatus for passive moments of the various parts of the body which serve their purpose admirably. All have been made by the patients themselves (for among 500 wounded there are bound to be a few who know enough about carpentering to carry out any doctor's ideas). All Dr. Besson's pieces consisted entirely of wood, very neatly finished off and furnished with counter-weights, except one which consisted of the main part of a bicycle immobilized on a stand, and with handles arranged so as to exercise the arms as much as the legs. Professor Besson is an expert on the utilization of decrepit bicycles, or their various portions, for mechano-therapy. Defunct inner tyres he sterilizes and uses for elastic traction to counter-act tendencies to abnormal positions in the intervals of treatment—i.e., in dropped wrist a very loose loop of strong comfortable stuff is passed round the arm above the elbow, and a strong hook attached; other loose loops with hooks are passed round each finger; the whole is connected by a piece of inner tube so looped as to connect all the hooks and hold the limb in normal position. If the patient gets stiff or tired, he just unfastens the loops for a while, but if the elastic loop is arranged so that the limb matches its fellow when it is in a position of normal relaxation, that will probably be the most comfortable position for most of the time.

Professor Bergonié's mechano-therapy room has a long bar running horizontally down the middle of it, and supported on uprights so as to be about 6 ft. high. Weights and pulleys are suspended over it; by means of foot-posts on the floor patients are placed in such positions that the particular muscles of the back or shoulders that need developing are called into action when they raise the weights. He sets men to dig in the garden for back exercises, and to wheel barrows for shoulder exercises, &c.

In conclusion, may I urge the need for this Section considering:—

(1) The appointment in each district of a special physio-therapist

to superintend the hospitals in that district, especially those smaller ones where there is some medical man who is keen on, but inexperienced in, the work. This Section has thoroughly competent members in all parts of the country—one cannot imagine the Committee having any difficulty in filling such appointments.

(2) That the X-ray work in hospitals should be so organized that, with the help of extra assistants, electro-vibreurs, &c., one man is competent to superintend both an X-ray and an electro-therapeutic department in the same way as one man manages to direct both in France. It seems necessary to make a stand for liberty to engage in private practice, especially in slack military times. It needs great self-sacrifice for a medical man to do military work at all. When a doctor who is devoting his time to this life has satisfied his conscience that he has done his best for his military patients, the least a war office can do in return is to refrain from interfering with the rest of his day.

(3) The equipment essential: As a basis for consideration for 500 wounded, I would like to suggest, say:—

- (i) One static machine.
- (ii) One diathermy apparatus.
- (iii) One sinusoidal apparatus.
- (iv) One large interrupted galvanic apparatus with ten leads in the electrical department, and one small apparatus for each ward.
- (v) One faradic apparatus on the Bergonié system with ten leads.
- (vi) Ten radiators of three sections each.
- (vii) Ten vibrators.
- (viii) Ten pieces for mechano-therapy.
- (ix) A mechanical massage apparatus with numerous leads.

I trust you will agree that with some such sort of equipment properly worked the average stay of the average patient in hospital ought to be halved. I believe the question of expenditure on initial outlay is an obstacle at the War Office. I can only see that it is the most terrible extravagance not to indulge in it.

(4) The question of defining the number of square feet of floor space necessary for the installation and for good ventilation. Cellars are not suitable; the department whose *métier* it is to throw muscles into vigorous contraction is the one which most of all needs plenty of oxygen.

The PRESIDENT: We are, I am sure, very much obliged for this most interesting paper. I am sorry the lateness of the hour prevents the possibility of a discussion on it.



## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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(December 17, 1915.)

### The King George Military Hospital—Radiographic Department.

By STANLEY MELVILLE, M.D.

I HAVE been asked to give you a short account of the equipment and arrangement of this department, to acquaint you with certain difficulties and perplexities with which we were confronted, and to ask for your friendly criticism and advice as to increasing the efficiency of the department in the future.

This building was designed for His Majesty's Stationery Office, but at the outbreak of the War was still incomplete, and was diverted to its present use on the advice of Sir Rowland Bailey, late Comptroller of that Office. The hospital is under military control, and is administered by officers appointed by the War Office, the present Commandant being Colonel Cottell. The Army Council undertook all structural alterations, while the Joint Committee of the British Red Cross Society and the St. John Ambulance Association made itself responsible for the entire equipment and for the personnel of the hospital. The visiting staff is entirely civilian and the services gratuitous, making altogether a somewhat unique and, I venture to think, a satisfactory experiment in the history of war hospitals.

Early in the year, the Joint Committee invited Dr. Harrison Orton,



Dr. Ironside Bruce and myself to join the honorary staff of this hospital and to undertake the necessary arrangements for the equipment of this department. At this time only part of the structural alterations had been effected, but our troubles began at once, for, on my being deputed to visit the building in order to decide as to the suggested accommodation, I discovered three somewhat small, ill-ventilated, and quite inadequate cupboards, one on each of the lower three floors, in which we were asked to house ourselves. Every available corner in the building appeared to have been allocated for some definite purpose, and the matter was somewhat serious, inasmuch as the equipment of this department was to form part of a special and noble gift from the British Farmers' Red Cross Fund.

We discovered, however, that the basement had not been definitely appropriated, though, indeed, as we first saw it, it seemed impossible that it could be utilized. Literally tons of rubbish covered the floors, where it was not already inches deep in water, and nothing was to be seen but row after row of bare concrete columns, set 18 ft. apart. Here we decided to settle, and we proceeded to stake out extensive claims in this "no man's land." The extent of our claims, while affording much amusement and opportunity for "leg-pulling" by the Committee, has not turned out to be greater than the needs of the department justify. Eventually, the department shaped itself into what you will see to-night, thanks in great measure to the kindly sympathy and help of Mr. Oakley Williams (secretary, for the Joint Committee, to the hospital), and also to the great interest and experience of Mr. Walter Goodwin, of the staff of the Director of Barrack Construction.

The department consists, as you will see, of five rooms and a waiting-room. Of the five rooms, two of them measure 18 ft. square, with an intercommunicating door, and are fitted as couch unit rooms. Each room is self-contained, and is equipped with a 14-in. coil, mercury and Wehnelt interrupters. A third room contains the screening stand, which is also fitted with an easy stereoscopic shift gear, and has a special fitment for examination of the head. Of the other rooms, one is used as a consulting room, and is lined with cupboards for the filing of plates and report cards. The dark room, which is a copy, on a large scale, of that at Brompton Chest Hospital, is most roomy and well equipped. I would draw your attention to the large teak washing tanks, which we have found most advantageous. In addition to these

rooms, there is, on the third floor, a special operating theatre, fitted with coil and mercury interrupter, to which I will refer later.

I have mentioned our early difficulties, but the serious difficulty of dealing with the main electrical supply, which was of the alternating type, presented itself. It was perhaps fortunate that three men were appointed whose views were identical upon this subject, and it was decided to convert the current by means of a large motor transformer ( $18\frac{1}{2}$  H.P.), which gave us a 200-volt continuous current. I do not think we have had any reason to regret our decision.

Another difficulty which presented itself was the thorny question as to whether portable apparatus should be provided for use in the wards. Upon this point we were asked to express our opinion, which was done in the form of a memorandum, in which we stated our objections, and gave the following reasons:—

Firstly, that on general principles we disapproved of portable apparatus, of the usual accumulator type, as being unsatisfactory from the radiographic point of view.

Secondly, that the disadvantages of such apparatus were not by any means compensated for by any increased comfort or advantage to the patient.

It was, however, suggested, if it could be shown to be generally desired, that cables should be run direct from the transformer to suitable points in the wards. Eventually this difficulty was overcome by the provision of special stretchers, one of which is available for your inspection. This stretcher has a strong loose canvas top, which can be introduced beneath the patient without seriously disturbing him, and can then be made rigid by means of the poles and end-pieces.

With regard to the apparatus itself, you will notice that we have made no attempt to aim at heroics, in the shape of instantaneous radiography for instance. Our aim has been that everything shall be subservient to two chief factors—namely, the comfort of the patient and rapidity in dealing with large numbers of cases. Thus, while a patient is being examined in one of the couch unit rooms, another patient is being prepared in the adjoining room, or they can be in use simultaneously if required. There is also an excellent system of intercommunicating telephones throughout the hospital, and it is easy to arrange for patients to be brought down in batches, and thus prolonged waiting is obviated. Thanks also to the kindly arrangement

of the Commandant, a large lift is reserved exclusively for our use during the whole time the department is open. As in most military hospitals, the work of the X-ray department though continuous, is liable to periodic rushes, and we find it perfectly easy to deal with from sixty to seventy cases in an afternoon. Facilities for examination of patients under the fluorescent screen by the resident medical officers are provided during certain hours in the morning.

There are two matters of interest to which I would in particular direct your attention. The first of these is the special operating theatre, which has been fitted with apparatus for direct operation under X-ray illumination. When Mr. Edmund Owen (who, alas! has not lived to see the results of his great labours) asked me to take over the secretarial work in connexion with this department, among other correspondence which he handed over to me was a letter from Mr. Maynard Smith, of St. Mary's Hospital. The latter had been invited to join the staff, and in his letter he strongly urged the importance of such a theatre, pointing out what an invaluable help similar provision in Dr. Harrison Orton's department at St. Mary's Hospital had been.

I had been debating in my own mind the provision of such a theatre, and had come to the following conclusions—namely, that the less the ordinary theatre is interfered with, and the simpler the special X-ray arrangements, the more likely would the surgeon be to appreciate and avail himself of the undoubted assistance which X-ray illumination affords. It was obvious that the couch would have to be constructed of wood and not of metal, and that it should be properly earthed; further, that the tube box should be readily movable, easily controlled, and fitted with an easily adjustable diaphragm. Such were the thoughts in my mind, but my views underwent some change when I had seen the director which my colleague, Dr. Ironside Bruce, had just designed. It was decided that the present was a favourable opportunity for the practical application of this director. Having regard to the fact that other military hospitals have already equipped, or propose to equip, similar theatres, the opinion of those present would be valuable on this subject. In this hospital up to the present time, Dr. Bruce's director—a most ingenious and theoretically almost perfect apparatus—has been in fairly constant use. Like many other ingenious devices, however, experience would appear to suggest that it has certain practical limitations. Apart from the fact that some-

what elaborate and costly apparatus is required, the practical limitations, in my opinion, are the following:—

Firstly, it does not do away with the necessity for the most careful localization of the foreign body beforehand.

Secondly, the patient has to be kept rigidly in position by means of straps and sandbags; consequently, if any change of the position of the patient is made by the surgeon, or at the request of the anæsthetist, the director ceases to be a true guide to the position of the foreign body.

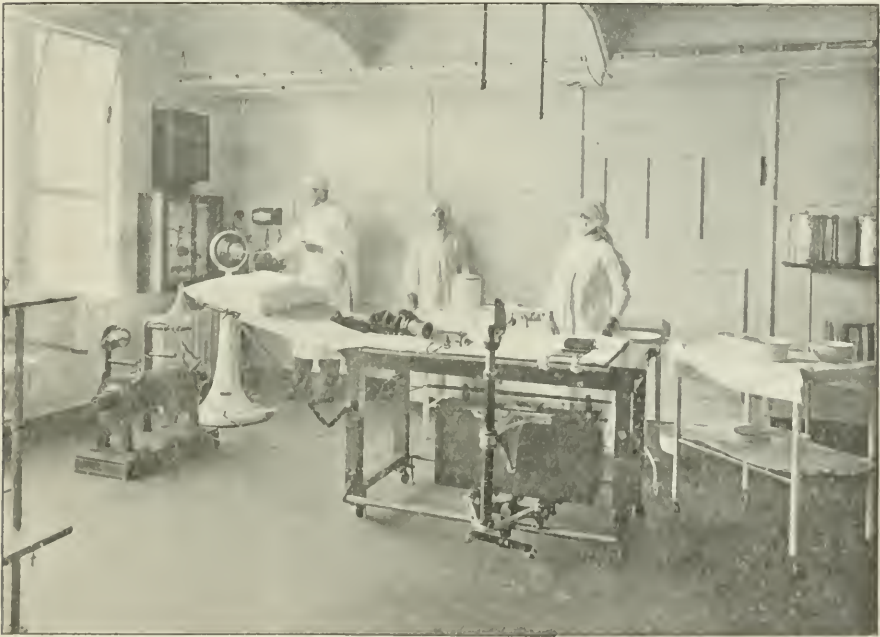


FIG. 1.

The illustration shows the X-ray operating theatre. The artificial lighting of the theatre is provided for by means of the almost continuous line of electric lamps placed at a convenient height on the walls, which give a soft but very efficient illumination. As is fitting in a operating theatre, the coil has been enclosed within an enamelled cabinet, but this has been removed, so as to show the arrangement of the coil, &c.

Thirdly, it would appear that in situations in which the surgeon cannot operate in the vertical line of the director, as, for example, in the perinæum, the value of the director is definitely restricted.



Such considerations suggest the addition to our armamentarium of the fluorescent screen designed by Dr. Harrison Orton. This small screen, which is about 4 in. square, is enclosed within a closely fitting frame of aluminium, and is covered by protective glass. The frame and its handle can be sterilized with the other instruments before operation. This screen, together with his ingenious localizing contrivance, will be shown to-night by Dr. Harrison Orton. The latter instrument, which is intended for use under conditions in which X-ray illumination is not available, is in the form of a metal cross with fenestrated arms, and is provided with a central opening through which a director can be passed. The arms of the cross, at the time of the operation, are applied to marks previously made on the skin at the time of localization of the foreign body.

The other interesting piece of apparatus, to which I would draw your attention, is the Bergonié vibrator for the detection of and guide at the time of operation for removal of magnetic foreign bodies in the tissues. Thanks to the kindness of Dr. Ettie Sayer, this apparatus has been lent to this hospital, and will shortly be placed in one of the operating theatres. This apparatus, of which much use has been made in France, gives an unmistakable sensation of vibration to the examining finger on approaching the foreign body. There is some difference in the periodicity of the alternating current supply in this hospital as compared with that in France, and this apparatus still requires some adjustment before it can be used.

With regard to the method of work adopted in this hospital, I do not think it differs materially from that done elsewhere. There would appear to be no question that the value of stereoscopic radiography is becoming more and more evident, and the demands for this method of X-ray examination are increasing. From the personal standpoint, I have ever been a strong believer in the stereoscopic method. Providing a known surface mark is made—by means of a small brass ring for instance—and the radiogram is carefully studied, it is very rarely that the anatomical relations of a foreign body cannot be made out with sufficient accuracy to determine the advisability of operating. At the time of operation, the surgeon is able to visualize the foreign body, and for this purpose we have placed a stereoscopic viewing box in one of the theatres, and others are shortly to be obtained.

Connected with this department are the ordinary photographic arrangements, which are in the skilled hands of Dr. Albert Norman,



with the assistance of Mr. H. F. Cheese, who is also in charge of the photographic side of the radiographic department, and to whose energy the department owes a debt of gratitude.

From the first this department has worked most smoothly. We have an excellent Sister in charge, whose experience in a similar capacity at Great Ormond Street Children's Hospital is invaluable. Under her are six most energetic ladies from the Voluntary Aid

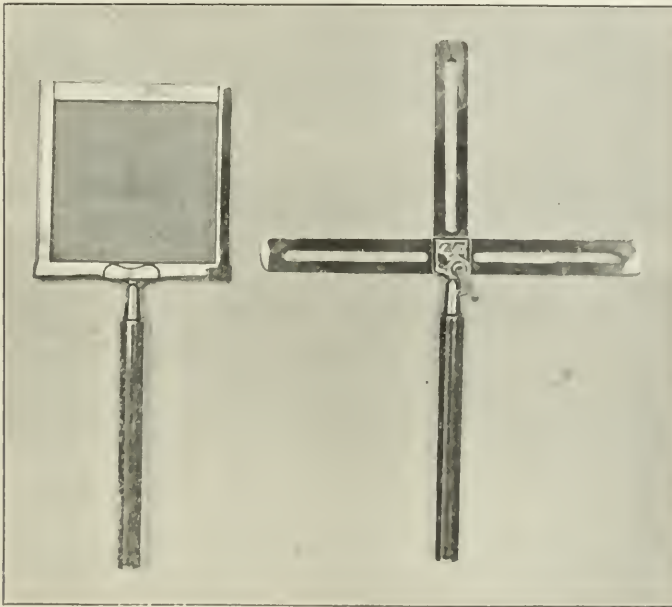


FIG. 2.

The illustration shows the small, sterilizable, fluorescent screen and sterilizable director designed by Dr. Harrison Orton.

Detachment, all of whom are becoming well skilled in the duties required of them; our only difficulty is to make them appreciate that some relaxation is essential both for the mind and the body.

Dr. HARRISON ORTON exhibited the sterilizable fluorescent screen and the localizing director which he had designed.

Dr. ETTIE SAYER exhibited the vibreur of Bergonié for localizing foreign bodies in the tissues and demonstrated its use.

The PRESIDENT demonstrated the use of his X-ray director in the operating theatre.

## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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(January 21, 1916.)

### A New Commutator Attachment for Rectifying the Current supplied to the X-ray Tube.

By Sir JAMES MACKENZIE DAVIDSON, M.B., C.M.

Mr. PRESIDENT, LADIES, AND GENTLEMEN,—

I have the honour this evening to bring to your notice a device which I have constructed for the improvement, as I hope, of the working of X-ray tubes. Now that it is worked out, the device is so extremely simple that it seems inexplicable to me that it should not have been done before. In early X-ray work the currents which were used to excite the tube were comparatively so weak that the disturbance and difficulty caused by the inverse current, or make current, did not arise. When strong currents came to be used, however, this make current, as all those present will be aware, became a source of trouble and anxiety to X-ray workers. Valve tubes are largely used, and in some hospitals, I believe, as many as four tubes, two at each terminal, are employed. These tubes themselves, however, are subject to disturbances, and at best are only makeshifts.

Lately, having to do a good deal of work among the wounded, I have been much troubled, when trying to get rapid exposures, by my tubes readily reversing. I am aware, of course, of the rectifying arrangement in the Snook apparatus, which makes an alternating current unidirectional through the tube, but this apparatus I do not possess. Therefore I set about some way of trying to overcome the inverse

current from the induction coil. At first it occurred to me that it might be advisable to try to prevent the production of the inverse current rather than adopt Dr. Morton's or Mr. Miller's arrangement for stopping it after its production. With this object in view I got Messrs. Watson and Sons, Ltd., to attach to a spindle of my little rotary dipper break a series of studs connected with resistances, and so arranged in relation to the position of the blade that when the blade was just dipping into the mercury to close the circuit in the primary, the finger attachment pressing on one of its sides, introduced into the primary circuit a resistance of from 40 to 60 ohms. As it rotated it came on to other studs, and gradually cut out the resistance, until, just before the break, there was no resistance interposing at all. As the output in the secondary is a function of the rate of change of the primary, this gradual building up of the saturation of the primary through these resistances resulted in no inverse current being produced; and an X-ray tube which would readily allow inverse current to pass if such current existed showed no signs of it at all when worked with this arrangement.

This led me immediately to consider that it would be still better if I could utilize this make current by passing it in its right direction through the tube; and as a preliminary to the construction of such a commutator I did several experiments with my break (which I shall briefly describe) in order to determine whether there was any appreciable lag between the moment of make and the occurrence of the make current in the secondary. To one of my breaks which had the spindle prolonged I attached a disk of cardboard, which rotated with the break; I then marked this disk at a certain point corresponding with the position of the dipper blade. The break was coupled up to a 10-in. coil in the usual way, and two wires were brought from the secondary terminals of the coil and fixed on either side near the periphery of the disk. When the break was started, and the current sent through the coil, there was a series of sparks in the usual way from the secondary terminals, which punctured the cardboard as it rotated. In this way I got punctures for the make and break current, and ascertained their exact position. I found that the make and break were practically instantaneous, and, further, that with the blade I was using I could obtain an angle of separation of very nearly  $180^\circ$ . It then became only a question of commutating these currents in the most convenient manner. I may recall that in 1898 I had employed a commutator for the current from the induction coil for my stereoscopic fluoroscope. In that case I used a

vulcanite disk with a metal spoke or radius, and so diverted the currents which were to illuminate each tube alternately. The working model which I have here this evening, although quite rough, will, \*I think, demonstrate that commutation can be carried out very simply, and that when a proper instrument is finished—this being in course of construction—we shall have an apparatus which will enable the ordinary induction coil to compete favourably with the Snook apparatus, which hitherto has been the most powerful means of exciting the X-ray tube.

The method I have adopted is to attach to the prolonged spindle of my break an insulating rod, about 10 in. long, made of wood or vulcanite, and from each extremity of this insulating rod there projects a light, rigid metal rod or wire, about 8 in. or 10 in. in length. To each end of these fingers, which have insulated bearings, are attached the secondary terminals of the coil by rubbing contact. Further, these rotating terminals of the secondary coil—as they really become—are adjusted to be parallel to the dipper blade, so that if these fingers are pointing vertically downwards, then the dipper blade is also pointing vertically downwards and into the mercury; and as they rotate together they always maintain the same relative position. The next thing was to ascertain the exact position occupied by these fingers at the moment when the dipper blade was dipping into the mercury. This was easily done by putting a little milliamperemeter into the circuit with a dry cell, and turning round until the meter indicated the current. At this point two supports, curved so as to represent a part of a circle, of which the fingers formed the radius (but with a clear space between the fingers and the supports) were introduced. Strips of metal lined these supports on their inner face, and they were electrically connected to the terminal to which the wires from the tube were attached. As the break rotated the current through the milliamperemeter remained constant until the blade came out of the mercury. Just at that point the fingers are rotated round at a considerable angle to the other side, and here the concentric arcs of the circle with the metal lining were fixed, and to these the break discharge sparked. Insulated wire was carried from this point to the make take-offs in a diagonal direction. It will be observed that by this simple construction, when the interrupter is started and the current in the primary turned on, the tube is illuminated by the make or inverse current as well as by the break current of the coil; and if I now proceed to connect the two take-offs of the break current together by wire, and allow the wire to the tube to come only from the make current take-off, the tube will be found to be quite



well excited by the make current alone. This illustrates forcibly how hitherto we have not only wasted this useful current, but have found it a serious drawback to our work by going through the tube in the wrong direction. With this arrangement I find that when the make current alone is passed through a low-resistance tube a current of 2 ma. is obtained. When the wires from the coil are connected directly to the tube so that the commutator is entirely cut out, a little over 4 ma. is registered, but owing to the inverse current getting through the needle vibrates so as to make the reading a little indefinite, and the tube, as you will see, presents a remarkable appearance, this being purposely chosen as a low tube in order to afford the severest test.

Hitherto the make current has been a source of serious wastage, not to speak of damage to the tubes, but with this arrangement, when properly made, it may be hoped that the wastage will be stopped, and that whatever make current may exist in any given coil will, so far as it is effective, go through the tube in the right direction and increase its output. The different appearance presented by this low tube when worked in the ordinary way, illuminating very badly, and giving, of course, unsatisfactory photographic results, as compared with the appearance presented when the commutator is attached to the break, resulting in a unidirectional current, will be readily perceived.

I feel that perhaps I ought to apologize for showing such a primitive apparatus for carrying out this idea, but during the War it is almost impossible to get any metal work done in reasonable time, and this must be my excuse. Simple though it is, however, it serves to demonstrate the point. We are now engaged in constructing breaks which will enable 50 or 60 amp., or perhaps 100 amp., to be used on the primary. When this is accomplished, I should hope that X-ray workers will have a means of exciting their tubes which will enable them to do work with the coil which hitherto has been impossible.

#### DISCUSSION.

The PRESIDENT: You will agree, ladies and gentlemen, that this has been a most interesting demonstration. And it must be obvious to all who work with X-rays what an advantage it would be if we could get rid of valve tubes. Whatever we may be able to do as radiographers with the mercury breaks, at any rate, a machine of this kind will be of enormous value in therapeutic work. Nearly all of us use the mercury break for treatment purposes, and to get rid of valve tubes in this kind of work alone would be a very great advantage indeed. It is quite clear that Sir James Mackenzie Davidson's device actually

does produce a unidirectional current for the ordinary induction coil. I shall be pleased to hear any comments on the device.

Mr. CECIL R. C. LYSTER : I think we are very deeply indebted to Sir James Mackenzie Davidson again—for this is not the first time he has brought something new before us—for introducing this very simple and efficacious means of saving us from the use of valve tubes, at any rate for photographic purposes. The device appears to give us considerable latitude as to the output of rays from the tube. I would suggest that Sir James should try to make his sparking machine as nearly noiseless as possible.

Dr. F. HERNAMAN-JOHNSON : I would like to ask if it would be possible to run the whole commutator in a gas chamber, as that might possibly silence it and increase its efficiency. This, of course, may be impracticable ; I only throw out the suggestion.

Dr. G. B. BATTEN : I would like to say, as one interested in home-made mechanical apparatus, that we have again to thank Sir James Mackenzie Davidson for bringing to our notice something which he started in a very simple way, and has gradually rendered more and more efficient. One small point which occurs to me is, that unless the spindle is made very long, it may be a little awkward to put an interrupter sufficiently near the coil to allow the same apparatus to interrupt both the primary and the secondary. Most installations are fitted with the wires overhead ; we do not like to alter that arrangement, and it might be necessary for us to have our interrupters rather high up. But probably Sir James, with his usual ingenuity, will get over that difficulty.

Sir JAMES MACKENZIE DAVIDSON (in reply) : I have to thank you for your kind reception of my demonstration ; and in reply to Dr. Hernaman-Johnson's suggestion to include the whole commutator in a gas chamber, I scarcely think it would be easy to carry this out, nor can I see any advantage whatever which would accrue from doing so. The noise will not be very great when the instrument is properly made, and ought in any case to be much less than that of a Snook machine. We have anticipated the precaution of observing sufficient distances in the machine we are now constructing and experimenting with. A large amount of hot ionized air is produced by the sparking to the "take-offs," which, when diffused, forms easy paths for "stray sparking." I should like to point out finally that should it be found that the "make current," from its lower voltage, heats the tube without a corresponding gain in increased X-ray output, it is quite easy to join the "make take-off" with a wire, and, cutting the crossed commutating wires, join them directly to the X-ray tube. In this way *only* the usual strong break current from the coil would be used to excite the X-ray tube.

(January 21, 1916.)

## The Use of the Simpson Light, with a Description of the Apparatus.

By E. P. CUMBERBATCH, M.B.

THE apparatus for the production of the Simpson light has already been shown at the November meeting (1915) of the Section of Laryngology. At that meeting Mr. Harmer read the notes of some cases that had been treated by the Simpson light, and I gave some particulars of the physical properties of the light, with notes of cases. During the past few months the Simpson light has been more fully investigated, and it was thought advisable that it should be more fully considered at the present meeting, especially as unscientific accounts of its properties have been appearing in the Press and erroneous information thus disseminated among the public.

### ORIGIN OF THE SIMPSON LIGHT.

Mr. Simpson was making a research (March, 1913) on the affinity of the rare metals for each other, and he noted that the light produced by the combustion of their ores in an electric arc had "curative effects on the workmen's hands." The preparation, from these ores, of electrodes suitable for combustion in an arc lamp proved to be a difficult and lengthy task, and Mr. Moore, M.I.M.E., who has been engaged in a research on this matter for more than two years, has succeeded in preparing those which are now in use.

### THE APPARATUS FOR PRODUCTION OF THE SIMPSON LIGHT.

The apparatus consists of an electric arc lamp and a resistance board. The latter is for the purpose of lowering the voltage and amperage of the current from the main when this is used for the lamp. A voltage of 40 to 50 and an amperage of 5 to 7 are required. The lamp consists of a pair of electrodes, a reflector, and a stand. The electrodes are the special feature of the lamp. They are made of certain ores, mainly *wolfram*, which is a tungstate of iron and manganese. They are fixed

in a horizontal position in suitable holders, which can be moved by means of a handcrew, so that the points of the electrodes can be brought into contact or separated. When the points are brought into contact and a current of the strength above-mentioned is passed along them, slight separation of the points will produce a brilliant arc at the intervening gap. A plain sheet of metal is placed behind the electrodes, or else a parabolic mirror. By the mirror the reflected rays are brought to a focus. Two forms of mirror are now made for use with the lamp. One is of metal covered with quartz. The object of the quartz is to absorb some of the heat rays. If this were not done, too intense a heat would be concentrated at the point where the light is focused. The other mirror is made of uncovered metal, and its curvature is such that the reflected rays are brought to a blunt focus at the situation in which the patient is placed. The stand is made of metal, and it can be raised or lowered or rotated so that the light can be directed where desired.

The arc is not steady like that formed between carbon electrodes, and it flares and splutters, and small incandescent particles are thrown off from the region of the arc. At the same time a white vapour or smoke rises into the air.

Treatment is applied by directing the light on to the part requiring it—either the unfocused rays (this is called the “open-air” method), or the rays brought to a blunt focus (the “medium-focus” method) or to a small focus (the “full-focus” method). The white vapour formed during the combustion of the electrodes can be inhaled into the respiratory passages, or it can be collected and suspended in water. This aqueous suspension is said to possess therapeutic value in some cases of colitis.

#### NATURE OF THE SIMPSON LIGHT.

The light produced by the Simpson lamp is composed of rays of three kinds: (1) heat rays; (2) visible light rays; and (3) ultra-violet rays. Arc lights are known to be sources of ultra-violet rays, but the arc of the Simpson lamp is a much more powerful source of these rays than the arc of the Finsen lamp, in which the electrodes are made of gas carbon. Professor Horton verified this by comparative measurements. He also showed that the Simpson light was richer in ultra-violet rays than the light produced by arcs in which the electrodes are made of aluminium, zinc, or cadmium. Further, the rays of the Simpson light range nearly twice as far into the ultra-violet region of



the spectrum as do those of the Finsen lamp. This means that there are rays of shorter wave-length in the ultra-violet light emitted by the Simpson lamp. We have no evidence of the existence of any new and undiscovered ray. Such therapeutic effects as the Simpson light is capable of producing more quickly than other sources of ultra-violet light are to be attributed to its greater richness in ultra-violet rays, while such effects as can be proved to be peculiar to the Simpson light are doubtless due to the presence of ultra-violet rays of shorter wave-length.

#### PHYSIOLOGICAL PROPERTIES OF THE SIMPSON LIGHT.

A sufficiently long exposure, the duration of which varies in different individuals, produces an erythema of the skin. The erythema is usually noted five or six hours after the exposure. It does not last long and generally it disappears after a day. Two minutes' exposure (open arc, 12 in. away) may produce this erythema. A longer exposure will produce a longer-lasting and more intense erythema, and "peeling" of the skin may then be the result. No bad effects have been noted on the patients' skin as the result of exposure to the light. Sometimes the patient complains of tingling and itching of the skin, and it may last one or two days. Pigmentation of the skin is sometimes left after a number of exposures. The light will produce a painful conjunctivitis if the eyes are exposed to it. The patient's eyes should be blindfolded and the operator should wear blue lead-glass spectacles.

The rays do not appear to pass through objects opaque to visible light, and Dr. Russ's experiments seem to show that it penetrates the tissues scarcely at all.

#### THERAPEUTIC PROPERTIES OF THE SIMPSON LIGHT. .

It is difficult, at the present stage, to make an inclusive statement of the morbid conditions for which the Simpson light is likely to benefit. It appears to clean and stimulate the healing of septic wounds. It has some germicidal action as shown by some experiments carried out by Dr. R. W. Allen. It has produced good results in some cases of lupus. When disease affects the deeper parts it usually, though not always, fails to benefit. Some cases of rheumatoid arthritis have been treated, most of these without result, though in a few considerable relief of pain was produced, particularly when the disease affected the finger-joints. Malignant growths have been exposed



to the light, but without effect, except occasional relief of pain. Better results have been obtained in two cases of rodent ulcer (*see* below). One case of tuberculous glands (without sinuses) in the neck benefited greatly; the swelling disappeared entirely from one side and nearly so from the other. The effect of inhalation of the vapour has been tried in one case of asthma and in three cases of nasal catarrh with sinusitis. The case of asthma materially benefited; the others did not improve.

Such is the experience of the Simpson light as obtained at St. Bartholomew's Hospital, where the lamp has been in use since July, 1915. Further particulars of some of these cases will be found below. It is hoped that others who have used the light will give their experience.

In conclusion, it may be said that the therapeutic properties of the Simpson light are due mainly to its ultra-violet rays. Such effects as it can produce *more quickly* than other sources of ultra-violet rays are to be attributed to its greater richness in these rays. Effects that can be shown to be *peculiar to* the Simpson light are to be attributed to the presence in it of ultra-violet rays of shorter wave-length. The greater intensity of the ultra-violet light of the Simpson lamp, and the consequent shortness of the exposure, and the possibility of therapeutic action by inhalation of the vapour, entitle this light to a claim for further investigation. Other sources of ultra-violet light should be compared with the Simpson lamp. Therapeutic results in given cases should not be attributed to the Simpson lamp exclusively till those other sources have been tried in similar cases, and other methods of treatment shown to be less effective.

The following are brief notes of a few cases treated by the "Simpson light," with good results:—

*Rodent Ulcer.*—Two cases were treated; one was very advanced, the greater part of the side of the face and upper jaw had been destroyed by the disease. Many forms of treatment had been tried. Forty-four applications of "Simpson light" were made during a period of three months. Great benefit was derived from them. Suppuration (which had been profuse) ceased, and in many parts the cavity had become lined with healthy epithelium. In the second case the patient had an ulcer on the lower eyelid; it had reappeared after previous healing under X-ray treatment. The ulcer healed again after six applications of "Simpson light."

*Lupus.*—Five cases have been treated. Two of these, in which the vestibules of the nasal cavities were involved, and a third in which an area

of skin, 2 in. by 2 in., on the leg was involved, were healed. In the fourth, the disease was very extensive, the floor of the mouth, tongue, gums, pharynx, larynx and epiglottis being extensively ulcerated. After thirty-three applications the floor of the mouth healed. The larynx was not improved by the treatment, but other parts were benefited by it.

*Asthma*.—One case of this disease, accompanied by nasal catarrh and polypi, was greatly benefited. The patient had suffered for five years and the disease had steadily progressed. After forty-four inhalations of the vapour (during four months) he improved greatly and was able to do full work. For six weeks he was practically free from asthma.

*Eczeṃa*.—One case, in which the hands and fingers were involved, was apparently cured after sixteen applications given over a period of twenty days. The rash disappeared, also the intense pain and itching.

For further particulars of these cases and others, see *Lancet*, January 8, 1916, p. 76.

(January 21, 1916.)

## The Physical Properties of the Simpson Light.

By SIDNEY RUSS, D.Sc.

DR. CUMBERBATCH brought to my notice a month or two ago the new electrodes with which he was working at St. Bartholomew's Hospital, and he indicated the necessity of some further observations on the physical nature of this light being undertaken. Since I had the facilities for doing so I have carried out a few investigations purely from the physical point of view.

I think that for our present purposes it would be well for us to realize what part of the spectrum we are working in; then it will clear away a number of false impressions to start with. If we go from the visible region of the spectrum to the limit of waves on the small scale, we have about seventeen octaves to work over. If we start from the visible region, which covers an octave, the first octave beyond that, we may say, is the region of ultra-violet light, with which most people are familiar. If we go into the next region it is very unfamiliar ground, and it is probable that the ground will remain unfamiliar for medical purposes because the fact is that such radiation has very little penetrating power, and any investigation upon it has to be made in a vacuum. If we start at the first octave, which constitutes visible radiation, the region in which we are interested this evening is the octave of wave-lengths, about 4,000 to 2,000 Angström units. There is a very big gap, marked on the slide "unmapped region," which constitutes nearly six and half octaves, before we come to the commencement of the very softest, that is to say, the least penetrating, X-rays that have so far been measured. So much for the region in which we are interested this evening.

The next illustration will show you the ultra-violet radiation from various types of arcs. The top one is the ultra-violet radiation from the ordinary mercury lamp. These photographs were taken by means of a quartz spectrograph, and the plates used were specially sensitive in the ultra-violet; they are not sensitive to red and yellow radiation at all. What strikes one is that the mercury radiation is split up into wave-lengths of a large variety. The next is an arc constituted of silver, and you see it has its own characteristic properties. The radiation falls into

well-marked bundles of light. The third is the radiation between two copper electrodes, and it is a comparatively rich source of ultra-violet rays. The rays go to the limits of the instrument—i.e., about 2,000 Angström units. The next is the radiation from two electrodes which Dr. Cumberbatch kindly gave me; these are the Simpson electrodes; the radiation from them is quite intense in this ultra-violet region. I will not at the moment dwell on the shortness of its waves, but I will do so presently. The last on the diagram is the carbon arc, and you will see that the ultra-violet radiation is a fairly intense source, that it gradually fades off in intensity, and falls into definite groups of lines. Those are not so clearly brought out here as in the succeeding photographs.

I now pass on to the question concerning the degree to which such ultra-violet radiation can penetrate human tissues. For this purpose I thought it best to use human skin. This was obtained and was cut into various thicknesses; it was then interposed between the source of light and the slit of the spectrograph. The next diagram shows you what happens when you try to pass these very short waves through human skin. No. 1 is a replica of the unscreened arc which I have just shown you, and the photograph was obtained with an exposure of five seconds. One-tenth of a millimetre of human skin was interposed in front of the slit, and again photographs were taken. I soon found that not very much radiation came through. In the next the exposure was increased from five seconds to two minutes, with the result that rays of wave-length shorter than about 3,500 Angström units are cut out by this thickness of skin. Another specimen was taken of  $\frac{1}{2}$  mm. thickness and interposed in the same way, and a photograph was obtained with an exposure of eight minutes instead of five seconds. Even so, you see the quantity is still further diminished, it does not extend to the same distance, and the intensity is not so great. A thickness of  $1\frac{1}{2}$  mm. was interposed and some radiation got through, but of course this radiation is much nearer the region of visible radiation. So much for the penetrating power of the very short wave-lengths through the human skin.

The next question which presented itself to me was, What are the essential constituents of this arc? Since tungsten was known to be a prominent constituent, I obtained some pure tungsten and compared the spectrum with the spectrum obtained with the Simpson electrodes. The purity of the tungsten used was practically assured, because it was the same material as is used for the metal filaments for lamps. Photographs were obtained from Simpson electrodes, and on the same plate

the spectrum obtained with pure tungsten. When one first looks at these two spectra, one says—and I think with justice—that they are apparently identical. We are not likely to commit ourselves to say more than that they are *apparently* identical—we should want to make much more elaborate measurements to see whether the intensities of these various lines correspond. The next slide will show you to what extent they are similar. These were obtained under different conditions of exposure: the bottom one is of pure tungsten with five seconds' exposure; the middle one is the Simpson electrode with five seconds' exposure; the top one is pure tungsten with ten seconds' exposure. You will note the gradual fading off of one into the other; and yet, if you look carefully, you see that there are some differences. I cannot say any more about these differences for the reasons I have stated.

There is only one other physical property of the radiation with which I have as yet concerned myself, and that is its relative intensity compared with that from the carbon arc. It is very difficult indeed to give any answer to the inquiry as to how many times as intense the radiation from the Simpson arc is as compared with that from the carbon arc; in fact, I think that no good would accrue from any statement of the kind being given, because we are not necessarily working over the same region of wave-lengths. The next two diagrams will show you the nature of the difficulty. The first is a spectrograph of the Simpson arc, and I subsequently used pure tungsten because I found the arc was then much steadier. The second is the spectrum of carbon with a very short exposure, which is in order to get some idea of the relative intensities in the more violet end of the spectrum. The exposures in each case are identical; they are graded from three to nine to twelve seconds, and similar numbers apply to the carbon arc. You can see there is more ultra-violet energy from the tungsten in this region—i.e., the longer wave-lengths—than from the carbon, and if we over-expose in this region and make our comparison in the region of shorter wave-lengths, the next photograph will show you that there is still more energy in this region from the Simpson arc than there is from the carbon arc. I am afraid I cannot go beyond that statement, and I do not think any purpose would be served if I were to make the attempt.



## DISCUSSION.

The PRESIDENT: We are all very much indebted to Dr. Cumberbatch and Dr. Russ for bringing this matter before us; and if there are any here who would like to make remarks with regard to the Simpson light we shall be glad to hear them.

Dr. J. H. SEQUEIRA: I have been asked by your Secretary, Dr. Gilbert Scott, to take part in the discussion. For sixteen years I have devoted special attention to the therapeutic use of various forms of radiation, and I trust I may be permitted to put before the Section a few facts which appear to have been ignored by the exponents of the Simpson lamp. The proper understanding of the use of concentrated actinic light in the treatment of disease is due to the late Professor Finsen. By a series of simple experiments he demonstrated that the penetration of living tissues could only be obtained by rendering them anæmic, and an essential part of the treatment to which his name is attached consists in compressing the parts subjected to light. It is the ignoring of this fact which demands close examination of the contentions urged by the exponents of the Simpson lamp. I am convinced by a number of observations already made with the apparatus now on trial at the London Hospital that the light produced by the Simpson lamp is very rich in actinic rays. Quite early in the development of phototherapy attempts were made to devise lamps which gave more actinic radiations than the carbon arc. A successful apparatus was that invented by Mr. Leslie Miller, in which an arc was produced between iron electrodes. The light was highly actinic, and it was used with plates of ice to compress and render anæmic the area under treatment. Carbon poles with iron cores were also used in my department, and these gave highly actinic rays. There were, however, technical difficulties which led to the abandonment of the apparatus. The most valuable advance hitherto has been the mercury-vapour lamp of Kromayer. The actinic value of the radiations produced by incandescent mercury vapour are too well known to be insisted on here. In the Kromayer apparatus the necessity of rendering the parts anæmic is recognized, and its therapeutic value in a number of cutaneous affections is undoubted. It has been used in my clinic for years with great success, but it has not ousted the Finsen lamp in the treatment of most forms of lupus. That radiations of high actinic value have a destructive influence on bacteria and superficial bacterial affections is well known. Application of this fact was recently made on a large scale in the sterilization of water for the large camp at Niagara Falls. It would therefore be remarkable if a light rich in actinic rays failed to affect beneficially certain superficial bacterial affections. It is claimed, however, that the Simpson lamp produces a radiation of great penetrative power, and it has been described—I presume with the authority of its exponents—in the *Times* of Monday, January 10, 1916, as "A New X-ray." I venture to say emphatically that the penetrative power of the light produced in the Simpson lamp is negligible, and that the radiations have nothing in

common with the X-rays. I make this statement as a result of the following experiments:—

(1) An irregular hole was cut in a sheet of paper, and the paper was laid on a piece of printing-out paper (P.O.P.) and exposed to the Simpson arc for three minutes at standard distance. The P.O.P. was unaffected—i.e., the rays will not pass through paper.

(2) Three "dental films," used for the examination of dental cases by X-rays, and consisting of a sensitized film wrapped in paper, were exposed, one half of each film being protected by a sheet of lead: (a) For half a second to an X-ray tube at 12 in. The half uncovered by lead was found on develop-

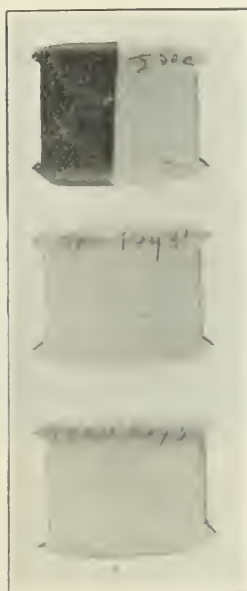


FIG. 1.

Dental films exposed to Simpson light. Three films were wrapped in paper, half of each being covered by sheet lead 1 mm. thick. (1) The upper film was exposed to an X-ray tube for half a second. The part of film uncovered by lead is black. (2) The middle film was exposed to Simpson arc for three minutes' open ray. (3) The lower film for three minutes' focal ray. The paper is in both cases as opaque to the rays as the lead.

ment to be black. (b) For three minutes to the open rays of the Simpson lamp at 12 in. On being developed the film was found unaffected. (c) For three minutes to the focal rays of the Simpson lamp at 12 in. On being developed the film was found unaffected.

These experiments prove that the rays from the Simpson lamp cannot influence a sensitized film through paper, and that they are in no way comparable with the X-rays.

(3) On examination of the arc produced by the Simpson lamp with Benoist's radio-chromometer there is no fluorescence. The radiations are not, therefore, X-rays.

(4) I cut out (i) a piece of aluminium 0.25 mm. thick, (ii) a piece of paper, (iii) a single layer of gutta-percha tissue, in the shape of my initials, and fastened them on my forearm with cotton wound round the limb. This was exposed for three minutes at 12 in. from the lamp at 3.30 one afternoon, and the aluminium, paper, and tissue were removed. By 6.30 p.m. an acute



FIG. 2.

Photograph of the writer's forearm. On it had been fastened by cotton thread a piece of human skin just removed from a spina bifida in shape of letter J, a piece of paper shape of H, and a piece of fresh frog's skin shape of S. The limb was then exposed to the Simpson lamp for three minutes at a distance of 12 in. The photograph was taken thirty-six hours later. An acute erythema was produced by the rays, but they could not pass through the pieces of human and frog's skin, nor the paper; even the cotton threads left transverse white lines well seen in the photograph.

erythema had appeared on the forearm, bearing on it my initials in white. The light had failed to penetrate any of the materials, and even the cotton threads left white lines on the skin. This proved that the rays given off excited an acute reaction but failed to penetrate aluminium, paper, a single layer of gutta-percha, and cotton thread. I have repeated the experiment, using (i) a piece of fresh human skin (taken a few minutes before from a spina bifida and kindly supplied by my colleague, Mr. Walton), (ii) a piece of fresh frog's skin, and (iii) a piece of paper. The result is identical. The rays from

the lamp failed to penetrate any of these tissues, while they set up an acute erythema around. Again, the cotton threads used to fasten the tissues left white lines. The reaction is identical with that seen on the neck of a lady who has worn a lace blouse while exposed to strong sun. The pattern of the lace stands out white on the "sunburnt" area.

(5) To exclude a heat-ray reaction, upon the forearm of one of the Sisters in my department were fastened (i) a square piece of glass, (ii) a circular disk of rock crystal, and the limb was exposed to the rays in the usual way. The glass entirely stopped the rays, but the rock crystal allowed them to pass and set up an erythema. Actinic rays alone have this property.

(6) Behind the lobule of the ear of a young patient I placed a piece of P.O.P. and exposed the ear to the Simpson lamp for three minutes. The

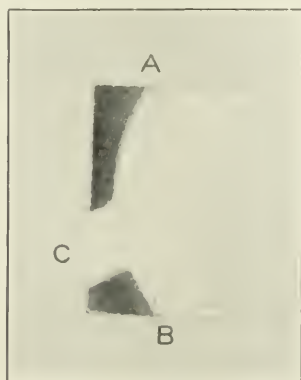


FIG. 3.

A piece of P.O.P. was strapped behind the lobule of the right ear of a boy and exposed to the Simpson arc for three minutes at a distance of 18 in. The line **A B** is the curve of the auricle, **C** the piece of strapping. The dark part is the projecting part of the paper. The rays failed to pass through the lobule of the ear or the strapping.

P.O.P. on being developed showed that the rays would not pass through the lobule of the ear nor through the piece of strapping which fastened the P.O.P. to the auricle.

I therefore conclude—(1) that the rays given off by the Simpson arc are actinic rays. There is no essential difference between them and actinic rays derived from other sources of light. (2) That, like other actinic rays, they have no penetrative power. They will not pass through fresh human skin, frog's skin, paper, cotton, or gutta-percha tissue. (3) That they are not X-rays and have nothing in common with X-rays. (4) That any deep effects which have been observed are due to counter-irritation, and are similar to those produced by the application of a mustard plaster or those seen from the use of light baths.



Dr. ALASTAIR MACGREGOR: Dr. Cumberbatch has gone over the subject of the results of treatment with the Simpson light at St. Bartholomew's Hospital so comprehensively that I, who have been working with him, have little to add. Both Dr. Cumberbatch and I started this treatment with open minds, though perhaps with a trace of scepticism, which is the correct attitude for a medical man investigating the results of a new treatment for which much is claimed. The first type of case which we had to treat by the light was that of rheumatoid arthritis, a condition which is not likely to inspire enthusiasm in regard to any treatment. One of the first cases (eczema) gave us hope, for we had a striking result. The patient was a man who had a history of eczema for nine months. He said his life was not worth living owing to the intense itching. The first four treatments seemed to have aggravated the condition, the irritation was greater, and there was a considerable reaction. The treatment was interrupted for three days and then resumed. After that the eczematous condition improved, and the eczema, along with the pruritus, which had been the bane of his existence, disappeared in twenty days. Since then there has been no relapse, so far as is known. There was another case which Major McAdam Eccles showed to the West London Medico-Chirurgical Society in November. It was a case of tuberculous disease of the elbow-joint with a three years' history. There had been four operations, and there were two discharging sinuses. One of the sinuses was healed on November 8, after thirteen applications, and has remained healed ever since; the other ceased discharging about January 6, after thirty-two applications, and is apparently healed. There is one case of exophthalmic goitre that attended at hospital this afternoon, and I give an account of it for what it is worth. The patient was treated from July to September with X-rays, but as she complained of increasing dizziness after each successive X-ray treatment this treatment was suspended. After an interval treatment by Simpson light was tried. The neck measurement, which was reduced from  $13\frac{1}{2}$  in. to  $12\frac{1}{4}$  in. under the X-ray treatment, is now further reduced to  $11\frac{7}{8}$  in. since the Simpson light treatment has been employed. The pulse, which was reduced under the X-rays from 140 to 120, is now as a rule 82 to 90. It has not exceeded 100 for a month past. The patient ascribes her marked improvement and general feeling of well-being to the Simpson light, but, of course, this improvement is open to more than one explanation. The Simpson light method of treatment is yet in its infancy, but I certainly think that in some cases it is a very useful application.

Dr. FINZI: I would like to ask Dr. Russ a couple of questions. The first is: Has he made a comparison as to intensity between the mercury vapour lamp and a window of quartz, which, it is understood, has been a very efficient source of ultra-violet rays? And the other question is this: Sir James Mackenzie Davidson showed us a rather overpowering little instrument which he described as a tungsten arc in nitrogen. If that could be put into a quartz lamp, would not that be likely to produce a more manageable arc than the Simpson arc?



Dr. BATHURST: It may be considered a small point, but I have noticed that both Dr. Cumberbatch and Dr. MacGregor referred to this treatment as the "Simpson rays." It has been shown by Dr. Russ, and emphasized by Dr. Sequeira, that there is nothing new in these rays, and we have to decide whether we shall dignify them by the name of "Simpson rays," instead of describing the treatment as that by Simpson light.

Mr. CECIL R. C. LYSTER: I was just about to speak on that very point. If there is any name that can be associated with treatment by actinic effect, it is that of Finsen; and these are only modifications of Finsen light, and we are using practically the same lengths as Finsen used. It is true the waves are derived from tungsten instead of from carbon, and some future worker may derive them from a more potent substance still; but there is nothing new about them. I was rather disappointed at Dr. Cumberbatch repeating the name "Simpson"; I think that name should have been dropped at once. We know they are ultra-violet rays; such they should be called, and I object to any name but Finsen's being associated with ultra-violet treatment.

Dr. RUSS (in reply): In answer to Dr. Finzi as to whether an examination of the mercury spectrum had been made with a quartz window, I say "Yes," and that the mercury lamp has been in clinical use in Mr. Lyster's department. I would touch upon the point which has just been mentioned, that is, what to call this radiation: the question is bound to crop up. We are not concerned with an essentially new part of the spectrum. To make assurance doubly sure, I wrote to the Secretary of the Royal Society on the question of scientific sanction for the adoption of a new name for this radiation. I received a letter from him to the effect that since this radiation occupied a well-known and well-recognized place in the spectrum, there was, of course, no question of it receiving any special name such as those that have been designated X-rays, Schumann rays, Gamma rays, and so forth. These have all been rays discovered in an essentially new region of the spectrum, and he made the suggestion, which has already been made here to-night, that the rays should be called by the name of the metal responsible for them. And if, as Mr. Lyster suggests, any new source of their derivation becomes known—and there are many yet untapped—they should be called after the substance which is responsible for their production.

Dr. CUMBERBATCH (in reply): I feel the meeting has fulfilled its purpose in clearing away some of the obscurity which has surrounded the Simpson light, which resembles that produced in an electric arc with tungsten electrodes. I stated in my paper that the rays appeared to exercise no penetrative power on opaque objects, and Dr. Russ has shown that it penetrates the tissues very slightly. Dr. Sequeira has demolished any idea one might have had about the presence of penetrative properties in the rays. I submit to Mr. Lyster's reproach for using the term "Simpson light," but I think I may be excused, because I did not use the expression "S-rays." Dr. Russ

has clearly shown that we ought to abolish the name "S-rays," because that implies a new discovery. Dr. Sequeira apparently saw an article which appeared in the *Times*, and which was headed "A New X-ray," but such a description does not occur in the original article which appeared in the *Lancet*.

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Sir JAMES MACKENZIE DAVIDSON exhibited a newly designed localizing couch.

## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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(February 18, 1916.)

### Preliminary Note on a New Method of Bullet Extraction.

By A. E. BARCLAY, M.D. (Captain; R.A.M.C., T.).

THE idea of removing foreign bodies by the direct guidance of X-rays is almost as old as the discovery of the X-rays themselves. As a house surgeon one resorted to the X-ray department with those cases of needles in the hand when one found difficulty, and doubtless many others had done the same. But although this procedure was useful, it was a difficult combination—the need for adequate light for operating and the essential accommodation of the retina for seeing the shadows in the dark being the most insuperable. Other troubles were the difficulty of maintaining asepsis in the dark, ignorance as to what one was picking up as well as the foreign body, and, perhaps more than anything else, the dangers of injury from the X-rays.

Partly from reading Captain Caldwell's<sup>1</sup> papers on the co-operation of the surgeon and radiographer in combined operations in the extraction of foreign bodies under X-rays, but more as the result of seeing operations and the scars which many of our soldiers carry in witness of the extensive operations, sometimes unfortunately ineffective, that have been undertaken to remove foreign bodies, it seemed to me that something more than the usual localizing of a bullet was demanded of the radiographer in a military hospital. That this demand has been recognized by others is evident from the multiplicity of instruments that have been devised by radiographers to assist surgeons in the removal of foreign bodies that have already been localized—e.g., the

<sup>1</sup> *Lancet*, 1915, i, pp. 1018, 1342.

telephone probe of Sir James Mackenzie Davidson, the electro-vibrator of Bergonié, our President's ingenious director, and various other devices. Not that I lost any confidence in the accuracy of my localization methods; however, quite a small alteration in the position of the patient or the limb might make a great difference by the time the surgeon had cut down to the required depth. Or the surgeon might not go down quite straight, as is only natural when retractors must be used. Or, as in the buttock, the skin mark that was made was capable of gross displacement by slight forces. These and other things suggested that some further guide was necessary—the operations seemed altogether too extensive for the extraction of a foreign body, provided absolutely reliable guides could be used. Accordingly, when I saw cases of foreign bodies deeply situated in fleshy parts I marked these cases as suitable for closer co-operation between the radiographer and the surgeon. Many of these were dealt with, resulting in considerable saving of time and cutting, by means of long Hagedorn needles (No. 1 size) pushed down through an iodined pad until the bullet was felt and seen to move on the screen.<sup>1</sup> One was at once struck with the great advantage of right-angled instruments (originally suggested for this work by Wullyamoz,<sup>2</sup> of Lausanne), the ordinary Hagedorn needle-holder being fairly satisfactory and enabling one to follow the course of the needle. It was sometimes surprisingly difficult, however, to keep straight and hit the target, especially at a depth of, say, 3 in.; little wonder, then, if a surgeon, working through retractors, wandered an inch or more from the line in such operations. But there were difficulties about this co-operation, for it was found that any movement on the part of the patient might displace the needle—on many occasions I was unable to feel the bullet with the needle after the patient had been moved to the theatre on a stretcher, even when, in some instances, under an anæsthetic.

There was also the trouble lest the foreign body, particularly shrapnel balls and pieces of lead, might be displaced by the operator without their being felt. In one such case the foreign body moved nearly 3 in., but was eventually quite easily felt with the assistance of a little pressure from the other side of the thigh.

<sup>1</sup> Originally suggested by Professor Roux.

<sup>2</sup> *Arch. of the Rönt. Ray*, April, 1912. In this article Dr. Wullyamoz described right-angled instruments, including scalpels, forceps, and hypodermic needles. The operations were open, done in part only by X-rays, and the operator had the fluorescent screen attached to his head—i.e., his face was directly exposed to the rays.

With this experience, I began to wonder whether an actual cutting operation was necessary for the removal of foreign bodies embedded in the flesh. Could not something be devised that would combine:—

(1) An instrument that would find its way down without cutting, i.e., apart from a primary skin incision;

(2) That would dilate a passage through which the foreign body could be removed;

(3) That would be sufficiently powerful to grip and extract the foreign body;

(4) That would allow a clear view of what was happening at the points;

(5) That would combine some electrical contrivance to replace the tactile sense—i.e., tell one that the instrument had laid hold of nothing except the foreign body—this being the most important feature;

(6) That would avoid damage in pulling out rough pieces;

(7) That would be sterilizable;

(8) That would afford protection to the operator?

Such an instrument would have to be made in the form of a forceps of some kind.

(1) Apart from a small skin incision, a blunt dissector would find its way down through muscle and fibrous tissue with comparatively little disturbance beyond the separation of the fibres. One of the blades of the forceps must therefore be a little longer than the other and be finished as a blunt dissector. In order that no fibres should slip in between the two blades, the shorter one must be hidden in the longer and fit right into it.

(2) The blades should be sufficiently strong to be opened out and used to dilate the channel made by the dissector point.

(3) To make the forceps powerful they must be very strong, and the ends should be so fashioned as to take a firm grip on round or rough pieces.

(4) To get a clear view, the prongs of the forceps must be at right angles to the handles in order that the hands might be out of the way. Also the prongs must be inclined to each other so that there would be a space of about  $1\frac{1}{2}$  in. between them at the top. A small fluorescent screen would be fitted above this gap on the forceps themselves.

(5) To replace tactile sense one of the prongs must be insulated,



while the other would be in direct connexion with the forceps. At the points they must be insulated from one another by a piece of fibre let into the spoon of the longer prong where the shorter one touched. Contact would be made, *not when the dissector touched*, but when the point had found its way down the side of the bullet, so that both prongs touched the foreign body. This would ring an electric bell. Then the blades would be opened—keeping the bell ringing all the time—until one saw on the screen that the bullet was enclosed and held by the forceps. If this could be carried out, nothing else could be picked up except the foreign body and it could be safely extracted.

(6) To avoid damage from rough and sharp fragments as they were extracted, it would be necessary to have some form of retractor. For this purpose another pair of forceps, of the mouth-gag type, would be made with a pair of blades whose ends would sheathe half round the prongs of the extracting forceps. When necessary these would be slipped down the prongs to the required depth, after the fragment had been gripped, and then expanded in the same way that an anæsthetist forces the mouth open.

(7) With the exception of the fluorescent screen, the whole instrument would be made of metal and would be sterilized. The electrical connexions would be covered with rubber and could also be sterilized. The fluorescent screen, which would not stand heat, would be encased in celluloid and could be kept in antiseptic solution.

(8) Before the operation commenced the ordinary localization would be carried out and a skin mark made. When this had been done the tube would be left centred exactly under the foreign body with the diaphragm closed down to a small square. Instead of gloves being worn, a sheet of X-ray-proof rubber with a hole about 3 in. square would be spread over or under the part, the hole being, of course, for the purpose of operating. There would be no need to have lead glass on the fluorescent screen as the operator would work from the side and would not have his face within the unprotected area.

So much I wrote before I had actual experience of the instrument; I was extremely hopeful, for the sake of the patients, that it would be successful. Not that one has any belief in the removal of every foreign body, but there are thousands of cases in which a surgeon does not feel justified in undertaking an operation for comparatively slight disability—a disability that may *or may not* wear off with time and which frequently makes all the difference to the patient. There are the

foreign bodies in the brain with which few surgeons care to interfere unless the condition is really urgent; the ghastly comminuted fractures with pieces of metal lodged among the fragments, and many other combinations of metal and human body, products of our twentieth century "civilization," with which every doctor in the land is now only too familiar.

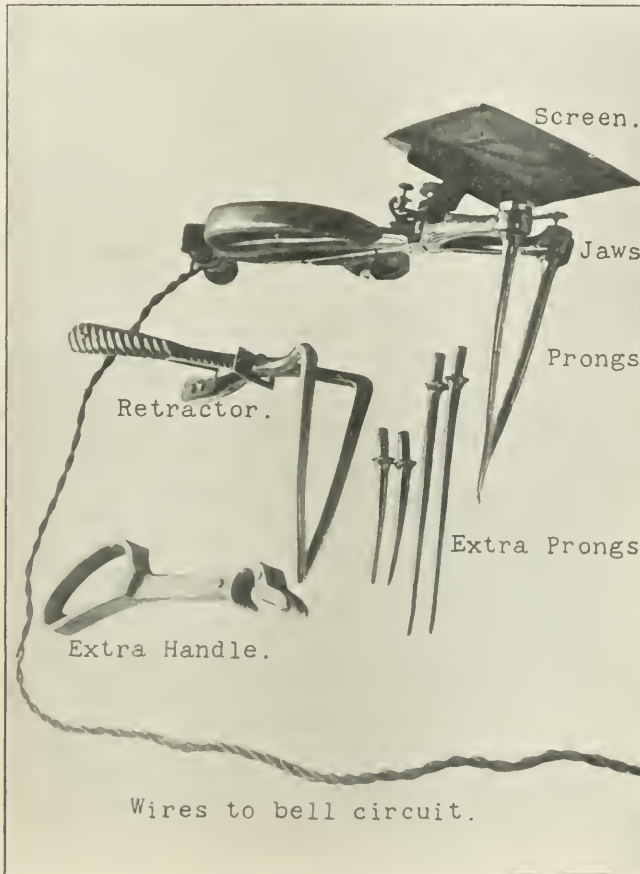


FIG. 1.

The bullet extractor. Four sets of prongs are made: (1) A long pair for foreign bodies seated deeply in muscles; (2) another long but much finer pair for brain work; (3) a short stout pair for working in parts where the fragment is embedded in fibrous tissue; (4) a short fine pair for extracting foreign bodies, such as needles. The retractor has so far been unnecessary. The extra handle fits on to the near jaw of the forceps, and is operated with the left hand.

Then, in civil practice—how many house surgeons can look back without misgivings as to the ultimate fate of certain needle cases, where, after long search, it may be, they found that elusive needle, but at a price, measured later on by destroyed function or possibly by the intervention of sepsis?

Naturally, in devising such a method, I expected to find that in practice there would be many difficulties, and in ordinary times I should have waited a long time before giving publicity to such an instrument as this. But it is already evident from the first cases that there is a very wide and immediate field of usefulness opened up. Imperfect as I feel sure the present instrument will be found, there is no time to be lost in giving others the opportunity of testing, and helping in the work of perfecting an instrument that will, I trust, play quite an appreciable part, not only in transforming many major into minor operations, but in rendering many a man once more fit for military service.

Naturally the earlier such an operation is undertaken the better, for there will be fewer resulting adhesions, and I hope that before the streams of wounded come in, this instrument will be in the hands of men who have already made themselves masters of its use. For, simple as it is in theory, it is by no means so easy in practice. Perhaps the best way of illustrating the difficulties will be by a brief description of my cases. This will also serve to explain the various modifications that practice has shown to be necessary.

*Case I.*—December 23, 1915: Piece of steel,  $\frac{3}{8}$  in. long, like a needle, in the thenar eminence of the hand. It had been missed at the operation the previous day. The wound was not exactly over the needle, and I could not work the instrument diagonally to the shadow of the needle. By rotating the hand the shadow was made to correspond to the wound. The prongs were again inserted and almost at once the bell rang and there was no difficulty in picking up the fragment and drawing it out. This extraction was performed with the forceps in its rough state and with large prongs.

*Case II.*—January 13, 1916: A minute fragment of steel,  $\frac{1}{4}$  in. long, like a needle, in the hand of a munition worker. It was so small that one took it for granted it must be superficial. It was not, as I found when the local anæsthesia failed. A general anæsthetic was given and in the meantime a localizing plate was taken which showed at once that the fragment was deep—in fact, it was well behind the middle of the metacarpals. The bell began to ring almost as soon as the instrument had burrowed down through the palmar fascia to the required depth, and the fragment was extracted fairly easily. This was the first trial with the fine prongs (fig. 2).

*Case III.*—January 16: Irregular piece of iron,  $\frac{3}{4}$  in. by  $\frac{1}{4}$  in., lying behind the femur, just below the great trochanter. In the sterilizer the insulating fibre button came out of the prong and one had to disconnect the bell till the bullet was felt. The bell was then put in circuit but was of value only when the prongs were separated. As soon as the fragment was touched it was seen to slip away about 2 in.—i.e., it was lying in a pocket of pus. It was not possible to make the shadow correspond to the wound of entry, and another incision was therefore made and the extraction was performed without difficulty.



FIG. 2 (Case 2).

Piece of steel in region of the interossei.

It seemed to be an easy matter to make the point of the prongs go down  $\frac{1}{2}$  in. at a time and then to open them out. The instrument was powerful enough for the work, but it was a strain on the hands trying to separate the points. To overcome this difficulty the handles were made like those of a pair of scissors, but this alteration was not completed till January 30.

*Case IV.*—January 16: Splatter of lead apparently touching the sciatic nerve behind the acetabulum. This case had been operated on, but the foreign body had not been found. It lay 4 in. higher up than the centre of the surgeon's incision; whether it had moved this distance before or during the operation I cannot say. As the long prongs had lost the fibre button a short pair had to be used in this case, and the difficulty was chiefly due to the wider angle between the prongs—i.e., nearly  $30^{\circ}$ —for although the blunt dissector penetrated the fat of the buttock, attempts to separate the prongs brought one of them out of the wound. The fat was divided with a knife till fascia was seen, and then there was no difficulty in getting down to the fragment. As soon as contact was made, however, the nerve was stimulated, and it was several minutes before the fragment was firmly gripped owing to the movements of the limb.

The imperfect contact with the foreign body caused a great deal of stimulation of the nerve—the electric bell circuit giving a current like that of the primary of a faradic coil, which one could easily feel through one's fingers. Probably there was also direct stimulation of the muscles by the prongs. The prongs were therefore painted with collodion, except at the points.

*Case V.*—January 20: Small jagged piece of shrapnel in the forearm, causing a persistent sinus—at least, this was the only fragment that was painful to the touch. It had been in four months, but there was no difficulty in getting to it or in extracting it under local anæsthesia. The operation took four minutes. From later experience I expect the fragment was lying in a pocket of pus and therefore came away quite easily.

*Case VI.*—January 20: A  $\frac{3}{4}$ -in. needle in the palm of the hand. It lay between the palmar fascia and the arch. The surgeon in an outlying town did not wish to touch it although it was giving pain. Local anæsthesia was quite efficient and the bell rang very quickly, but on six or seven occasions a strand of the fascia as well as the needle was gripped. When I realized what it was I turned the forceps to another position, and then succeeded in getting hold of the needle by itself. It took altogether seven minutes to extract.

*Case VII.*—January 23: Rifle bullet lying against the outer side of the femur just above the condyle. (Four months' standing aseptic case.) After the incision had been made the electric bell was found to be out of order, and when this was repaired the limb had moved and the first position could not be found again. It was apparently futile to attempt to work diagonally. Another incision was made and no difficulty was encountered in again splitting a way through the tendon of the tensor fasciæ femoris, and the bullet was easily pulled through the opening (fig. 3).



This case emphasizes the importance of fixing the part as rigidly as possible before making the incision. The forceps slipped off the nickel casing several times before they obtained a firm grip. The difficulty of obtaining a good hold was mainly due to the twitching of the muscles.



FIG. 3 (Case 7).

Bullet lying against the outer side of the femur above the condyle.

*Case VIII.*—January 23: Rifle bullet in thigh, lying against the femur at a depth of 2 in. (Seven months' standing aseptic case.) No serious difficulty encountered in this case except that the muscles twitched whenever contact was made and there was difficulty in getting hold of the bullet—probably

a nerve was in contact with the bullet or with the prongs. The current for the electric bell could not be reduced lower than  $2\frac{1}{2}$  volts—two dry cells.

*Case IX.*—January 23: A damaged rifle bullet lying about 1 in. deep in the thigh. No difficulty of any kind was encountered except that, after the bell had rung, the bullet tended to jump first to the one side and then to the other—i.e., the overlap of the longer prong was not sufficient. The electric shock made the muscle contract each time the bullet was touched.

*Case X.*—January 30: Very small fragment of shrapnel in the back. (Seven months' aseptic case.) It was quite superficial, cased in a sheath of fibrous tissue and freely movable. It had to be pulled out with its sheath. The bell did not ring; it happened to be out of order, but it is doubtful if it would have rung with so dense a fibrous sheath. The case was much more suitable for ordinary excision, and would not have been undertaken if one had looked at it beforehand and had not relied on the statement that it was a rifle bullet.

*Case XI.*—January 30: Rifle bullet in lumbar region,  $1\frac{3}{4}$  in. deep and lying on the lamina of the first lumbar vertebra. There was no difficulty in splitting the fasciæ and the bell rang quite quickly, but the bullet slipped first to one side of the prongs and then to the other, and it was difficult to catch it. It was evident that one point of the prongs ought to be considerably longer than the other. The extraction took rather less than fifteen minutes.

*Case XII.*—January 30: Rifle bullet in thigh, 2 in. deep. Previous operation not successful, and bullet about 2 in. away from the centre of the scar. Patient refused general anæsthetic, and the local anæsthetic was unsuccessful, even for the skin incision. In spite of his pluck the patient moved badly every time the bullet was touched, and the muscles were so powerfully contracted that it was a great labour to separate the blades, and one could not get a proper grip of the bullet. After a number of attempts the patient at last consented to have chloroform, and there was then practically no delay in the extraction. It turned out subsequently that this patient's objection to a general anæsthetic was that he was on furlough and did not want his time curtailed. Within half an hour he insisted on going home with the bullet in his pocket. The wound did not become septic.

It is useless to attempt operation unless the anæsthesia is perfect—a small movement upsets the alignment, and the muscular contraction makes the operation infinitely more difficult—it becomes a trial of strength between the patient's muscles and those of the operator. To overcome the difficulty of the electrical stimulation of the nerves and muscles a relay was introduced—the suggestion of Mr. Wild, one of the house physicians at the Manchester Royal Infirmary, where this work has been carried out. The current passing to the prongs is now

only from one dry cell, which is ample to pull over the relay that forms the contact for the bell circuit. Instead of the interrupted bell current there is now the single galvanic shock to the nerves and muscles from one cell, which will probably not cause any movement of the foreign body. There will possibly be no need to paint the prongs with collodion.

*Case XIII.*—February 8: Many shrapnel wounds in the lumbar region. Persistent sinus, and considerable pain—four months' duration. The fragments apparently responsible for the symptoms were those shown on the plate submitted as A, B, and C, and the depths were  $3\frac{1}{4}$  in.,  $2\frac{1}{2}$  in., and  $1\frac{1}{2}$  in. respectively. After consultation with Lieutenant-Colonel Southam, R.A.M.C., it was decided to attempt removal of fragments A and B, the small chain of shadows, C, not being sufficiently clearly seen on the screen. Under the anaesthetic the fragment A was observed to move, and was probably embedded in adhesions in very close relationship to the kidney, but no blood came from the kidney after the operation. § A: (1) Very considerable difficulty was encountered in this case from false contact of the bell; it rang quite definitely in response to contact, but it continued to ring—i.e., the leakage of current across the tissues kept the relay drawn over and the bell continued to ring, but in a hesitating manner. (2) It was not at all easy to see the shadow of the foreign body—it was a rather thin and rough fragment of shell—and one had to rely chiefly on the electric bell, and as this was unreliable it made the extraction much more difficult than it should have been. (3) Before incision, a needle was put down to see whether the passage would be clear of the transverse process, but when the prongs were expanded, the dense fascia about the bone tended to force the points below the lower pole of the foreign body, and considerable difficulty was encountered. When the cause of this was realized, the incision was enlarged upward, and another passage made as close as possible to the transverse process, and the foreign body was then picked up fairly easily. (4) The motor for the break seized up and had to be freed. (5) The X-ray tube showed signs of exhaustion and had to be changed. § B: This collection of fragments was removed through a separate incision. They were not at all easy to see, yet, although the bell was so unreliable, the three largest pieces were extracted with very little delay. The case with which they were taken out suggested that there was suppuration around them.

The whole operation took rather more than an hour, but, considering the difficulties encountered, this is not surprising—the maintainance of false contact through the tissues after true contact, coupled with the difficulty of seeing the foreign body and detecting the cause of the failure to lay hold of it, accounting for the greater part of the delay.

An adjustable resistance was put into the relay circuit, and tests for false contact carried out with normal saline.

*Case XIV.*—February 13: Fragment of shrapnel, which had previously been missed, lying on the anterior margin of the glenoid fossa. It had been embedded for ten months and had never been septic. Major Burgess, R.A.M.C., made the incision and the bell rang fairly soon, but there was no certainty in getting contact, and it was soon evident that the fragment was embedded in a dense case of fibrous tissue, from which it could not be separated. The depth was only  $1\frac{1}{2}$  in., and, after a trial with the extractor, the sheath was gripped with a Spencer Wells' forceps under X-rays, and, with considerable difficulty, the fragment was eventually felt and finally removed from the dense sheath, in which it lay embedded, by the ordinary method.

So far as one could judge, the electrical apparatus worked admirably, and there was neither shock nor false contact. The case was evidently not one suited for extraction, as the rough coral-like surface of the fragment was too firmly adherent for any possibility of separation from its casing, except by ordinary methods. The value to the surgeon of working under X-ray guidance was very evident in this case, for even when the capsule was fixed by the forceps it was almost impossible to feel the fragment in it.

All the "War" cases were old injuries, and it is reasonable to suppose that extractions would be easier when undertaken shortly after the injury, before fibrous tissue had formed.

There has been no bleeding worth speaking of in any of the cases, and all the aseptic ones have healed by first intention.

Naturally, there have been numerous alterations in the instrument, but these have all been of a minor nature—e.g., the gripping surfaces of the prongs, the overlap of the points, the fixing of the fibre insulation, and so on. The next instrument will be made considerably stronger in the joint; the jaws of the forceps will be stronger, and the prongs will be a little thicker at the upper ends. There is no object in keeping the instrument as light as possible, and there are definite advantages in greater strength—i.e., firmer grip and less likelihood of straining, and of the points of the prongs being forced into false contact in burrowing, especially in removing fragments from among new-formed callus and fibrous tissue. An extra handle for attachment to the far end of the jaw that is operated by the thumb has also been made, and this will greatly assist the opening of the jaws in parts where some force is required, as in the lumbar fascia.



## THE USE OF THE INSTRUMENT.

The method of using the instrument is simple. After fixing the patient, localizing and marking the skin accurately, and disposing extra protection for the operator, the skin is sterilized and the area surrounded by sterilized towels, &c. A tourniquet above and below is an advantage in steadying the muscles. An incision about 1 in. or  $1\frac{1}{2}$  in. long is made down to the fascia and exactly over the shadow, the prongs are inserted and the lights are turned off. With the occasional guidance of the Rays and frequent dilatation of the prongs, a way is made down to the *right* side of the bullet until the bell rings. Then the foreign body is freed all down this side—the bell ringing nearly all the time. (The freeing of the fragment would probably be unnecessary in recent injuries.) The prongs are then withdrawn about  $\frac{1}{2}$  in., and the other side of the foreign body is also freed as far as possible—the bell of course not ringing. Then, returning to the right side of the bullet and making the bell ring again, we select the proper part of the foreign body to grasp. The prongs are then opened, and the bell kept ringing as much as possible, until it is seen that the prongs have embraced the bullet. Until the relay was put into the circuit it was often difficult to pick up the bullet because of the jumping of the fragment caused by muscular contraction in response to the shock. The absence of shock and the increase of the overlap of the longer prong should make the picking up of the foreign body a much easier matter. In fact, it is likely that if they are not fixed by adhesions, bullets will naturally slip into the grip when the longer prong is used as a retractor while the forceps are being opened—i.e., the elasticity of the tissues will be of assistance.

So far, no fragments have been removed that have necessitated the use of the dilator, and, when once the bullet or fragment has been firmly gripped, it has been extracted without any difficulty—except in Case XII, when the absence of anæsthesia made the extraction through the fascia lata impossible.

It all sounds simple, and it is comparatively simple when the instrument is perfected, to anyone who is accustomed to X-ray work—but the radiographer is not, as a rule, a good surgeon, and, vice versa, there are few surgeons who are good radiographers. The radiographer, if he does this work, must act under the guidance of the surgeon, or he will make mistakes; he is likely to be at fault both in surgical anatomy and asepsis; if he is a true radiographer he will not be able to keep his



fingers off the adjustments and switches. And if the surgeon uses the extractor he will probably need much more illumination than a trained radiographer, and is likely to wreck all the X-ray tubes in the department, and may possibly even burn the patient. In the simpler cases, the assistance of a house surgeon in making the incision and closing the wound appears necessary, while the radiographer extracts the bullet ; in difficult cases, involving perhaps the exposure of, say, the subclavian artery or removal of a piece of rib or a portion of the skull, this would be done by the surgeon, while the radiographer adjusts the beam of Rays, obliquely if necessary, so that he can carry out the extraction from such position as is indicated by the surgeon as being free from danger.

#### LIMITATIONS.

What are the limitations of the method? On the radiographer's side, the fragment must be easily visible on the screen—the depth at which it lies makes little difference, but he must have free access to it : if necessary, portions of ribs will have to be excised, the skull trephined, or, with the use of the prongs as dilators, the long bones may possibly have to be trephined and bullets taken out of the medullary cavity or from masses of callus.

In Cases X and XIV there were fragments of iron, eaten into and with a surface like coral. They were so firmly encased and entangled that no extractor could possibly free them from their sheath. On the other hand, in Cases III, V, and XIII there were similar fragments, pitted and rusted in the same way, and yet they came away quite easily, for, in the septic cases the sheath does not enmesh the foreign body. Presumably, therefore, old-standing aseptic cases are not suitable for extraction, although old-standing septic cases can be dealt with by this method. But apparently this applies only to fragments of iron, as the nickel-cased bullets and lead fragments (Cases IV, VII, VIII, IX, XI, and XII) were freed and extracted without much difficulty, for nickel and lead are not eroded, and therefore do not form the same intimate adhesion as iron fragments. As yet there is no experience of recently embedded fragments of iron, except the bits of steel in the hand, but one would expect that, if extraction was done within a few weeks, there would not be sufficient fibrous tissue to interfere with it.

If one works in close concert with the surgeon there should be few limitations from the X-ray point of view in recent cases, but as to the limitations and disadvantages from the surgical side, I am not competent to speak.

The obvious advantage is that in most cases, even when the foreign body is deep-seated, its removal becomes a minor operation, and probably, with practice, a speedy one. It is a *closed* operation, and the damage to tissues is limited to a narrow track—a glance at the cases in the convalescent camps and Red Cross Hospitals will convince anyone of the importance of the scar after the removal of bullets or other operations. And not the least advantage is that when sepsis is active or latent, a track only is opened up, and surrounding tissues are not necessarily infected. In the brain it should be quite invaluable, but as yet no cases have been attempted; one would expect it to be much the same as the very simple matter of taking bullets out of a loaf of bread.

In conclusion, I wish to thank my colleagues of the Second Western General Hospital, and Major Mackenzie, R.A.M.C., and the staff of the Heaton Park Command Dépôt, for their interest and advice in this experimental work, and for kindly placing cases at my disposal. And I am indebted to many friends for suggestions, but especially to Mr. English, the local manager for the instrument makers, who has spared himself neither time nor labour in converting an idea into an instrument that will, I trust, play more than a small part in the saving of the wastage from this War.

#### DISCUSSION.

Dr. HARRISON ORTON: The principle of operating with the aid of the X-rays has been employed at St. Mary's Hospital for some years, and many foreign bodies have been extracted in this way by Mr. Maynard Smith. At the suggestion of this surgeon, an X-ray operating theatre has been provided at the King George Military Hospital and is in almost daily use. The procedure employed at St. Mary's Hospital is very similar to that described by Captain Barclay. Captain Barclay's forceps I consider extremely ingenious, and I have no doubt that they will save much time in certain cases. I congratulate him on the ingenuity shown in their construction. The provision of a special X-ray operating theatre does away with one of the objections mentioned by Captain Barclay, since the working of the main X-ray department is not interfered with. I think that all large military hospitals should be provided with such a theatre.

Dr. STANLEY MELVILLE: I wish to join in congratulating Captain Barclay on the ingenious instrument which he has just exhibited. It is difficult, even if not improper, at so early a stage to offer criticism, but the significance of the invention interests me greatly. Captain Barclay's "bullet extractor" has the honour of being the first combined surgical and X-ray appliance yet produced,

and most unmistakably bears upon itself the stamp of the radiographer. If for no other reason than this Captain Barclay well merits the highest tribute of praise. At the King George Hospital much good work has been done by means of Dr. Ironside Bruce's "director" and the small sterilizable fluorescent screen of Dr. Harrison Orton. Whether the surgeons at that hospital will avail themselves of the instrument under discussion in addition to the present armamentarium will be a matter for the future to decide. It seems a pity for some reasons that the method adopted in France is not more universal in this country. In France, the radiographer in a large number of cases himself proceeds to remove the foreign body, if localization shows it to be lying in the more superficial tissues, leaving the more difficult cases for the surgeon—namely, those in which the foreign body lies in difficult or dangerous positions. The time of surgeons seems to me to be often unduly taken up with trivial matters of the kind, and they are very much over-worked as it is. I hope to hear that Captain Barclay's instrument is doing all the good work that its inventor and the members of the Section wish for it.

The PRESIDENT: I congratulate Captain Barclay on his ingenious bullet extractor, which will undoubtedly be of great advantage in dealing with the extraction of projectile fragments from the tissues, at least in some situations. The difficulty which Captain Barclay has experienced with muscular contractions for stimulation by the bell circuit may perhaps be got over by using the Mackenzie Davidson telephone probe instead of a bell.

Dr. H. A. ECCLES: Can Captain Barclay quote a price for his "bullet extractor"? I think it will be a most valuable help not only in the large military hospitals supplied amply with funds, but also in the smaller ones where funds are not so abundant. It is not the privilege of all of us to be attached to the larger hospitals, and therefore I venture to inquire the cost.

Captain A. E. BARCLAY (in reply): I thank the members of the Section for their sympathetic reception of my experimental work with the instrument. In my opinion the most valuable point consists in the saving of extensive operative proceedings, even when fragments are deep-seated; it is a closed operation, and needs no more preparation than that which is necessary for a small superficial operation. The necessary degree of asepsis can readily be obtained, even in an X-ray department of the ordinary character, without any alteration whatever. I quite agree with Dr. Harrison Orton that the proper place for the removal of foreign bodies is the X-ray room, but I suspect that the surgeon is the proper person to do the operation. Perhaps, as Dr. Stanley Melville has suggested, the simpler cases will fall to the lot of the radiographer, while the difficult cases will be reserved for the surgeon. Certainly, in difficult cases where there is danger of injuring important vessels, a surgeon should be at hand, and, in the event of an accident, the case will at once be transferred to the operating theatre. Not that I anticipate trouble, but it must not be forgotten that the work is as yet experimental, and we do not yet know the

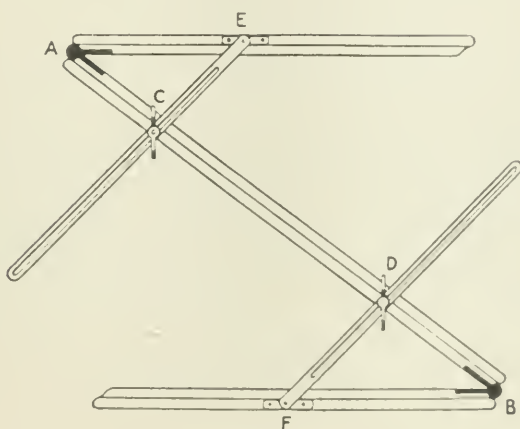
limits of safety. Dr. Eccles inquired as to the cost; the instrument shown is the first one made, and I do not know how much it has cost. Probably it will work out to between £10 or £12 complete. The makers are Messrs. Claudius Ash, Sons and Co., Ltd., of London, Manchester, &c.

(February 18, 1916.)

### An Adjustable Screen Support.

By N. S. FINZI, M.B.

As the method of avoiding any injury to the hands in doing X-ray work is to keep them out of the path of the Rays as much as possible, even when protective gloves are being worn, I always arrange my screen



An adjustable screen support.

so that it is supported over or on the part to be radiographed, and for this purpose, until about a year ago, I used various sizes of sandbags, blocks, and small cushions. As the arrangement of these was rather troublesome I designed the little instrument I am showing this evening as a screen support. The support consists of three boards hinged together at A and B in the form of a Z. On the top and bottom boards at E and F are pivoted slotted pieces of iron, and these are clamped to the middle board at C and D by large fly-nuts with spring washers. The tension is so arranged that the apparatus will support



the weight of the screen, and possibly a sandbag as well, without collapsing, and yet by a little extra pressure can easily be collapsed to the correct height needed for the part under examination. It is also easy to lower one end more than the other so as to get any oblique position of the screen required. The width of the boards is either 3 in. or 4 in., and they should not be made more than  $\frac{1}{2}$  in. thick; less than this is advisable in order to be able to get the apparatus flat enough to support the screen for the hand and wrist. The hinges must be wide and go right across the wood. The slotted pieces must be very rigid and strong. The apparatus has been found to save a great deal of time in practice.

(February 18, 1916.)

### Diathermic Fulguration.

By W. J. TURRELL, M.D.

THE following is a new technique for fulguration with the diathermy current. In this method of treatment the patient lies on an auto-condensation couch, holding two handles attached to one of the terminals of the diathermy apparatus; the other terminal is attached to the metal plate beneath the couch. A current of about 500 milliamperes is administered, and the operator approximates to the wart, or other tissue to be treated, any blunt-pointed metal instrument, such as a probe or artery forceps, without any grounding wire or attachment of any kind. When the instrument is about 2 cm. or 3 cm. from the growth an oscillatory discharge in the form of short, intensely hot sparks takes place between the tissues and the instrument held in the hand of the surgeon and through him to and from earth. The patient experiences a sharp stinging sensation; the pain is so slight, however, that it is possible to treat a port wine mark on the eyelids without the patient blinking or flinching. A very similar method has been described in the use of the high frequency current; but with the ordinary high frequency current the voltage is much in excess of the amount required, and the amperage is too low. With the higher voltage a longer spark is obtained, which renders the application less accurate and more painful.

This new technique possesses very definite advantages over the usual method of bipolar diathermic fulguration, in which the patient is



attached to one terminal of the apparatus and the surgeon applies the treatment with an instrument attached to the other terminal. With the bipolar method it is impossible to ascertain the amount of current which will pass during the treatment. With the use of the auto-condensation couch the patient is previously charged with the required amperage, and the amount is registered on the amperemeter in the usual way; the charge is also evenly distributed throughout the whole body of the patient, and may be drawn off from any point required. Finally, it is a great convenience to the operator to use an instrument which is free from all wires and attachments and is inactive until the moment it is brought to the requisite distance from the patient.

This form of diathermic fulguration is not advocated for the destruction of large growths, but it has been found extremely efficient in the treatment of warts, moles, acne spots, port wine marks, acne rosacea papules, xanthelasma palpebrarum, and many other similar conditions.

Dr. E. P. CUMBERBATCH: I have tried the method of applying the diathermic cautery spoken of by Dr. Turrell as "diathermic fulguration," but with a different arrangement and connexions of the apparatus. One cable from the diathermy apparatus was earthed, the other was attached to a pointed metal electrode. The electrode was directed close to the tissue that was to be destroyed so that a number of sparks passed on to it, producing superficial cautery. I tried the method two years ago on a number of cases of nævus. Good results were obtained if the nævus did not lie under the skin. Only superficial cauterization was produced.

(February 18, 1916.)

Dr. FLORENCE STONEY showed the following lantern slides; one from the Anglo-French Hospital, No. 2, at Cherbourg, and the others from cases at Fulham Military Hospital:—

(1) Sequestra in a humerus after comminuted fracture of the upper end of the shaft. The sequestra cast a denser shadow than living bone (the sequestra were confirmed by operation, being removed by an incision through the deltoid).

(2) A sequestrum in a fractured radius. There are two fragments: one throws a normal shadow and is living bone, the other throws a denser shadow, and was diagnosed as a sequestrum (confirmed by operation).

(3) Myositis ossificans of the ligamentum patellæ. It caused very little inconvenience.

(4) Osteo-chondroma of the shaft of the femur (confirmed by operation and pathological report).

(5) Shell in the occipital lobe removed by Captain Lee, R.A.M.C., with the help of the X-ray screen.

(6) Broken rifle ball in the axilla ; suppuration had continued for a year after amputation at the shoulder-joint. The wound healed almost directly after the ball was extracted by Major Parsons, R.A.M.C.

(February 18, 1916.)

### **An Apparatus to assist the Examination of Children by the X-rays.**

By STANLEY MELVILLE, M.D.

THE photographs I exhibit demonstrate a method I have found useful at the Victoria Hospital for Children, especially in the X-ray examination of the alimentary tract. The difficulty of making routine examination of infants owing to their inherent tendency to flex their thighs upon the abdomen and their innate dread of falling, suggested the method I have adopted with success. A piece of three-ply wood, 25 in. by 12 in., is lightly padded with wool, and to this the infant, lying on its back, is carefully bandaged. The result has exceeded expectations, for not only has the examination been much simplified (examination in the vertical is as simple as in the horizontal position), but the infant, feeling secure, no longer wriggles and cries, but usually sleeps throughout the examination.

## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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(February 18, 1916.)

### Case of Leontiasis Ossia.<sup>1</sup>

By REGINALD MORTON, M.D.

THIS patient was admitted to the West London Hospital under Dr. Beddard for some thoracic symptoms, and, when well enough, came to the X-ray department. Owing to the dense masses of bone it was difficult to get completely satisfactory radiograms, and while little detail shows through these masses it will be noticed that the exposure has been sufficient almost to obliterate the cervical vertebræ. Little, if anything, seems to be known regarding the ætiology of the disease. Erichsen, in the tenth edition of his "Surgery" (published 1895), refers to it in the diagnosis of acromegaly as follows: "Leontiasis ossia is characterized by great and irregular enlargement of the face and bones, the two upper jaws being, as a rule, first affected. Masses of bone project externally and also fill the antra and block the nose. The affection very slowly spreads to the other bones of the face and even to those of the cranium. The new bone is spongy in structure. In the few recorded cases the disease has begun in early life, but nothing definite is known as to its causation."

<sup>1</sup> The following note from the "Practitioner's Encyclopædia" (General Medicine), p. 308, is of interest: "Leontiasis ossia, a much rarer disease, affecting the bones of the skull and sometimes of the face. Owing to the increase in size of the skull, notably of the forehead and upper facial bones, a suggestion of leonine aspect may be acquired. Optic atrophy may occur as a result of the overgrowth of the bones at the back of the orbit. A famous instance of this disease was the 'elephant man,' who died in the London Hospital after living there some years. His skull is preserved in the Hospital museum."

It will be observed that the accompanying radiograms might have formed the text of the above description. I might add that, some years ago, a part of the bony outgrowth was removed from the lower jaw. Needless to say, the effect was purely mechanical and temporary; the course of the disease has not been affected in any way.



FIG. 1



FIG. 2.

Leontiasis ossia.

(February 18, 1916.)

### Displacement of the Aortic Arch.

By REGINALD MORTON, M.D.

THE second case, about which I hope to get some information, is that of a man, aged 63, who came under the care of Dr. Saunders, complaining of some difficulty in swallowing, also in breathing after slight exertion. Opaque food was checked at the level of the arch of the aorta, but not enough to constitute any real obstruction. In the radiogram the column of food is broken at this point, but a few minutes after this was taken the upper portion had passed down to the stomach.

It will be noticed that there is no localized dilatation of the arch,

but it is widened out and displaced so that the whole of it comes into view; although normally, in the oblique position, all we see is a straight, ribbon-shaped shadow with a rounded top. This is the second time only that I have seen this condition, and in neither case has there been any opportunity of ascertaining the real cause of this displacement



Deformity of the aortic arch.

of the aortic arch. I have seen but one reference to it in the course of my reading, which reference I have put away so carefully that it cannot be found. I remember, however, that the writer stated the condition was due to the presence of a new growth. I think radiologists should be on the look-out for these cases and do what is possible to find some explanation for the condition.



(March 17, 1916.)

## DISCUSSION ON EXPERIMENTS AND EXPERIENCES WITH THE COOLIDGE TUBE.

Opened by ROBERT KNOX, M.D.

THE construction of the Coolidge tube is now familiar to all; it is therefore only necessary to describe the uses to which it may be put in radiography, radioscopy, and X-ray therapeutics, and to touch on a few practical points in its manipulation.

Considering firstly the use of the tube in radioscopy, it may be briefly dismissed with the remark that it possesses, amongst other advantages over other tubes, that of extreme adaptability, being readily adjusted to suit the requirements of the case under examination. It can be used continuously for long periods, without fear of damage, with the currents likely to be required for a prolonged screen examination. The experience gained in therapeutic work warrants the statement that a current of 4 ma. to 5 ma. can be continuously passed through the tube for many hours. This quantity of current should be ample for any screening examination. It is well, however, to issue a word of warning on behalf of the patient and of the operator. Both must be efficiently protected, and the radiologist must not forget to cut down the time of examination to the minimum lest in his enthusiasm he may damage his patient or himself.

In radiography the Coolidge tube will be found to possess advantages which at present can hardly be estimated accurately. But as time elapses and the use of the tube becomes more general, radiologists will find that in this tube they possess an instrument of precision far in advance of any previously produced. Its first great advantage is the ease with which it can be adjusted to conditions suitable for radiography of various parts. By regulating the heating current in the accessory circuit the tube can be arranged to give rays of varying penetrative power. Another advantage is its capacity for taking very large discharges from the secondary of the coil or transformer; these may be used for instantaneous exposures, or long exposures with heavy currents may be given.

I consider it the ideal tube for single flash exposures—nearly all the

radiographic results shown to-night were obtained with a high-tension transformer—but it is equally adaptable to a large or small coil outfit. In the latter case the inverse current must be as nearly as possible suppressed if the best results are to be obtained. In this respect the single impulse coil with its absence of inverse current will be very useful in work with the Coolidge tube.

In order to show how the Coolidge tube may be manipulated in radiography, I have had a number of experiments carried out, the results of which I shall briefly describe to you. In passing, it may be well to point out that so far these experiments have been elementary in character, the object being to ascertain the best working conditions of the tube in radiography and radioscopy. From these observations it is possible to indicate the advantages of the new tube—no other tube would have given similar results in the time taken for these experiments. One tube only has been used for all the experiments shown, and it was frequently adjusted from a very soft condition to a moderately hard one in a few seconds. It is hoped that with more exhaustive experiments it may be possible to lay down definite rules for the production of particular results; in other words, it may soon be possible to standardize radiographic exposures, and it is also hoped that some real advance may be made in the vexed question of the measurement of X-ray dosage.

#### PARTICULARS OF EXPERIMENTS.

The slides which illustrate this part of the paper were all obtained from the dried femur. A penetration gauge was employed, consisting of a strip of aluminium 140 mm. long and 20 mm. wide, divided into seven squares, the first of which is 1 mm. thick, each of the others being twice the thickness of the preceding one, the seventh being made of a heavy metal equivalent to aluminium 64 mm. thick. In each of these squares small circular lead plugs are let in. The gauge when radiographed shows white spots on a dark ground, and the slides taken direct from the radiogram show black dots on a lighter ground. The lead plugs are inserted in numbers, 1 to 6; they have no relation to the thickness of the aluminium steps.

#### *Development of the Slides.*

In most of the radiograms shown, the exposures have all been made on one plate, or the plates have been developed all together; special mention is made in those cases where the plates have been developed separately.

*Experiment I.*—Four plates were taken with different heating currents in the filament circuit, the regulator in the primary circuit remaining unaltered. An exposure of 5 sec. was given to each plate. The plates were developed in the same dish, each receiving the same time allowance. With a heating current of 4.42 amp. the tube was so soft that only an outline of the bone was obtained, the penetrometer

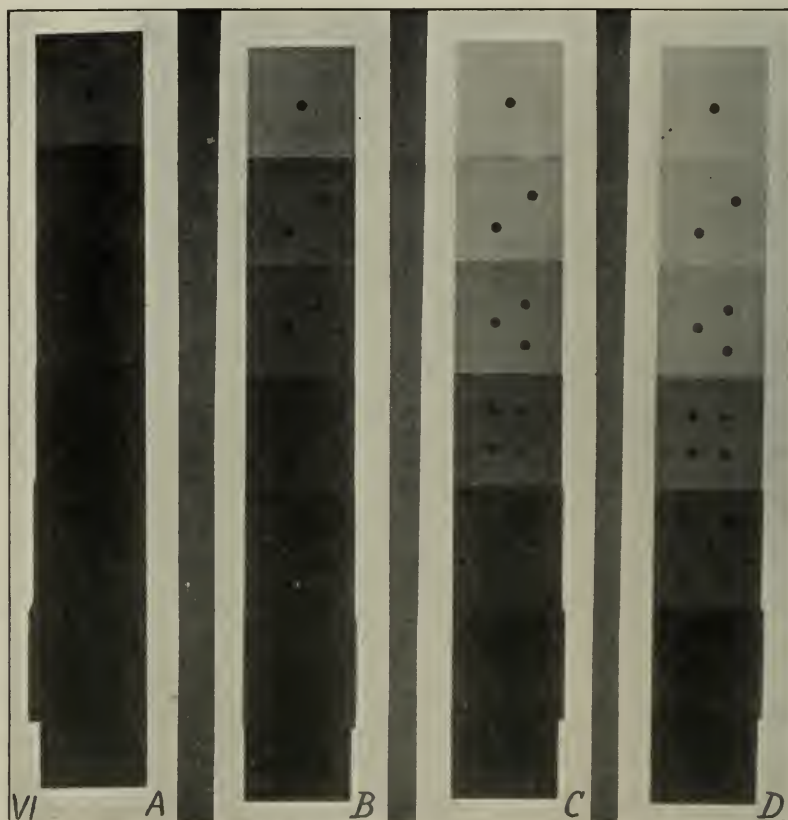


FIG. 1 (Experiment II).

recording one spot. The other three, with heating currents of 4.3 amp., 4.17 amp., and 4 amp. respectively, all gave good results, though the third and fourth were greatly over-developed. A second batch of four plates taken under the same conditions, but each developed separately, gave a good result in three of the plates, which were taken with heating currents of 4.3 amp., 4.17 amp., and 4 amp. The details of these experiments are as follows:—

		Heating current amperes		Current in primary amperes		Current in secondary milliamperes		Time of exposure in seconds
A	...	4.42	...	45	...	30	...	5
B	...	4.3	...	39	...	25	...	5
C	...	4.17	...	30	...	15	...	5
D	...	4.0	...	20	...	5	...	5

Regulator in primary circuit constant. Time of exposure constant. Heating current varied; note the influence of heating current upon (a) primary current, and (b) secondary current through tube, resulting in a drop from 30 ma. to 5 ma. The spark-gap in these experiments was not taken into account. Its importance as an indication of the penetration will be shown later.

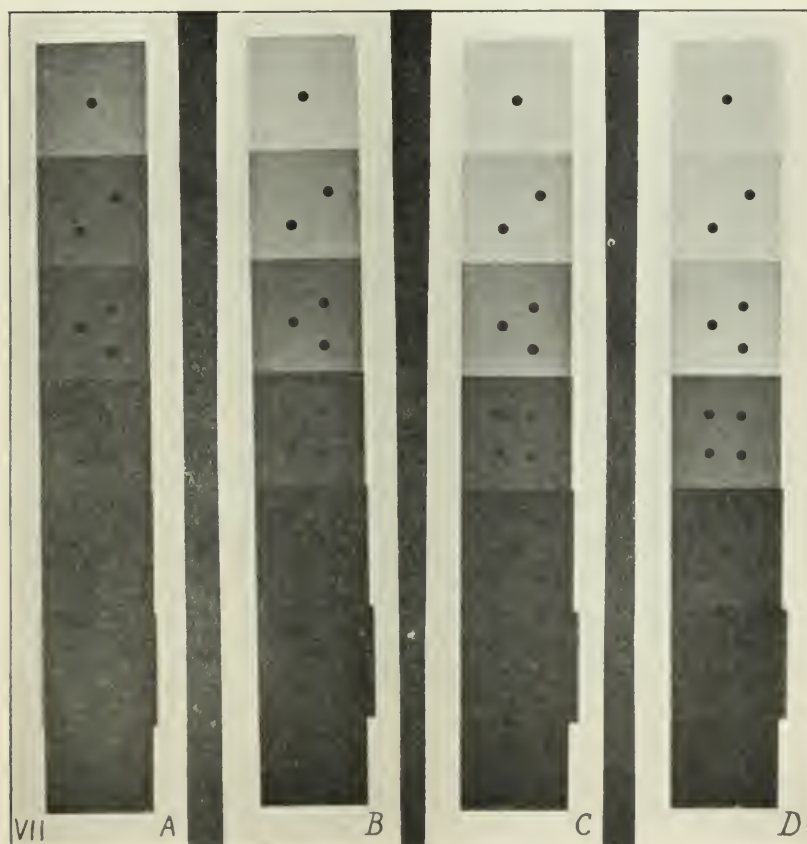


FIG. 2 (Experiment III).

*Experiment II* (penetration gauge, four exposures).—In this experiment the heating current was kept constant at 4.2 amp., the regulator in the primary being moved so as to vary the current, and the time of exposure adjusted to keep the milliampere seconds constant. The slide shows the great increase of penetration with the harder rays.

		Heating current amperes		Current in primary amperes		Current in secondary milliamperes		Exposure seconds		Milliampere seconds		Approximate spark-gap between points
A	..	4.2	...	25	...	12	...	30	...	360	...	1 in.
B	...	4.2	...	27	...	14	...	26	...	364	...	1½ "
C	...	4.2	...	30	...	17	...	21	...	357	...	2¼ "
D	...	4.2	...	32	..	20	...	18	...	360	...	3¾ "

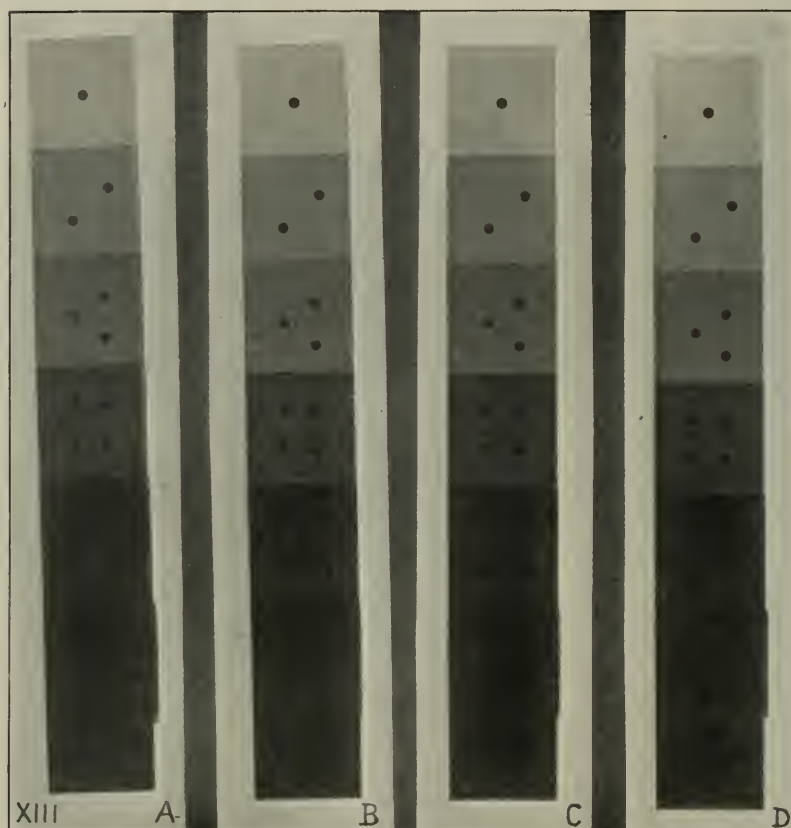


FIG. 3 (Experiment IV).

*Experiment III.*—The next experiment was carried out to show the effect of variations in the time of the exposure. The heating current was 4.2 amp., the milliamperes 24, and the approximate spark-gap 2½ in. The exposures given were 3 sec., 6 sec., 12 sec., and 24 sec. respectively. This resolved itself into an attempt to find the best exposure time under the existing conditions and need not be further discussed. It might be noted that eight times the length of exposure in *D* gives practically the same result as when 3 sec. is given in *A*. On the lower registers the penetration is greater with the longer exposure.



*Experiment IV.*—This was carried out with the object of ascertaining the effect of varying the distance of the plate from the anticathode; the exposures given were approximately proportional to the square of the distance, and the resulting negatives were practically all similar. The details of the experiment are given as follows :—

	Heating current amperes			Amperes	Milliamperes			Seconds	Milliampere seconds			Spark-gap
A	...	4.2	...	23	...	12	...	30	...	360	...	1 in.
B	...	4.2	...	27	...	14	...	26	...	364	...	1½ "
C	...	4.2	...	30	...	17	...	21	...	357	...	2¼ "
D	...	4.2	...	32	...	20	...	18	...	360	...	3¼ "

*Experiment V* (head of femur and penetration gauge).—In this experiment the heating current was varied and the regulator adjusted to get the same spark-gap in each case, the exposure time being arranged to keep the milliampere seconds constant. The negatives are not identically<sup>1</sup> the same. The following table gives the full particulars of this experiment :—

Heating current amperes		Current in secondary milliamperes		Exposure seconds		Milliampere seconds		Spark-gap
4.3	...	36	...	3.0	...	108	...	3¼ in.
4.2	...	25	...	4.4	...	110	...	3¼ "
4.1	...	20	...	5.5	...	110	...	3¼ "
4.0	...	12	...	9.0	...	108	...	3¼ "

*Experiment VI.*—Two negatives were taken with short exposures, with 30 ma. in the tube and a spark-gap of 4½ in., the exposures being approximately ¼ sec. and 1 sec. A second pair of negatives were taken with a much shorter spark-gap (about 2 in.), with 45 ma. in the secondary, and exposures of 5 sec. and 20 sec. respectively. The four negatives were on a 15 in. by 12 in. plate. The first, A, was rather under-developed, but any one of the other three could have been developed into a good negative. The full particulars were :—

Heating current amperes		Milliamperes		Spark-gap inches		Time seconds		Milliamperes seconds	
Set A	{ 4.3	...	30	...	$4\frac{1}{8}$	...	$\frac{1}{4}$	...	$7\frac{1}{2}$
	{ 4.3	...	30	...	$4\frac{1}{8}$	...	1	...	30
Set B	{ 4.2	...	45	...	2	...	5	...	225
	{ 4.2	...	45	...	2	...	20	...	900

This experiment shows the difference in the spark-gap in the two sets, and the remarkable differences in the milliampere seconds exposure, varying from 7½ ma. to 900 ma. sec.

<sup>1</sup> This has also been pointed out by Mr. Schall in a paper recently read before the Röntgen Society.



FIG. 4 (Experiment V).



FIG. 5 (Experiment V).



FIG. 6 (Experiment V).



FIG. 7 (Experiment V)



FIG. 8 (Experiment VI).



FIG. 9 (Experiment VI).



FIG. 10 (Experiment VI).



FIG. 11 (Experiment VI).





FIG. 12 (Experiment VII).



FIG. 13 (Experiment VII).



FIG. 14 (Experiment VII).



FIG. 15 (Experiment VII).



FIG. 16 (Experiment VIII).



FIG. 17 (Experiment VIII).



FIG. 18 (Experiment VIII).



FIG. 19 (Experiment VIII).



FIG. 20 (Experiment IX).



FIG. 21 (Experiment IX).



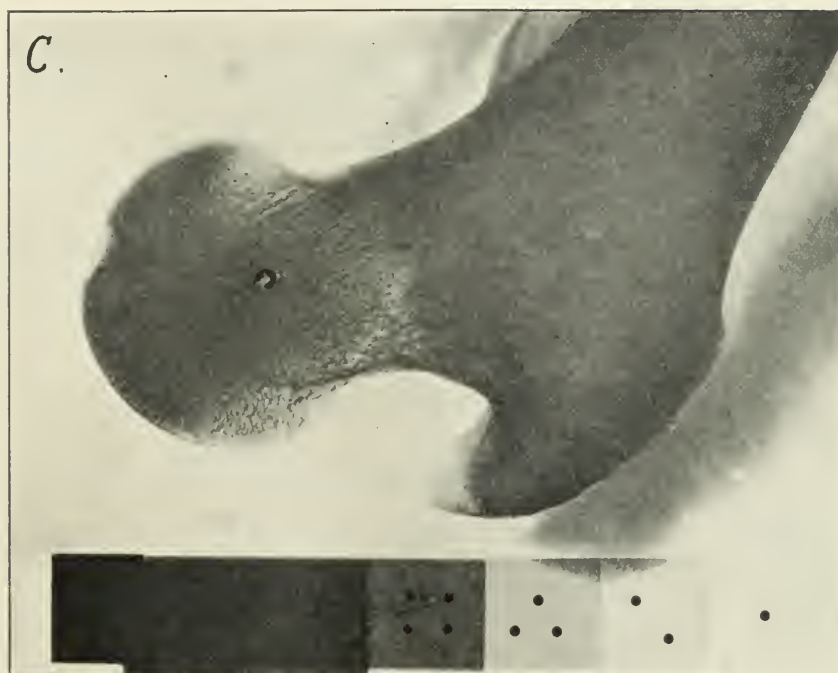


FIG. 22 (Experiment IX).



FIG. 23 (Experiment IX).

*Experiment VII.*—It has been suggested that the Coolidge tube is not so good for radiographic work as an ordinary tube. To determine this point four radiograms were taken, two (*A* and *B*) with a Macalaster-Wiggin tube with a very sharp focus, and two (*C* and *D*) with the Coolidge tube used in the other experiments. The exposures were the same in each, and the other factors were also equal. The result is shown in figs. 12, 13, 14 and 15. All four are good negatives. The two taken with the Macalaster-Wiggin tube are, if anything, the better plates, but they do not demonstrate any marked superiority.

*Experiment VIII* (fig. 16).—Meat about  $\frac{3}{4}$  in. thick, wrapped round bone above and below.

		Heating current amperes		Spark-gap		Milliamperes		Seconds
A	...	3.75	...	$4\frac{3}{4}$ in.	...	$4\frac{1}{2}$	...	12
B	...	4.0	...	$1\frac{3}{4}$ „	...	12	...	30
C	...	3.92	...	$3\frac{1}{4}$ „	...	9	...	10
D	...	4.12	...	$6\frac{1}{4}$ „	...	27	...	$1\frac{1}{2}$

*Experiment IX* was a repetition of Experiment VIII with shorter times of exposure, the former being considerably over-exposed.

#### THE COOLIDGE TUBE IN THERAPEUTICS.

The Coolidge tube has been shown to be of proved utility in radiography, and if its reputation depended on that alone it would have amply substantiated the claims made for it, but it possesses still greater advantages when used in therapeutic work. To those of us who have been engaged in this work practically from its infancy, it is hardly too much to say that the Coolidge tube is by far the most efficient tube we have yet had the opportunity of using, and it would be difficult to conceive of any improvements which would be likely to make it more efficient. Within reasonable limits of safety, so far as the tube is concerned, it may be used for many hours daily, and at the end of the day still be able to continue its output of uniform radiations.

The following experiments were carried out with a view to ascertaining the best conditions for the working of the tube. Since the Coolidge tube has been used at the Cancer Hospital we have had three tubes; the following figures will show the working value: 2,228 doses have been given in a period of several months, amounting in all to

310 hours, at an average of 8 min. per dose; most of these were given through 3 mm. or 4 mm. of aluminium. Of the three tubes one was worn out after having been used for over 800 exposures; it was repaired at a cost of £20, the original price being £25. Taking the cost of the tube at £25, this works out at an average of  $7\frac{1}{2}$ d. per dose, not an excessive price to pay for a deep irradiation. During the time these tubes were in use two other tubes were damaged, but this was the result of accident incidental to all tubes, and the damage was not done while the tubes were actually at work.

#### APPARATUS.

The apparatus used in the experiments consisted of:—

(1) *High-tension Transformer.*

(2) *Large Induction Coil.*—Spark-gap estimated at over 20 in. This was used in all its combinations—namely, one-, two-, and three-point electrolytic interrupter, three windings of primary, and in various combinations of these factors. The mercury interrupter was also tried with the three primary windings.

(3) *Small Coil.*—Sixteen-inch spark-gap specially constructed for therapeutic work. A mercury jet break was used. It was worked with coal gas as the dielectric.

The high-tension transformer in its present condition was quickly ruled out of the experiments, it being found that for ordinary therapeutic work the coil outfit was by far the most efficient and economical.

The best all-round conditions were obtained with: (a) The large coil, working on No. 1 primary, and the two-point electrolytic interrupter; (b) the small coil, with the mercury interrupter.

The conditions under which the *large coil* was worked were: Heating current, 3.95 amp.; amperes in primary, 6; spark-gap, 7.5 in.; milliamperes, 6; time, 4 min. to produce a pastille dose at 10 in. from the anticathode of the tube.

*The Small Coil* (16-in. spark-gap).—Heating current, 4 amp.; primary current, 4 amp.; milliamperes, 4; spark-gap, 9 in.; time, 4 min. to produce 10 X or tint B. The interrupter was worked at its lowest speed.

This was found to be the best average condition for therapeutic work, and is the arrangement we generally use with the Coolidge tube. There are variations from this which can be obtained for special occasions; the length of the spark-gap can be increased. This might

be done with a number of variations in the apparatus. The longest spark-gap obtained was 13.25 in. This was obtained with the large coil, working on No. 3 primary, and the mercury interrupter, with 7 amp. primary current and 0.8 ma. in the secondary. The heating current was 3.5 amp.

Time factor in dosage: Working at a distance of 10 in. from the anticathode, the shortest time in which a pastille dose could be obtained was  $2\frac{1}{2}$  min. This was done in two combinations of apparatus, namely:—

(1) *Large Coil*.—Two-primary winding. Two-point electrolytic interrupter; heating current, 3.8 amp.; primary current, 7 amp.; spark-gap, 8.7 in.; milliamperes, 4.5; time,  $2\frac{1}{2}$  min.

(2) *Small Coil*.—Heating current, 4 amp.; primary current, 8 amp.; spark-gap, 6 in.; milliamperes, 12; time,  $2\frac{1}{2}$  min.

*Heating Current*.—The range employed in the large number of experiments varied from 3.2 amp. to 4 amp. in all possible combinations at our disposal, and in the endeavour to find out the best working conditions a large number of experiments were carried out. The results obtained are charted on a scaled paper, but I shall only refer to it, as it is much too intricate and at the present time too unstable to allow of any definite statements. The charts on the wall will give some idea of the amount of time which has been spent on these investigations, which are still in the initial stage and which, when completed, may produce some useful data upon which to base our future work.

TABLE TO SHOW VARIOUS COMBINATIONS OF APPARATUS USED AND TIME TO PRODUCE THE B TINT ON SABOURAUD AND NOIRE PASTILLES.

(*Sabouraud pastille 10 in. from Coolidge anticathode.*)

Primary in amperes	Spark in inches	Milli-amperes in tube	Heating current in amperes	Time in minutes to colour Tint B	Apparatus employed
3.5	8.5	3.9	4.0	8.0	Small coil; mercury break
4.0	9.0	4.0	4.0	4.0	Small coil; mercury break
6.0	7.5	6.0	3.95	4.0	Big coil; No. 1, primary; No. 2, electrolytic
7.0	8.75	4.5	3.8	2.5	Big coil; No. 2, primary; No. 2, electrolytic
8.0	6.0	12.0	4.0	2.5	Big coil; No. 3, primary; mercury break
9.0	4.0	18.0	4.0	3.0	Big coil; No. 1, primary; No. 2, electrolytic
15.0	7.25	4.75	3.5	10.0	Transformer
16.0	7.0	2.0	3.2	14.0	Transformer
17.0	8.25	2.5	3.475	12.0	Transformer
17.0	8.0	3.75	3.45	10.0	Transformer
17.0	5.0	10.0	3.75	5.0	Transformer



The next stage of our investigations took the form of a number of experiments with a fixed and rotating tube. The development of the rotating tube is the outcome of an attempt to improve upon the technique at present used in the treatment of deep-seated diseases. The suggestion to rotate the tube was made by Mr. C. E. S. Phillips; the mechanical details of the apparatus have been worked out and the instrument made by Mr. St. George Caulfeild; the experiments which illustrate this communication have been carried out for me by Mr. Caulfeild and Mr. Westlake at the Cancer Hospital. In order to explain the manner in which the results have been obtained, it will be necessary to describe briefly the apparatus employed.

In passing it will be well to point out that the apparatus is at present in the experimental stage; but that as time elapses and experience dictates the future form which this instrument may take it will be modified; the underlying principles which led to its construction will, however, remain unchanged.

The experiments conducted so far have been made in order to ascertain the best method of using the new apparatus. The employment of X-rays for the localization of foreign bodies directed our thoughts to the practicability of employing the central ray of the focus tube in therapeutics, the object being to direct the ray to a definite spot in the interior of the body. The principles employed in localization are quite applicable to the perfection of a technique for therapy, and rules found to be of value in the former are equally applicable to the latter. The principle on which the tube-stand is based is that of the simple rotation of the tube over the part to be irradiated, the beam of rays emerging from the aperture in the tube box being projected into the interior of the body at an angle which can be determined beforehand. The result of this angular projection of the sheaf of rays when the tube box is rotated is to describe a circle upon the skin surface; proceeding from this circle the converging rays meet at a point **X**; at this point the rays cross to diverge, and to emerge from or to be absorbed by the tissues beyond the focus point. The central ray is ascertained, the tube fixed to the stand, and by mechanical adjustments the rays may be projected into the interior of the body with an accuracy which is as surprising as its projection is simple. Moreover, the rays can be centred at any point and at any depth from the surface.



*Mechanical Details of the Apparatus.*

The machine consists of a vertical bar, with a bar **B** bolted horizontally across its lower end forming an inverted **T**. This **T** is slung by a hook from the ball-bearing **C**, through which passes the shank of the eye into which the hook fits. The shank carries the pulley **D**, which is rotated by a belt from the clock **E**. A carrier **F** slides along one end of the arm **B**. This carrier carries the tube box **G** slung from the axis **W**. A weight **H** slides along the other end of arm **B** in order to balance the weight of the tube box at any angle or distance from the centre **A**. A winch **K** raises or lowers the tube box. The carrier **F** has a scale of degrees showing the angle to which **G** is tilted. The bar **B** is scaled to show the distance the axis **W** is from the centre **A**. An insulated brass circle **L** is carried on the bar **A** and serves to collect the high-tension current from the spring **M** which is attached to the frame of the apparatus. When using a Coolidge tube another brass circle is fitted at **P**. **L** is connected to one end of the tube. The other high-tension terminal is at **N**, a spring which makes contact with the shank of the main eye-bolt, which is connected electrically to the other end of the tube. An apparatus for finding the direction of the ray is provided with equivalent scales to enable the main apparatus to be set to any required angle.

In its present form the apparatus is not quite accurate in its adjustments; the following diagrams explain the degrees of error which may occur if care is not exercised to adjust it accurately. This is shown in the two diagrams thrown on the screen, and need not be described in detail.

*Vertical Error.*—Let **AO** be the vertical arm, **OP** the plumb-line. If **AO** is not vertical it will assume position **AX**. The horizontal bar will then be at **XC**. The tube box is at a constant angle,  $45^\circ$ . If **AOP** is vertical the pencil of rays **BM** will strike at **M**. When **AXC** is the position of the bar, the same pencil of rays will strike the plumb-line which now hangs from **X** at point **F**, and the position of the ray on a skin plate at **KM** will be **K** instead of **M**. Circles on the skin plate would be larger than estimated by the length **KM** (as estimated on the drawing). Focus points would be deeper by the length **MN**.

*Angular Error.*—Suppose the setting of the tube to have a constant angular error, **CTA**. At a flat angle the focus point would be **C** instead of **A**. At a steep angle the focus point would be **D** instead of **B**. A skin circle would be less in radius by the length **FC**, and in a steep

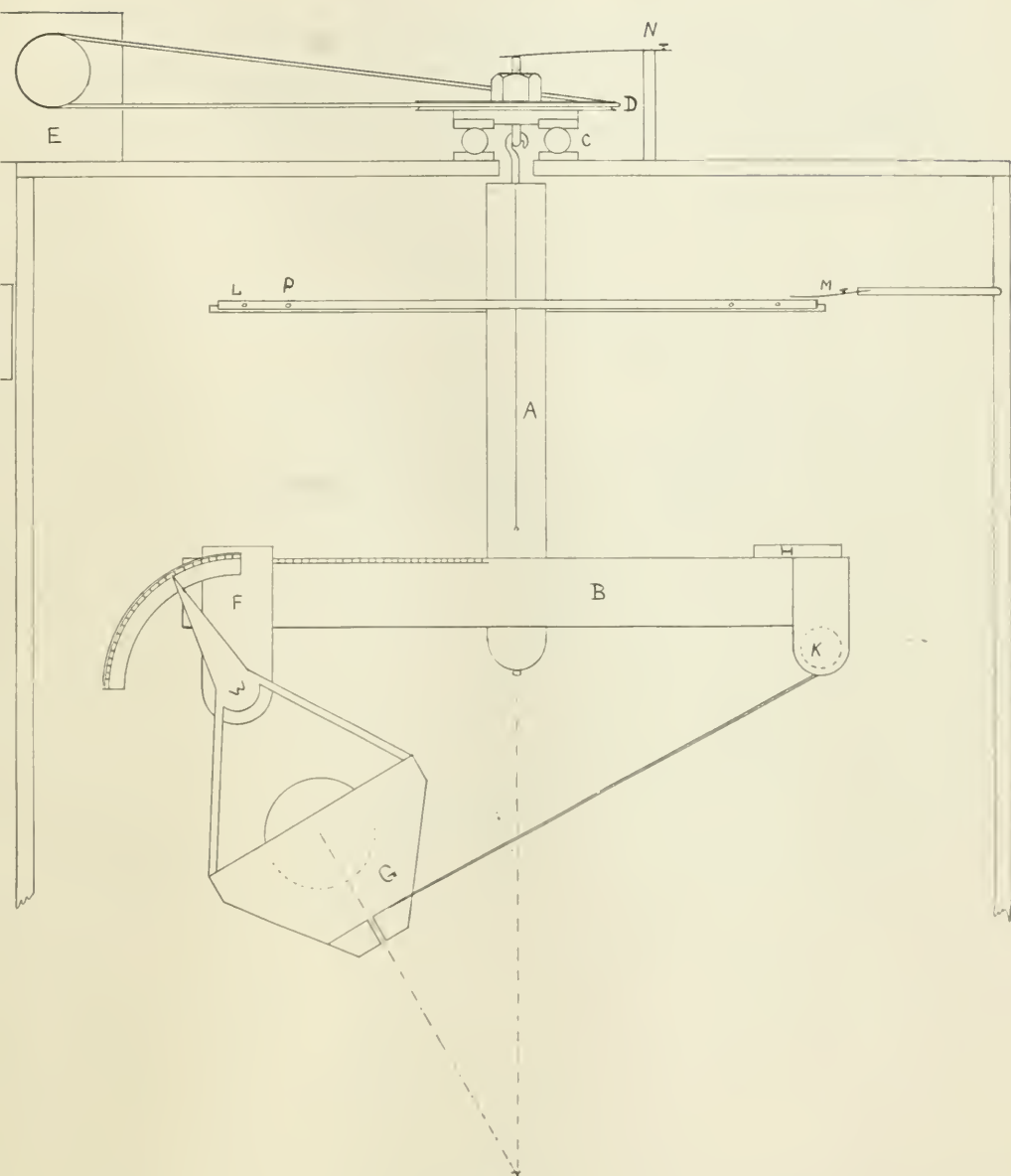


FIG. 24.

Mechanical details of the apparatus.

<sup>1</sup> This and figs. 25, 26, 27 and 28, illustrating Dr. Knox's paper, are inserted by the permission of the publishers of the *Archives of Radiology and Electro-Therapy*.

position by the length **ED**. The flatter the angle the less the vertical error and the greater the horizontal error.

*Error in Scale Adjustments.*—There can be an error in reading the angular scales on the apparatus in its present rough form of nearly  $1^\circ$ . The tube box cannot be adjusted to a greater accuracy than  $\frac{1}{2}^\circ$ . If

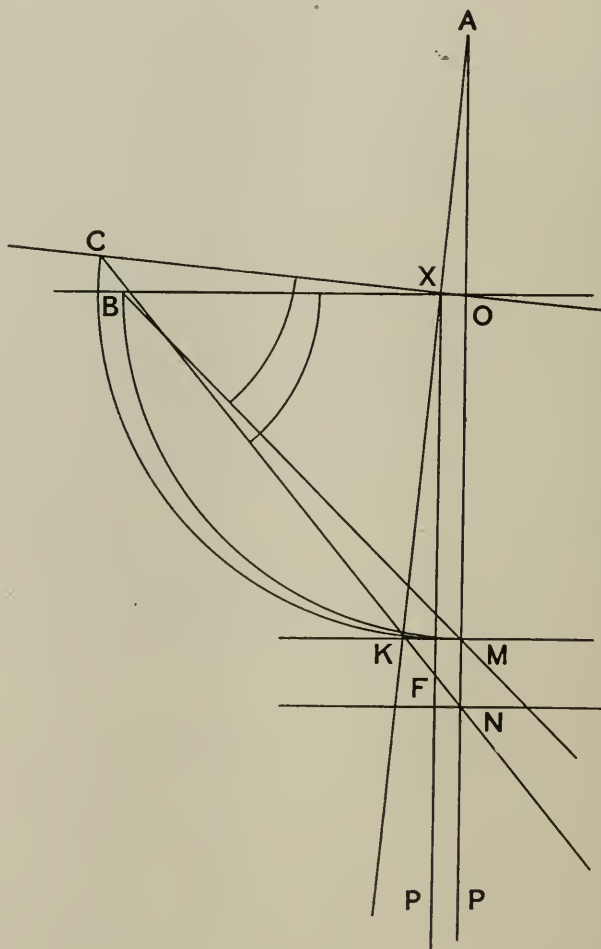


FIG. 25.

Diagram to illustrate vertical error.

these two errors are additive there would be a total angular difference of  $1\frac{1}{2}^\circ$ —i.e., with a focus point adjusted at  $35''$  below the horizontal, the actual focus point might be at  $36\frac{1}{2}''$ , the angle of the tube being  $28\frac{1}{2}^\circ$

instead of  $30^\circ$ . A skin plate at  $30''$  below the horizontal would have its circle  $\frac{3}{4}''$  larger or smaller than the estimate.

The following experiment illustrated in fig. 27 explains the method of demonstrating the action of the rotating tube: The tube is set at an angle of  $32\frac{1}{2}^\circ$ , the skin surface is  $14\frac{1}{2}$  in. from the anticathode, the

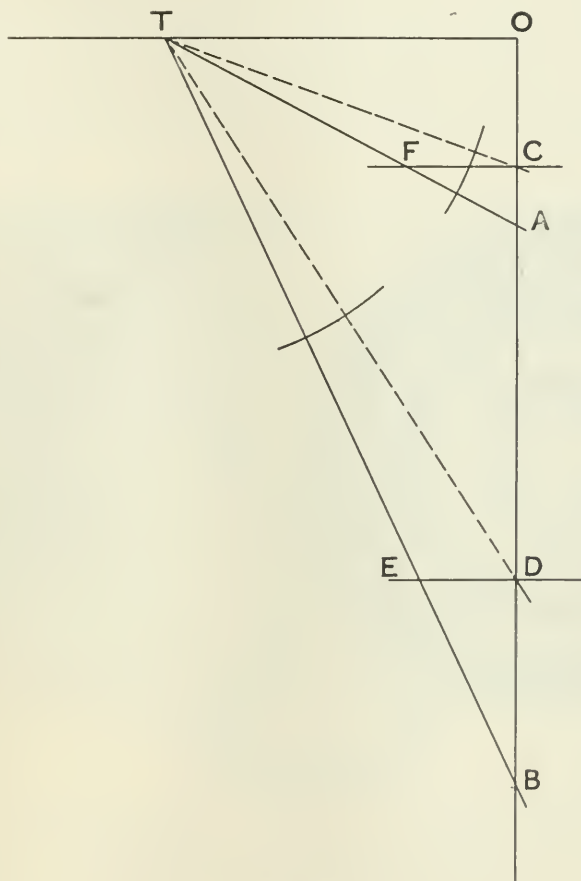


FIG. 26.

Diagram to illustrate angular error.

focus point is at  $26\frac{1}{2}$  in. from the anticathode. Fig. 27 represents photographic records taken with this setting, and shows three surface circles, with the rays converging to the same focus on a plate underneath and at right angles to the surface plate. The plates remained in the same position during the three exposures.

In another experiment the anticathode was set at  $13\frac{1}{2}$  in. from the skin; the plate representing the tumour to be treated was 19 in. from the anticathode, this being the area of maximum irradiation, the size of the circle being  $5\frac{1}{2}$  in. mean diameter. It was found that the tumour plate received a dose of 151 X, while any measured portion of the skin plate received 12 X. The total time taken to procure the 151 X was 192 min. The relative proportion of tumour dose to skin dose was 12.5 to 1. The dose was given from a moderately soft tube and no filters were used. It will be shown later that the proportion of focal dose to surface dose varies considerably when the radiations pass through tissues.

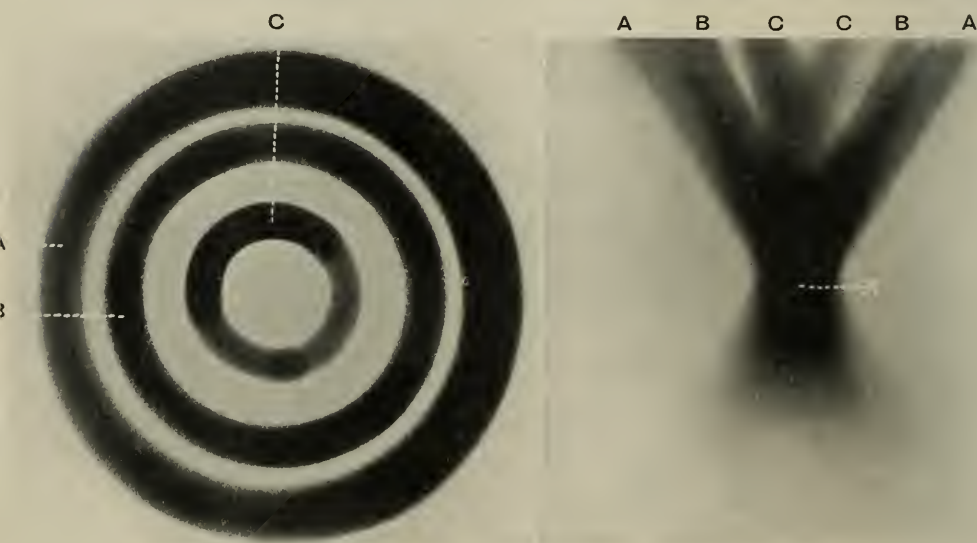


FIG. 27.

In experiments conducted to ascertain the percentage of radiation absorbed by the tissues, beef-steak was employed.

The actual time taken to produce these results may be greatly shortened when harder tubes are used and larger outputs from the generator utilized, but it is reasonable to expect that the relative proportions of dosage indicated will be maintained.

The description explains fully the mechanical features of the apparatus. These are susceptible of several improvements, which will be added as experience in its use teaches their necessity. The advantages claimed for this therapeutic localizer are: (1) Accuracy of application. (2) The possibility of repeating at subsequent exposures



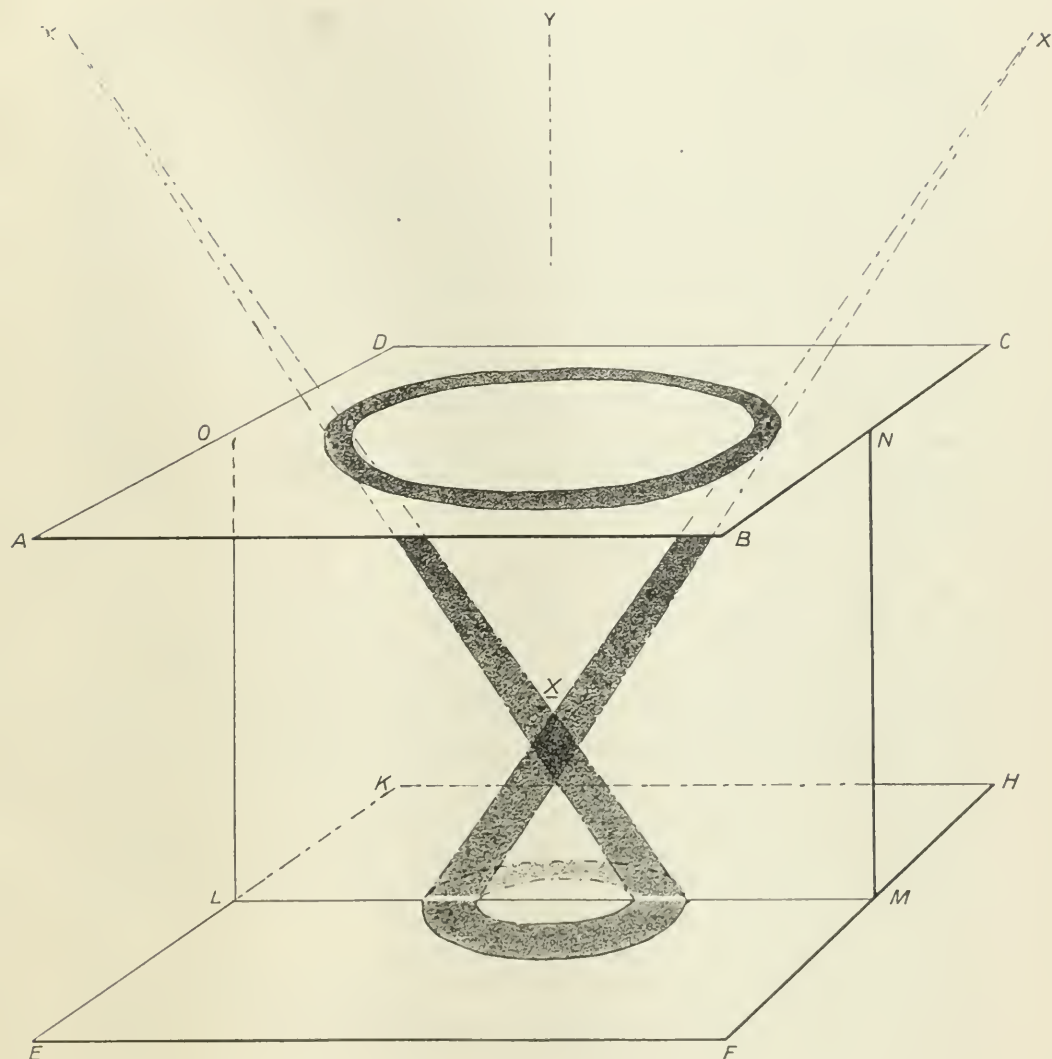


FIG. 28.

To show method of placing plates. Let  $ABCD, EFGH$  be two parallel planes and  $LMNO$  a plane vertical to them. Let the centre of rotation  $Y$  pass through the plane  $LMNO$ . Let  $X$  be any pencil of rays proceeding from an anticathode  $X$ , the tube being fixed at a constant angle to, and rotating round the centre of rotation,  $Y$ . Then the rotating ray  $X$  will describe the circle shown on the plane  $ABCD$ . The rays from opposite points in the circle of rotation will converge at  $X$  and diverge beyond it to describe the circle drawn on the plane  $EFGH$ .

the dose primarily given. (3) The radiations can be varied in depth and latitude in order to influence all portions of a growth; if desired, the focus point can be fixed in front of, or behind, or at the centre of the growth. (4) By changing the size of the circle two or more doses can be directed to the centre of the growth (*see* figs. 27 and 28). A centrally situated growth can be approached from all aspects of the body and the maximum dose administered on a point selected beforehand. Thus, a tumour of the mediastinum may be treated from four aspects with accuracy at any depth from the skin surface. By changing the focus point the whole area of the tumour can be irradiated to the necessary extent, the surrounding tissues receiving also an estimated dose. By this method it is also approximately possible to estimate the dosage received by the tumour. The dose on the skin is estimated in the usual way. The chief field of action of the rotating tube will be in the treatment of deep-seated disease. It will be possible to attack a growth from all aspects, and to administer at will a maximum dose to selected areas of the growth.

Given suitable tubes of great penetrative power, it should be possible to produce effects similar to those obtained when a radium tube is buried in the substance of a growth. It is not intended to suggest that X-rays should take the place of radium; both are useful, and the one may be used to supplement the other. In many cases of new growths it is impossible to place a radium tube in their substance, and when radiations from the skin surface obtained from radium alone are quite ineffectual. In these cases the X-rays may be projected into the new growth from several aspects, the tumour receiving a dose of sufficient activity to produce therapeutic effects. The new method will enable us to reach tumours in any part of the body.

It was found experimentally that the sheaf of rays from the X-ray tube could be directed to any part and at any angle within the structural limits of the apparatus. The tube stand was constructed to carry a Coolidge tube, and the following experiments were carried out. The chief object of these was to determine the relative proportion of radiations administered at the skin surface and upon a point 4 in. from the skin. The investigations therefore took the form of:—

(a) A comparison between the filtered and the unfiltered ray through air and through beef.

(b) A comparison between the action of the radiations from a fixed tube and a rotating tube, through (i) air, and through (ii) beef. In each set of experiments a filtered and an unfiltered ray was employed.

(c) In the course of these experiments several interesting points were observed in relation to the absorption power of tissues upon radiations, filtered and unfiltered, with the fixed and rotating tubes. These can only be briefly referred to here, but the experiments are being followed, and may later be communicated. A general observation was obtained which may have a definite significance—namely, all radiations, filtered or unfiltered, through air or beef, with fixed or rotating tube, with a short or a long exposure, and in all degrees of hardness, converge on the charted drawing to a common level, between the second and third inches from the surface, 14 in. or 15 in. from the target of the tube.

Proportion of skin dose to focus dose with rotating tube: No filter, 1 to 13; 3 mm. filter, 1 to 12. Through beef, 4 in. thick: Without a filter, 3 to 4; with a 3 mm. filter, 1 to 2.

The value of the Coolidge tube in therapeutic work was amply demonstrated, the tube being in action for hours on end and showing only the slightest variations.

#### METHOD OF ESTIMATION OF THE DOSAGE.

A slow-contact Wellington bromide paper, quarter-plate size, was used. A standard scale was prepared, based on the time taken to change a Sabouraud pastille from the A to the B tint, ten graduations of colour being obtained. The same make of paper was used and the same strength of developer employed for the development of the strips used in the experiments; the same developer was used for each set of experiments. The accuracy of this method of measurement is open to question, but as the results are experimental and comparative only, the errors common to the method will apply to all the experiments, and should in no way prejudice the comparative results. A small hole,  $\frac{1}{2}$  in. in diameter, cut in a thick sheet of lead was used as a diaphragm in all the experiments (rotating). The distance from the target to the diaphragm was 6 in., from the target to the skin, 12 in., and to the focus point 16 in. An exception was made to this measurement in the experiment of the penetration through beef with the fixed tube. All the experiments were carried out with the Coolidge tube. A 16 in. coil with a mercury break was used in the majority of the cases, a high-tension transformer being employed in a few. On account of the variations between the two types of apparatus it was thought better to use a coil outfit for all. The differences between the two, the heating

effect of the transformer on the tube, and the lesser output of hard rays when compared with those from the coil gave data which, when completed, may form the basis of another communication. It is sufficient for the present to state that the practical conclusion arrived at from these comparisons was, that for general value the coil is, far and away more efficient for deep therapeutic work; it gives a more penetrating ray at the cost of much less primary current and with a smaller milliamperage. The coil was a 16-in. one, especially wound for therapeutic work, the break was a large mercury jet one with gas dielectric. The conditions under which the coil and tube were worked are given with each set of experiments.

EXPERIMENT A.—COMPARISON OF THE EFFECT ON UNFILTERED AND FILTERED RAYS THROUGH 4 IN. OF BEEF.

EXPOSURE 60 MIN				EXPOSURE 50 MIN			
NO FILTER				3MM FILTER			
% ABSORB- TION	% PENETR- ATION	X DOSE		X DOSE	% PENETR- ATION	% ABSORB- TION	
		240		10 IN			
65	35	84	PHOTO	11	40	60	PAPER
91	9	23	BEEF	12	23	77	STEAK
95	5	12		13	12	88	
97	3	7		14	5	95	

PENETRATION OF UNFILTERED & FILTERED RAYS THROUGH  
FOUR INCHES OF BEEF STEAK

MERCURY BREAK, COOLIDGE TUBE

HEAT 4 AMPS., AMPS. 4, MA 4, SPARK-GAP 9 IN.

FIG. 29.

Twenty 3-min. runs of tube; total  
exposure, 60 min.

Ten 5-min. runs; total  
exposure, 50 min.

Four pieces of beef 1 in. thick; paper on top and between each piece of beef;  
one paper at lowest level, 4 in.

In the experiments with the fixed tube, the diaphragm had a larger opening, a 2-in. hole being used. The distances were the same as in the experiments with the rotating tube, with the exception already mentioned. If the larger opening had been used in the rotating experiments, the readings at the skin level would have been slightly higher, as the same strip of paper would have been exposed to the radiations for a somewhat longer period. This point will be experimentally dealt with at a later date.

CURVES CHARTED TO SHOW THE PREVIOUS EXPERIMENT.

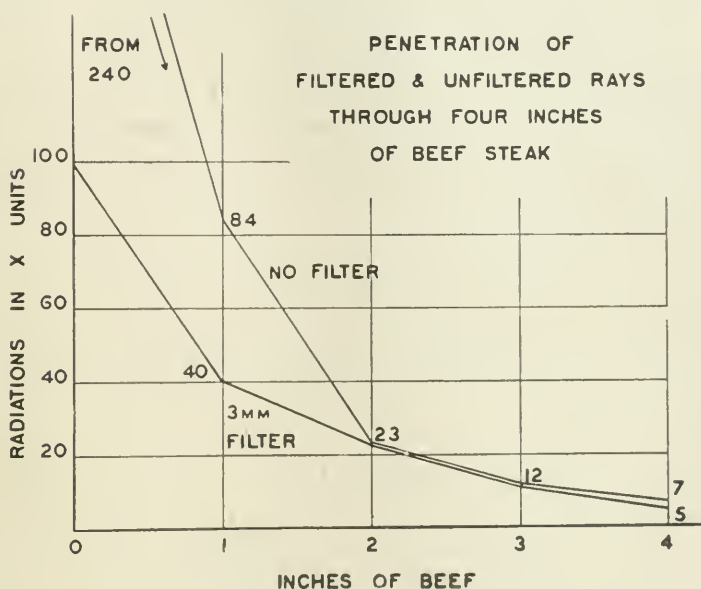


FIG. 30.

The curves representing the filtered and unfiltered rays come to the same value 2 in. from the skin, and follow each other down to the beginning of the fourth inch, where the filtered ray has rather less value than the unfiltered rays. This showing may be the result of a slight experimental error. The unfiltered skin dose is 2.10 X; focus dose, 7 X in 60 min. The filtered skin dose is 100 X; focus dose, 5 X in 50 min. The distance from the Coolidge target, 10 in., 11 in., 12 in., 13 in., and 14 in.



EXPERIMENT B.—RADIATION THROUGH AIR WITH A FIXED AND A ROTATING TUBE,  
FILTERED AND UNFILTERED RAYS BEING EMPLOYED.

*The results obtained are shown in the charts.*

FIXED TUBE					ROTATING TUBE				
		No filter	3 mm. filter				No filter	3 mm. filter	
Skin	...	43	...	29	...	5	...	6	
1	...	29	...	23	...	7	...	7	
2	...	24	...	20	...	9	...	14	
3	...	19	...	19	...	27	...	32	
Focus	...	17	...	18	...	65	...	73	

Five exposures of 3 min. ; 15 min. in all	Three exposures of 10 min. ; 30 min. in all	Eight exposures of 14 min. ; 112 min. in all	Eight exposures of 30 min. ; 240 min. in all
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Sixteen-inch coil ; Coolidge tube ; amperes, 4 ; milliamperes, 4 ; heat, 4 ; spark-gap, 9. Proportion of skin dose to focus dose : filtered, 2 to 1 ; unfiltered, 3 to 1.

Coolidge tube and transformer : amperes, 17 ; milliamperes, 6 ; spark-gap, 7. Filtered greater than unfiltered. Proportion of skin dose to focus dose about 1 to 12.

CHART TO ILLUSTRATE EXPERIMENT B.

RADIATION THROUGH AIR.

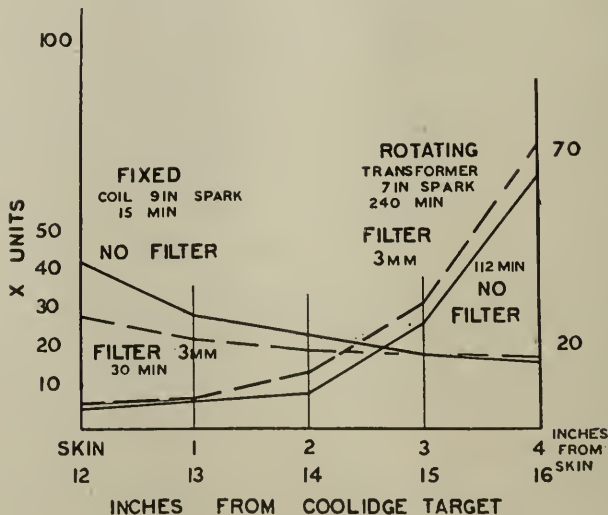


FIG. 31.

DETAILS OF EXPERIMENT : RADIATIONS THROUGH 4 IN. OF BEEF, THE ROTATING TUBE BEING USED AND THE RAYS UNFILTERED ; AND FILTERED THROUGH 3 MM. ALUMINIUM.

			NO FILTER			
			Soft		Medium hard	Hard with filter
Skin	...	...	5	...	15	5
1 in.	...	...	1	...	7	1
2 in.	...	...	—	...	5	—
3 in.	...	...	5	...	15	12
4 in. (focus)	...	...	6	...	20	10
			Two hours			
			A		B	C

Only B and C shown on the slide.

Rotating tube : no filter, two hours' run, 15-in. coil, Coolidge tube, mercury interrupter ; through beef, 12 in. to 13 in., and 14 in. to 15 in., and 16 in. from target.

Soft tube : amperes, 4·5 ; milliamperes, 4 ; heat, 3·8 ; spark, 6.

Medium hard tube : amperes, 5 ; milliamperes, 4·5 ; heat, 3·3 ; spark, 8.

This experiment is not charted separately, but is given on several of the other tables.

TABULATED LIST OF A NUMBER OF EXPERIMENTS WITH FIXED AND ROTATING TUBES.

*Filtered and unfiltered radiations through 4 in. of beef.*

Type of apparatus	Tube fixed or rotating	No filter or 3-mm. filter	Spark in inches	Distance of target to skin and focus	Minutes' duration of experiment	Dose on skin	Dose on focus	Minutes to produce 1 X on skin	Minutes to produce 1 X on focus
Coil    ...	<i>Fixed</i>	N.F.	9	10 in. 14 in.	60	240	7	0·25	8·5
„    ...	„	3 mm.	9	„    „	50	100	5	0·5	10·0
„    ...	<i>Rotating</i>	N.F.	8	12 in. 16 in.	120	15	20	8·0	6·0
„    ...	„	3 mm.	8	„    „	120	5	10	24·0	12·0
Transform.	Fixed	N.F.	6	„    „	132	220	1	0·6	132·0
„	„	3 mm.	6	„    „	180	120	1	1·5	180·0
Coil    ...	Rotating	N.F.	6	„    „	120	5	6	24·0	20·0
„    ...	Fixed	N.F.	6	„    „	95	161	5	0·59	19·0
„    ...	„	3 mm.	6	„    „	130	118	5	1·1	26·0

*Experiment C.*—Comparison of the percentage of rays passing through air and beef with a fixed tube, with filtered and unfiltered rays. The effect of the filter so far as the penetrative power of the ray is concerned was practically negligible. It appears to effect the purpose for which it is used : it cuts down the skin dose. Through 4 in. of beef the time taken to obtain 5 X was 95 min. with the unfiltered ray, while with the filtered ray it took 130 min. to obtain the same value.

The dose on the skin paper, however, was 118 as against 161. In the experiments through air with the filtered and unfiltered rays the time taken to produce much more intense effects was much less, but it took twice as long to produce the same result at 4 in. with the filtered rays, the skin dose with the filtered ray being much less.

COMPARATIVE RESULTS OF FILTERED AND UNFILTERED RAYS FROM THE FIXED TUBE THROUGH BEEF AND AIR.

*Fixed tube, Beef and Air—no filter ; 3 mm. filter ; 12 in., 13 in., 14 in., 15 in., 16 in.*

		BEEF			AIR				
		No filter		3-mm. filter	No filter		3-mm. filter		
Skin ...	...	161	...	118	...	43	...	29	
1 ...	...	50	...	38	...	29	...	23	
2 ...	...	22	...	20	...	24	...	20	
3 ...	...	10	...	8	...	19	...	19	
Focus ...	...	5	...	5	...	17	...	18	
		95 min.		130 min.	15 min.		30 min.		
		Amperes, 4.5; milliamperes 4; heat, 3.8; spark, 6.				Amperes, 4; milliamperes, 4; heat, 4; spark, 9.			

CHART TO SHOW RESULTS OBTAINED WITH RADIATION THROUGH AIR AND BEEF.

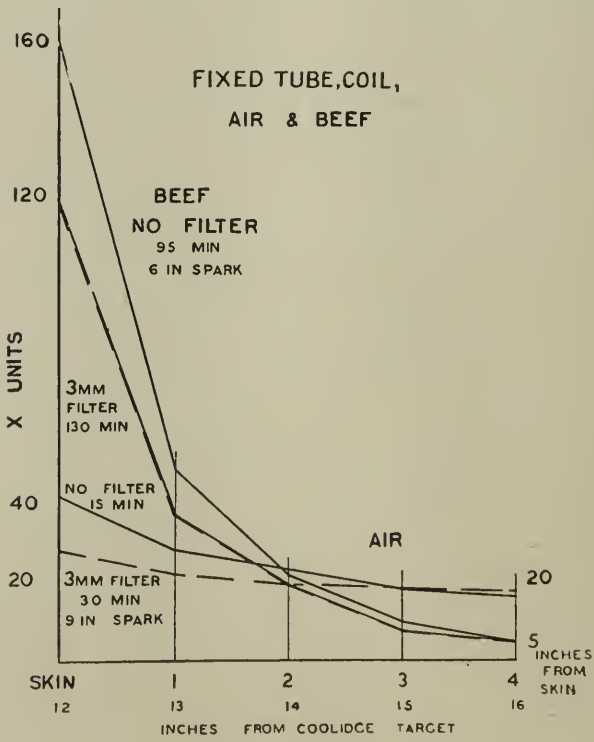


FIG. 32.

EXPERIMENT C.—COMPARISON OF THE EFFECTS OF FILTERED AND UNFILTERED RAYS THROUGH 4 IN. OF BEEF, WITH FIXED AND ROTATING TUBES.

<i>Fixed and rotating tubes through 4 in. of beef.</i>				<i>Filtered and unfiltered, 12 in., 13 in., 14 in., 15 in., 16 in. from target.</i>			
FIXED				ROTATING TUBE			
	No filter		3-mm. filter		No filter		3-mm. filter
Skin ...	161	...	118	...	15	...	5
1 ...	50	...	38	...	7	...	1
2 ...	22	...	20	...	5	...	0 ?
3 ...	10	...	8	...	15	...	12
Focus ...	5	...	5	...	20	...	10
Nineteen runs of 5 min.; 95 min. in all				Thirteen runs of 10 min.; 130 min. in all			
				Two runs of 1 hour; 120 min. in all			
				One run of 2 hours; 120 min. in all			

Angle, 41°; mean skin circle, 6 in.; 16-in. coil; mercury interrupter. With fixed tube the average readings were: amperes, 4·5; milliamperes, 4; heat, 3·8; spark-gap, 6 in. Rotating tube: amperes, 4·0; milliamperes, 4·5; heat, 3·8; spark-gap, 8 in.

The chief result in this comparison is the diminution of the skin dose when the rotating tube is used; in the unfiltered radiation it is 161 against 15 (10 against 1), in the filtered, 118 against 5 (40 against 1); whilst the focal dose is 1 to 4 in the unfiltered, and 1 to 2 in the filtered.

CHART TO ILLUSTRATE EXPERIMENT C.

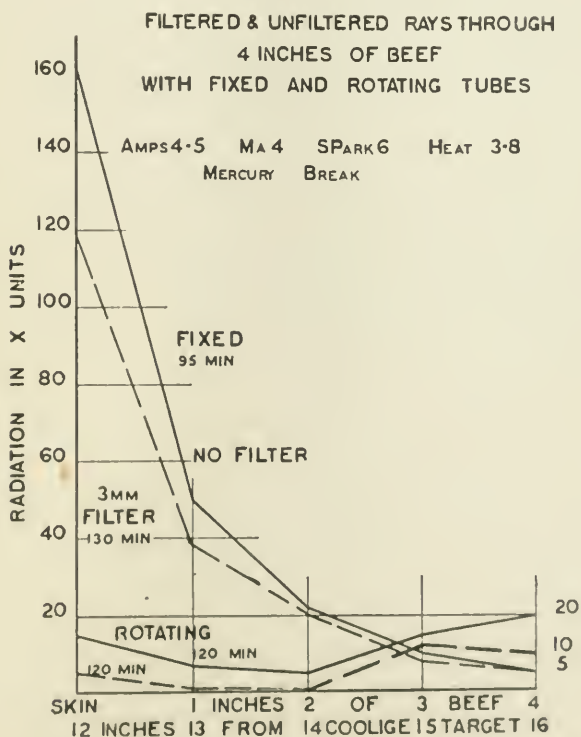


FIG. 33.

The final experiment deals with the comparison of the results obtained with radiations from the rotating tube through beef and air. Rays filtered and unfiltered.

			COIL THROUGH BEEF			TRANSFORMER THROUGH AIR		
			No filter		Filter 3 mm.	No filter		Filter 3 mm.
Skin	...	...	15	...	5	5	...	6
1 in.	...	...	7	...	1	7	...	7
2 in.	...	...	5	...	—	9	...	14
3 in.	...	...	15	...	10	27	...	32
4 in. (focus)	...	...	20	...	10	65	...	73
			120 min.		120 min.	112 min.		240 min.
			Angle, 41°; skin, 6-in. circle; amperes, 4; milliamperes, 4·5; heat, 3·8; spark, 8.			Angle, 41°; circle, 6-in skin; amperes, 17; milliamperes, 6; heat, 36; spark, 7.		

Filtered and unfiltered curves nearly the same. Absorption of most of the rays is due to the beef. Curves obtained with rotating tube through beef all drop in the last inch. This is not due to faulty colour estimation as it occurred in three separate experiments, even after adjusting the apparatus.

CHART TO ILLUSTRATE RADIATIONS FROM ROTATING TUBE THROUGH AIR AND BEEF.

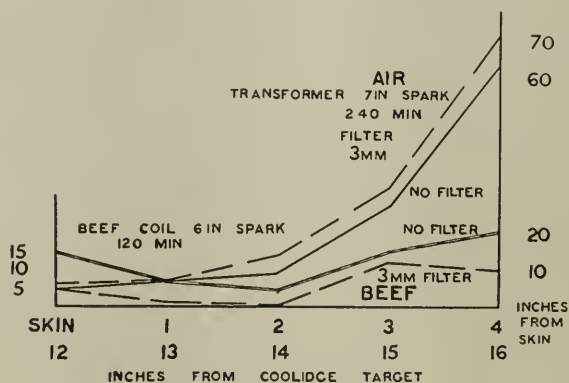


FIG. 34.

In conclusion, I wish to express my thanks to Mr. St. George Caulfeild for the valuable help he has given me in constructing the rotating tube-stand, and to both Mr. Caulfeild and Mr. Westlake for the trouble they have taken in conducting these experiments. My thanks are also due to Mr. C. E. Holland, M.A., for the help he has given me in the preparation of the radiographic part of this paper, and especially for the lantern slides of the radiograms which he has made. I trust I have been able to demonstrate the value of the Coolidge tube



in radiography and X-ray therapeutics. The experiments dealt with in this paper could have been carried out with no other tube in present use. The advent of the Coolidge tube has opened up many interesting fields of experimental research, and it is my hope that members of this Section will combine in an endeavour to standardize X-ray exposures and therapeutic dosage, possibilities which the new tube seems to justify.

Sir JAMES MACKENZIE DAVIDSON.

I do not know that I can contribute much to this discussion, because I had an unfortunate experience with my Coolidge tube. I got one of the early ones, and it punctured almost immediately, and had to be sent back to America, and I only got it back a little time ago. To-night I propose to show you some photographs of the tube which have surprised me, and I thought they might interest the members of this Section.

The tube was placed on a stand, and the pin-hole was made through a sheet of lead. The diameter was 2 mm., and it was punctured with a needle 2 mm. in diameter, and with a rymer at each side of the plate it was rymered down, so that the pin-hole was at the end of two openings. That could be put into any desired position. The plate was put as a lid on a lead box, so that no scattered rays affected the plate.

Here is a pin-hole photograph, and you will see that a large amount of rays is given off from the whole target. That accounts for the lack of sharpness in some of the photographs which I have taken with this tube: this tube gives off X-rays right to the very stem of the target. X-rays are given off from the back as well as the front. I cannot explain that, but such is the fact. With a 20-min. exposure you see just an indication of the glass. The glass gives off X-rays very feebly. With the ordinary tube the rays do not spread on the target, but the glass of it gives off a good many more X-rays than does the Coolidge tube as can be seen in this pin-hole photograph on the screen.

I wanted to see whether the scattered rays on the target stem were easily stopped, therefore I covered a plate with tin-foil before taking the photograph. The rays were merely a little weakened, otherwise they passed through the pin-hole. I show you a representation of the pin-hole photographs which you can see stereoscopically.

The use of the Coolidge tube is somewhat dangerous unless the

operator protects himself; there is no part of the tube near which you can stand without exposing yourself to the rays—rays more or less powerful.

I believe that some workers have complained of want of definition in X-ray photographs with it, but from Dr. Knox's remarks I gather that he has not experienced this. I do not know what type of tube he uses, but if there is blurring in ordinary photographs with the Coolidge tube, I think it is explained by the fact that many more X-rays are diffusely sent off from the whole target; and the fact that the rays come off so richly from the back of the tube makes it unsafe to stand behind the tube, since it does not shield the worker.

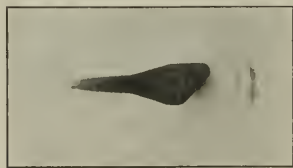


FIG. 1.



FIG. 2.

Fig. 1.—Pinhole photograph of a Coolidge tube in action. The photographic plate was enclosed in opaque paper envelopes as used in X-ray work. This photograph shows that X-rays are given off by the cathode and by the whole surface of the target or anode, extending even along the supporting stem.

Fig. 2.—This photograph was taken in the same way as fig. 1, only that the tube was placed obliquely—the exposure was less—and so the intense spot indicates the principal focus on the target, but there is the same diffuse production of X-rays spread over the surface of the target and along its supporting stem.

DR. HARRISON ORTON.

I have made no very special experiments with the Coolidge tube, but I have used it for fourteen months for deep therapy. I feel sure that nobody who has used this tube for deep therapy will return to the ordinary X-ray tube. Firstly, there is an enormous saving of time, for without unduly pressing the tube or over-heating it, it is easy to turn a pastille in two minutes. Secondly, there is the possibility of always being able to adjust the tube to give off the same quality of ray as in a previous case, and so you can repeat an exactly similar dose in any given case.

Dr. Knox did not make any special mention of dosage, and it may be interesting to state that for cases of ringworm (using Hampson's scale), if the pastille is turned to tint 14, it is ample, with the Coolidge tube, to produce epilation without redness. Working with a 3-mm. aluminium filter, I have been accustomed, when using an ordinary X-ray tube, to give a dose at one sitting of 20 X in uterine conditions and when treating tumours through the skin, but I find you cannot do this with safety when using a Coolidge tube: you should go a point or two below this in order to avoid dermatitis. But by giving 10 X in one dose, and after an interval another 10 X, the 20 X dose is given without causing dermatitis.

In starting a Coolidge tube it sometimes behaves rather peculiarly until the anticathode has become hot. It is advisable therefore to start with a current of about 4.4 amp. through the filament, and continue that until the anticathode is hot, before passing a heavy current through the tube. I think that a failure to do this is responsible for some of the punctures which have occurred. Another point is, that before the anticathode becomes thoroughly heated, the tube is not absolutely constant, and if you adjust the tube for a certain penetration, then when the anticathode becomes very hot the penetration will fall somewhat, and it should be readjusted. But if when the anticathode is thoroughly heated you adjust it, it will then run for hours on end with a current of 4 ma. or 5 ma. running through it, without any change in penetration of the rays. It is said by the makers, and probably it is true, that when the anticathode is cold the tube suppresses inverse current: but this is not true when it is heated. When this is the case the tube will behave as badly as any ordinary one if the inverse current is not thoroughly cut out. Since all valve tubes cut off some current in the right direction, I think that the new instrument which Sir James Mackenzie Davidson has described will be very useful in enabling us to get more power.

I have not done therapy with the electrolytic break, but with a large mercury break, 20-in. coil, and penetration of 8 to 9 Benoist, I cannot get more than 4 ma. or 5 ma. through the tube.

With regard to the cost, the original tube I got fourteen months ago I still have in use. About six months ago it began to behave peculiarly, and that is what first drew my attention to the valves being defective and some inverse current getting through. As I thought it would break, owing to its irregular behaviour, I ordered another. However, on putting in new valves the old tube has run quite steadily, and I have not yet had to use the new one.

With the Coolidge tube one requires no apparatus for running water to cool the tube, neither does one need a rhythmic interrupter, and I think the cost will not be found to be excessive. With care, the life of this tube seems to be a very long one. I have never forced mine. I know some pass as much as 10 ma. to 20 ma., but for therapy I do not pass more than 5 ma. or 6 ma., and with that current the tube has been in almost daily use, and has run steadily for over a year.

Dr. N. S. FINZI.

I am afraid I have no figures nor experiments to bring before you on this subject: my observations on it will be purely clinical. With regard to radioscopy, I find one can get fairly good results with the Coolidge tube, but I am not satisfied that one can obtain the very best detailed work with it compared with the ordinary tube with a tungsten target. I do not think one gets the same fine detail in the case of the lungs, for instance. For bismuth meal work, of course, the Coolidge tube is ideal. Whether it is the bluntness of the focus, even in the fine focus Coolidge tube, or the secondary rays given off from the tube, or the fact that one does not get the same spectrum of rays, I cannot say. I am inclined to suspect that some of the medium soft rays which one gets from the ordinary X-ray tube are not so numerous, that is to say, the rays which are most useful for taking delicate negatives. One can get exceedingly soft rays and exceedingly hard ones, but whether with this tube we get so much of the middle ratios I am inclined to doubt. I have no figures upon which to base this statement: it is an impression, and I hope physicists will be able later on to enlighten us upon the point. If one has a feeble apparatus, no doubt it is better to use a Coolidge tube, because it has nearly double the efficiency for the same current through the coil as compared with the ordinary X-ray tube; at any rate it is considerably more efficient.

With regard to radio-therapy, I am afraid I have been working on rather heroic lines—that is, I have been getting the dosage down to as short a time as I could conveniently manage. My ordinary time now for a pastille dose (5 H), measured through  $3\frac{1}{2}$  mm. of aluminium, is  $2\frac{1}{2}$  min.; for 7 H. it is  $3\frac{1}{2}$  min.; for 8 H. 4 min. The same amount of rays used without a filter give a pastille dose in 50 sec. I have successfully used this for epilation: the whole five areas of exposure, with the use of the Kienböck method, were done in well under half an hour. The exposure times varied from 48 sec. to 54 sec. each, the variation



being due to the fact that the tube was hotter for some exposures than for others. As the target gets hotter the tube gets softer, unless one alters the heating current. The current I passed through for these exposures was 10 ma. to 15 ma. Of course, the anode gets absolutely white-hot, and glows like a tungsten lamp, and in order to keep the glass of the tube cool, I have got an air-blower. If I were setting up a new one, I should have an air-sucker, on the principle of the vacuum cleaners; this would have the additional advantage of sucking away the ozone and the nitrous fumes which attend this work.

As to the dosage, I agree with Dr. Harrison Orton that one cannot give quite such a big dose of filtered rays with the Coolidge tube as with the ordinary tube. I used to find that I could give about  $7\frac{1}{2}$  H. to 8 H. to most patients with the ordinary tube, but with the Coolidge tube this goes down to about 7 H.

I am convinced that no one who has used the Coolidge tube for treatment will care to go back to ordinary tubes, unless it can be proved to them that the Coolidge will not give as good results. I think the results are neither better nor worse than those we used to obtain with the use of the older tubes, but the conveniences of working with the Coolidge tube are enormous: you can set your tube to do a certain thing in a certain time, and you can rely on it doing the same thing each time.

With regard to the break to be used for a Coolidge tube, I have used the mercury break and I have used the Wehnelt break, and I infinitely prefer the last-named: the steadier voltage which one gets with it seems to suit the Coolidge tube better, and one can work it harder without sparking round the tube. With the mercury break I find a great tendency towards sparking round the tube, or across the alternate gap, if the tube is worked hard. I find that if a mercury break is used the best method of working is to use very rapid interruptions and a low induction in the primary of the coil. For therapy with the ordinary tube I prefer the highest induction and the slowest break, but with the Coolidge tube the conditions seem to be reversed. Another observation is, that if you are using a mercury break and, keeping the heating current through the spiral constant, you increase the current through the primary of the coil, the milliamperes in the tube decrease, because the secondary voltage is increased and the tube gets harder. If you are using a Wehnelt break, this does not occur to the same extent—i.e., the secondary voltage is not increased to the same extent by increasing the current in the primary.



As to cost, my first tube lasted me four and a half months, and then the filament broke : I think it was broken when I received it, but not badly enough to prevent working. My second tube failed two days ago, having lasted me thirteen months, and then it went wrong by accident : I was doing some experiments with it and sparked it. I think the Coolidge tube will not be found to be more expensive than the ordinary tube.

Dr. SABERTON (Harrogate).

I have not made any experiments with the Coolidge tube, so my remarks will be purely clinical and confined to a narration of my experiences with this tube. Speaking from the radiographic point of view, I have not succeeded in obtaining so good a negative with the Coolidge as with an ordinary tube ; there appears to be a lack of definition or sharpness and a want of fine detail. This may be due to the fact that the tube used was one of the first to come over to this country, and had a very diffuse focus ; perhaps later models have a sharper focus. Another explanation may lie in the fact that the rays emitted by the Coolidge tube are of a uniform type or vibration, whereas the vacuum of an ordinary X-ray tube will vary during an exposure and emit rays of varying rates of vibration ; this latter characteristic appears to me to be a valuable factor in the production of a good negative. I have had considerable experience in the therapeutic use of the Coolidge tube. In cases of tinea I have found it necessary to push the dose beyond the Sabouraud B tint in order to produce epilation, my experience in this respect being exactly the converse of that of Dr. Orton and Dr. Finzi. The cases of tinea have been treated with fairly soft rays corresponding to a reading of 6 on the Bauer qualimeter. I gave the usual Sabouraud dose to the first three or four patients and failed to produce epilation. In the next two cases the Sabouraud B tint was exceeded ; this dose produced a partial epilation. To the next patient I gave about  $1\frac{1}{4}$  Sabouraud, and this produced complete epilation without any erythema. I fail to understand why one worker finds it necessary to give less than the usual epilation dose and another has to augment the dose beyond the normal to obtain the same result.

In cases of pruritus I find unfiltered rays between 6 to 7 Bauer most successful. For purposes of deep therapy the Coolidge tube appears to me to be ideal to work with, and one obtains most gratifying results. I am accustomed to run the tube with an alternative spark-gap on my

apparatus of 27 cm. to 30 cm., corresponding on the Bauer scale to a reading between nine and ten units, this being the maximum output of the apparatus. With 3 ma. passing through the tube and measuring the dose after filtration through 3 mm. of aluminium, it takes me 10 minutes to obtain a full Sabouraud dose. I have repeated this dose once a fortnight on the same area of skin and it produces excellent pigmentation.

I was very pleased to hear Dr. Knox remark on the desirability of standardization of dose in X-ray therapy. I regret that so many of our members still appear to measure their dose on the tube side of the filter, and cannot understand why this practice is persisted in, especially as it is easier and, in my opinion, more scientific to measure one's dose after filtration.

With regard to the terms "penetration" or "penetrating rays," these have always appeared to me unfortunate expressions. I may be dull, but I fail quite to understand it. One can appreciate the fact that a certain proportion of the rays, whether soft or hard, are absorbed in passing through the tissues, but I do not understand the statement that soft rays are absorbed by a few centimetres of tissue. One is accustomed to take radiograms of the thickest part of the body with a soft tube; there is no question of penetration—we get our radiogram.

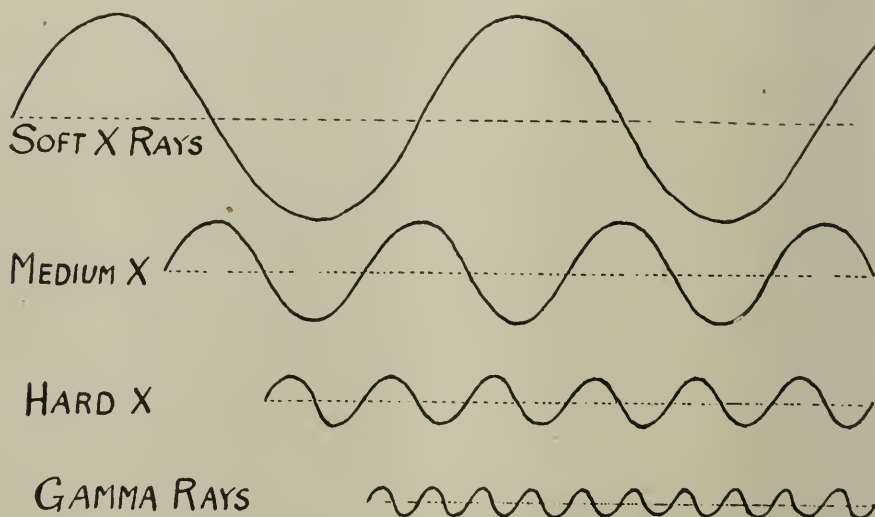
Dr. Knox ruled out the high-tension transformer with the Coolidge tube for therapeutic work, though we are told it is excellent for radiographic work. I presume this is due to the high-tension transformer having insufficient voltage to back up a 10-in. or 12-in. alternative spark-gap on a Coolidge tube, and shortening between the high-tension collectors. Provided a high-tension transformer will back up a 10-in. alternative spark-gap, I do not see why it should be ruled out for work with the Coolidge tube. My work with the Coolidge tube has, so far, been done with a specially wound 16-in. coil and large Dreadnought interrupter.

Dr. Finzi appears to be going in for speed work in therapy, but I am not sure that in this the time factor is not an important one. Assuming that one gave a "flash" treatment, would it have the same effect as an exposure of 8 min. or 10 min., although the amount of rays in both cases received by the tissues is the same? Personally, I should not expect the effect on the cells to be the same as with a time exposure.

As regards cost, the Coolidge tube well repays its expense. I have used one daily for seven or eight months, but unfortunately a few days ago it was accidentally broken. Apart from an accident, I see no reason why it should not have run on indefinitely.

Dr. S. Russ.

I am very much interested in the discussion on the Coolidge radiation, and I should like to show you a slide which deals with an aspect rather different from those which have been touched upon in the discussion. To some of us, the great interest in the advent of the Coolidge tube is the possibility it has opened up of producing X-rays of the same penetrative power as the gamma rays of radium.



Scale of wave-lengths; 1 cm. represents 0.71 Angström unit.

The possibility of producing X-rays of the same wave-length as the gamma rays was put to a very stringent test by Professor Rutherford and his colleagues in Manchester, and they published their results in the *Philosophical Magazine* of last September. They found that whereas they could produce X-rays of as short wave-lengths as the gamma rays of radium B, the minimum wave-length they obtained was rather more than twice that of the hardest gamma rays from radium C. The diagram shows the difference in the wave-lengths of soft, medium and hard X-rays. The ratio of wave-lengths for the hardest gamma rays from radium C and the hardest X-rays which have so far been produced is as 0.71 is to 1.72. The authors give good reasons, in their paper, for believing that there is very little possibility, under ordinary experimental conditions, of producing X-rays of shorter wave-length than  $1.7 \times 10^{-10}$  metres.

With regard to one or two questions which have been raised in the discussion, I have been interested to find that several observations I made myself, purely from the experimental side, have been noticed by other people. Two were especially noted by Dr. Orton: firstly, that as the tube runs, there is a slight drop in its working potential difference. It does drop in voltage, and you have to readjust your current in order to maintain the character of the radiation. There is also the question of it carrying the inverse current. I remember that when I got my tube, which I have had for eighteen months, the manufacturer told me that one feature of the tube was that it would not carry an inverse current. So when it had been running a few minutes, I suggested that we should try it in that particular; the result was that the milliamperes registered a slight increase on reversing the direction of the current.

With regard to the point Dr. Finzi raises, as to the difference in the composition of the beam in the two kinds of tube, personally, I think that if there are differences, with the same strength of current, they are very small, not sufficient to account for the differences he noticed radiographically. I should be much more inclined to attribute those differences to the fact which Sir James Mackenzie Davidson brings out in his pin-hole photographs—namely, that there is radiation from the whole stem of the anticathode. This very point which Sir James brought forward was noticed by Mr. Coolidge himself, because in a recent paper in the *American Journal of Roentgenology* he suggests, as an explanation, that the electrons, as they come from the heated spiral, are repelled from the negatively electrified glass surface and so find their way to the anticathode and there produce X-rays.

I should like to add that my personal feeling is that we owe a great debt of gratitude to Mr. Coolidge for his invention. I think it is the most wonderful X-ray tube that has ever been constructed, and I feel sure that this is only a beginning of what we are going to hear of the Coolidge tube in the future.

Dr. HARRISON ORTON (in reply).

I cannot give any explanation of the different experience as to dosage. A short time ago Dr. Codd came to see me about dosage, and I told him 14 on the Hampson scale was suitable for epilation, and he tells me this evening that he has always used this dose for epilation and found it quite successful. Dr. Finzi also tells me he finds it



desirable to go below the ordinary dose for filtered rays. I do not know why Dr. Saberton should have to give so much. I have myself never exceeded the Sabouraud tint B in a case of ringworm (i.e., 16 on Hampson's scale), and I should not consider it safe to do so with any tube, presuming, of course, that the pastille is exposed in the usual position.

Dr. N. S. FINZI (in reply).

With regard to the time factor, I think it makes not the least difference. I seem to be getting the same results in  $2\frac{1}{2}$  minutes with this tube as I formerly obtained with the old type of tube in 10 minutes. And if you could get the same into the tissues with a flash I do not see why the result should not be the same. I think the dosage difference as between Dr. Orton and Dr. Saberton is probably due to working the tubes at a different hardness, and the fact that hard rays come out in much larger quantity from the Coolidge tube as compared with the ordinary tube. I think it is really a difference in the spectrum, which varies very much according to the degree of hardness.

Dr. ROBERT KNOX (in reply).

Most of the questions raised have already been dealt with. I agree with what Sir James Mackenzie Davidson has said with regard to protection from the rays when using the Coolidge tube: we cannot protect ourselves too much.

I agree with Dr. Orton as to the desirability of giving two 10 X doses instead of one dose of 20 X, and I prefer to wait a day or two before giving the second dose.

I am sorry to have given Dr. Saberton a wrong impression; I have simply ruled out the high-tension transformer used in the experiments. I believe in this kind of transformer so long as you can back up the tube and get a long spark-gap, and I think that they do that in America. We were unfortunate in having a transformer which was not properly attuned. I shall have some alterations made in it, and then I hope it will work very well.



## Electro=Therapeutical Section.

President—Dr. W. IRNSIDE BRUCE.

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### New Treatment of Tuberculosis.

DR. GEORGES TH. PANOUPOULOS, of Athens, has sent to the Section the following note of what he believes to be a new treatment for tuberculosis, in the hope that members will try it and communicate their results to him. Members who are interested in the subject may communicate direct with: Dr. Georges Th. Panoupoulos, Chateaubriand 12, Athens.

The procedure consists in the destruction of Koch's bacillus by means of ozone formed in the lungs under the agency of the inspiration of pure oxygen and the simultaneous action of hard X-rays.

The application of my method is as follows:—

Before radiation is started the patient is made to breathe pure oxygen by means of an apparatus which is made so that the entry of atmospheric air into the lungs is prevented. This is of very great importance, because the presence of the large amount of nitrogen contained in the atmospheric air, during the action of the rays, would convert the nitrogen in the lungs into oxide of nitrogen, which would have an injurious action on the tissues. After five minutes' inspiration of the oxygen, the action of the hard X-rays or other radio-active bodies should be commenced, the patient continuing to breathe the oxygen throughout the treatment.

In order to avoid X-ray burns, a filter consisting of an aluminium plate 0.5 mm. to 2 mm. in thickness should be interposed.

The duration of the action of the X-rays, the intensity of which varies from  $2\frac{1}{2}$  H. to 8 H. at each sitting, is from ten to twenty minutes.

The X-rays (or other radio-active substances) convert a very small portion of the pure oxygen which fills the lungs into ozone, the bactericidal action of

which is well known. To this very important action of the ozone must be added the combined action of radio-activity and of the oxygen, which insures :—

(a) The increase of the beneficial oxidation of chemical combinations of the blood (hæmospherine, hæmoglobin, &c.), due on the one hand to the presence of a large quantity of oxygen, and on the other to the ionization of the oxygen, the oxidizing and diffusive action of which is increased as a result of the catalytic action of the X-rays.

(b) The influence brought to bear on the tubercles and the bacilli themselves.

I began the application of this method of treatment with the approval of eminent physicians of Athens in the month of March, and the results I have obtained are very encouraging.

As soon as I am in a position to work with two apparatus at once I shall induce the X-rays from two sides, so as to insure a "cross-fire" action of these rays in the lungs. I have every reason to believe that I shall then have increased not only the quantity of formed ozone, but also the activity of the X-rays and of the oxygen.





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