

---

Memories of Early Days in Solid State Physics

Author(s): N. F. Mott

Source: *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences*, Vol. 371, No. 1744, The Beginnings of Solid State Physics (Jun. 10, 1980), pp. 56-66

Published by: The Royal Society

Stable URL: <http://www.jstor.org/stable/2990276>

Accessed: 12/03/2010 02:19

---

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=rsl>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).



The Royal Society is collaborating with JSTOR to digitize, preserve and extend access to *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences*.

## Memories of early days in solid state physics

BY N. F. MOTT, F.R.S.

*Cavendish Laboratory, Cambridge*

*Mott, Sir Nevill. Born Leeds 1905. Studied theoretical physics under R. H. Fowler in Cambridge, in Copenhagen under Niels Bohr and in Göttingen. Professor of Theoretical Physics in Bristol 1933–54, and Cavendish Professor of Physics, Cambridge 1954–71. Nobel Prize for Physics 1977. Author of several books and research papers on application of quantum mechanics to atomic collisions and since 1933 on problems of solid state science.*

My interest in the subject began in 1933 when I left Cambridge to take up the chair of theoretical physics in Bristol. In Cambridge I remember that A. H. Wilson wrote an Adams prize essay on metals which led to his book, *The theory of metals*, and I have a curiously vivid recollection of R. H. Fowler explaining to C. D. Ellis that most semiconductors were what we would now call extrinsic, the electrons coming from impurities, and Ellis replying ‘very interesting’ in a tone of voice that implied that he was not very interested. Although a lecturer in the Faculty of Mathematics, as were at that time all theorists in Cambridge, I spent much of my time in the Cavendish, and in spite of R. H. Fowler’s massive book on Statistical Mechanics, Rutherford’s Cavendish was not the place where a young man would naturally turn to the theory of electrons in solids. I do not remember being very interested myself, but I do remember going to a course of lectures by the late Ebenezer Cunningham of St John’s College on electrons in metals when I was an undergraduate;† our textbook was O. W. Richardson’s *Electron theory of metals*. I remember being struck by the point in the book and in the lectures that the Hall effect gave an indication of the number of free electrons in a metal, often near one per atom, and that at least one quantity, the ratio of thermal to electrical conductivity, could be calculated; but there were many unexplained observations, particularly the long mean free paths and the absence of any large contribution to the specific heat from the electrons. But most of all I wondered how it could be that the atoms of a metal gave up their electrons, so that they became free, while in an insulator they remained fixed in position and unable to move.‡ This was of course cleared up by Wilson’s work.

When I went to Bristol in 1933 as Professor of Theoretical Physics, two influences turned me towards electrons in solids: Harry Jones (see the previous paper) had

† Probably in 1925; J. C. Slater in his *Scientific biography* records that he (and Dirac) attended the same course in 1923.

‡ My recollection contrasts with Wilson’s in this volume of discussions at Leipzig, and the feeling that an insulator is a ‘poor’ metal. As far as I can remember, we were aware at this period that in ‘insulators’, temperature would be capable of allowing electrons to move.

a theory explaining the Hume-Rothery† rules in metals and Herbert Skinner was engaged in his experimental work on the soft X-ray emission bands of light metals. I think that when I went to Bristol I must have been aware of Sommerfeld's explanation of the absence of a major contribution to the specific heat from the electrons, assuming that they obeyed Fermi–Dirac statistics, and the treatments by Bloch, Peierls and Wilson of electrons moving in a periodic field, the long mean free paths, the separation of the energy states into zones and the distinction between metals and non-metals. Looking back on it, I am surprised how easily everyone accepted this strange theory, in which all the electrons, in insulators as well as in metals, were treated as free, and insulators appeared as materials in which all the energy states in each zone were either occupied or empty, so that a current due to electrons moving in one direction was exactly balanced by movement in the opposite direction. The papers by Bloch and Peierls tell us something about that. Moreover, the approximation in which each electron moved in the average field of all the others was very easily accepted; I suppose Hartree's success with the 'self-consistent' fields for atoms had a lot to do with this. More detailed treatment of the effect of the very large interaction term  $e^2/r_{12}$  had to wait till later.

The monumental and comprehensive report in the *Handbuch der Physik* by Sommerfeld & Bethe appeared in 1933 and formed a basis on which other workers could build. This report, I believe, had an immense influence on solid state physics. Apart from the effects of the interaction term  $e^2/r_{12}$  everything seemed to be there, worked out in detail. Whether I had seen this report when Harry Jones showed me his work in 1933 I cannot remember, but I do remember the impact that Jones's work made on me. He supposed that all the electrons in an alloy could be treated as free, that the effect on their energies, particularly of those with states in  $k$ -space near the Fermi surface, could be calculated as in the theory of Bloch, and so be depressed when that surface approached the boundaries of the zone. An alloy would therefore take up a structure such that the boundary of a zone lay just outside the Fermi surface. (I do not believe we used the term 'Fermi surface' at that time, only 'Fermi energy'. The idea of the Fermi surface was, however, well brought out in Bethe's article which contains very clear illustrations of their shape calculated for certain cases.) I think my enthusiastic acceptance of the theory was typical of my generation at that time; quantum mechanics was new, all physics and chemistry lay there to be explained and if by making approximations and neglecting even large terms (like  $e^2/r_{12}$ ) one could account for something that had been observed, the thing to do was to go ahead and not to worry. I remember many discussions with Hume-Rothery himself, who shared my point of view.

Turning now to Skinner's work (some of it with O'Bryan) on X-ray emission by the light metals lithium, sodium and magnesium, the observations showed bands of the form illustrated in figure 1*a*. The theory of non-interacting electrons, as set out in Sommerfeld & Bethe's article and neglecting correlation, predicted a form of the band, corresponding to the density of states, as illustrated in figure 1*b* – so

† See note on Hume-Rothery at end of the article by H. Jones.

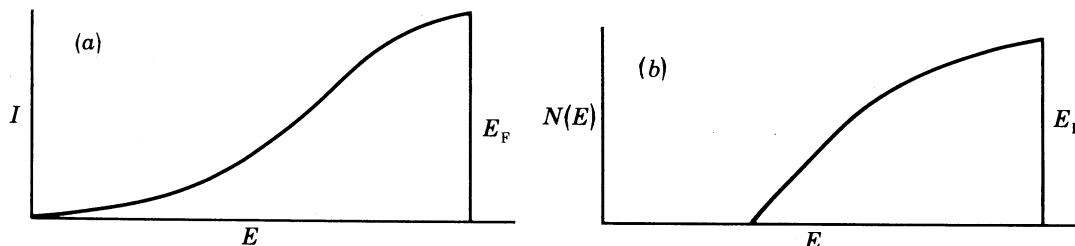


FIGURE 1. (a) Intensity of X-ray emission spectra plotted against energy in volts for certain light metals (from H. M. O'Bryan & H. W. B. Skinner (1934), *Phys. Rev.* **45**, 379). (b) Theoretical form of bands.

long as the transition was to the  $L_{III}$  state (of p-symmetry) so that no selection rule prohibited the transition from the s-like states at the bottom of the band. On the other hand, what was observed as in figure 1a. The sharp cut-off at high energies was in fact very marked, as was its broadening at high temperatures predicted by Fermi-Dirac statistics, but the low energy limit showed a marked tail. In a paper<sup>(1)</sup> published in 1934, Jones, Skinner and I put forward the explanation of the fact that the term  $e^2/r_{12}$  did not destroy the sharp cut-off, but did affect the low-energy limit: an electron excited just above the Fermi energy (by an energy  $\Delta E$ ) and could interact (collide) with an electron just below it, so that both changed their states, by a kind of Auger effect. But the number of electrons with which it could do this was proportional to  $\Delta E$ , so the lifetime of such an excited electron was long, varying (as we found) as  $1/(\Delta E)^2$ . This tended to infinity as  $\Delta E$  became small, so the Fermi energy was sharp, even when electron-electron interaction was taken into account. On the other hand, a 'hole' in the Fermi distribution near the bottom of the band could be filled by a process in which any electron with energy above it moved down into it, giving up its energy to another electron. Thus the lifetime would be short, and the electron's energy according to the uncertainty principle would be weakly defined, leading to the observed tail.

There are two comments that I should like to make on this. The first is that Sommerfeld's prediction of a linear electronic specific heat, and equally Jones's theory of alloys, really did imply that the idea of a sharp Fermi energy was something that corresponded to physical reality and not just a consequence of neglecting the interaction between electrons. But it needed the visual evidence provided by Skinner's work to make me (at any rate) see that there was something here that we really had to explain. The second is that we failed to go on and show that the Fermi surface was a real concept. This awaited the work of Landau<sup>(2)</sup> (1956), and its experimental determination (for copper,<sup>(3)</sup> by Pippard in 1957) and the development of the de Haas - van Alphen effect for the purpose, mainly by Shoenberg, † together

† An excellent account of experimental methods of measuring the Fermi surface is given by N. W. Ashcroft & N. D. Mermin, *Solid state physics*, (New York: Holt, Rinehart & Winston, 1976), pp. 263-281.

with his extensive exploration of its form through this same effect starting before World War II. Nonetheless, we often implicitly assumed it. For instance, in Bristol my research student Baber<sup>(4)</sup> showed in 1937 that collisions between electrons, all in states within an energy *ca.*  $kT$  of the Fermi surface, yielded a term in the resistivity proportional to  $T^2$ ; the proof assumed that the wavenumber  $k$  near the Fermi energy was a quantity with physical reality, and this was equivalent to assuming the existence of the Fermi surface. Peierls's memorandum shows that this result was anticipated by Landau.

Interaction between electrons plays an essential role in our understanding of ferromagnetism. This was of course first shown by Heisenberg<sup>(5)</sup> in his paper of 1928, which was essentially a theory of a non-metallic ferromagnetic material, which ascribed ferromagnetism to an abnormal sign of the exchange integral between two atoms. In Bristol, in the period 1933–8, our interest in metals made us start from the Pauli theory of the paramagnetism of a metal; here the electrons excited above the Fermi energy, of which the number per unit volume was  $N(E_F) kT$  per unit volume, each contributed  $\mu_B^2/kT$  to the paramagnetism, giving a total susceptibility  $\chi$  equal to  $N(E_F)\mu_B^2$ . Here  $N(E)$  is the one-electron density of states,  $\mu_B$  the Bohr magneton and  $E_F$  the Fermi energy. In Bristol, influenced by the experimental work of Potter and Sucksmith, I put forward in 1935 a model<sup>(6)</sup> for the transition metals in which a narrow d-band overlaps a wide s band, as in figure 2, so that the density of states at the Fermi energy could be large, and in ferromagnets the saturation moment a non-integral multiple of the Bohr magneton. This was supported by results already existing on the effect on the paramagnetism of palladium of alloying it with certain metals, such as silver with one extra electron, which could be expected to displace the Fermi energy in figure 2 to the right, thus decreasing  $N(E_F)$  and  $\chi$ . Ferromagnetism, and particularly the non-integral number of Bohr magnetons per atom shown by nickel, cobalt and iron, was in our thinking the result of a 'Weiss molecular field' which in nickel and cobalt, at any rate, polarized the spins of as many d-electrons as possible in one direction. The Weiss field arose because two electrons of which the spins pointed in the same direction must have an antisymmetrical orbital wavefunction, so that the probability that two electrons approached within a distance  $r_{12}$  of each other tended to zero for small values of  $r_{12}$ . This idea goes back to Bloch's paper of 1929. This meant a decrease in the energy contributed by the repulsion  $e^2/r_{12}$  between each pair of electrons with parallel spins. † Stoner<sup>(8)</sup> in 1938 published the first of a series of papers using this model, and developing its consequences in detail, which came to be called the Stoner model. ‡ His obituary notice published by the Royal Society says that this

† Bloch<sup>(7)</sup> in 1929 first saw that exchange correlations (as distinct from correlations of antiparallel electrons) would enhance paramagnetism, and should give ferromagnetism for a very extended lattice. Wigner pointed out that this was not likely to occur for nearly free electrons (see for instance Mott & Jones<sup>(9)</sup>, p. 141, and Seitz<sup>(10)</sup>, p. 602). The use of the equation for the susceptibility

$$\chi = \chi_0/(1 - \chi_0 I),$$

with  $I$  an exchange term, is due to the post-war work of Anderson and Hubbard.

‡ See note on Stoner's work at the end of this article.

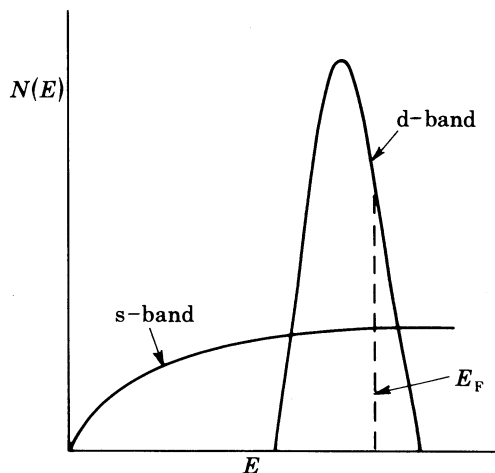


FIGURE 2. Proposed density of states in nickel (from Mott & Jones,<sup>(9)</sup> p. 192.)

model took a long time to gain recognition. A ferromagnet that could be described in this way came to be called an ‘itinerant ferromagnet’. I think there was some conflict in the development of ideas between this concept and those who tried to reconcile it with the concept of localized moments, for instance by describing nickel as a mixture of states with the configurations  $3d^{10}$ ,  $3d^9 4s^1$  and  $3d^8 4s^2$ .

J. C. Slater<sup>(11)</sup> in his *Scientific biography* (p. 191) makes the remark that in ‘studying the work of Mott and Jones one got the impression that they felt that the potential actually occurring in the energy band theory was a small one, which could be handled by perturbation theory; and this affected the thinking of the English school of physicists so that even now [1976] they are trying to get valid results relating to energy-band theory from simplified models...’. This perhaps refers to pseudopotential theory. But as regards our work in 1933–6, we were very much impressed by the work of Wigner & Seitz<sup>(12)</sup> in obtaining exact wavefunctions  $\psi_0$  for the ground state of metallic sodium, and by the fact that, except near the nucleus, they were flat. Also we used the approximation

$$\psi = e^{ikx}\psi_0$$

for other  $k$  values. It was this flatness that led us to think that the free electron approximation might be a good one, except of course for d-bands. Whether this influenced ‘the English school’ I am unable to say. Slater, of course, was the real leader in the use of computers, as they developed, to calculate energy bands by using exact wavefunctions, and in particular in including the effect of correlation.

In 1933, Klaus Fuchs† came to Bristol, as a political refugee from Nazi Germany.

† Fuchs later worked in Los Alamos, and after the war as head of the theoretical group at Harwell. He was convicted in 1950 for communicating secret material to the Russians, and imprisoned for 9 years.

He was my research student for 4 years, and worked among other problems on the elastic constants of the alkali halides, another problem in which we made the assumption that the wavefunction midway between the atoms is flat (see ref. 9). Fuchs at that time was shy and not very communicative; I do not remember that we ever discussed politics, though his interest in the Soviet Union was evident.

I add a few words on metal–insulator transitions, though most of this is post-war. At a conference in Bristol in 1938 when the Wilson theory of metals and insulators was under discussion, de Boer and Verwey (Philips, Eindhoven) pointed out that according to the theory NiO should be a metal, with a half-filled d-band, while in fact it was a transparent insulator. Peierls said that this must have its origin in electron–electron interaction. I think that this problem worried me throughout the war period (there were other worries!) and in 1949 I published a paper<sup>(13)</sup> using a screening argument to show that the transition from a NiO-type insulator to a metal, induced for instance by pressure, should be discontinuous. I do not now like the argument that I gave then, and prefer that of Brinkman & Rice, who used the concept of Hubbard bands (see ref. 14). At that time, however, although I had learned about antiferromagnetism from Néel (on a drive in his car in the Vosges at the 1938 Strasbourg conference on magnetism), I failed to realize that this kind of material could be antiferromagnetic in the insulating state. Slater,<sup>(15)</sup> in 1951, showed very clearly how, on the basis of full and empty bands (Wilson theory) taking different wavefunctions for the two spin directions, such an antiferromagnetic material could be an insulator. He failed, however, to show how it would remain an insulator above the Néel temperature. Slater’s admirably thorough development of band theory gave him little sympathy with my more intuitive attempts to describe metal–insulator transitions, as is apparent from his biography, confirmed by my memory of his comments on a lecture I gave at M.I.T. in the 1950s.

I shall now turn to my recollections of the beginning of the application of quantum mechanics to semiconductors, in so far as I was concerned with it. Ronald Gurney† came from Manchester to join my group in Bristol in 1935. My recollection of what turned us to non-metallic materials was primarily the series of papers on colour centres (Farbzentren, F-centres) in alkali halides, which came from R. W. Pohl’s school in Göttingen. These papers were short, clear and on the whole free of theoretical interpretation. Pohl was said to despise theoretical physics as ‘*Papierphysik*’, but he told me in a letter, shortly before he died at the age of 90, that it was a great satisfaction to him that our group in Bristol, already quite well known in solid state theory, should have shown so much interest in his work; according to him, the atomic physicists of the great Göttingen tradition despised work on defects in solids as ‘*dirty physics*’. I suppose this must have referred to James Franck in the

† Ronald W. Gurney was born in Cheltenham in 1898. While in Princeton from 1926 to 1928 in collaboration with E. U. Condon he put forward the accepted theory of alpha decay, proposed independently by Gamow. He was the author of *Ions in solution*, *Elementary quantum mechanics* and, with Mott, *Electronic processes in ionic crystals*. He had a remarkable talent for expounding physical ideas with the minimum of mathematics. He died in New York in 1953.

pre-Hitler period before he left; I hardly imagine it could have been Max Born, in view of his own work on alkali halides.†

The way that Gurney and I developed the subject is set out in our book (Mott & Gurney, *Electronic properties of ionic crystals*, Oxford University Press, 1940). We did not mean to write a book on semiconductors. We had before us the papers of A. H. Wilson, showing the difference between metals and non-metals, and how semi-conducting behaviour could be due to discrete levels produced by impurities. We had also, of course, Born's work on the cohesive forces in alkali halides, the concepts of Schottky and Frenkel defects in alkali and silver halides and also that of the Frenkel exciton (Frenkel,<sup>(16)</sup> see also Peierls.<sup>(17)</sup> Frenkel's contributions,‡ particularly these two, seem to me to be of major importance; I met him in Leningrad in 1934, but I do not think I fully realized the significance of the exciton concept till I started working with Gurney. It is not mentioned in Mott & Jones,<sup>(9)</sup> published in 1936.) My most vivid recollection of what started us writing a book is discussions with Gurney on the nature of an F-centre. At first we thought it was the 'self-trapped electron', or 'electron trapped by digging its own potential hole', which had been introduced by Landau<sup>(18)</sup> in 1933, and the hypothesis was put forward by Gurney & Mott<sup>(19)</sup> in 1937 and also by Von Hippel<sup>(13)</sup> in 1936. This self-trapped electron would now be called a polaron, and we gave a correct qualitative description (Mott & Gurney, p. 87) of the hopping process for such particles, but not of their band motion at low temperatures, which I think was first proposed by Holstein in 1958. But we ran up against all sorts of difficulties in developing this idea. If F-centres were self-trapped electrons, where was the positive charge? We knew that in alkali halides ionic conduction took place through the movement of 'Schottky defects' – that is vacant cation and anion lattice sites – and surmised that excess metal ought to be taken up as vacant anion sites, with the electron 'somewhere'. The presence of excess metal gave colour to alkali halides and that meant that it produced F-centres. At first we speculated that the Landau self-trapped electron might be held by the field of a vacant anion site, but I think that it was Gurney who came up with the simplifying suggestion that the electron was *in* the vacancy.

I do not know who first used the effective mass approximation for the energy levels in a non-polar semiconductor, though Kohn first set out to justify it. Since Mott & Gurney is about ionic crystals, it is not mentioned there, though we recognized that the potential seen by an electron at a considerable distance from an F-centre must be  $-e^2/\kappa r$ , where  $\kappa$  is the dielectric constant, either static or at high frequencies. This comes out most clearly in our discussions of polarons, and on the relaxation of the medium round a centre after trapping. In fact, the book now

† See also article 15.

‡ A description of Y. I. Frenkel's work by his son, V. J. Frenkel, is given in *Archive for History of Exact Sciences*, vol. 13, p. 1 (1974). Perhaps his main contributions are his book on liquids, with his early realization of the similarities in the atomic packing between liquids and solids, the Frenkel exciton, described in detail long before evidence for it began to accumulate, work on the electron theory of metals before quantum mechanics, and the Frenkel defect. See also footnote to paper by Smoluchowski in this volume.



reads as a treatise on what we should now call 'deep levels', a subject only now coming into prominence among semiconductor physicists. Copper oxide was our 'typical' semiconductor. I believe it was only during the war, as a result of wartime work in the United States that it was first realized that silicon and germanium were semiconductors and not 'poor' metals. F. Seitz comments as follows:

I guess I deserve as much credit as anyone for getting the physics of silicon and germanium, including the determination of basic parameters, on the right track. This was done early in the war, as is indicated in the last paragraph starting on page 8 of Volume 15 of the Radiation Laboratory series.

The background history of the situation seems to be the following. It was well-known from early radio days that raw commercial (metallurgical) silicon had active spots that could provide excellent rectification in the usual cat-whisker arrangement. In fact, such rectifiers were used for experimental work as research progressed into the multi-megacycle range. Silicon rectifiers were adapted for radar work by British Thompson-Houston on a classified basis in the late 1930's and by the Germans, presumably at about the same time. In fact, there is an article in the *Zeitschrift für Physik* describing the observational characteristics of such rectifiers in 1939 or 1940. The British material was relatively crude and was very heavily doped with aluminium to give it some degree of uniformity and to make it p type.

The British technology was taken over in the United States as a result of the generation of cooperative programs early in the war, and I was asked, probably in the middle of 1941, if I would establish a research group at the University of Pennsylvania which would attempt to find out more about the workings of the material. Perhaps I should add that the Bell Laboratories was also deeply involved and had a group of metallurgists under J. H. Scaff which was trying to refine metallurgical grade silicon by selective crystallization – a fairly hopeless task at that time. This was long before the days of zone refining. I decided that we ought to start completely anew and persuaded the Dupont company to initiate a program to produce a relatively pure grade of silicon from purified silicon tetrachloride.

We began experiments in Philadelphia on the effect of various additions to this material, using whatever elements we could lay our hands on and carrying through a combination of conductivity (four-electrode) and Hall effect measurements at various temperatures. This gave us a chance to map out the general characteristics of the material containing various additions. Needless to say, our initial interest was to cover as much ground as possible rather than to achieve precision to many decimal points.

I should add that in the course of this work we did some rudimentary research with fairly pure germanium obtained from the University of Chicago. We also discovered some papers in the literature describing research carried out by physical chemists at Cornell University in the 1920's on the physical properties of relatively pure germanium prepared there. An analysis of their relatively

crude measurements of conductivity as a function of temperature showed that their measurements had actually extended into the intrinsic range and gave a reasonably good value for the band gap.

Lark-Horovitz and his group at Purdue entered the field after much of our basic work was completed. As I recall, he made a trip to the Radiation Laboratory to learn if there were programs which could be farmed out to his laboratory in Purdue. It was proposed that he cooperate with our group. He and one or two of his colleagues spent several days with us and then returned to Purdue to duplicate the basic equipment.

The work on silicon and germanium fanned out very rapidly once relatively pure silicon became universally available through the development of an appropriate production unit at Dupont. One then had a reasonable basis for standardization of measurement.

On the Purdue work, Professor Yearian of that university comments as follows:

By the time we junior members of the research group had received our security clearances and the laboratory was organized in early 1942, it was very clear that Lark-Horovitz was convinced that Ge should be a semiconductor. So far as I am aware there was no experimental evidence at that time to confirm this idea. Lark-Horovitz had obtained a few samples of Ge from various chemical supply companies and simple tests with 'cat whisker' contacts showed that although many such contacts were essentially ohmic, some gave good rectification, strongly suggestive of a semiconductor. He immediately instituted a program of electrical resistivity and Hall effect measurements as a function of temperature. These, and later thermoelectric power measurements, confirmed that Ge is indeed a semiconductor. So far as I can recall, this had been accomplished before the end of 1942.

Continuation of the experiments, as the quality of our materials improved, showed the p or n effect of different impurities and permitted determination of impurity and intrinsic energy gaps, density of carriers, mobility, etc.

The ultimate success of the project was dependent on obtaining high purity Ge for experiments on controlled doping. This became possible through a collaboration with the Eagle-Pitcher Lead Co. of Joplin, Missouri to obtain high purity  $\text{GeO}_2$ . The oxide was treated by hydrogen furnace reduction in our laboratory.

## DISLOCATIONS

Papers describing plastic flow and work-hardening of solids were published independently by Orowan, Polanyi and G. I. Taylor in 1934. I have always been struck by the fact that it was after the discovery of the nucleus, nuclear disintegration and the neutron that physicists first turned their attention to the explanation in terms of atomic movements of so familiar a phenomenon as the ductility of metals. I was in Bristol at the time, already committed to 'solid state', and I remember

being impressed by Taylor's paper particularly (apart from its merit for me that it was in English and the others in German). Taylor considered only what we now call 'edge dislocations'. He assumed that during the deformation of a single crystal: (a) dislocations were generated by a process unspecified on parallel lines random in space; (b) that they moved all on the same plane; (c) Each one moved a distance  $L$  so that the strain,  $s$ , was given by  $s = NLb$ ,

where  $N$  is the number of dislocations per unit area and  $b$  is the 'Burgers vector'. The stress  $S$  was supposed to be a consequence of the elastic interaction between dislocations and inversely proportional to the mean distance ( $N^{-\frac{1}{2}}$ ) between them; thus

$$S = \text{const.} \times bGN^{\frac{1}{2}},$$

where  $G$  is the shear modulus. It followed that

$$S = \text{const.} \times \sqrt{s},$$

a relation in accord with experiment for single crystals of face-centred cubic metals.

I remember talking to G. I. Taylor and asking him where the dislocations come from and why slip bands were formed. Of course he was perfectly aware of these difficulties and insisted that his paper was 'just a model' and not a description of reality. However, it was successful also in predicting quantitatively the stored energy in a work-hardened material. For my part, what I learned from this paper was that the resistance to shear of a crystal was a consequence of internal stresses which hindered the movement of dislocations. Nabarro and I<sup>(21)</sup> in 1939 tried to use this idea to explain the process of work-hardening. The 'Preston-Guinier zones' or incipient precipitates produced strains, which would hinder the movement of dislocations. In this work we had to take into account the flexibility of dislocations; it was three-dimensional, in the sense that Taylor's work was not.

The subsequent flowering of the dislocation model belongs of course to the decade after the war. The key points were, J. M. Burger's papers on screw and edge dislocations, the 'Frank-Read source' both in the theory of crystal growth and for the generation of dislocations, Cottrell's concept of 'pinning' by impurities, supplementing the Peierls force; observation of static dislocations by various forms of decoration (Lacombe, J. W. Mitchell, Amelinckx) were important steps, and finally there was the observation of moving dislocations by Peter Hirsch and co-workers, using electron microscopy of thin films. I shall always remember the day when his research students came into my room in the Austin Wing of the Cavendish, saying 'Prof., come and see a moving dislocation'. These developments are treated in the final memoranda in this volume.

*Note on E. C. Stoner (1899-1968) by N. F. Mott and E. P. Wohlfarth*

Edmund Stoner is best remembered for the collective electron or Stoner model of ferromagnetism, though by no means all of his work was in this area; in fact in a paper published in 1924 he suggested what was later called the Pauli exclusion

principle, at a time when neither the electron spin nor quantum mechanics were known. Collective electron magnetism is the form of magnetism which occurs when the electron spins responsible are associated with itinerant electrons in a partly filled band. On this concept Bloch pointed out in 1929 that exchange correlation between free electrons with parallel spins would enhance the paramagnetism of a metal, and might lead to ferromagnetism. Mott in 1935 introduced the model of overlapping s and d bands, and Slater<sup>(22)</sup> in 1936, made a major attack on the problem, using a partly filled energy band for copper and considered various exchange integrals as causing ferromagnetism. Stoner's major work<sup>(23)</sup> in collective electron ferromagnetism began in 1938, though his earlier work<sup>(24)</sup> (1936) considered overlapping bands. In 1938 he introduced a 'molecular field' and the great merit of his work is that he took the model seriously, calculating (for instance) susceptibility-temperature curves in detail. The model was developed after the war, particularly in collaboration with his pupil E. P. Wohlfarth. Now in 1979 one sees no contradiction between applying the model of localized electrons to the rare earth metals, and the collective electron model to the transition metals, but Stoner's model encountered a good deal of opposition, perhaps because the concepts of spin waves and the resulting law  $M = M_0(1 - \alpha T^{\frac{3}{2}})$  for the magnetic moment at temperature  $T$  were more easily derived from the localized Heisenberg model. With the realization perhaps that the Fermi surface was a real physical quantity and in a ferromagnet was different for the two spin directions, the virtues of the model were widely accepted, and it is a pleasure to record here how often Stoner was right.

## REFERENCES

- (1) Jones, H., Mott, N. F. & Skinner, H. W. B. 1934 *Phys. Rev.* **45**, 370.
- (2) Landau, L. D. 1956 *Soviet Phys. JETP* **3**, 920.
- (3) Pippard, A. B. 1957 *Phil. Trans. R. Soc. Lond. A* **250**, 325.
- (4) Baber, W. G. 1937 *Proc. R. Soc. Lond. A* **158**, 383.
- (5) Heisenberg, W. 1928 *Z. Phys.* **49**, 619.
- (6) Mott, N. F. 1935 *Proc. phys. Soc.* **47**, 571.
- (7) Bloch, F. 1929 *Z. Phys.* **57**, 545.
- (8) Stoner, E. C. 1938 *Proc. R. Soc. Lond. A* **165**, 372.
- (9) Mott, N. F. & Jones, H. 1936 *Theory of the properties of metals and alloys*. Oxford University Press.
- (10) Seitz, F. 1940 *Modern theory of solids*. New York.
- (11) Slater, J. C. 1975 *Solid state and molecular physics: a scientific biography*. John Wiley.
- (12) Wigner, E. & Seitz, F. 1933 *Phys. Rev.* **43**, 804.
- (13) Mott, N. F. 1949 *Proc. phys. Soc. A* **62**, 416.
- (14) Mott, N. F. 1974 *Metal-insulator transitions*. London: Taylor & Francis.
- (15) Slater, J. C. 1951 *Phys. Rev.* **82**, 538.
- (16) Frenkel, J. 1931 *Phys. Rev.* **37**, 17.
- (17) Peierls, R. E. 1932 *Annl. Phys.* **13**, 905.
- (18) Landau, L. 1933 *Phys. Z. SowjUn.* **3**, 664.
- (19) Gurney, R. W. & Mott, N. F. 1937 *Proc. phys. Soc.* **49**, 32.
- (20) Holstein, T. 1959 *Ann. Phys.* **8**, 343.
- (21) Mott, N. F. & Nabarro, F. R. N. 1940 *Proc. phys. Soc.* **52**, 86.
- (22) Slater, J. C. 1936 *Phys. Rev.* **49**, 537, 931.
- (23) Stoner, E. C. 1938 *Proc. R. Soc. Lond. A* **165**, 372.
- (24) Stoner, E. C. 1936 *Proc. R. Soc. Lond. A* **154**, 654.