DAVID SHOENBERG

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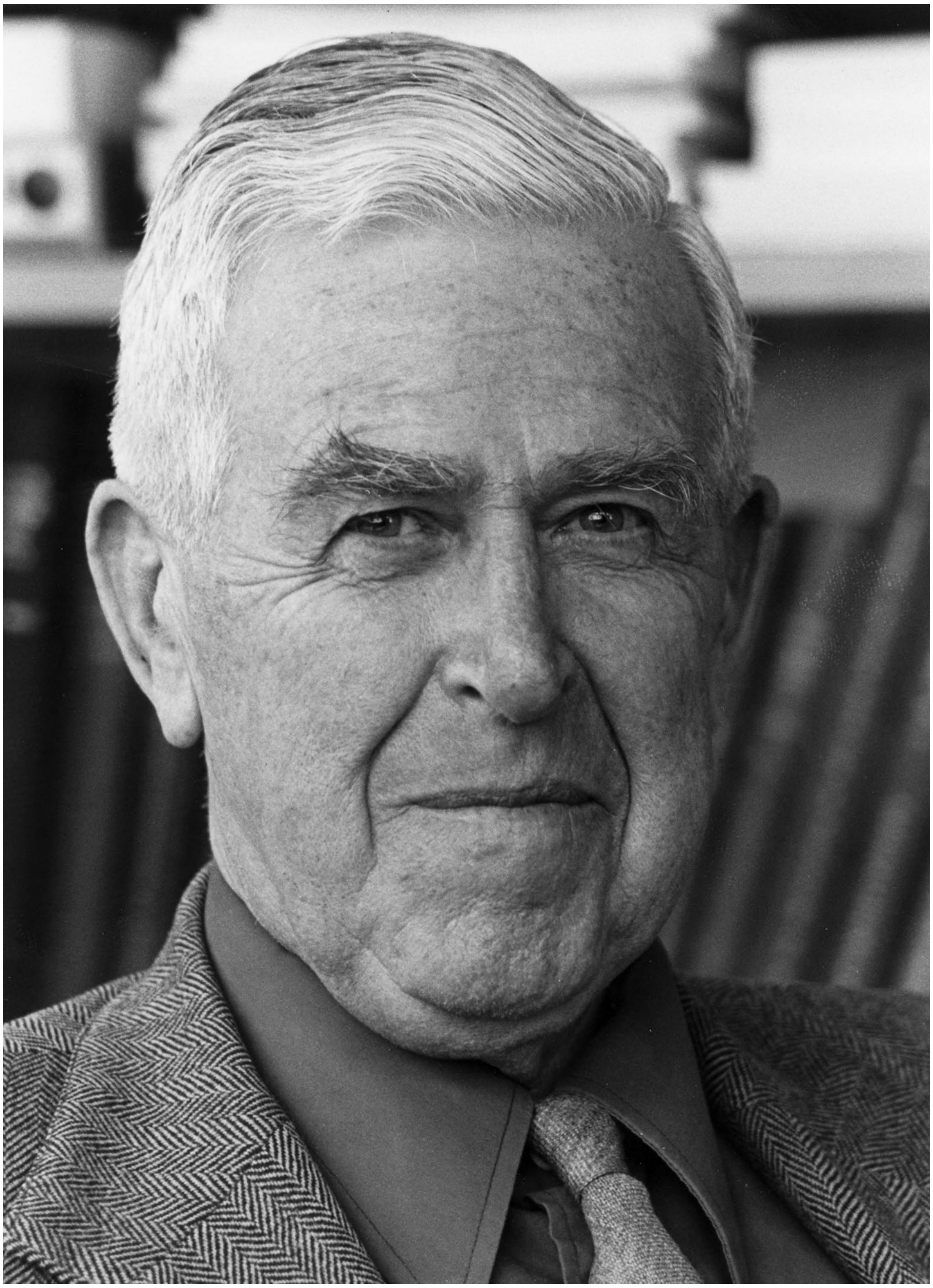

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Elected FRS 1953

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David Shoenberg was the last surviving pioneer of British low-temperature physics, having outlived Nicholas Kurti FRS, who died at the age of 90 in 1988, and J. S. (Jack) Allen, who was nearly 93 when he died in 2001. At the time they began work, before World War II, liquid helium was a rare commodity. In England it was made first in Oxford (1933), with Cambridge following a year later; L. C. Jackson in Bristol could liquefy hydrogen, which was used occasionally to prime a small helium liquefier. Before the outbreak of war halted academic research, all three centres had made significant discoveries in magnetism, superconductivity and superfluidity. P. L. Kapitza's expansion engine regularly supplied liquid helium for Cambridge and, on a few occasions, the other two; it was the forerunner of S. C. Collins's design of commercial liquefier, which, with generous postwar government support for research, made the USA the most prolific performer in low-temperature physics. But the English laboratories built on their prewar distinction and were soon active once more after 1945, with Shoenberg a leading figure. The work he initiated, particularly on the de Haas–van Alphen effect, has continued to spread across the world and into realms of thought he never contemplated in his active years.

BIOGRAPHICAL OUTLINE

David's father, Isaac Shoenberg, was from Pinsk, a predominantly Jewish city within the Russian Pale. As a highly talented engineer he was permitted to move to St Petersburg, where he designed and set up the first radio stations in Russia. It was there that David was born in December 1910, but his birthday became 4 January 1911 when, after the revolution, the Gregorian calendar was adopted. He was the fourth of five children, survived by his younger sister, the psychiatrist Dr Elizabeth Shoenberg. Happening to visit England for a conference one winter, in about 1912, Isaac was delighted to find it still green, and the people criticizing king and government without looking over their shoulders. But he was well established in St Petersburg and only came again in July 1914 when invited by A. N. Whitehead FRS to a research fellowship at the Royal College of Science. He jumped at the chance of realizing a dream, a higher degree in mathematics. Intending to stay for three years, against all advice he brought his family; almost immediately they were caught up in the war. A defect of sight made him ineligible for military service, and he helped the war effort with his science. When peace was declared in 1918 and he was contemplating a return to Russia, reluctantly because of the civil war there, he was unexpectedly invited to take British citizenship as a reward for his services. He remained, with a junior post at Marconi that prospered so that he rose to being head of patents and then joint general manager. He was tempted to the Columbia Gramophone Company, which through mergers evolved into EMI. There he built up a superb team of electronic engineers, guiding them and making the important decisions that standardized European television and led at the BBC to the world's first television service (1936) in time for an outside broadcast of the coronation of George VI. Isaac was knighted in 1962, shortly before his death; David was already a lecturer in Cambridge and director of the Royal Society Mond Laboratory, a Fellow of the Royal Society as his father never was.

David was only three years old when he came to England, with a child's Russian as his native language. Isaac was fluent in English, though with an accent he never lost. He spoke Russian with his wife but encouraged his young family to speak English. Enough Russian was spoken for David to grow up bilingual, and his first year back in Russia (1937–38) gave him full command, except perhaps in modern idioms; his conversational use of formal literary Russian was a real pleasure to his friends there.

At the age of eight years he went to Latymer Upper School in Hammersmith where, during the next 10 years, he was well grounded in mathematics and science, with so little attention to the classical languages that he had to take the Littlego on entry to Cambridge. He won a scholarship to Trinity College and went up in October 1929 intending, with his father's approval, to read mathematics. After one year, with a first in Part I, he switched to physics at the Cavendish Laboratory (22)*, which was then, under Sir Ernest Rutherford FRS (later Lord Rutherford; PRS 1925–30), enjoying an orgy of discovery—the neutron, the positron (very nearly) and artificial disintegration all surfaced during his student days. A first class in physics Part II ensured that he could continue as a research student, and in October 1932 he began under the supervision of P. L. Kapitza FRS. He had, as a boy, met Kapitza through his father, and the work on magnetism that Kapitza was engaged in attracted him more than the nuclear physics which was the normal goal of a new Cavendish graduate but which, it seemed to David, might be running out of excitement after its intellectual explosion of the previous year (19).

A succession of awards from Trinity College provided financial help and culminated in a senior 1851 Scholarship, which lasted until 1939. He had become a Cambridge man to the bone and indeed never left except for occasional sabbaticals and frequent shorter visits abroad. In March 1940 David married Catherine (Kate) Felicitée Fischmann, who was some five years older. Her ancestry was Russian but her grandfather and father had worked in Brussels; born a Belgian, she had taken British nationality before her marriage. She was a physiology graduate of University College London and worked in Cambridge on tissue culture, at the Strangeways Laboratory among other places, and she did not abandon her research interests on marriage, except for necessary intermissions to bring up her children. There were two

* Numbers in this form refer to the bibliography at the end of the text.

daughters, Ann (Mrs Eugene Bourgeois) and Jane (Mrs Peter Gatrell) and between them Dr Peter Shoenberg, a psychiatrist like his aunt Elizabeth. David and Kate soon settled in a good-sized, single-storey house at 2 Long Road and lived there happily for the rest of their lives.

At first they were on the outskirts of Cambridge, but when Addenbrooke's Hospital moved from the centre of town and expanded, as it has done since about 1970, they were soon at the heart of an active medical and research community. Despite this, and because the house and its neighbours have large gardens, it remained a secluded haven where research students were entertained in the summer and where friends from abroad always seemed welcome. In about 1978, however, Kate began to experience such back pains as kept her more and more confined to home, later almost continuously to her couch in the sitting room, where visitors and her research friends would drop in. David had help to supplement his devoted care and continued his own work with little apparent distraction. In the 1990s she was given a new medical regime and began to walk again, appearing at social gatherings to the astonishment of her friends. An old friend of theirs and mine, B. S. Chandrasekhar, visiting in July 2003, told me they had enjoyed a mile's walk in the neighbourhood; at 97 years old she was perhaps more mobile than David, and apart from occasional forgetfulness carried her years well. A month later, with little warning, she died. David had not recovered from the shock and grief before a slight stroke made his speech hard to follow; because he was already very deaf, communication was difficult. Early in March 2004 he suffered a more severe stroke and died a few days later.

It became clear at the very well attended funeral, and from the conversation of old students who had come to Cambridge for it, how much affection David had inspired. Apart from the ideas with which he had started them on their careers and the encouragement that had got them over sticky passages, they remembered gratefully his habit of asking speakers to clarify a point when they themselves had been afraid of doing so and revealing their ignorance. Especially they valued his help in writing clear accounts of their work, and his insistence on plain expression and continuity in the development of an argument. A striking aspect of his nature, on which several commented independently, was that they had never seen him angry.

The Royal Society Mond Laboratory

Kapitza persuaded Rutherford that he needed space for his magnets and dynamo, as well as to develop low-temperature facilities. The Royal Society generously provided money for a new laboratory (21), which was opened in February 1933, a few months after David had begun research, and he was the first student to work there (19). At Kapitza's suggestion he set about measuring the magnetostriction of a bismuth single crystal, not in the direction of the applied magnetic field as Kapitza had done, but transverse to the field where the effect is even smaller. With considerable help he managed to devise a hydraulic amplifier that could detect length changes as small as 5×10^{-8} cm—an achievement in a new research student unremarkable for deftness (1). In this respect he belonged to an important class of experimenters more gifted in devising and interpreting experiments than in performing them; to be fair, he was a less extreme example than J. J. Thomson FRS (PRS 1915–20) (Davis & Falconer 1957) or Heinz London (FRS 1961) (20), both experts in circumventing their severe practical incapacities but perilous if let loose with the equipment.

If the scientific value of this experiment is limited, it was important to David for more than the confidence it gave him. Bismuth had been chosen because it shows pronounced magnetostriction and because it can rather easily be grown in large single crystals. By accident he was thus introduced to the material that started him on the de Haas–van Alphen effect (dHvA from now on), which turned out to be the love of his research life. His other principal research topic also arose accidentally when in 1933, his first year of research, he was invited to talk to the Kapitza club on superconductivity.*

Kapitza's direct influence on David ceased abruptly when in August 1934 he went to a conference in Russia and was not permitted to leave. His detention provoked a storm of recrimination and high-level correspondence, while Kapitza's personal dismay was assuaged only by building a new research institute for him just outside Moscow. The story is told in some detail by Wilson (1983). The generator used for producing impulsive magnetic fields languished in the new Mond laboratory until it was sold to the Soviet authorities at twice its value, thus providing the Mond with a comfortable endowment and Kapitza with what proved to be a white elephant. In contrast, a second model of Kapitza's helium liquefier was made in Cambridge and sent to him with his old assistant, Emil Laurmann, and a senior technician, Harry Pearson, who assembled it before returning to Cambridge. The liquefier was a mainstay of Moscow low-temperature research; Laurmann was David's skilful assistant until his death in 1954 (13), and Pearson after the war became chief laboratory assistant in the Cavendish.

David had just finished his second year of research when Kapitza's detention left him to his own devices. Rutherford was officially in charge of the Mond; J. D. (later Sir John) Cockcroft (FRS 1936), his deputy, was little concerned with low-temperature physics—his only point of contact was the hydrogen liquefier he had helped Kapitza to design, and this was rarely used. So David went his own way and by 1935 had achieved enough to enter the annual prize fellowship competition at Trinity College. His rather impressive fellowship thesis survives, but it did not succeed against that of Victor Rothschild (later Baron Rothschild), and he was undoubtedly disappointed. It must have been a small consolation when, 18 years later in 1953, they were elected together to Fellowship of the Royal Society. In 2001 he was proud to be made an honorary Fellow of Trinity.

Since 1947, however, David had been an Official Fellow of Gonville and Caius College. He was a conscientious supervisor of the college's undergraduates until 1960 when, at his own request, he became a supernumerary fellow so that he could give all his time to research. As a regular luncher in college he would be found in the Combination Room, surrounded by helpers, doing the crossword in *The Times*. Elsewhere in the room Ronald Fisher FRS was similarly engaged and disdaining assistance. Beyond his teaching duties and these social activities David was not deeply involved in college matters, and the traumas suffered by James Chadwick FRS and Nevill Mott FRS in their masterships did not inspire him to dive into the parochial maelstrom. He was temperamentally averse to becoming involved in making decisions when the facts were unclear, especially when personal feelings were at the heart of the problem. But when he felt sure of a need, whether it was a friend or colleague in difficulty or, as in the years after his retirement, Jewish scientists were persecuted in Russia for trying to emigrate, he would give all the help he could, even if on one occasion it brought him into direct conflict with Kapitza. All the same, his family and the life of research in the Mond were where he could thrive, and this way of life continued until the Cavendish moved to west Cambridge in 1973, where he re-established the congenial pattern.

* For 12 years from 1922 Kapitza organized regular discussion meetings at which almost every distinguished scientist gave an informal account of his ideas. David reconstituted the club after the war and ran it himself until 1958. On the occasion of Kapitza's return to Cambridge in 1966 David called a final celebratory meeting (21).

The years between 1933 and the outbreak of war in 1939 saw much activity in the Mond. Jack Allen had arrived from Toronto in 1935, Rudolf (later Sir Rudolf) Peierls (FRS 1945) in the same year and Harry Jones (FRS 1952) in 1937, the last two on one-year Assistantships in Research funded by the Royal Society. Allen succeeded Jones in 1938 and stayed during the war as a lecturer, but left for the chair of physics at St Andrews in 1947. Peierls, among his other theoretical activities, helped David in his preliminary dHvA studies, while Allen & Jones (1938) discovered the fountain effect in superfluid helium, Allen & Misener (1938) having already noted other aspects of superfluidity at the same time as Kapitza (1938) in Moscow. Superconductivity had also taken centre stage since Meissner & Ochsenfeld (1933) had revealed the perfect diamagnetism of superconductors and the London brothers (London & London 1935) had formulated the equations that have been used ever since. David performed a series of magnetic measurements on rings and cylinders that were useful at the time but added little of permanent value (2, 4).

It was the year 1937–38 that set David on his life's work. A lecture by K. S. (later Sir Kariamanikkam) Krishnan (FRS 1940) alerted him to the potential value of the torsion balance for magnetic measurements in a uniform field, and when he went to Moscow for the year he applied it to the dHvA effect in bismuth at liquid helium temperatures. His systematic study (5) eclipsed that of de Haas & van Alphen (1931) and inspired L. D. Landau (ForMemRS 1960) to develop a more detailed theory. It was their intention to publish jointly, but before they were ready Landau was in prison—where he would probably have died but for Kapitza's insistent remonstrations with the Soviet authorities (B5)—and eventually his analysis was prepared by Peierls as an appendix, duly acknowledged, to David's experimental paper. While Landau was still at liberty he gave David considerable help in the little Cambridge tract *Superconductivity* (B1) that he wrote in Moscow. It is a peculiarity of this, almost the first book on the subject, that the London equations get hardly a mention. Years afterwards, when I asked him why, he said that Landau thought them ill-founded. His disapproval cannot have lasted long for he and Ginsburg made them a central feature of their phenomenological theory (the 'G–L' equations; Ginsburg & Landau 1950) and in 1957 I was passionately attacked by them, although without animus, for suggesting a non-local variant of the London equations. But this is by the way. While in Moscow, David also translated Landau & Lifshitz's first version of *Statistical physics* (B2); it is much shorter than its postwar successor, part of their great *Course of theoretical physics*, and more readily accessible to inexperienced readers.

On returning to Cambridge, David performed his single most important research on superconductivity, as will be described in more detail below. There was no more low-temperature work until 1945. He never said much about his wartime activities, but he wrote a short account for his children. W. L. (later Sir Lawrence) Bragg FRS had stayed in Cambridge, and David worked with him on mine detectors, which, like the instruments of modern treasure-seekers, sensed metals by a change in inductance. However, he spent more time on electronically timed fuses for anti-aircraft shells; like much war-work this development was never put into use, but at least he was awarded the MBE in 1944. In that year he and Jack Allen were appointed to university lectureships and, until two years after the war, they shared the direction of the Mond. When Allen left, David took over completely and remained in charge until he retired: as Reader from 1952 and as Professor from 1973.

After the war he rarely experimented alone, and indeed had little need to do so because he could rely on Emil Laurmann and after him on Harry Davies, both technically skilled craftsmen; but David was always present and in control of his experiment. After Harry died in 1970, David was content to leave everything to his students, whose general programme he continued to supervise until he retired. He then set about expounding his work and what it had grown to in the hands of followers worldwide; the outcome was the substantial volume *Magnetic oscillations in metals* (B4), which will long serve as the memorial of a lifetime's pioneering. The Festschrift, *Electrons at the Fermi surface* (Springford 1980), covers a wider range but contains little that does not owe a great deal to David's example.

As he approached retirement it seemed likely that his branch of low-temperature physics at Cambridge, and perhaps the whole low-temperature section, would soon close down. But in 1974 Gil Lonzarich (FRS 1989) arrived as a postdoctoral visitor from the University of British Columbia, where he had studied for his PhD under Andrew Gold, one of David's outstanding students; he immediately created a local sensation by showing how to achieve major enhancements of the scope and precision of dHvA studies. With stronger fields, lower temperatures, strange compounds and high pressures new prospects became apparent and continue to be revealed. David gave his full support and took an active interest for as long as he could, but gradually he began to move more slowly and lose his hearing until he was only an infrequent visitor. He could still, at his 90th birthday celebrations, give an entertaining reminiscent lecture, and up to the end could be found at home reading Russian classics and *The Times*, but his crossword skill had deserted him.

Traveller and interpreter

David was a keen traveller, always a welcome speaker at international conferences such as the biennial Low Temperature Conferences that began at Massachusetts Institute of Technology in 1949 and still continue. He was fluent in Russian, comfortable in German, and adequate in French; at a meeting in Japan he made preliminary remarks in Japanese, but no more than that. His command of Russian was especially valued by research students in the immediate postwar period, when Russian journals in English had been discontinued and the now-prevalent translations had not begun. Mond researchers had the almost unique advantage of an interpreter who was not simply prepared to read off a new paper in English with the technicalities impeccably translated, but even seemed to enjoy the exercise. I only once knew him to pause, while translating a paper on resonators, to remark with a puzzled expression that here was a word he had only met in connection with manure. When he explained that in this context it meant the virtue, or 'goodness', it was clear that it also meant the quality factor, Q, of the resonator. His duplicated version of the important paper of Ginsburg & Landau (1950) was circulated beyond the Mond and, among other benefits, acquainted John Bardeen (ForMemRS 1973) with ideas that influenced his progress towards the now classical Bardeen–Cooper–Schrieffer (BCS) theory of superconductivity (Bardeen *et al.* 1957).

David's year in Moscow (1937–38) was scientifically very rewarding; but without the friendships at Kapitza's institute, especially of Shura Shalnikov and his wife, he might have been lonely, because the political atmosphere was bleak. A tiny incident epitomizes the feeling—after Landau was imprisoned, David took to the institute secretary a joint paper they had written; the secretary blanched: 'I cannot type a paper by an enemy of the people'. Postwar visits were less tense; the first, in 1964, was with an official delegation led by Vivian Bowden, and Tam Dalyell MP, also in the group, has told of the enthusiastic bear-hug with which Kapitza greeted David.

His next year-long visit abroad was in 1953–54 with the whole family to Delhi, where David had undertaken to help in starting low-temperature research at the new National Physical Laboratory of India. K. S. Krishnan, whose torsion balance had proved so useful in Moscow, was director and had arranged the invitation and finance. It was an opportunity to advance the dHvA work, and David's account of the year (23) illustrates his capacity for making friends and enjoying new environments. He received many invitations for extended visits to Tokyo, Pittsburgh, Göttingen and universities in Canada and the USA, where he lectured and enjoyed research collaborations.

Research

There were two themes in David's research, superconductivity and the dHvA effect, and it is convenient to discuss them separately rather than chronologically. There is one paper in superconductivity of outstanding importance (6), a study of mercury colloids that demonstrated the limited penetration of a magnetic field as predicted by the London equations, and showed clearly how the penetration depth λ varied with temperature. According to the equations the field should decay below the surface so that it is reduced by a factor $e^{-z/\lambda}$ at a depth z, and λ might be expected to be 10^{-5} cm or less. This is so small as to be detectable only with difficulty unless the specimen is also so small as to present great measurement problems. A single sphere of diameter 10^{-5} cm would be ideal, and a large uniform collection of spheres would give detectable magnetization. A mixture of mercury and chalk, finely ground, is a good approximation to the ideal, only the uniformity being in question. David's starting point was a pharmaceutical preparation then used in the treatment of syphilis, and successive passes through a disintegrator eliminated large droplets that would have dominated the measurements and ruined their interpretation.

For the measurements David devised a magnetometer that would work in a uniform field and in the restricted space of a thin Dewar flask. It proved useful for several later investigations until about 1950. Two identical search coils, one close above the other on the same vertical axis and oppositely wound in series with a galvanometer, were mounted in the Dewar with the field-producing solenoid outside. Fluctuations in the applied field produced no response, but when the sample was jerked from one coil into the other a ballistic throw showed the magnitude of the magnetic moment. In the normal state of the mercury there was none, but as the temperature was reduced below the transition temperature, T_c (about 4.1 K), the decrease in λ produced a steady increase in the diamagnetic moment. Although too little was known about the size distribution of the mercury spheres to allow absolute determination, relative values were reliable and the temperature variation of λ was revealed for the first time. It was later realized (7) that the results agreed well with an ingenious, but highly dubious, two-fluid model proposed by Gorter & Casimir (1934), which suggested that λ^2 should vary inversely as $1 - T^4/T_c^4$. This meant that changes in λ, easier to measure than relative values, could be translated into absolute values on the assumption that the behaviour of mercury was typical for all superconductors. No problems seem to have arisen from this assumption. Rather surprisingly the BCS theory leads to a very similar formula for λ except at temperatures so low that changes in penetration are hard to discern.

It is not necessary to use minute samples to measure changes in λ. Casimir (1940) wound coils on a mercury core as closely as was feasible and measured the change with temperature of their mutual inductance as the effective gap between sample and coils altered with λ. The oddity of no observed change led David to repeat the experiment when low-temperature work resumed after the war (10). Before he and Laurmann got the equipment assembled, Casimir found an explanation for his null result, but they continued and satisfied themselves that tin and mercury both followed the pattern that had been revealed by the colloids*. They had steered clear of the design error that had led Casimir astray. At the same time Maurice Désirant (8) and, a little later, Michael Lock (Lock 1951) were using David's magnetometer, with improvements, to measure variations of λ with thin wires and evaporated films. They overcame considerable difficulties to achieve reasonably satisfactory results, but the principal lesson was that precise measurements of penetration depth are very tricky.

In his earliest studies of superconductivity David had measured the magnetization of a long cylinder in a transverse field with a Sucksmith balance. Distortion of the field around the cylinder makes the superconducting phase unstable above half the critical field strength, and a finely divided structure of alternating normal and superconducting regions pervades the sample. In practice a rather stronger field is needed to initiate this intermediate state in a pure sample because of the positive surface energy at the normal–superconducting phase boundaries. Désirant and Shoenberg (9) used the improved magnetometer to show further delay in the onset of the intermediate state when very thin cylinders were used, and they made rough but plausible estimates of the surface energy with the help of Landau's theory (Landau 1943). When Désirant returned to Belgium, David did no more on this problem. He completely rewrote his tract (B1) and in its new form (B3) it introduced superconductivity to the greatly enlarged generation of low-temperature physicists. His last involvement with superconductivity was spurred by Fröhlich's theory (Fröhlich 1950) in which electron–lattice interactions provided the principal mechanism for the transition. Emanuel Maxwell had found (Maxwell 1950) that the transition temperature of mercury changed with isotopic composition, and Fröhlich realized that his theory predicted something very similar, the transition temperature varying inversely as the square root of isotopic mass. It was obviously desirable to check whether other metals behaved similarly, and the Atomic Energy Research Establishment at Harwell was in a position to provide small quantities of separated tin isotopes. The story (Pippard 1987) of mainly good-humoured rivalry with Oxford over possession of the samples needs no repetition. Michael Lock used David's magnetometer, with David closely supervising, while I helped in a minor way with analysing and presenting the results (11). Fröhlich's prediction was very nearly, but not perfectly, verified and with similar results from other superconductors the electron–lattice interaction took its place at the heart of superconductivity theory, even though Fröhlich's version has been discarded in favour of the marvellously inventive BCS theory. By now David had succumbed to the lure of dHvA, his exclusive research interest to the end.

David's own account of how his ideas, and those of others, developed is to be found in his book (B4); it will be sufficient here to draw attention to important stages showing how what was originally perceived as yet another peculiarity of that recognized anomaly among metals, bismuth, developed into a major contributor to the understanding of solids. The possibility of oscillating magnetization in the more-or-less free electron gas was discovered by Landau

* To avoid 'frozen-in' magnetism they used a large Helmholtz pair of coils, made by winding a few turns of wire on children's wooden hoops, to neutralize the Earth's field. It gave others of us in the Mond a simple pleasure to hear the occasional shout from David to Laurmann (who was deaf), 'Switch off the Earth's field, Emil Janitch!'

(1930) in the course of his elucidation of metallic diamagnetism. If one considers only motion in the plane normal to the magnetic field, ***H***, the energy of a free electron can take quantized values that are odd multiples of $\mu_B H$, μ_B being the Bohr magneton, which is also the magnetic moment associated with electron spin. In semi-classical terms the orbits permitted to the electrons are such as enclose a half-integral multiple of the flux quantum h/e. Landau saw that as H was changed and the energy levels moved through the Fermi level the energy of the whole electron assembly would undergo periodic fluctuations, and consequently the magnetic moment would fluctuate with field strength*. At the time Landau only commented briefly on the possibility of oscillations, being convinced by Kapitza and others of his colleagues that there was no hope of producing a field that was both intense and uniform enough, let alone steady in strength, to exhibit them. When de Haas & van Alphen (1931) observed oscillations of magnetization in bismuth they were not ignorant of Landau's recently published work, but they seem not to have noticed this passing remark or not to have realized its relevance; it was Peierls who drew attention to it.

It is curious that van Alphen, working under de Haas's supervision in Leiden, where liquid helium was available, should have taken so much trouble with his measurements at liquid hydrogen temperatures but made few, and published none, at liquid helium temperatures where the oscillations are more pronounced. No further work was done at Leiden, so that David and Zaki Uddin had no competition when they took up the problem in 1936 (3). It was, however, David's work with a torsion balance in Moscow (5) that gave enough precise data to show that the electrons obeyed, rather well, Fermi statistics with a tensorial effective mass. This was the theoretically expected behaviour for small pockets and overlaps at corners of the Brillouin zone; it was not with this primarily in mind that the work was undertaken, but simply to make an investigation that could be finished in the short time available.

As far as David was concerned this might have ended his involvement with dHvA, had it not been that Jules Marcus discovered the effect in zinc (Marcus 1947), which is by no means an eccentric metal like bismuth. Other metals might similarly have small overlaps, and indeed it was not long before dHvA was found to be rather common. There was now no turning back, and by 1952 David, using the torsion balance, had made provisional studies of dHvA in gallium, tin, graphite, cadmium, aluminium, mercury and thorium (12). At the same time Verkin, Lasarev and Rudenko were making similar explorations and adding magnesium and beryllium to the catalogue; David's paper lists several of their publications in Russian. In the course of this trawl an important instrumental advance occurred to him, perhaps the last legacy from Kapitza, who had dedicated many years to the impulsive generation of intense magnetic fields. The disadvantages of a short-lived field become a positive advantage when the magnetic moment is oscillatory, because the rapid changes in magnetization induce electromotive forces in a coil, which can be amplified, displayed on an oscilloscope and photographed to give a wealth of quantitative information in a single shot lasting a few milliseconds (15). The successful realization of this idea owed much to Emil Laurmann, who designed a 1000 μF capacitor bank and its discharge mechanism (capacitors had become more reliable than in the days when Kapitza rejected them) and made the exquisite ivory specimen holder that could fit into the narrow field coil and also be turned from outside the cryostat to alter the orientation of the specimen. For all that, it took three years from the conception to record an oscillation,

* It is not clear at what stage it was generally recognized that the magnetic moment, when plotted against H^{-1}, would oscillate with a constant period; David denoted the reciprocal of this period by F.

mainly because he wasted time hoping to find the effect in sodium. In the end it was lead that gave definite results, which were thoroughly investigated by Andrew Gold in what proved a notable piece of work, as will be described below.

In 1951–52, while this development was in progress, Lars Onsager (ForMemRS 1975) spent a sabbatical year from Yale in Cambridge and occupied a chair in David's large office. Although otherwise engaged, he offered occasional characteristically gnomic advice on the interpretation of data, but only at the moment of departure did he give David a short paper on the theory of dHvA, leaving no time for discussion of what proved an exceptionally valuable contribution. It is of interest to recall something of the background. In the early years of quantum mechanics, after Bloch had explained the movement of electrons through a crystal lattice, H. A. Bethe (ForMemRS 1957) wrote at length about electrons in metals; his pictures of energy surfaces in *k*-space, based on the tight-binding model, became well known, with their strong departures from spherical shape and contacts with the zone boundaries (see Mott & Jones 1936, p. 70). In the course of time, and especially after the war, theoretical attention shifted from metals to semiconductors, and to the small pockets of holes and overlaps of electrons for which a quadratic dependence of energy on wavenumber was a good first approximation (as it had been for dHvA in its early history). Shockley (1950) was aware that this was inexact even for semiconductors, and made a start on the task of describing electron motion in geometrical terms that might be applied to any form of energy surface, rather than the restricted, but analytically easier, algebraic form. Onsager (1952) also geometrized the problem and with great economy applied semiclassical quantization to show that any permitted orbit must, like a free-electron orbit, enclose a half-integral multiple of the flux quantum. The cross-sectional area of a plane section of the Fermi surface, cut normal to $\boldsymbol{H}$, determined the oscillation frequency contributed by the electrons on that cross-section, but the only frequencies that would appear on the record would be those due to sections of extremal area. Here, in principle, was a scheme by which a sufficiently complete set of dHvA data could be transformed into the shape of a Fermi surface, however complicated. The same theorem was independently discovered by I. M. Lifshitz (ForMemRS 1982), but this was not known in the West until some years later. Lifshitz & Pogorelov (1954) devised a formal procedure for turning dHvA data into a Fermi surface shape. It was applied by David's student Max Gunnersen (Gunnersen 1957) to his measurements on aluminium, but unsuccessfully; the idea is mathematically sound but in practice virtually useless. The first significant application of Onsager's theorem was by Gold (1958).

The oscillations in lead were so many, and varied with angle in so complicated a way, that their unravelling and interpretation were far from straightforward. A good guess, however, made all the difference: Gold considered a spherical Fermi surface, corresponding to four free electrons per atom, dissected it by the Brillouin zone boundaries into many portions, and reconstructed them into a number of separate surfaces, one of which resembled a tubular structure surrounding all the edges of the primitive zone. A reasonable correspondence could be found between the observed dHvA frequencies and extremal sections of these surfaces after they had been rounded off. The success of this manoeuvre encouraged Walter Harrison to propose it as a rather general first approximation for polyvalent metals (Harrison 1960). A more sophisticated development, the pseudopotential (Heine 1969), has provided a deeper rationale and has had wide application to theories of condensed matter, including liquid metals. In the search for theoretical approximations that alone allow manageable calculations in any but the simplest examples, the dHvA effect has had an important role both as inspirer of ideas and as verifier of theoretical techniques. An especially interesting overview, at the time that the field was beginning to show its possibilities, is to be found in David's and other's presentations to the 1960 Fermi Surface Conference (15).

In his talk he concentrated on his recent success in finding dHvA in copper, a metal both intrinsically important and challenging because laboratory-grown single crystals gave no oscillations; moreover, it had posed theoretical problems because the valence bands lie so close below the Fermi level, and various attempts to calculate the band structure disagreed. Recently, however, he had tried with a copper whisker and found oscillations (16). Their variety left no doubt that the Fermi surface was distorted enough to make contact with the centre of the (111) Brillouin zone faces. From these early observations came the now-standard names for the orbits: neck, belly, dog's bone, lemon and four-cornered rosette (a misnomer). After copper, both silver and gold were found to behave similarly, and could more easily be grown into crystals that showed the oscillations well. Several research students devoted time to careful plotting of the Fermi surfaces and correlating their shapes with magnetoresistance and other properties.

The quest for precision was notably advanced when a postdoctoral American visitor, Phil Stiles, arrived just as John Hulm, a former research student then at Westinghouse, presented David with one of their first superconducting solenoids. Phil developed the electronics that provided the necessary finesse to map out the tiny departures from sphericity of the Fermi surfaces in the alkali metals (17). It was not long before impulsive magnetic fields gave way, in Cambridge and elsewhere, to the steady fields of superconducting solenoids; these had the special advantage that they could be locked by a superconducting short-circuit into a stable condition, so that the phase shifts produced by sample rotation could be used to reveal tiny changes in extremal area.

It can hardly be doubted that the greatest contribution of this work to physics in the broad sense lay in the acquisition of data leading to criticism and subsequent refinement of theoretical techniques applied to condensed matter, and that the emphasis on precise measurement paid handsomely. Incidental to this programme, however, were other observations of considerable intrinsic interest, not to be overlooked simply because they had less impact beyond themselves. As David admitted in his acceptance of the London medal (18), it was the fascination of the phenomenon itself, rather than any fundamental value, that kept him enthralled. Out of many incidentals discussed fully in his book (B4), two deserve brief mention here and two call for a little more. The first pair are collision damping (chapter 8) and magnetic breakdown (chapter 7), the second are electron spin (chapter 9) and magnetic interaction (chapter 6). No further references will be given because they are fully covered in the book.

Collision damping

The oscillations are frequently not as strong as might be expected, and their variation with temperature is as if a constant x—the Dingle temperature—were added to the actual temperature. Impurities and dislocations seem to be the cause, having a greater effect on the breadth of the Landau levels than on the electrical conductivity; thus an apparently pure sample, to judge by its conductivity, may still be significantly weakened in its dHvA behaviour.

Magnetic breakdown

This is a very interesting and complicated effect that is passed over briefly because, although it was first observed by Michael Priestley when a research student of David's, David himself was never closely involved. Priestley's observation was that an oscillation in magnesium needed, for explanation, an orbit too big to fit inside a Brillouin zone. David drew attention to this anomaly at the Fermi Surface Conference (15), and it is a source of real regret that we in the Mond failed to see for ourselves the explanation, which was proposed by Cohen and Falicov and refined by Blount: if the lattice potential responsible for Bragg reflection at a zone boundary is weak, a process analogous to electron tunnelling may allow the reflection to be overridden. Normally breakdown, when it occurs at all, is incomplete so that at every point in its orbit at which Bragg reflection might occur the electron may be reflected or may carry on. A great variety of new orbits now appears, and the conductivity of the metal in a magnetic field may be strongly affected. A certain amount of progress has been made in sorting out the possible manifestations of breakdown, but the formidable complexity of the theoretical problem has discouraged taking it much further. It is worth noting that while he was in India in 1953–54 David started J. S. Dhillon on dHvA work with bismuth and zinc; they drew attention to the marked loss of amplitude in zinc at stronger fields (14), but an explanation had to await the discovery of magnetic breakdown.

Electron spin

Quantization of the orbital motion of a free electron in the plane normal to $\boldsymbol{H}$ allows the energy to take odd multiples of $\mu_B H$ while the spin splits each level by $\pm\mu_B H$; thus, even multiples of $\mu_B H$, including zero, are the permitted energy levels for a free electron, leaving the oscillation pattern unchanged except for a reversal of phase. Such a reversal had been noted by David in his early bismuth work, and this is a surprise because the low effective mass of the electrons involved means that the orbital splitting is much wider; an explanation in terms of spin must allow a g-factor for the spin magnetic moment considerably greater than 2. Such an effect had been observed in semiconductors and explained in terms of spin–orbit interaction, and something similar affects electrons in orbit. If at a Bragg reflection spin–orbit interaction is a significant factor, the phase of reflection, and hence the effective length of the orbit, may be different for the two directions of spin. In general each orbital level is now doubled, with corresponding changes to the phase and harmonic content of the oscillations. There is much detail in David's book of his own and others' studies of the effect, and of possible ways of sorting out ambiguities of interpretation when the spin splitting exceeds the orbital splitting. The g-factor may be very large, a value of 170 having been suggested for zinc, which, like bismuth, exhibited what seemed at the time the wrong phase.

Magnetic interaction

When David, in the course of his first study of noble metals, observed that the harmonic content was stronger than he expected (the impulsive field method is especially sensitive to the higher frequencies) he attributed it to the demagnetizing effect of the sample shape but was soon convinced that it was an intrinsic nonlinearity; the mean field acting on an orbiting electron is not the applied field $\boldsymbol{H}$ but $\boldsymbol{H}+4\pi\boldsymbol{M}$, or $\boldsymbol{B}$ (David never espoused rationalized units). The magnetization may be small, but so is the period of oscillation in a noble metal, and the difference between $\boldsymbol{B}$ and $\boldsymbol{H}$ may be comparable to, or even greater than, the period, so that the line shape is seriously distorted. What was originally a sine wave is sheared, often to the extent that the slope is reversed and $\boldsymbol{M}$ looks set to be a three-valued function of $\boldsymbol{H}$. This is, of course, impossible and the resulting instability is similar to that described by the van der Waals equation for an imperfect gas. Like the gas, a unique solution is imposed by the discontinuous jump of ***M*** to the most stable value; the magnetization curve is changed from a sine to a saw-tooth, and in this way the strong harmonics are generated. With the stability of a superconducting solenoid the line shape can be plotted out in detail, and examples are shown in David's book. It is clear from the length of the chapter that he was strongly attracted to this process—in a first paper on the theory I christened it the Shoenberg effect, and believe this is therefore the correct name though he was too reticent to use it*. He was attracted not solely because it was an excuse for elegant measurements but because it gave him an opportunity for the sort of analysis he enjoyed, the sort of skill conferred by a first year of Cambridge mathematics. He spent a few months in Ottawa with Ian Templeton, a master of precision in observation, and sorted out many fine details of the phenomenon.

A further complication was revealed by Condon, who used flat crystals of beryllium set normal to ***H***. It might have been thought that the demagnetizing effect would restore the field on the electrons to ***H***, but Condon showed that it was thermodynamically better for the sample to divide into domains in which the values of ***M*** alternated from one to the next, in a manner reminiscent of the intermediate state of superconductors. Beryllium also exhibits magnetic breakdown, and Condon's resistance measurements show the extremes of behaviour that dHvA and its complications can give rise to.

More could be written about the sidelines of dHvA that gave David pleasure, but enough has been said to show the wealth of interest of his research. He was an experimental physicist of the old school, fascinated by phenomena themselves and devoted to probing their secrets. He was skilful in devising instruments for particular purposes, even if he was content to leave details to others. He showed little interest in theoretical matters beyond what he needed, but never hesitated to ask advice or, for that matter, to give it when asked. Above all, he had the gift of choosing the right problems for himself and his students—the gift of luck which, as Pasteur said, 'ne favorise que les esprits préparés'.

Honours and awards

1944 MBE
1953 Elected FRS
1961 Guthrie Lecturer, Physical Society
1964 Fritz London award
1973 Doctor *honoris causa*, University of Lausanne
1980 Rutherford Memorial Lecture, India and Sri Lanka
1988 Krishnan Memorial Lecture, New Delhi
1991 Hughes Medal
2001 Honorary Fellow, Trinity College, Cambridge

* In this he differed from Nevill Mott, who enjoyed talking about 'the Mott transition', in contrast to Fritz and Heinz London, who both spoke shyly of 'the equations named after my brother and myself'.

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