MARY HESSE

Hermeticism and Historiography: An Apology for the Internal History of Science

In her pioneering study of the natural magic and hermetic tradition in relation to sixteenth- and seventeenth-century science, Frances Yates specifically disclaims the intention of contributing to the history of science proper: "with the history of genuine science leading up to Galileo's mechanics this book has nothing whatever to do. That story belongs to the history of science proper . . . The phenomenon of Galileo derives from the continuous development in Middle Ages and Renaissance of the rational traditions of Greek science."[1] More recently, however, she has made bolder claims for the relevance of the hermetic tradition:

I would thus urge that the history of science in this period, instead of being read solely forwards for its premonitions of what was to come, should also be read backwards, seeking its connections with what had gone before. A history of science may emerge from such efforts which will be exaggerated and partly wrong. But then the history of science from the solely forward-looking point of view has also been exaggerated and partly wrong, misinterpreting the old thinkers by picking out from the context of their thought as a whole only what seems to point in the direction of modern developments. Only in the perhaps fairly distant future will a proper balance be established in which the two types of inquiry, both of which are essential, will each contribute their quota to a new assessment.[2]

These two quotations serve well to introduce some issues in the historiography of science which deserve attention from philosophers. My interest

AUTHOR'S NOTE: This paper originated from discussions in the Research Seminar on "Science and History" at King's College, Cambridge, organized by Dr. P. M. Rattansi and Dr. R. M. Young. I am glad to express my indebtedness to them and to other participants in the seminar, especially Dr. C. Webster and Miss F. A. Yates, but I would stress that they are in no way responsible for the views expressed here.

[1] *Giordano Bruno and the Hermetic Tradition* (London: Routledge and Kegan Paul, 1964), p. 447.

[2] "The Hermetic Tradition in Renaissance Science," in *Art, Science, and History in the Renaissance*, ed. C. S. Singleton (Baltimore: Johns Hopkins Press, 1968), p. 270.

in them was first aroused by a practical demarcation problem in the history of science. As the discipline has emerged from a certain scientific parochialism to take its place in general history, it is inevitable that some tension has arisen between the so-called "internal" and "external" approaches to the science of the past. On the one hand historians in the tradition of Cassirer, Collingwood, and Koyré have tended to regard the history of science as the history of rational thought about nature, evolving according to its own inner logic, and requiring for its understanding only the attempt on the part of the historian to "think the scientist's thoughts after him," what Miss Yates calls the "continuous development . . . of the rational tradition." This type of historiography of science has produced such masterpieces as Duhem's *Le Systéme du Monde*, Burtt's *Metaphysical Foundations of Modern Physical Science*, and Koyré's *Etudes Galiléennes*, and the bulk of papers which have appeared in the specialist journals in the last few decades. On the other hand, there is the view of science as an irreducibly social and cultural phenomenon, subject alike to rational and irrational influences, to magic as well as mathematics, religious sectarianism as well as logic, politics and economics as well as philosophy, and which is itself one of the major causative influences upon the general historical scene and inseparable from it.

These views are not in themselves incompatible, and no one would wish to deny that there is truth in both of them. But two further types of consideration have tended to bring them into conflict. The first is the claim frequently made by philosophers of the history of science that some particular view of the nature of science is implicit in every study of its history, and the second is the claim made or implied by some proponents of the integration of science with general history that the notion of its internal history as a history of pure concepts independent of "nonrational" factors is a delusion. The passages quoted from Miss Yates are themselves examples of both these claims, since they imply some specification of the "rational tradition" which is contrasted with other factors influencing scientific development, and suggest additionally that the history of this tradition has been distorted by being written from a forward-looking point of view "picking out . . . only what seems to point in the direction of modern developments." Both these elements in Miss Yates's analysis depend on an implicit philosophical position with regard to the nature of science, and the second calls in question the autonomy of internal history.

A more explicit statement of the effect of a philosophy of science on the

historiography of science is given by A. R. Hall. Discussing Merton's influential sociological interpretation "Science, Technology and Society in Seventeenth-Century England,"[3] he describes Merton's view that long-time changes in science are primarily to be ascribed to social factors, and contrasts this with the implicit view of the intellectual historians that new intellectual attitudes are not "generated by or dependent upon anything external to science . . . the history of science is strictly analogous to the history of philosophy." Hall goes on:

Profoundly different historical points of view are involved. . . . To suppose that it is not worth while to take sides or that the determination of the historian's own attitude to the issue is not significant is to jeopardise the existence of the historiography of science as more than narration and chronicle. For example: how is the historian to conceive of science, before he undertakes to trace its development; is he to conceive it as above all a deep intellectual enterprise whose object is it [*sic*] to gain some comprehension of the cosmos in terms which are, in the last resort, philosophical? Or as an instruction-book for a bag of tricks by which men master natural resources and each other? . . . I have deliberately given an exaggerated emphasis to these rhetorical questions in order to indicate the violent imbalance between two points of view that one simply cannot ignore nor amalgamate.[4]

The second claim, that the notion of an independent internal history of science is a delusion, has been pervasive in recent literature, and is even beginning to act as a subtle disincentive to young scholars against working in the more traditional areas of the history of scientific ideas. Attempts to integrate the external and internal approaches abound, but detailed and critical analyses of the claim that internal history is inadequate are more difficult to find. Two recent brief statements of it may be taken as typical. The first occurs in Christopher Hill's reply to debates centering round his analysis of the role of puritanism and capitalism in the scientific revolution: "I am impenitent in my conviction that it is right to try to see society as a whole, and wrong to consider men's work and thought as though they existed in separate self-contained compartments."[5] The second occurs in Robert Kargon's Preface to his *Atomism in England from Hariot to Newton*:

. . . most historians of atomism . . . deal with their subject as if it existed, so to speak, in a void. In these works, atomism is treated as an ideologi-

[3] *Osiris*, 4 (1938), 360.
[4] "Merton Revisited," *History of Science*, 2 (1963), 1.
[5] "Debate: Puritanism, Capitalism and the Scientific Revolution," *Past and Present*, no. 29 (1964), 97.

cal development of a few major figures. Absent are truly *historical* relations between men and ideas; all stress is placed upon internal philosophical and scientific developments . . . Atomism becomes a concept developed by philosophical titans and not *real men*, facing *real problems*—social, political, theological, and personal, as well as scientific.[6]

These various examples are enough to show that the issues between internal and external history of science are also issues involving the relations of philosophy and history of science. Those who see a philosophy of science in every history of science may look to it to provide the definition of "genuine science" which serves to demarcate internal history, and if possible to guarantee its autonomy. Or, if their philosophy of science is a pragmatic and instrumental one, they may use it to demonstrate the nonindependence of internal history, for if science is an epiphenomenon of society or technology, then necessarily autonomous internal history is a delusion. The two claims tend, however, to work in opposite directions with respect to the relations between the history and the philosophy of science. The first imposes on historians the duty of being self-conscious and critical about their implicit philosophy, while the second results in increasing affiliation between history of science and general history rather than either science or philosophy, and consequently loosens the tie between the history of science and the philosophy of science.

These implications of the two claims are my excuse for embarking upon a discussion of internal and external history on this occasion, although I fully realize that it is presumptuous of a philosopher who does little more than keep up with trends in history of science to attempt to pass judgment

[6] Oxford: Clarendon Press, 1966, p. vii. Other comments on the relation of internal and external history are more guarded on the question of internal autonomy. Reviewing L. Pearce Williams's *Michael Faraday*, T. S. Kuhn notes that his "predominant concern" is "with Faraday's scientific ideas and their philosophical background," and comments that the "extrascientific events in Faraday's life" might have been exploited to give a more plausible picture of Faraday the man, without suggesting that Williams has failed to reveal an intrinsic connection between these extrascientific events and the scientific ideas themselves (*British Journal for the Philosophy of Science*, 18 (1967), 148). In even more tentative vein, Henry Guerlac deplores excessive introspection, on grounds of public relations with historians: ". . . if Syracuse does little to explain Archimedes, perhaps Greek culture as a whole may do so at least in part. And certainly, for the general historian, Archimedes does something to explain Syracuse. . . . if we concentrate exclusively on what has been called the internal history of science, on the filiation and unfolding of scientific ideas and technics, we may end up writing for ourselves alone, or for ourselves and the philosophers of science." A. C. Crombie, ed., *Scientific Change* (London: Heinemann, 1963), p. 876. See also A. W. Thackray's article "Science: Has Its Present Past a Future?" for a stimulating characterization of internal history as itself a product of social withdrawal on the part of twentieth-century historians of science.

on a domestic debate among the historians. However, one of the functions of philosophy in relation to history of science may be to suggest a few conceptual guidelines through what is a very complex and many-sided question.

To make that question manageable, I shall consider in particular in the last sections of this paper certain aspects of seventeenth-century science, where various new interpretations, of which Miss Yates's is one, are currently in the field alongside the internalist view we have inherited from Burtt, Koyré, Dijksterhuis, Butterfield, and their successors. What that internalist view is can be sufficiently indicated by reference to the chief characters of the story: Copernicus, Kepler, Galileo, Descartes, Boyle, and Newton, who are pictured as engaged, with supporting cast, in metaphysical, theoretical, and experimental argument whose internal rational structure is relatively independent of personal biographies, cultures, and politics. But first I shall consider some more general philosophical points arising from the relationship of external and internal history.

II

Out of the complex of issues already raised it is useful first to make a distinction between the historical *occasions* upon which scientific developments take place, and the *character* of those developments themselves. That there should be any activity describable as scientific obviously depends on a certain stability in some part of society, a certain degree of literacy, and a certain desire for intellectual pursuits. All these conditions are closely dependent on social environment. But in themselves they do not necessarily produce any conflict of interests between the external and internal approaches to science, or threaten the internal autonomy of scientific ideas. These more intimate relationships are involved in the question of how far external or nonrational factors influence or determine the character of the science done and the scientific conclusions reached, and it is this question that I shall be exclusively concerned with here.

A further distinction in the kinds of "external" factors involved is suggested by comparing the comments quoted in the last section from Hall and Yates. Hall contrasts internal and external in the traditional terms of intellectual and social; Miss Yates on the other hand implies a more subtle classification, in making the distinction not so much in terms of thought in general as opposed to social pressures, as between a particular kind of thought, the "rational tradition," and a variety of other kinds of mental or

ideological influence. It is tempting to express Hall's distinction in terms of the current philosophical distinction between *reasons* and *causes*: social, political, and psychological factors act as causes (or partial causes) of the contemplation and acceptance of particular kinds of scientific theories, while what have traditionally been called internal factors provide reasons, and would serve to define Miss Yates's "rational tradition." But between these two Miss Yates suggests a third: the influences coming from an alternative tradition of thought or ideology, which can hardly be called mere unconscious causes, and which she would yet wish to distinguish from rational factors. But if the distinction between the occasion and the character of scientific development is kept in mind, the distinction between Hall's and Yates's types of "external" factor is seen to be more apparent than real. If some social, political, or psychological factor is to influence the character of scientific theory, there must be some sense in which it becomes an object of thought, even if perhaps it has to be called an "unconscious reason," or a rationalization, or even a bad, though conscious, reason. Some examples may help to illuminate this point.

Miss Yates's ideological tradition of natural magic and hermeticism undoubtedly provides in principle material for intellectual factors influencing the character of science which are by no means unconscious or merely causal, and which their proponents would regard as reasons, though Miss Yates seems to regard them not just as bad reasons, but even as *irrational* (not "genuine science"). On the other hand, some factors which look purely causal and unconscious at first sight may on more careful inspection reveal intellectual and rational components. For example it is suggested that familiarity with practical machines was a partial determinant of corpuscular mechanism, and that the existence of hydraulic and heat engines played a similar role in early nineteenth-century thermodynamics. If these were factors in determining the character of their contemporary science, they were not merely causal jogs on the mental processes of scientists, but owed something to the rational consideration that machines of various kinds might be macroscopic analogues for more fundamental elementary processes. There are, however, other cases where we are more disposed to regard the external factors as unconscious and therefore more nearly causal. It is interesting that the examples that come to mind are also highly controversial as historical explanations, for it is more difficult to establish unconscious causality upon scientific ideas than intellectual influence. Hill, for example, has suggested that a possible cause for Harvey's change

of mind about the primacy of the heart and the primacy of the blood may have been the actual transition from monarchy, analogous to the heart, to republic, analogous to the blood.[7] And F. E. Manuel hints that Newton may have been more receptive to the idea of attraction because he may as a child have felt himself deprived of natural affection on the remarriage of his mother.[8] We may regard Hill's suggestion as bad history, and the influence suggested, if it had been good history, as bad science, but it is not at all clear that, for Harvey, this may not have been in principle a perfectly respectable analogical argument. Only Manuel's suggestion seems to preserve pure unconscious causality.

All of which is to say that the classification into internal and external influences on the character of science is by no means as simple as it looks at first sight. We seem to require some more fundamental framework in terms of which to discuss it; therefore let us return to Hall's suggestion that the historian's assessment of the various kinds of influence will depend upon his conception of what science is, and that external and internal history are committed to incompatible conceptions. He seems to have in mind the conflicts that might arise from the claim that sociological or psychological interpretations such as those just mentioned are *sufficient* explanations of theoretical development. Such a claim would have, of course, much wider ramifications on the intellectual scene than merely in the historiography of science. Let us, however, ask a more limited question, namely, is our approach to the historical question of interaction between the various factors that influence science predetermined by some prior philosophical analysis of the structure of science? In the course of this discussion some light may be shed on the obscure notion of the "rational tradition" and on the distinction between external and internal factors.

It is certainly the case that philosophical analyses of the structure of science have an effect on its historiography, even when historians disclaim any concern with philosophy of science. J. Agassi has engagingly described

[7] C. Hill, "William Harvey and the Idea of Monarchy," *Past and Present*, no. 27 (1964), 54. C. Webster opposes to Hill's view the internal explanation that it was Harvey's "failure to substantiate the Aristotelian idea of the primacy of the heart in embryology which led him to doubt other facets of the heart's primacy." "Harvey's *De Generatione*: Its Origins and Relevance to the Theory of Circulation," *British Journal for the History of Science*, 3 (1967), 274.

[8] *A Portrait of Isaac Newton* (Cambridge, Mass.: Harvard University Press, 1968), pp. 83–85.

the unhappy effects of *inductivism* in history of science.[9] An inductivist historian has implicit Baconian philosophical allegiances, concentrates on describing "hard facts" and experiments, reconstructs past arguments to fit an inductive structure, and judges past theoretical conceptions as true or false, significant or fit for ridicule, depending upon what are now acceptable theories. This is the prime example of what Miss Yates calls history read according to premonitions of what was to come, and if it is the type of historical tradition she has in mind, she is fully justified in asking for a corrective, although not necessarily in looking for the corrective in "nonrational" factors. Inductivism also produces the type of history which most obviously leads to conflict with externalist interpretations, for if the development of science consists of exhaustive inventories of hard facts, and careful generalizations to laws and theories which are fully and uniquely warranted by the facts, then although the particular facts selected for study might depend partly on external factors, the nature of the conclusions would not. The story of science would indeed unroll inexorably and cumulatively according to its own internal logic, and any externalist claims to provide additional causal explanations would be otiose or false.

A more plausible version of inductivism which leads to the same consequences might be called *naive realism*. This holds that theories properly arrived at are simply and perennially true, without specifying that they are necessarily arrived at by strictly inductive means. They might, for example, be justified by consilience of concepts in Whewell's sense, or they might be the product of perennial metaphysical principles as in seventeenth-century rationalist science. A quasi-Kantian view of science as flowing wholly from a priori categories is another variation on the same theme. Any such view, if correct, would justify autonomous internal history, since all imply that the character of science is determined solely by intellectual factors.

But no philosopher and few historians would now subscribe to any of these views of science, and certainly none of them can be equated with the views held by twentieth-century intellectual historians. Their view seems to come nearer to what Agassi calls *conventionalist* history, in which the theoretical system of each period is seen from its own standpoint as creating its own intellectual world in which facts themselves are interpreted wholly according to criteria of internal coherence. An extreme form of conventionalism (which is the only form considered by Agassi) implies that

[9] "Towards an Historiography of Science," *History and Theory* (The Hague), 2 (1963).

scientific theories not only are insulated from external social factors, but are also immune to intellectual and empirical constraints making for modification and evolution of their closed logical systems. But conventionalism in this form would hardly be adopted by any philosopher or historian if it implies that theoretical systems are immune to change, for it is universally agreed that both facts and alternative theories make for change, and most internal historians have taken full account of the resulting problem situations and conflicts which Agassi enjoins them to notice.

In a less extreme form of conventionalism, however, it might be held that changes of world view, insofar as they can be accounted for historically at all, are *wholly* functions of intellectual factors, and this seems indeed to be the philosophy of science which Hall ascribes to the intellectual historians in the passage quoted above. Such a view would provide an alternative source for the "rational tradition" of science to that described by Miss Yates as "forward-looking" history written in the light of modern science. For what is "rational" in a particular period may be different from what is now regarded as "scientific," but it may be recoverable as rational for that period when a historian sympathetically immerses himself in the literature of the period, and learns to think in terms of its own rules of argument and to use its criteria of truth. This is the approach now frequently adopted by internal historians, but it seems to need more specification than that just given to it, for in this sense the anthropologist might even claim to find a rational tradition among the Azandi witches. What seems to be closer to the actual philosophy of intellectual historians such as Cassirer, Collingwood, and Koyré, however, is the view that what counts as rational at any period is a timeless characteristic which shows itself to the historian who follows the rationally intelligible thought of the scientists of the past, and which transcends the cultural peculiarities of particular historical periods. Collingwood's version of this view, however, sometimes seems to include the recognition that it is the historian's own thought that structures the thought of the past—the timeless rationality of history is his own; thus Collingwood comes nearer to Miss Yates's "forward-looking" history.[10]

It is tempting to characterize this last approach as *neo-Hegelian*, not only because several of its practitioners stand recognizably in a Hegelian tradition of historiography, but because it shares with Hegel's philosophy

[10] See the quotation from an unpublished manuscript in the Editor's Preface, *The Idea of History* (Oxford: Clarendon Press, 1946), p. xii.

of history the emphasis on history as the history of thought, developing by the exercise of human reason according to implicit and autonomous logical constraints. Just as Collingwood remarks that Hegel's philosophy of history is most successful when applied to the history of philosophy,[11] it might be held that it is also the appropriate approach to history of science seen as a sequence of intellectual structures developing according to internal rational criteria. On the other hand the epithet "neo-Hegelian" must be used with care, for there are at least two important respects in which Hegelian historiography differs from that of the internal historians of science. In the first place the dictum that all history is the history of thought applies to *all* history, not just to the history of intellectual endeavors such as philosophy and science. In other words, a Hegelian history of the social and political factors influencing science could be written as easily as a history of scientific ideas, for all factors, internal and external, would be seen as the products of human reason operating upon the relationships between man and the natural world. Hence, strictly speaking, Hegelian history is neutral on the issue of the intellectual autonomy of science, although this freedom of interpretation has hardly been exploited by the historians of science who stand nearest to the Hegelian tradition.

Secondly, a strictly Hegelian history of science would be more restrictive in its characterization of the "internal logic" of science than the internal historians would generally be prepared to accept. For the Hegelian logic is dialectical, and would commit internal history to the view that science proceeds by revolutionary alternations of thesis, antithesis, and synthesis, rather than by accumulation or evolution. Traces of such a view may indeed be found, for example in the title of Koyré's *From the Closed World to the Infinite Universe*, but there is little evidence in general that internal historians have structured their work according to a dialectical logic in any but the loosest sense of being prepared to accept the occurrence from time to time of radical conceptual revolutions. Koyré's own effective restriction of the source of new conceptual frameworks to *intellectual* systems already found in the history of thought (the seventeenth-century revolution as a revival of Archimedes and atomism, for example) seems rather to be a *historical* judgment about the relative importance of ideas and techniques in the seventeenth century than a product of any a priori structuring of scientific development according to a particular view of its logic.

Again, if we consider the *deductivist* analysis of science which has been

[11] *Ibid.*, p. 120.

almost universally accepted by philosophers of science until recently, it is clear that it entails no claim to internal autonomy nor any necessary conflict between the standpoints of external and internal history. This view has been characterized by a radical distinction between the sociology and psychology of science and its logic, or as it is sometimes expressed, the contexts of *discovery* and of *justification*. *How* a hypothesis is arrived at is not a question for philosophy of science; it is a matter of the individual or group psychology of scientists, or of historical investigation of external pressures upon science as a social phenomenon. The question for philosophy or logic is solely the question whether the hypotheses thus "nonrationally" thrown up are viable in the light of facts, that is, whether they satisfy the formal conditions of confirmation and falsifiability adumbrated by deductivist philosophers. Although this view places a heavy straitjacket on the philosophy of science, it appears to exert no restraint at all upon its history, much less to cause any possible conflict with the external approach. It allows historians to take seriously as scientific whatever theories were contemplated in the past, arrived at by whatever external or internal influences, and however apparently bizarre, just so long as these theories were treated according to the deductivists' logical criteria. The use of the terms "logical" and even "rational" in this analysis is indeed far narrower than in the intellectual historian's "internal logic of science," or his view of the history of science as the history of man's rational thought about nature. For deductivism characterizes all influences leading to discovery as nonlogical or even nonrational, and leaves the whole context of discovery to the efforts of the historian without offering him any criteria of distinction between kinds of influence on discovery. And since for given evidence, a theory satisfying the deductive criteria is never unique, even the kinds of concepts adopted are open to nonlogical influences. Hence within deductivism as a view of science it would even seem impossible to make the distinction between intellectual and social influences on discovery, and a fortiori no general conflict could be generated between them, and no general claim to internal autonomy sustained. Perhaps the only external view which would in principle conflict with deductivism would be one in which every deductive as well as nondeductive argument was interpreted indifferently as a psychological or sociological epiphenomenon, but surely no such approach to history has ever been seriously practiced, even among Freudian or Marxist historians.

What has in fact happened is not that a deductivist approach to internal history has conflicted with an external history, but that all forms of historical investigation, internal as well as external, have led to radical questioning of the deductive view of science itself. For they have revealed the impossibility of drawing any sharp line between the contexts of justification and of discovery, between the "rational" arguments as defined by deductivism and the psychological and cultural processes which determine what kinds of theory are contemplated, and even between the "hard facts" which must be respected as tests of theory and the way these facts were interpreted in a given cultural environment.[12] It is no accident that the current attacks upon all these entrenched dichotomies of deductivism come either from historians of science (explicitly and recently from Kuhn, but also implicitly from Duhem) or from philosophers who are deeply immersed in history of science and conduct their discussions by means of detailed case histories (Popper in his later writings, Buchdahl, Feyerabend, Hanson, Harré, Lakatos, Toulmin). Some of these writers have moved to a position similar to that described above as conventionalism, with stress on the role of intellectual and even in some cases inductive factors[13] in scientific development, but without any implication that external causes of change are excluded, or that there is any intrinsic conflict between the approaches of internal and external history.[14]

I have used the comments of various historians on the relations between internal and external history to suggest several general conclusions which may now be drawn together:

[12] Cf. T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 8.

[13] Particularly in N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958); R. Harré, *Theories and Things* (London: Sheed and Ward, 1961); M. B. Hesse, *Models and Analogies in Science* (Notre Dame, Ind.: University of Notre Dame Press, 1966).

[14] Kuhn has given an interesting analysis of the external-internal distinction from an external sociological rather than an internal intellectual viewpoint, including an attempt to explain the apparent autonomy of internal history. The account depends on his analysis of the pre-paradigm and paradigm stages of a science; in the mature paradigm stages, the scientific community is a sociological entity whose structure and practices embody the paradigm of rationality for that group: ". . . the practitioners of a mature science are effectively insulated from the cultural milieu in which they live their extra-professional lives. That quite special, though still incomplete, insulation is the presumptive reason why the internal approach to the history of science, conceived as autonomous and self-contained, has seemed so nearly successful." "Science: The History of Science," *International Encyclopedia of the Social Sciences*, ed. David L. Sills (New York: Macmillan, 1968), p. 81. This analysis, however, still evades the question whether the scientific group is to be defined in terms of rationality or rationality in terms of the scientific group.

1. The notion of internal history as generally understood involves essentially the conception of a "rational tradition" and of "intellectual factors."

2. The suggestion that the rational tradition should be defined in terms of a particular philosophical analysis of science is too simple a view of the relations between philosophy and history of science. Of the accounts discussed, only inductivism or the various forms of a priorism would provide a complete specification of rationality with respect to science; conventionalism and deductivism leave open many questions about the development of theories and about intellectual influences, questions which can only be answered historically; and what I have guardedly called neo-Hegelianism seems to presuppose an autonomous rational tradition, but little indication is given by its practitioners of how this is to be recognized, except perhaps by the subjective experience of the historian.

3. Among analyses currently adopted by philosophers of science, namely deductivism and various species of conventionalism, there is much room for feedback between historical investigations and philosophical accounts of scientific structure.

4. None of these views exclude the possibility that external factors may be partial causes of particular scientific developments, including particular concepts and theories, none claim a priori autonomy of internal history, and none entail any necessary conflict between external and internal interpretations.

5. Internal history is not necessarily history read forwards in the light of what is to come, picking out particular precursors either of modern theories or of modern analyses of rational method, for it may be practiced by "thinking men's thoughts after them" according to their own theories and criteria of rationality. But this method involves some judgment by the historian about what in the past is to count as "rationality."

No general specification of "rationality" seems, however, to be forthcoming from the philosophical analyses discussed, and therefore we cannot expect that there will be any general answer to the question whether there is a relatively autonomous internal history. All such questions must be asked in particular cases, which is to say that they are essentially historical questions. I therefore turn now to some specific recent examples in seventeenth-century historiography with these questions in mind. I wish to stress particularly at this point that what I am attempting here is *not* to make firsthand historical judgments, for which I have no competence, but

rather to ask how far the evidence cited by historians themselves supports any attack upon the received internal tradition.

III

The significance of recent studies of the hermetic and natural magic tradition in seventeenth-century science is precisely that they seem to erode the intuitive distinctions in terms of which external history and internal history have been understood, by pointing to a set of factors conditioning the character as well as the occasion of science, which if not intellectual are certainly mental, and which are also closely related to important sociological considerations. They have therefore raised sharply the question of the nature of internal history in general, and in particular the autonomy and adequacy of the received tradition of the mechanical philosophy. The suggestion does indeed arise that this tradition is an unacceptable distortion, since it fails to account for a large element of thought about the natural world that even a neo-Hegelian view would regard as relevant. Hence it is sometimes made to appear that there are fundamental objections to the notion of internal history in general. My philosophical discussion has already indicated that such general objections are misplaced; I shall now suggest that they are not supported by the evidence as cited by the historians of this particular case either.

First it should be remarked that interest in hermeticism and magic is no external imposition upon the received history of the mechanical philosophy, for many historians had previously noted odd features of this story which seemed to intrude unexplained in the internal development of concept, theory, and fact. To take just one example, in a set of essays entitled *The Making of Modern Science* in 1960, Charles Raven, alone of all the contributors who concentrated on the mathematical and physical sciences, remarked: "Immensely important as they have been in establishing a reign of law and an urban and materialistic society, [Copernicus, Kepler, Galileo, and Newton] neither initiated the emergence nor gave rise to the transformation of modern man. The heliocentric cosmology was less disturbing than the rejection of spontaneous generation, or creation as an act rather than a continuing process, and of witchcraft, astrology, and magic."[15] "Witchcraft, astrology, and magic" point better to the general climate of thought and belief in which modern science arose than do the

[15] "Living Things in the Frame of Nature," in *The Making of Modern Science*, ed. A. R. Hall (Leicester: Leicester University Press, 1960).

antiseptic details of the Democritan-Archimedean tradition. And they remind us of the context in which it is natural to find references to the writings of Hermes, Orpheus, Moses, and other pseudo-priscine authors scattered through Copernicus, Gilbert, Kepler, Wallis, and Newton, to find More immersed in the Cabala, Glanvill writing in 1666 a work entitled *A Philosophical Endeavour towards the Defence of the Being of Witches and Apparitions*, and Boyle seriously discussing the effectiveness of amulets, sympathetic powers, and the magnetic cure of wounds.

The hermetic writings were a group of Gnostic texts actually dating from the second and third centuries A.D., but believed in the sixteenth century to be contemporary with or earlier than Moses, and originating in Egypt. They consequently carried all the ancient authority so much revered in the Renaissance; they were quite non-Aristotelian in spirit and hence reinforced any antischolastic tendencies of Renaissance thought; and since they were in fact written in the Christian era, they contained some elements of Judaism and Christianity which were regarded as prophetic and so enhanced still further their authority. P. M. Rattansi epitomizes well the main tenets of hermeticism in contrast with the careful distinctions maintained in medieval scholasticism between the natural and the marvelous, the magical, and the miraculous:

> For Hermeticism, by contrast, man was a *magus* or operator who, by reaching back to a secret tradition of knowledge which gave a truer insight into the basic forces in the universe than the qualitative physics of Aristotle, could command these forces for human ends. Nature was linked by correspondences, by secret ties of sympathy and antipathy, and by stellar influences; the pervasive nature of the Neo-Platonic World-Soul made everything including matter, alive and sentient. Knowledge of these links laid the basis for a 'natural magical' control of nature. The techniques of manipulation were understood mainly in magical terms (incantations, amulets and images, music, numerologies).[16]

It is of this tradition that he says on the previous page: "It was not completely vanquished by the rise of the mechanical philosophy. Without taking full account of that tradition, it is impossible . . . to attain a full picture of the 'new science.' "

There is here, as in other unguarded comments on the hermetic tradition,[17] more than a hint of the notion that by adding to the picture all in-

[16] "The Intellectual Origins of the Royal Society," *Notes and Records of the Royal Society*, 23 (1968), 132.

[17] Cf. F. A. Yates: "If we want the truth about the history of thought, we must omit nothing" (*Giordano Bruno*, p. 204). Fortunately few historians attempt the impossible task of carrying out such injunctions in practice. Their history is infinitely more sensitive than some of their throw-away methodological remarks about it.

fluences that fed into it, of all conceivable degrees of relevance, we get nearer to some form of complete description or complete understanding of the "whole picture." But the view of history as complete description, or "telling it like it was," is an error analogous to the error of inductivism in science. It presupposes that history is a search for hard facts, which are relatively independent of each other, and that the full picture is attained by accumulating as many of these as possible. Even the claim to get nearer the full picture by adding more factors should be treated with caution. Throwing more light on a picture may distort what has already been seen, and certainly judgments of relative significance are required if the picture is not to become flat and overcrowded. The historian's task is not to spell out in tedious detail every minor writing or trivial biography of forgotten figures, or every twist and turn of the social scene which had some bearing on the personnel and institutions of science. Even Miss Yates visibly and sensibly wilts before some of the details of her magico-cabalist authors. In the present context, the historian's task is rather, first, to follow up the loose ends of the received internal tradition where necessary to explain oddities and answer unanswered questions and, second, to investigate such other factors related to the science of the period as have their own intrinsic interest, perhaps because they are opened up from the side of general history, or because they have an importance or fascination of their own. It is then a historical question whether the received internal tradition should be jettisoned or modified, and whether some relatively autonomous understanding of the nature of "rationality" in seventeenth-century science emerges.

In order to discuss this it is useful to distinguish among the elements found by historians in the hermetic complex three themes whose relation to the history of science ought to be considered separately. These are (1) the social and political affiliations of certain religious sects, and the schools of Paracelsian and Helmontian doctors and chemists, (2) the full-scale hermetic and natural magic tradition as a way of thought and life in such writers as Paracelsus himself, Bruno, and Fludd, and (3) the doctrines of extended spirits and powers of matter which persisted even in later seventeenth-century science in opposition to corpuscular mechanism.

Of the first of these factors it does not seem to be anywhere claimed that they provided more than the occasion and the motivation for certain de-

velopments connected with the new science.[18] The enthusiastic fervor, utopianism, and even revolutionary character of certain religious sects do indeed throw much light on the institutional expressions of science. Such sectarian figures as Hartlib, Dury, and Comenius helped to encourage Baconian allegiances in the early Royal Society, and the anti-establishment circles in which they, and some other founding fathers of the Society, moved go some way to explain the suspicion with which the Society was viewed by Royalists and Churchmen in the Restoration period. But none of this seems to impinge essentially on the internal tradition of the mechanical philosophy. Similarly, when Rattansi explores the reasons for the popularity of Paracelsian and Helmontian medicine in England in the mid-century, he concludes: "Paracelsus and his chemical doctrines were brought into prominence because of factors that do not belong purely to the internal history of chemistry . . . but must be referred to the larger social and political environment."[19] In other words the Paracelsian iatrochemists stand to some extent *outside* an "internal history of chemistry" which is presupposed here to be independent of the social environment which brought them into prominence. Again, in the debates which have followed the related theses of Merton and Hill regarding the influence of puritanism on seventeenth-century science, several commentators have remarked that the argument suffered from too little conceptual clarity about what was to count as "science" (and indeed as "puritanism"). Far from suggesting a restructuring of the internal tradition, these debates presupposed its existence, and the disputants were counseled to look at what had been achieved in internal history in order to acquire some internal specification of what "science" is.[20]

The case with the second and third elements of the hermetic complex is different, because here it is not a question of interacting social factors, but

[18] Apart from the classic studies of R. K. Merton, and C. Hill's *Intellectual Origins of the English Revolution* (Oxford: Oxford University Press, 1965), recent partly "external" studies of these matters are to be found in M. Purver, *The Royal Society: Concept and Creation* (Cambridge, Mass.: MIT Press, 1967); R. H. Kargon, *Atomism in England from Hariot to Newton* (Oxford: Clarendon Press, 1966); P. M. Rattansi, "The Helmontian-Galenist Controversy in Restoration England," *Ambix*, 12 (1964), 1; and C. Webster, "English Medical Reformers of the Puritan Revolution: A Background to the 'Society of Chymical Physitians,'" *Ambix*, 14 (1967), 16.

[19] "Paracelsus and the Puritan Revolution," *Ambix*, 11 (1963), 31.

[20] Cf. A. R. Hall, "The Scholar and the Craftsman in the Scientific Revolution," in *Critical Problems in the History of Science*, ed. M. Clagett (Madison: University of Wisconsin Press, 1959); H. F. Kearney, "Puritanism, Capitalism and the Scientific Revolution," *Past and Present*, no. 28 (1964), 81, and "Puritanism and Science: Problems of Definition," *Past and Present*, no. 31 (1965), 104.

of intellectual factors which might be held to be necessary ingredients of the history of science seen as the history of thought. Their close relation to the social factors just discussed has obscured the fact that the real challenge to the received internal tradition comes not so much from "external" factors concerned with social and political pressures as from within history seen as "thinking men's thoughts after them." Miss Yates excludes them from the "rational tradition," and yet they are undoubtedly in men's heads, and presupposed in much of their literature. Can an internal history which has neglected them be defended?

There are two ways in which it might be defended. The first is to peer into the internal tradition to see whether any implicit specification of "rational science" for the seventeenth century can be detected which will serve to distinguish and perhaps isolate the activities described in this tradition from hermetic thought. The second is to look for the explicit comments of each tradition on the other. Both methods are illuminating, but the first alone is indecisive, since it reveals a notion of "rationality" that is at best a loosely clustered family resemblance concept, some elements of which seem almost as remote from our views of rationality as do the tenets of hermeticism. If we are to heed warnings not to read back our methods into the past (a sin only less grievous than inductivism), it is difficult to see how to draw the line between what legitimately concerns internal history and what does not.

IV

Close attention to the intellectual context of seventeenth-century science has revealed a multiplicity of ways in which contemporary writers themselves saw the new philosophy. No simple inductivist or deductivist account or any mixture of these is sufficient to do justice to the complexity of either their theory of rationality or its practice. For striking illustration of this we need go no further than the first chapter of Professor Sabra's book *Theories of Light from Descartes to Newton*.[21] In Descartes's optics alone he finds no fewer than six interpretations of rational method:

1. The method of the *Discourse*, which is supposed to be but is not the method of the *Dioptric*.

2. The method explicitly described in the *Dioptric*, which consists of arguing from possibly false assumptions to observation; that is, it is a spe-

[21] A. I. Sabra, *Theories of Light from Descartes to Newton* (London: Oldbourne, 1967).

cies of "saving the phenomena" which is not concerned with the true nature of light.

3. One method actually used in the *Dioptric*, of deduction of phenomena from claimed metaphysical principles.

4. Another method actually used in the *Dioptric*, of circular argument from effects to causes to predictions of further effects in which supposed causes are held "sufficiently proved" by the truth of their consequences.

5. Descartes's own specification of this circular argument in terms of the ancient mathematical method of analysis and synthesis: in order to solve a problem, look for that from which a solution would result, and again what that would result from, until we come to something known or a first principle. Then by synthesis retrace our steps deductively until we reach the solution.

6. But Descartes knows that in many problems knowledge of the general principle does not give sufficient specific information about the original problem, and so he proceeds by analogy with other cases whose solutions are known.

Thus within one topic Descartes almost runs the whole gamut of "methods" proposed in seventeenth-century science. And to this list may be added several interpretations of Baconian method, both in Bacon's own work and in the tensions between the inductive and hypothetical methods in the early Royal Society, culminating in Newton's Baconian claim to "deduction from the phenomena."

In addition to all these contemporary attempts to characterize scientific method, it is clear in many expositions and apologies for the new philosophy that a certain kind of content as well as certain kinds of arguments is regarded as necessary. Science is to be mathematical, mechanist, and hence nonanimist and nonteleological; in other words it is to deal with facts interpreted in a certain way. This is seen, for example, in various discussions of the nature of the primary qualities, most consistently and pervasively in Descartes. But his is not the only system in which the conceptual substance as well as the method of the new philosophy is essential to its specification. Even Bacon, who officially rejects any anticipation of the nature of the most general forms until after the inductive ladder has been ascended step by step, seems forced by his own method to specify in advance of what kinds the general forms must be, and vacillates at various times between a quasi-atomism, a continuum theory based on density and rarity as the primary qualities, and even the Paracelsian principles sulphur

and mercury. Hooke, similarly, in attempting a reconstruction of Bacon's method in his "philosophical algebra," presupposes a fundamental theory of primary mechanical vibrations, which he elaborates not inductively but hypothetically in his explanations of elasticity and of light.[22] Method and metaphysics are inseparable in seventeenth-century science; hence it is useless to seek for any perennial paradigm of rationality in the study of seventeenth-century method.

Even "concern for the facts" cannot be taken easily as a common ingredient of the new philosophy. On the one hand Galileo expresses disinterest in the actual form of projectile trajectories, and Descartes in the actual behavior of colliding particles, once the mathematical principles are established; and on the other hand Bacon, Mersenne, More, Boyle, Digby, Glanvill, and a host of lesser figures accept as factual some instances of magic, sorcery, telepathy, and sympathetic cures, and sometimes try to rationalize them in terms of mechanical explanations. These attempts were highly artificial, and with the subsequent success of the mechanical philosophy the alleged facts themselves dropped out of the purview of science, perhaps prematurely. That mathematical and mechanical science become to some extent constitutive of facts is seen clearly in Mersenne, for whom this framework determines what is to count as a natural phenomenon—all else is to be rigorously excluded as the sphere of the supernatural and miraculous.[23]

But when all this has been said, our intuition remains that however varied may be the explicit and implicit methodologies of seventeenth-century science, they are still worlds away from hermeticism. This intuition is in fact confirmed by several examples, cited by our group of historians, of intellectual dispute between adherents of the two traditions. In fact it soon becomes clear in their work that the hermetic tradition does not provide merely an extra factor to be noted and added to the picture, but rather its importance is that it provides the occasion for some conscious self-definition of the new science in the course of vigorous repudiation of the hermetics and all their works.

This can be illustrated from several examples of dispute. In an exchange

[22] On method and mechanism in Bacon and Hooke, see my "Francis Bacon," in *A Critical History of Western Philosophy*, ed. D. J. O'Connor (London: Collier-Macmillan, 1964), p. 141; and "Hooke's Philosophical Algebra," *Isis*, 57 (1966), 67; cf. also R. Harré, *Matter and Method* (London: Macmillan, 1964).

[23] See R. Lenoble, *Mersenne; ou La naissance du mécanisme* (Paris: J. Vrin, 1943), p. 7.

of polemics with the English Rosicrucian doctor Robert Fludd, Kepler dissociates himself from the interpretation of mathematics found in the hermetic writers.[24] Kepler does indeed himself believe in a mathematical harmony of the cosmos as the image or analogue of God and the soul, but his geometry is Euclidean, his conclusions require proof, and they must correspond with facts (that is, the kind of facts Kepler inherited in Brahe's planetary observations). According to Fludd, on the other hand, Kepler merely "excogitates the exterior movement. . . . I contemplate the internal and essential impulses." Fludd *complains* that geometry is dominated by Euclid, while arithmetic is full of "definitions, principles and discussions of theoretical operations . . . addition, subtraction, multiplication, division, golden numbers, fractions, square roots and the extraction of cubes." There is, he goes on, no "arcane arithmetic," no understanding of the significance of the number 4, deriving from the sacred name of God.[25]

In less measured tones than Kepler, Mersenne devotes himself to combating the arrogance and impiety of the terrible magicians.[26] Their arbitrary numerologies do not even agree among themselves; they do not understand that words are mere *flatus voces*, merely conventional signs or sounds, not images or causes. The proportion of the planetary distances may exhibit harmony, but whether it does or not is a matter of fact, not of cosmic analogies. Moreover, astrology, magic, and the Cabala are not just harmless games, they reduce human freedom to cosmic determinism and hence are morally reprehensible. Although some alleged examples of sorcery may be facts, use of sorcery is morally detestable; the magicians are guilty of arrogance and impiety in their claim that the human intellect is divinely inspired and is the measure of things. When Fludd replies to this onslaught with equal violence, Mersenne requests Gassendi to take up the cause, and he, slightly reluctantly but for friendship's sake, drops what he

[24] The documents have been presented by W. Pauli, "The Influence of Archetypal Ideas on the Scientific Theories of Kepler," in C. G. Jung and W. Pauli, *The Interpretation of Nature and the Psyche*, English translation (London: Routledge and Kegan Paul, 1955), p. 151. See also A. G. Debus, "Renaissance Chemistry and the Work of Robert Fludd," *Ambix*, 14 (1967), 42, and "Mathematics and Nature in the Chemical Texts of the Renaissance," *Ambix*, 15 (1968), 1.

[25] Debus, "Mathematics and Nature in the Chemical Texts of the Renaissance," p. 17; Pauli, "The Influence of Archetypal Ideas on the Scientific Theories of Kepler," pp. 196, 102ff.

[26] Lenoble, *Mersenne*; Yates, *Giordano Bruno*, chapter XXII.

is doing in order to study Fludd's writings.[27] That is a measure of the externality of the hermetics at this period to the new philosophy.

Another such polemical exchange is Seth Ward's *Vindiciae Academiarum*, written in reply to an attack upon the academic activities of the University of Oxford by John Webster.[28] Webster berates Oxford for its neglect of the new science, citing indifferently as representatives of that science Bacon, Copernicus, Galileo, Paracelsus, Boehme, Fludd, and the Rosy Cross. Ward replies with careful distinctions between the true natural language or universal character "where every word were a definition and contained the nature of the thing," and "that which the Caballists and Rosycrucians have vainly sought for in the Hebrew." Hieroglyphics and cryptography were invented for *concealment*, grammar and language for *explication*. Magic is a "cheat and imposture . . . with the pretence of specificall vertues, and occult celestiall signatures and taking [credulous men] off from observation and experiment. . . . The discoveries of the symphonies of nature, and the rules of applying agent and materiall causes to produce effects, is the true naturall magick." Both Mersenne and Ward take Aristotle for an *ally* against the magicians: it is not Aristotle, rational though wrongheaded, who is the enemy of the new philosophy, but "the windy impostures of magick and astrology, of signatures and physiognomy."[29]

Rattansi characterizes the situation accurately when he contrasts "the emotionally-charged and mystical flavour of Hermeticism, its rejection of corrupted reason and praise of 'experience' (which meant mystical illumination as well as manual operations), and its search for knowledge in arbitrary scriptural interpretation," with "a sober and disenchanted system of natural knowledge, harmonized with traditional religion," and goes on: "To move from one to the other was to change one conceptual scheme for ordering natural knowledge to another, with an accompanying shift in the choice of problems, methods, and explanatory models."[30] The change of

[27] G. Sortais, *La Philosophie Moderne depuis Bacon jusqu'à Leibniz*, vol. 2 (Paris: P. Lethielleux, 1920), p. 43.

[28] S. Ward, *Vindiciae Academiarum* (Oxford, 1654); J. Webster, *The Examination of Academies* (London, 1654).

[29] Ward, *Vindiciae Academiarum*, pp. 22, 34, 36; Lenoble, *Mersenne*, p. 146.

[30] Rattansi, "Intellectual Origins," p. 139. Rattansi also notes several examples of *conversions* from hermeticism to the mechanical philosophy in the 1650's, including Boyle, Barrow, and Charleton (p. 136). But these seem to have been conversions from certain animist concepts in their theories of matter rather than from what I have called the full-blooded hermeticism of Paracelsus, Bruno, and Fludd. I shall discuss the significance of these theoretical conceptions in the next section. Again, the picture becomes

sensibility is also a contemporary view. For example Glanvill: "among the Egyptians and Arabians, the Paracelsians, and some other moderns, chemistry was very phantastic, unintelligible, and delusive . . . the Royal Society have refined it from its dross, and made it honest, sober, and intelligible . . ."[31] And Sprat's plea for a "close, naked, natural way of speaking" is directed as much at the "Egyptians" as at the Aristotelians.[32]

In view of all this, any suggestion of a *confluence* of hermeticism and mechanism into the melting pot of the new science would be a mistake. In all that constituted its essence as a way of thought and life, hermeticism was not only vanquished by the mid-century, but had provided the occasion for the new philosophy to mark out its own relative independence of all such traditions. The style of argument required in the polemics is itself significantly different from that adopted in domestic scientific disputes. It involves rhetoric and ridicule, and appeals to theological and moral principle, and sometimes political and pragmatic test. Thus, Allen Debus shows too great a tolerance and fails to highlight the element of conflict in a study of the work of Robert Fludd when he concludes: "I do not believe that it is sound to dismiss the work of these men as valueless for the rise of modern science, as has often been done, simply because they were not right in our terms. The work of Robert Fludd had been taken quite seriously in the second quarter of the seventeenth century and it had resulted in a major confrontation between the supporters of the mystical neo-Platonic universe and representatives of what we would call a more modern outlook."[33] That the hermetic tradition had a large popular following in the mid-century and even later does not amount to "influence" upon the development of rational science, much less call in question its autonomy, nor does the fact that it provoked leading protagonists of that science show that internal history which neglects it is necessarily distorted.

The error of cumulative history is one to which conventionalists are particularly prone, and its effects on historiography are the opposite from those of inductivism. Instead of selecting only those factors which lead to modern developments, the conventionalist is tempted to try to select as many factors as possible, to improve the total internal coherence of his story, as if to know all is to understand all. In his indictment of the inductivist, or whig, interpretation of history, Butterfield allowed that history must be selective, but required that judgments of importance should be the judgments of the period, not our judgments.[34] But how is one to follow this advice in the history of seventeenth-century science? Are we to take Mersenne's judgments of importance or Fludd's? To reply that the seventeenth century itself clearly accepted Mersenne's view rather than Fludd's is not sufficient, for this would be like relying on the popular verdict of Athens upon Socratic philosophy to dictate our judgments of intrinsic importance. So long as we select science as our subject matter, we are bound to write forward-looking history in the limited sense that we regard as important what we recognize as our own rationality, having some historical continuity with our own science. This does not imply, as we have seen, that we impose our own theories or even our own views of method on the science of the past. And if it seems in danger of becoming a circular definition of internal history as that which is continuous with our science according to our internal history, the only cure is to look more closely at the record to see whether the relative autonomy of internal history can be maintained in spite of possible disturbing factors. Pursuit of the hermetic hare has surely so far shown that in regard to the seventeenth century it can.

V

When all this has been said, however, it remains true that some of the language, the problems, and the concepts of natural magic persist in the new science, though in sterilized form. The oddity of the conceptual background of the internal history remains when it is read merely in its own terms, but it is now possible to treat the strange concepts as providing alternative theories, to be tested and argued for in accordance with recognized patterns of rationality. Gilbert, for example, adopts the explanatory analogy of "soul" for the magnet, but only after rejecting it for the attrac-

unduly blurred when "hermeticism" is said to be one of the traditions of "scientific inquiry" that must be taken account of (p. 140).

[31] J. Glanvill, *Plus Ultra* (London, 1668), p. 12.

[32] T. Sprat, *The History of the Royal Society of London* (London, 1667), section XX; see also section III.

[33] Debus, "Renaissance Chemistry and the Work of Robert Fludd," p. 58. See also his "Mathematics and Nature in the Chemical Texts of the Renaissance," p. 2, where he speaks of neglect by historians of science of "non-modern views of the role mathematics should play." But it is inconceivable that historians should not make judgments about that "non-modern role," and it has yet to be shown that its neglect has seriously distorted our view of the internal tradition. Miss Yates's comments on Bruno's mathematics are less conventionalist: "Bruno is not at all in the line of the advance of mathematical and mechanical science. Rather he is a reactionary who would push the Copernican diagram or a compass invention back towards 'mathesis.'" *Giordano Bruno*, p. 324.

[34] H. Butterfield, *The Whig Interpretation of History* (London: Bell, 1931), p. 24.

tion of electrified bodies on experimental grounds, and because for him "soul" is the only available principle of order and harmony which are so obviously exhibited by magnets. He does indeed quote favorably "Hermes, Zoroaster, Orpheus" as recognizing a world soul, but only to parallel his own conclusions, which come from experiments, not from philosophical speculations and ancient books: "we do not at all quote the ancients and the Greeks as our supporters."[35] Again, Bacon sometimes speaks of the powers of matter in terms of "desire," "aversion," "instinct," and in his later works inanimate bodies have "perceptions," but such expressions are usually found in the context of discussions of Democritan atomism, in which Bacon is fully aware of the difficulties of accounting for such phenomena as cohesion, impenetrability, and magnetism in terms of passive matter and motion alone.[36] Even so, his quasi-animism cost him the approval of Mersenne, who placed the line between old and new "between Campanella and Bacon on one side, and Galileo and Descartes on the other."[37]

Examples could be multiplied; a few more will have to suffice. When Henry More recants his allegiance to Cartesianism, this is not only in fear of its theological implications, but also because he sees that it will not work in detail even in mechanical examples.[38] In this he is followed by Leibniz, who introduced into mechanical philosophy that thoroughly legitimized offspring of sixteenth-century animism, the *vis viva* of mechanical systems. Even Gassendi is not a pure Democritan atomist. For example, he holds that attraction is a principle of motion equally fundamental with impulse, and explains it in terms of an attracting body emitting material rays which "grasp" the object attracted. There is no mechanical account of how this action takes place; in fact motion toward a magnet is ascribed to a quasi-soul in the object which is stimulated into motion by the magnetic emanations.[39]

Preeminently, of course, as has most recently been emphasized by J. E.

[35] W. Gilbert, *De Magnete* (London, 1600), Preface and p. 309. I have discussed Gilbert's animism in relation to his experimental method in "Gilbert and the Historians," *British Journal for the Philosophy of Science*, 11 (1960), 1, 130.

[36] For Bacon's relation to the natural magic tradition, see P. Rossi, *Francis Bacon, from Magic to Science*, trans. S. Rabinovitch (London, Routledge and Kegan Paul, 1968).

[37] Quoted in Lenoble, *Mersenne*, p. 12; see also *ibid.*, p. 30.

[38] This has been brought out in an unpublished M.Litt. thesis of P. M. L. Moir, "The Natural Philosophy of Henry More" (Cambridge, 1967).

[39] P. Gassendi, *Syntagma Philosophicum*, *Opera*, II (Lyons, 1658), p. 450.

McGuire,[40] Newton provides many examples of appeal to ancient wisdom and exploitation of nonmechanical concepts. Some of this is present in fragmentary form in the published work, but the attempt to understand its real significance in Newton's own thinking has demanded study of manuscripts neglected until very recently, and still mainly unpublished. About the light thrown by them on Newton's thought, three points may very briefly be made in the present context.

1. Those of Newton's ideas which were unavailable to his eighteenth-century successors were not historically significant for the development of eighteenth-century Newtonian science, which can therefore be studied independently of them. When such disciples as Desaguliers, s'Gravesande, Maclaurin, Musschenbroek, and Pemberton came to write epitomes of the *Principia*, these are indeed sometimes set in a theological framework, but it is one which is far removed from the explicit hermeticism of Newton's unpublished writings. The point is so obvious as to be hardly worth making that the public Newton is necessarily more significant for internal history than the private.

2. What I have called the sterilization of hermetic and Neoplatonic influences has gone very far in Newton even in his unpublished manuscripts. He does indeed suggest that the ancients attributed the power of gravity to atoms, and derived the proportion of the weights and distances of the planets from the inverse-square law for musical strings in unison, but, he goes on, they asserted this "without telling us the means *unless in figures* [my italics]: as by calling God harmony representing him and matter by the God Pan and his pipe."[41] Hence Newton's view is that we have to rediscover these things in order to decode the ancient myths which will tell us nothing on their own, although having decoded them we may indeed take comfort from the correspondence of our discoveries and theirs. Again, Newton seems to have had a conception of a continuum of being ranging from inert matter through immaterial, quasi-spiritual forces and principles to God, and it is certainly possible to find in this traces of the Dionysian celestial hierarchy of spirits ascending and descending between earth and heaven, mediated through the whole history of the concept of the Great Chain of Being. But now the terrible power of the spirits is exorcised, the

[40] J. E. McGuire, "Transmutation and Immutability: Newton's Doctrine of Physical Qualities," *Ambix*, 14 (1967), 69; "Force, Active Principles, and Newton's Invisible Realm," *Ambix*, 15 (1968), 154; "The Origin of Newton's Doctrine of Essential Qualities," *Centaurus*, 12 (1968), 233; and J. E. McGuire and P. M. Rattansi, "Newton and the 'Pipes of Pan,'" *Notes and Records of the Royal Society*, 21 (1966), 108.

[41] Quoted in McGuire and Rattansi, "Newton and the 'Pipes of Pan,'" p. 118.

laws of God are found throughout the hierarchy, and they are accessible not through mystic communion of the mind and the spirits, but through patient experimental investigation. Newton may use the image of microcosm and macrocosm, but the analogy is now that of man and created nature on the one hand, and the mind of God on the other, not that of Renaissance man bestriding the universe, his mind the measure of all things.

3. The details of Newton's philosophy of nature are not required for an understanding of the internal history of physics and astronomy any more than is Bruno's magical interpretation of the circles of Copernicus.[42] But this philosophy has its historical importance in answering a different kind of question, namely, how has one preeminent scientific thinker sought to reconcile a particular theology with a particular science? The same question might also be asked about Bruno, and has indeed been asked by Miss Yates, but just because Newton's conceptions are nearer to our own than Bruno's (and this is not a temporal matter, for Aristotle's are nearer to our own than Bruno's), Newton's proposed solution is more historically significant for us. The fortunate accident that large amounts of Newton's manuscript material survive does not so much enable us to complete the picture of early modern science, or even of Newton's own biography, as enable us to ask and answer a new set of questions which have for us their own intrinsic interest.

COMMENT BY ARNOLD THACKRAY

Dr. Hesse has presented us with an important but profoundly puzzling paper. Its importance lies in her pioneering discussion of the implications carried by the notions of "internal" and "external" history. With a masterly touch she exposes the—apparently insuperable—philosophical problems attending any attempt to construct a general defense of an autonomous internal history of science. I do not wish to comment on this first part of her paper, but simply to stand in awe of such commanding expertise. Things are different when I turn to the second, and puzzling, part of the argument.

Here, if I understand correctly, she wishes to defend on particular grounds and for a particular historical period that internalist view for which she has just denied any possibility of a general and abstract defense. But her particular defense *necessarily* demands appeal to just those general

[42] For which see Yates, *Giordano Bruno*, chapter XIII.

principles she has exposed as insufficient. Indeed, unless Dr. Hesse is resorting to that inductivism she herself deplores, I for one cannot understand how she finds it possible to defend a particular case without resort to general principles (however heavily disguised). Hence my puzzlement.

Dr. Hesse argues that for intellectual historians such as Collingwood (and presumably Koyré) "the timeless rationality of history" which they seek to impose is not in fact timeless but rather their own particular rationality. She admits that this approach is arbitrary and unsatisfactory. She also concedes that "no general specification of rationality seems . . . to be forthcoming." Yet Dr. Hesse then proceeds as if the general specification existed and the approach were adequate!

Her argument depends heavily on "the internal tradition of the mechanical philosophy"—as for instance in the statement that "the historian's task is . . . first, to follow up the loose ends of the received internal tradition." Now it is by no means self-evident (save possibly to a determined and modern-minded physicist) what such phraseology implies—the early Gilbert? the later Robert Boyle? Newton in his more theological moments? Again, Dr. Hesse's description *assumes* but does not prove that no rational(!) seventeenth-century figure curious about nature would adopt any other mode of approach. Similarly, she assumes rather than demonstrates the validity of the traditional internalist position when she writes that "so long as we select science as our subject matter, we are bound to write forward-looking history. . . ." If this were true (I do not think it is), one might reasonably inquire why any historian worth his salt should waste his time on such a necessarily anachronistic pursuit. Similar internalist assumptions are evident in the passages about how "our intuition" shows the difference between seventeenth-century science and hermeticism, about how science is to be "mathematical, mechanist" (alas for botany, zoology, geology, chemistry, etc.), and about how it is legitimate "to investigate such other factors related to the science of the period as have their own intrinsic interest" (according to whose timeless criteria of intrinsic interest?).

Though Dr. Hesse's fascinating paper raises a host of further questions, there is not time to pursue them. Therefore let me briefly reemphasize my unease.

Were Dr. Hesse merely saying "chaqu'un à son gout," her own particular preference for traditional internal history would be entirely unexceptionable. But in fact she wishes to be prescriptive, as her dismissal of