PHYSICS IN THE SIXTIES

PHYSICS
in the Sixties

Edited by

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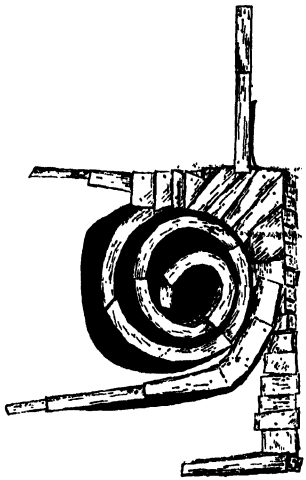

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Preface

In 1956 plans were made to build in King's College, Newcastle upon Tyne, a new physics laboratory and in March, 1956, Sir Basil Spence and Partners were appointed architects. The teaching block was completed in September, 1960, and the research block in November, 1961. On March 19th, 1962, the laboratory was opened by Professor P. M. S. Blackett, F.R.S. In addition to Professor Blackett's speech touching on matters of crucial importance to the development of physics in British Universities, a series of lectures on aspects of physics of especial interest were given. These lectures were given to representative gatherings of industrialists, school science masters (especially from the North-East), and other members of the University staff. It was felt that the lectures might have a wide appeal, covering as they do a broad spectrum of physics. It is hoped that the lectures may convey something of the extreme range of investigations now proceeding in Universities in this branch of science.

I am pleased to express my thanks to my secretary, Mrs. E. Large, and to my colleagues, Dr. E. E. Schneider, Dr. N. Thorley, and Dr. D. C. Tozer, for their help with the production of this volume.

S. K. Runcorn.

Contents

Preface v

1 Organisation Problems of Scientific Research in the Universities: —Prof. P. M. S. Blackett 1

2 Matter and Force after Fifty Years of Quantum Theory: —L. Rosenfeld 9

3 Controlled Thermonuclear Fusion: —Sir John Cockcroft 31

4 Nucleosynthesis and the Origin of the Solar System: —Prof. F. Hoyle 47

5 The Upper Mantle Project: —Prof. G. D. Garland 61

6 History and Future Applications of Superconductivity: —Prof. C. J. Gorter 79

7 The State of Physics: —Prof. R. E. Peierls 97

I

Organization Problems of Scientific Research in the Universities

P. M. S. BLACKETT

When an institution increases markedly in size, changes must be made in organizational methods. This is true of a government department, an industrial firm, a university and a university science department. I will mainly confine myself here to the post-graduate research and teaching in a university science department, though I shall also refer to undergraduate teaching.

Naturally I am drawing widely on my own personal experience as a university teacher and research worker, so my remarks may be unduly conditioned by my particular experiences. An even greater risk is that I may be taking too much the view of the man-at-the-top, for I have been head of a department, or rather three different departments, for just thirty years, and so have exercised great, but rather undefined, powers. I vividly recalled something which happened at the Princeton bi-centennial celebrations in 1946. That great British industrial scientist, Dr. C. K. Mees, creator of the Kodak Research Laboratories in the United States, gave a brilliant lecture on the right way to run an industrial research laboratory which was, of course, the way his laboratory was run. Then a young scientist from Mees's laboratory got up and spent half an hour telling us with great clarity what it was really like—as seen from the working researcher's point of view.

Much of what I have to say is generally accepted doctrine, but other parts, I think, are not. Anyway, it has been useful to me to formulate my views on these important matters, so I hope it will also be to those who hear or read them.

I am very convinced of the extreme value of associating research and teaching. In fact a large fraction of the most creative scientists of Britain are engaged in both, and it is essential to retain this. Moreover, the welcome increase of postgraduate specialized training provides a valuable part of the work of the teaching staff and one in very close and valuable relation with their researches.

Now it is clear that since before the War a numerical revolution has taken place in the production of trained scientific and engineering manpower in Britain. In 1938 the annual output was 5,000: in 1960 it was 17,000, and the present target is given as 30,000 by the early 1970's: this implies a rate of increase of 7 per cent a year over the next decade. A large part of this rapid expansion is taking place in existing university departments, which are therefore growing rapidly in size, generally at a rate of 5-10 per cent a year. Compared with 1938, an average physics, mathematics or chemistry department has nearly four times as many undergraduates and a rather greater increase of staff—thus giving a welcome improvement in the staff/student ratio. On the other hand, the number of senior posts, particularly professorships, has not increased as fast. Taking all university subjects, the ratio of professors to total staff was 1:5 in 1938 and only 1:8 to-day. In pure science the ratio is now only 1:9 or 10.

There cannot be much doubt that this failure of the number of senior posts to keep pace with the increase of the other staff, so reducing the chances of promotion and the exercise of individual initiative, has been a major cause of the loss of many outstanding scientists to the United States and to not a little general disgruntlement among university staffs. I consider that it is important that the number of scientific professorial posts be increased during the next decade to about 1 in 5 of total staff, thus achieving the general university figure of 1938.

Let us consider the position in the thirty-five university departments of physics in Great Britain. In 1961 there were about 700 total academic staff and some 65 professors, giving a ratio of about 1:10. So to achieve the above objective, about 60 extra chairs are needed (that is, nearly a doubling) apart from the number required by the planned general expansion of the

A Modern Physics Laboratory at King's College, Newcastle upon Tyne

science departments by 70 per cent in the next decade. So the total number of physics chairs by the early 1970's should be more than 200. To-day, out of 35 university departments of physics in Great Britain, 17 have a single professor, 8 have two, 5 have three, 3 have four, 1 has five and 1 has nine. Taking all sciences, technology and medicine together, the number of chairs has increased from about 870 in 1958 to 975 in 1961, that is, by about 105 in 3 years or 4 per cent a year. More than a doubling of this rate of increase with be required in the next decade to compete with the planned increase of the universities as a whole and with the restoration of the former ratio of professorial to total staff.

What are the reasons for the failure of the universities to increase the number of chairs to keep pace with the expansion of the departments? In a few cases there may have been a lack of suitable candidates: however, I very much doubt if this is an important factor except perhaps in the smaller, more remote and least well-endowed departments. Nor is it often a matter of finance: for example, the cost of upgrading a readership to a chair need only be a few hundred pounds a year. Nor does there appear to be any rigid University Grants Committee regulation against an increase of professorships.

The real reason, I think, for this serious failure is to be found mainly in the innate conservatism of universities as a whole. Of course, the universities other than Oxford or Cambridge have been influenced historically by the Continental tradition of a department with one well-paid professor and a number of badly paid juniors. In Scotland, where this tradition was strongest, I am told the creation of new chairs in some Scottish universities still requires an alteration of the statutes and the agreement of the other Scottish universities.

Fears have often been expressed that too many professors will make university senates too big and so unwieldy, and too many science professors will alter the balance of the government of a university. The first difficulty can be mitigated by suitable administrative changes and, as regards the second, it is to be remembered that to-day there are 520 arts professors to only

340 science professors. The higher professor/total staff ratio of 1 : 6 for arts compared with 1 : 9 for science, arises, I think, mainly because of the larger number of small arts departments with a professorial head but few other staff.

In the cases of a few individual professors of physics opposition to increasing their number may have been based on a reluctance to share power and prestige. More reputably, there may have been some fear of the administrative problems of the multiprofessorial department. These problems exist, and I will now consider them, specifically in relation to physics. Other big experimental departments such as chemistry and engineering clearly have somewhat similar but not identical problems. But mathematics departments again have fewer problems because of the lack of experimental research.

I would like to emphasize here that my advocacy of many more professorships in no way implies that I think a professorship should be a normal promotional grade for all members of the staff. I strongly oppose this: but I do think that there will be found available men and women of outstanding gifts to fill the new posts which I think are needed over the next decade.

Consider a moderately large physics department with an average of 100 undergraduates taking honours physics in each of three years, giving an undergraduate population (not counting students from other departments) of 300. Taking the present average staff/student ratio as 1 : 7 or 8, as seems accepted as reasonable, there will be about 40 staff in the department. Accepting further that 1 in 5 of the staff is a professor, there will be 8 professors. What should be the relation of these professors to each other and what part should they play in the three main functions of the staff—undergraduate teaching, postgraduate teaching and direction of research? There are clearly several possible arrangements which can give satisfactory results.

My own preference at present is on one hand for a completely unitary undergraduate teaching organization for a single unspecialized B.Sc. degree in which every member of the staff, including professors, takes part in the teaching: and, on the other hand, for a number of research units with the maximum possible autonomy at the postgraduate-level. In general, I think that every professor, if he wants to, should direct such a unit: in our imaginary department there will then be about eight such independent research units with an average total of five academic staff each.

Experience shows that on the average such a research unit with five academic staff may require half a dozen technicians of different grades, and if successful should attract a dozen or more research students and other research workers, so giving an average total population of, say, 25. In the Department of Physics in the Imperial College of Science and Technology, where there are 11 units, the total numbers in each range from about 12 to 45.

I would like to mention here an experiment in the architectural planning of a large physics department which we have made at Imperial College. In order to promote the maximum internal scientific and social cohesion of these research units, the new Physics Building was designed to allow so far as possible the allocation of a compact area of the building to as many units as was architecturally possible. The academic staff, including the professor together with the technicians and research workers, of a unit are all accommodated in this area. This architectural division of the research space of the whole department into more or less defined areas, housing separated research units, has a possible disadvantage in perhaps making more difficult re-allocation of space at a later date. However, its advantages are considerable over the extreme alternative, which is to house all the heads of groups in one area of the building and to distribute their respective research laboratories more or less at random over the whole building.

In my experience to-day, the average minimum size for a research unit is just about what I have mentioned, that is, five academic staff with supporting technicians and research students. Remembering that the staff are only part-time research workers, and in view of the problems of keeping up with the literature, attending conferences, etc., it is seldom possible for a much smaller group to make an outstanding contribution to a research subject in the front line of advance. Smaller and weaker groups can easily become sub-viable: they can fall below the critical size for self-sustaining research progress. These considerations apply, of course, only to groups of normally gifted workers: special arrangements can always be made for the outstanding lone worker, a C. T. R. Wilson, a Glaser or a Mossbauer.

Such a multi-professorial department can be directed in one of three main ways: one of the professors can be permanent head of the department—this is the traditional Continental solution: one of the eight can be temporary chairman—this is often done in the United States—or some form of committee can run the department. There are advantages and disadvantages in all these methods. It must be remembered that excessive departmental democracy can be very time wasting and so impede research. The main function of the central direction in relation to the research activities of the eight units is to distribute to the units the available money for research expenses received from the university, to allocate the academic staff vacancies, and, in conjunction with the university, to fill them: and finally, to allocate each summer to the units the new research students from the main teaching department. On the other hand, I think that the heads of the research units should take almost complete responsibility for acquiring outside finance, for example, from research councils or industry, etc., and for attracting research students and visitors from other institutions at home and abroad.

I would like now to refer to university research in border-line subjects. There is no doubt that the present method of finance by the University Grants Committee, which involves basing its grant to a university mainly on the number of undergraduates, does make difficult the development of specialized and border-line fields of research. Since there are few, if any, undergraduates, there are few staff and so little research, unless large outside funds are available. On the other hand, it seems likely that the importance of, and the interest in, these ever-arising new border-line subjects is likely to increase in the future.

In my view, it is generally a mistake to attempt to found an undergraduate school, giving a specialized B.Sc., at too early a stage of a new subject, simply in order to acquire the staff to make a research school. For one thing, the best students from school will seldom enter for such unfamiliar subjects: of more importance is the now general recognition that the undergraduate teaching course should be mainly concerned with the basic sciences, with only a minor degree of specialization in the last year. Training in the vitally important, but highly specialized new subjects, comes much more efficiently in the early post-doctorate years. A convenient way of dealing with some of these new subjects is to make a group working in such a field into a unit in one of the major departments of physics, chemistry, etc. As already outlined, the academic staff of these units would all take part in the general undergraduate physics teaching, but would be free to develop, as thought fit, the postgraduate specialized training required by their subject. A great advantage of this organic relation between a highly specialized research unit and a major teaching department is that it provides a potential supply of home-grown postgraduates. For example, in our imaginary department of physics with an output of about 80 graduates a year there will usually be up to 20 who can be considered as fit for a research studentship, and so an average of 2 or 3 for each of the 8 assumed research units. In addition, a good research group will normally attract at least as many or more from other universities, particularly those overseas. In the past it has been the lack of direct and formal right of access to a major teaching department which has often made such subjects as astronomy, meteorology and geophysics into the cinderellas of our university system. Sometimes, such departments have very few British research students, so that they have to rely mainly on overseas students. It is interesting to note that, because radioastronomy in both Cambridge and Manchester did grow up as sub-departments of major departments of physics, there has been a continuous flow of first-rate British physicists into this subject.

2

Matter and Force after Fifty Years of Quantum Theorv

L. ROSENFELD

1. Matter and Force

If one had asked any physicist about fifty or sixty years ago what was his view of the structure of the physical world, he would not have hesitated to say that it was based on an essential duality, on the interplay of two fundamental entities, which he would have called matter and force. This philosophy was propagated on the Continent in an immensely popular book by Büchner entitled *Stoff und Kraft*; it also gained favour in this country, under the less solemn guise of the character of the ideal physicist endowed by Tait with the name of Hermann Stoffkraft.

There was a general consensus that the structure of matter was atomistic: matter must somehow consist of elements all identical to each other, to which the old traditional name of atoms was given, although it was clear that they were not at all indivisible as the name implies, but must themselves have some kind of structure. However, our friend the physicist would have warned us that this view about the constitution of matter was entirely hypothetical: there was no direct evidence for the existence of the atoms, but only inferences from various chemical and physical phenomena.

About the structure of the atoms he would have been still more reticent. Opinions on this point had varied widely in the course of the century. At the beginning, Laplace had said that enquiry into the structure of atoms was hazardous speculation, useless to the progress of the arts. In fact, Laplace achieved the masterpiece of developing an atomic theory of matter without making any assumption about the actual structure of atoms, just by skilfully developing the consequences of the short-range character of the atomic forces. However, it was soon apparent that the atomic structure must be essentially governed by electromagnetic forces: atoms must consist of parts carrying electric charges of both signs. And by the end of the century, direct evidence was even forthcoming on the nature of the carriers of negative charge, the electrons, which had been separated in the form of cathode rays in electrical discharges in rarefied gases. One learned at that time how to ionise the atoms, how to split them into their electrical constituents, positive ions and negative electrons. One found that the electron has a mass extremely small compared with the total mass of the atom, so that practically all the mass must be attributed to the carrier of the positive charge. This was about all that one could then say definitely about the structure of the atoms.

As regards the nature of force, ideas had also fluctuated greatly. The original conception was that the atoms, in which was condensed, so to speak, the properly material constituent of the world, were held together by another entity, not material (though emanating from matter), which had the generic name of force, and could manifest itself under many different forms. The great achievement of the nineteenth century was to reduce this variety of specific forces, those governing the elastic properties of bodies, including the optical phenomena, those producing the chemical bonds, those exerted by electrical and magnetic bodies, to an essential simplicity: with the exception of gravitation, which remained something of a mystery and was cautiously kept aside, all forces then known were eventually traced to an electromagnetic origin. This result was only reached, however, after a long process of development, implying a radical change in the conception of force itself.

The starting point had been Newton's formulation of the law of gravitation as an immediate interaction between material elements, varying as a certain function of the distance between them. This description of gravitation as a "central force" between structureless elements of matter was in Newton's mind only a provisional one, inasmuch as it was absurd, from the physical point of view, to imagine that material bodies, like the planets of the solar system, could act instantaneously on each other at any distance without intermediary. In spite of Newton's warning, the epigones soon indulged in the facile expedient of regarding the concept of central force as fundamental, and inventing all kinds of specific central forces, with different laws of variation with distance, to account for the various kinds of phenomena.

As soon, however, as one started speculating on the electrical structure of the atoms, the way opened to an electrical interpretation of the chemical affinities and the cohesive forces determining the mechanical behaviour of matter. Still, this first unified account within the framework of central force physics left out one important class of phenomena: as the elastic properties of matter became better known, and the transverse nature of the vibrations of light was recognized, it appeared extremely difficult, if not impossible, to account for the optical phenomena by means of any imaginable elastic medium, obeying the same laws as the known material substances. Thus arose the vexed problem of the optical "aether", to the elucidation of which all the resources of central force physics were applied in vain.

Decisive progress was made by Faraday and Maxwell when they developed, in the realm of the electromagnetic phenomena, the more physical idea of a field of force, involving the intervention of a medium to transmit the force between the interacting bodies by successive and continuous propagation. By exhibiting the possibility of production of electromagnetic waves identical in character with those of light, the introduction of the field concept brilliantly completed the unification of the physical forces; but though in a sense it simplified the aether problem, it did not solve it. The problem was simpler inasmuch as one had to conceive only a single, universal medium with the function of transmitting all forces between the material constituents; but the question of the nature of this medium remained as acute as ever.

In fact, when considered from the point of view of the field concept, it even raised, as Maxwell shrewdly perceived, a peculiar dilemma. It was natural, according to the atomistic conception of matter, to regard the aether itself as a material medium of definite atomic structure; but then, its atoms, which would be separated by some distance, would have also to interact between themselves through this distance in order to transmit forces between the atoms of ordinary matter, and the account of such interaction would necessitate the introduction of another transmitting medium: there was clearly no means to stop the argument from proceeding *ad infinitum*. This difficulty was shunned by hopefully assuming that it would be sufficient to treat the aether as a continuous medium, endowed with suitable material characteristics, without enquiring into its possible atomistic structure. Faraday and Maxwell held the view that the atoms of gross matter were somehow floating in the continuous aether, or were perhaps just peculiar condensations of the substance of the aether moving through it. This conception had the happy consequence of leading Maxwell, in the analysis of the properties of dielectrics, to ascribe to the aether a more essential role than to the atoms of matter accidentally spread through it, and thus to conceive the production of a displacement current in the free aether by the same mechanism which gave rise to a polarisation current in dielectric media.

This is just one example of the remarkable fecundity of the seemingly so vague and hypothetical ideas on which theoretical speculation in the physics of the nineteenth century was based. At the same time, it also helps us to understand why the conclusions reached, even by great physicists like Maxwell and Boltzmann, about the structure of matter could never carry full conviction. Thus, the success of Maxwell's theory was no argument in favour of Maxwell's special conception of the aether and its relation to matter. Scepticism in such respect was most strongly voiced by Hertz, whose discovery of the electromagnetic waves gave Maxwell's theory its physical basis: Maxwell's theory, he said, is just Maxwell's equations. This statement gives pregnant expression indeed to the feeling of resignation produced by the persistent failure to construct any acceptable mechanical model of the aether.

2. *Statistical mechanics*

The aether problem, however, was not the only one confronting the physicists in their attempt to draw a dynamical picture of the physical universe. The task of the atomistic theory of matter, of accounting for the properties of gross bodies from the view that they are systems of atoms, interacting with given forces, had been accomplished by Boltzmann and Maxwell through the introduction of special methods to cope with such mechanical systems consisting of an immense number of constituents. As Maxwell said, these methods were borrowed from the Department of Statistics: when you have to cope with assemblies of a very large number of individuals, the fate of each individual becomes irrelevant; you are only concerned with the average fate of such assemblies, and you are led quite naturally to concepts of probability and statistics.

Now, the application of statistical methods to large mechanical systems yielded in particular a very simple and striking result about the way in which the energy of such a system is distributed on the average between its elements: each degree of freedom receives the same average amount of kinetic energy. It was therefore a matter of serious concern that such an immediate consequence of the general laws of mechanics sharply conflicted with an inference, apparently just as direct and convincing, about the structure of atoms, drawn from the evidence of the optical emission spectra. The emission of light was naturally interpreted as resulting from vibrations of the charged particles of the atoms around their equilibrium positions. On this view, the immense number of lines of a single emission spectrum, corresponding to different frequencies of oscillation, seemed to indicate that each atom would have an extremely large number of degrees of freedom. On the other hand, the specific heats of gases at ordinary temperatures, interpreted on the basis of the above-mentioned equipartition theorem, showed that the atomic interactions at such temperatures involved only a few degrees of freedom of each atom.

This riddle of the specific heats was regarded by the more critical minds of the time as so disturbing as to cast doubt on the very idea of the atomic constitution of matter. Boltzmann, of course, waved aside the difficulty: he was a man of strong faith. This one failure, which one could blame on our ignorance of the inner structure of atoms, could not outweigh the abundant evidence in favour of the atomistic conception furnished by the successful derivation of the laws of thermodynamics and of many thermal and mechanical properties of bodies. It is understandable, however, as we shall see presently, that other people took a different view.

The use of statistical methods in accounting for the thermal properties of matter raised another issue of a more philosophical character. The structure of mechanics and electro-magnetic theory was essentially deterministic: the physicist could pride himself that if you gave him the initial conditions required by the type of differential equations he had to solve, he could predict in a unique way the behaviour of the system for all future times, or (if there was any curiosity for this) uniquely reconstruct its past behaviour. From this point of view, the introduction of probabilities in thermodynamics was naturally regarded as an imperfection: statistics was introduced because there were no means of ascertaining the state at any instant of systems consisting of immense numbers of atoms; probability, as Poincaré put it, was a measure for our ignorance.

The contrast between deterministic and statistical description was not confined to the academic realm of methodology, however: it had immediate physical consequences of the greatest importance. It was the statistical element in the account of the time evolution of a large system that reflected itself in the irreversibility of this evolution, contrasting with the reversible character of its purely dynamical behaviour. Thus, the application of statistical considerations in this case could not be excused as a mere matter of practical convenience, or practical necessity: it appeared as a logical requirement for the interpretation of the second law of thermodynamics on an atomistic basis.

3. *Tensions and Contradictions*

Let us stop at this stage, and, on the basis of the survey we have made so far, try to visualize the state of mind of physicists at the close of the last century. One sometimes hears the view that they were then in a sort of euphoria: they had mechanics to describe the motions of the atoms and electromagnetic theory to account for the forces causing these motions and their propagation through the aether; thus, in principle at least, everything was explained by these great "classical" theories (the word "classical" suggesting perfection), and there was nothing more to do than to make precise measurements in order to put the last touches to the picture. It is clear from what we have seen that nothing is farther from the truth: what a physicist would then have felt, would be not harmony, but tension: his conscience would have been troubled by the realization that the whole conception of the physical universe was jeopardized by unresolved contradictions.

The endeavour to analyse the mechanism of force transmission between material bodies in terms of the field concept, while highly successful so far as it was confined to a formal description of the electromagnetic actions, seemed doomed to failure when concerned with finding a physical basis for this description in the form of a mechanical model of the transmitting medium. The hypothesis of the atomic constitution of matter was found at the same time to give convincing account of many properties of the bodies and to lead to contradictory inferences from thermal and optical evidence about the number of degrees of freedom of the atomic structures. Moreover, the necessity of using statistical methods in the treatment of the large systems of atoms constituting the material bodies of ordinary experience precluded any explicit fulfilment of the deterministic account of the evolution of the world, which was regarded as the ideal of physical description.

These deep-seated difficulties, it will be noticed, had a common origin: they all arose from the unquestioned requirement that the structure of the material world, including that of the universal aether, should be describable in mechanical terms. Even the most imaginative pioneers, men like Maxwell and Boltzmann, did not manage to free themselves from the mechanistic tradition which had dominated physical thinking since the days of Huygens and Newton. It is true that in many respects the ideal of a mechanical explanation of the world had been an extremely fruitful source of inspiration; but by the end of the nineteenth century, the intractable character of the accumulating contradictions led to a growing suspicion that the mechanistic philosophy had somehow (but nobody could tell how) overreached itself.

Among those who drew up in open revolt against mechanicism, there are two whose heretical views are especially instructive. One of them is Ostwald, who thought not only that there were no atoms, but that the whole of physics could be built up entirely on the sole principle of conservation of energy: all processes of the physical world could be adequately described and analysed just as transformations of energy. Now, this was going too far, and it was easy enough to expose the insufficiency of Ostwald's arguments. Ostwald's was a romantic nature; he was carried away by his enthusiastic advocacy of monism in physics: he wanted to found the whole of physics on a single principle, and he was too hasty in deciding that the universality of the energy concept was sufficient justification for adopting the principle of conservatism of energy as the only basic one. Still, there was a sound intuition in his stressing the significance of conservation laws as the expression for a fundamental aspect of the physical phenomena. He had the merit of bringing forward this view at a time when the prevalence of mechanistic considerations tended to focus attention on the detailed analysis of structure and change and to obscure the energetic aspect.

The other heretic whose independent views deserve more sympathetic consideration than they usually find is Mach. It is very unfair to remember Mach only as "the man who did not believe in atoms", without enquiring why he didn't, although he was careful to tell us, and his reasons were very interesting. Mach could not imagine that such a naïve picture of matter as a mechanical system of atoms could account for the complication of the processes that we actually observe; in other words he cast doubt on the possibility of localising the atoms and describing their motions in a simple-minded mechanical way. He emphasised the possibility that in order to account in detail for the behaviour of matter, we would need not ordinary dynamical co-ordinates, but quite different parameters, corresponding to a mode of description which would not be mechanical.

4. *Field Theory*

The reaction against mechanicism did not stop at the critical attitude evidenced by Ostwald or Mach: it soon took a more constructive turn, aiming at a new conceptual framework from which the contradictions inherent in the mechanistic one would be eliminated. The direction in which to look for a suitable starting point was actually forced upon the physicists by the desperate situation of the aether problem. In view of the deadlock in the search for a mechanical model for the propagation of electromagnetic fields through space, it was tempting indeed to treat these fields as primary entities, sufficiently defined by the laws of their interdependence and ponderomotoric action. At first, in the minds of Hertz or Lorentz, for instance, this tactical decision did not imply any deliberate abandonment of the mechanistic ideal: it was simply recognized that Maxwell's theory furnished a self-sufficient tool for the analysis of the phenomena, thus allowing the troublesome problems of the mechanical substructure to be shelved. It was tempting, however, to swing to the opposite extreme, and to start speculating whether what we call gross matter could not just be the manifestation of some peculiar distribution of electromagnetic fields.

At this juncture, the advocates of the supremacy of the field idea received encouragement from the brilliant completion of electromagnetism achieved by Einstein, and the ulterior extension of the principle of relativity leading to a field theory of gravitation. Einstein's ideas, for all their greatness, did not contribute to resolving the dilemmas of the dualistic view of the universe. What Einstein did was rather to round off the picture of classical physics by disclosing the true invariance properties of spatio-temporal description and dynamical laws, and incorporating the force of gravitation in the same general concept as the electromagnetic forces. It is true that in exhibiting the relativity of simultaneity, and more generally the role of the gravitation field as a metrical field, Einstein, developing to its last consequences the view initiated by Hertz, definitively removed from physics the aether problem: the primary constituents of the universe were now the fields continuously distributed in space and time, and the structure of which was sufficiently defined by their differential equations. However, any such universal field theory faced the task of fitting into the field description the existence and properties of the atomic constituents of matter: the problem of the material structure of aether was just replaced by the no less formidable and no less elusive one of the field structure of matter. Indeed, the problem of matter, in this formulation, proved as much of a blind alley as the aether problem, and the idea of reducing all phenomena to effects of space-time distributions of fields, however attractive it might seem, could achieve nothing in the way of relieving the tensions in the matter-force dualism.

In one important respect, however, the establishment of the theory of relativity exerted a powerful influence on the later developments leading to the solution of the contradictions in the world picture: for the first time since Newton's days, one met here a physical problem whose solution depended on epistemological considerations. Neither the physical insight of Lorentz nor the mathematical power of Poincaré could untangle the contradictions of the electrodynamics of moving bodies; Einstein in one stroke solved the riddle by recognising that the problem was essentially related to the definition of the very concepts of space and time underlying all physical and mathematical analysis of the phenomena. Only on rare occasions are physicists thus forced to reflect on the deeper meaning of the concepts they use without analysing them because they belong to common human experience; but each of these occasions heralds the beginning of a new era in the history of science, marked by an essential improvement of the adaptation of our mental representations to the reality of the external world. Such an occasion presented itself again, as we shall see, with respect to the description of atomic processes; and the great example of Einstein, as well as his direct intervention in the critical discussion of the new conceptions, contributed in no small measure to their elucidation.

The method that Einstein used in his epistemological analysis of the relativity of time has been popularised by the well-known fictions of observers, one at the station, others on passing trains, exchanging light signals, an operation which no traveller or station master would ever dream of performing, but whose consideration nevertheless is a great help to imagination in visualising the implications of the logical argument and convincing us that, however unfamiliar they may be, they are at least not in contradiction with common experience. This is the famous method of *Gedankenexperimente*, of ideal experiments, whose considerable role in the history of scientific discovery had been forcefully pointed out by Mach.

According to Mach, the function of such ideal experiments is to exhibit the relationship between the abstract concepts (in Einstein's case the concepts of space and time) used in physical description and the data of immediate experience to which they originally refer. It is only by making this relationship explicit that the significance of the concepts—as code-words, so to speak, for definite manipulations of specified apparatus—is brought to light, thus allowing us to eliminate the paradoxes which result from the indiscriminate application of insufficiently analysed concepts outside the range of their validity, and to arrive at a more accurate formulation of physical laws. Einstein was very much influenced by Mach's ideas, and his ingenious use of ideal experiments in all his work served in turn as a model for the founders of quantum mechanics.

5. *Quantisation and structure of radiation*

The decisive new element which was brought to bear on the problems of matter and force was the discovery of the quantum of action by Planck. Planck was led to this discovery in studying the thermodynamics of electromagnetic radiation, in which one was confronted with a difficulty of the same kind as that of the specific heat problem, only still more glaringly apparent. The distribution of energy among the modes of oscillation of the radiation field is not at all in conformity with the equipartition theorem, but deviates more and more from the distribution expected on this basis as the frequency increases or the temperature decreases. The occurrence of a maximum in the observed distribution for a certain ratio of frequency and temperature points to the existence of a new fundamental constant of the dimension of a mechanical quantity of action. Planck recognised that there exists in fact an element or quantum of action, such that in any radiative process the action involved can only change by one such indivisible quantum: for radiation in thermal equilibrium, this leads to deviations from equipartition of the kind just needed to account for the energy distribution found experimentally. In the following years this quantisation, this introduction of an atomicity of action, was extended to mechanical systems performing simple oscillatory motions, such as the atoms in a crystal, and it was shown that also for such material systems, the problem of the specific heats could thus be solved by appealing to the quantal character of the process of energy exchange: a striking indication that the scope of Planck's law was truly universal.

Without leaving the domain of radiation, however, another momentous consequence of the existence of the quantum of action was brought to the fore by Einstein, who applied the quantisation idea to the constitution of radiation itself. He gave strong arguments to show that the structure of the radiation field had a peculiar corpuscular character, in the sense that the energy contained in it was not distributed continuously all over space as assumed in the classical Maxwell theory, but was somehow concentrated in the form of photons or quanta of energy and momentum: such photons could be absorbed or emitted by charged material systems in elementary processes obeying the mechanical conservation laws.

This bold conception of Einstein raised, however, a problem of quite a novel character: how had one to understand this dual aspect of a definite physical entity? For there was no question, of course, of just abandoning Maxwell's conception of the electromagnetic field and returning to a purely corpuscular theory of light. Einstein himself knew perfectly well that in doing so he would not get, for instance, Planck's formula but only an approximation to it: the photon concept was an idealisation, valid only for very high frequencies. Moreover, Planck, who spent the rest of his life in efforts to minimise the importance of his own discovery, was stressing the necessity of upholding the classical theory of radiation as the indispensable basis for the explanation of the optical phenomena of interference and diffraction.

Yet, all efforts to ascribe to the electromagnetic field properties which could have simulated its corpuscular features were of no avail: it was impossible to arrive at any reasonable picture of the elementary processes of interaction between matter and radiation. Now, we know that the duality of behaviour of light is irreducible: its paradoxical character, from the classical point of view, has forced us to a radical revision of the most fundamental conceptions of physics, and brought about a revolution in our view of the universe comparable to that which Galileo and Newton initiated when they created modern science.

6. *Structure and quantisation of matter*

The crisis did not only arise from the duality in the structure of light, but also from the equally surprising discovery that a similar duality was exhibited by the elementary constituents of matter. This discovery came at the end of a long development, in the course of which our ideas of the structure of matter took definite shape. It started with Rutherford's fundamental discovery that the positive charge of an atom is concentrated in a nucleus of much smaller dimensions than the atom, so that the negative electrons had to be imagined moving around this nucleus at distances very large compared with its dimensions. Why was the establishing of this picture of the atom of such momentous importance? Because it made it absolutely clear that atomic stability, revealed by all chemical experience, was outside the competence of classical physics. In fact, according to classical electrodynamics, such a planetary system of charged particles as the Rutherford atom would radiate its energy and collapse in a relatively short time.

We were now confronted in the description of material systems with a failure of classical description as irremediable as that of the classical theory of the radiation field. In the latter case, the quantisation of action had just restored stability by preventing the accumulation of the radiation in the lowest modes to be expected on classical theory. Bohr saw that in the case of atomic systems, nothing short of the same radical departure from classical ideas could save the situation. He introduced quantisation here in the form of his famous postulates, according to which a mechanical system can only exist in certain stationary states, whose energies are determined by appropriate conditions of quantisation of action, and radiative transitions can only take place between such stationary states with emission or absorption of a quantum of radiation of the appropriate frequency.

Bohr's postulates not only gave the clue to an understanding, in principle at least, of all chemical and optical properties of matter, but above all exhibited the universal function of the quantum of action as the stabilising element in the structure and interaction of matter and radiation. However, much work remained to be done in order to incorporate these postulates in a logical way in the framework of a general theory. This task was strenuously pursued during several years from the point of view of the quantisation of the mechanical behaviour of systems of particles. It might have been accomplished in this way, but historically its completion was notably accelerated by an unexpected new development.

The quantisation of the dynamical equations carried out by

Heisenberg was a rational algorism for determining the stationary states of atomic systems and the probabilities of radiative transitions between such states: Heisenberg claimed that by thus providing a direct approach to these characteristics of the atomic processes, he had eliminated from the theory all features (such as the orbits of the atomic electrons) which were inaccessible to observation. However, as Pauli was quick to remark, he had overlooked situations in which trajectories did fall within the range of observation; after all, we do observe the orbit of the moon, although there is no reason not to apply to the motion of this body around the earth the same rules of quantisation as those of the electrons around the nucleus of an atom. This objection clearly pointed to an essential incompleteness of Heisenberg's quantum mechanics.

It was at this critical stage that help came from a line of thought entirely foreign, and even in a sense antagonistic, to the dynamical considerations pursued by the school of Bohr. The starting point of this attack on the quantum problem had been Louis de Broglie's bold speculation about a fundamental analogy between matter and radiation, manifesting itself. among other things, by the existence of the same duality of aspects for matter as for light: matter was known so far only under its particle aspect, but we ought to expect it also to exhibit wave properties similar to those of light, and connected with its dynamical properties by similar quantum relations. The question of the physical nature of the matter waves was left by de Broglie very much in the dark, but the striking elegance of his formal results stimulated Schrödinger to further speculation in the same vein. The similarity of structure between particle mechanics and geometrical optics, pointed out long ago by Hamilton, suggested that the wave aspect be the fundamental one, whereas the particle trajectories would correspond to an approximate description of the same type as the rays of light. The potential functions of mechanics would then enter into a generalized wave-equation for matter in the role of parameters determining a variable refractive index for the waves. The quantised stationary states of atomic systems found a natural interpretation as the proper modes of wave-motion defined by Schrödinger's equation. Schrödinger himself was sanguine about the prospects thus opened for a pure field theory of matter, from which all features of discontinuity implied by the concept of particle and the quantum of action would be eliminated: thus, quantum transitions would just be resonance phenomena in the interference of the waves corresponding to the two stationary states involved, and the particles themselves would be identified with 'wave-packets' formed by the superposition of the proper modes with appropriate amplitudes and phases. Closer examination, however, soon disclosed fatal weaknesses in this view: the proposed interpretation of the quantum transitions did not allow, at least not naturally, for the induced emission and could therefore not lead to Planck's formula for the equilibrium distribution of radiation; the wave-packets associated with systems of particles could not at all be pictured in ordinary space, since the wave-functions depended on the coordinates of all the particles, and moreover, even a wave-packet in ordinary space was generally too unstable to represent a single particle permanently confined to a small spatial domain.

What sealed the fate of Schrödinger's spirited attempt at tilting the balance towards the field aspect of matter, however, was the recognition that the mathematical scheme of his wave theory was actually equivalent to that of Heisenberg's quantum mechanics: the wave-function appeared as an operator effecting the transformation between a mode of description in terms of position co-ordinates and another in terms of quantum numbers of stationary states. Since both modes of description were equally abstract, equally remote from immediate physical intuition, there was no reason to favour either of them as in any sense more fundamental than the other. We were faced with regard to matter with the same duality of structure as for light, and it thus seemed at first sight that the duality of matter and force had now been doubled by a further duality of wave and particle aspects for matter as well as force.

The true implications of the situation, however, unfolded themselves with dramatic swiftness. Through their fusion, the two modes of description of matter clarified and strengthened each other. It became clear that the part played by the matter waves in the framework of the theory is an essentially statistical one: they cannot, except in limiting cases, give a concrete image of particles, but represent varying statistical distributions of particles in space and time. Such distributions, however, can be very unlike those governed by the laws of classical dynamics: for the superposition principle of wave mechanics allows for the occurrence of diffraction patterns analogous to those exhibited by Roentgen rays—a theoretical inference soon impressively verified by Davisson and Germer's fortuitous discovery of such a diffraction effect in the reflection of electrons from crystalline surfaces.

Besides the dynamical laws, modified by quantisation, the account of the behaviour of material particles requires another principle, specifying the symmetry properties of the distribution of any system of such particles between their individual states: for the electrons, in particular, this specification takes the form of an exclusion principle, prescribing that no two electrons can be in the same state. The exclusion principle, like the superposition principle of matter waves, is not amenable to classical pictures, and when combined with the latter leads to still more startling consequences. When applied to systems of identical particles interacting according to a given law, it gives rise to an additional interaction of a novel kind, called exchange interaction to remind us that it has its origin in the identity of the particles, and not at all because it could be attributed to any actual permutation of their locations. These exchange forces are of tremendous practical importance: they account for the homopolar chemical bonds, the internal field responsible for the magnetic behaviour of metals, and other essential features of atomic interactions. For our theme, however, the occurrence of such forces immediately related to the wave properties of matter is of decisive significance: it illustrates the fact that the matter waves can transmit force just as well as the electromagnetic waves.

The further development of quantum theory, involving the incorporation of relativistic features, such as the spin of the electron and the occurrence of positively charged anti-electrons, confirmed this inference and made it more precise: just as the electromagnetic field transmits interactions between the electrons, the field consisting of a pair of electrons of both signs transmits an interaction between photons; just as the electron is the source of an electromagnetic radiation and is acted upon by this field, a photon, in the presence of some system capable of exchanging momentum with it, can create a field consisting of an electron pair and experience a force from such a field. As more and more constituent entities of the universe were discovered, forming the present array of so-called elementary particles, the same mutual relationship was found to hold between any couple of such entities: under its field aspect, the one entity transmits some specific interaction between particles of the other kind; as a particle, it acts as a source of the field of the other kind and is acted upon by this field. Thus, the antagonism of matter and force vanishes from the scene, to be replaced by a universal particle-field duality affecting equally each of the constituent entities.

7. *Complementarity*

It now remains to reconcile ourselves with this peculiar duality of aspect, which had been such a puzzle since Einstein had found it in electromagnetic radiation, and which is not easier to grasp for being recognised as the common and universal basis of the conceptions of matter and force. Clearly, we are here confronted with an epistemological problem of the same fundamental nature as those of relativity theory. What do we mean by applying such words as 'particle' and 'field' to the same entity although they denote plainly contradictory conceptions? This problem was solved by Niels Bohr through the introduction of a new logical relation which he called complementarity.

We have here to do with a truly revolutionary advance in the theory of knowledge, with a widening of the frame of ordinary logic allowing us to make use of contradictory concepts in such a way as to avoid any mistake which a trivial violation of the principle of contradiction would entail. The possibility of achieving this follows from the simple remark that every one of our concepts has a limited scope of validity, and that you only get into difficulties when you try to give a concept an absolute meaning: for then you risk coming into conflict with the contradictory concept, which may also have its own range of validity. In such cases, the two concepts are said to be complementary: you must then enquire into the limits of their legitimate application, and if you are always careful of keeping within these bounds you do not run any risk of error. Thus, any apparent contradiction implied by a duality of aspects resolves itself and the duality appears as a true synthesis of the two aspects in a complementary relation to each other.

The concrete case of the complementarity of the particle and field aspects will accordingly require a detailed analysis of all the concepts used in connection with each of these aspects, in order to bring out the mutual limitations imposed on their applicability by the physical situation. To this end, it is essential to return to the very definition of such physical concepts. In order to realise what words like position and momentum actually imply, the only method is to go back to the imaginary experiments, the idealised manipulations of measuring apparatus, which allow us to assign to a physical object a definite position or momentum. This is the *raison d'être* of Bohr's famous discussion of measuring processes, by which he disclosed the origin of the reciprocal limitations, expressed by 'uncertainty relations', to the determination of quantities pertaining to the complementary modes of description. He could trace all such limitations to the law of the quantum of action, applied to the interaction between the system investigated and the measuring apparatus: it is the quantal nature of this interaction which prevents us from controlling with unlimited accuracy the two complementary types of behaviour of the system.

The kind of limitation we here meet is by no means an imperfection in our account of the phenomena: ideally, there is no limit in the accuracy with which we can ascribe any single characteristic to the system under investigation. What is implied by the complementarity relations, and so beautifully elucidated by Bohr's argument, is a much more radical transformation of our whole conception of scientific description. The object of such description is to communicate results of our experience in a language so chosen as to convey to fellow investigators a unique meaning, allowing them to reproduce, if they wish, these results or to draw safe conclusions from them. To be intelligible in this very practical sense, physical experience must therefore be formulated in terms of the classical modes of description, which have been elaborated precisely for this purpose. Experience about atoms, however, is necessarily of an indirect character, and the complementarity we here find between the classical modes of description is not of our choosing: it is forced upon us by the laws of nature and the peculiar relationship in which we, compelled as we are to use large amplifying devices to get down to processes on the atomic scale, stand to the world of atoms.

It should be clear that the explicit reference to ourselves (or at least to beings of comparable complication of organisation) in the last statement does not entail any restriction of the objective character of our account of atomic phenomena. This objectivity is fully guaranteed by the very requirement that this account should be reduced, in the last resort, to statements referring to immediate experience, common to all observers. In this connexion, the introduction of complementary relations appears, not as a hindrance, but as an indispensable logical tool to secure the correct use of an objective terminology.

In such a perspective the problem of determinism or statistical causality appears in a new light. Here again, the decision is not one of free choice or of appeal to philosophical considerations, which are nothing else than philosophical prejudices. That the fundamental laws of atomic physics are essentially and irreducibly statistical is a clear consequence of the whole situation just outlined: statistical causality is just the type of causality adapted to the formulation of these laws in a language objectively intelligible.

8. *Retrospect and prospect*

In the view of the universe to which the exploration of the atomic world has thus led us, hardly anything remains of our friend Stoffkraft's ideal. His picture of a universe governed by the rigidly determined interplay of matter and force was a *fata morgana.* Instead, we have learned to look upon atomic processes as individual responses of atomic systems to our experimental probings, following definite statistical expectations, and presenting us, according to our approach, one or the other of their complementary aspects. Yet, Herr Stoffkraft would be pleased to see that the classical conceptions, to whose elaboration the great masters of his time had devoted so much effort, still play a most essential part in the modern theories as providing the only rational language in which these theories can be formulated.

The criticism of nineteenth century trends voiced by Mach and Ostwald played no part in the modern developments. The two rebels lived long enough to witness the first experiments evidencing individual atomic processes, and to acknowledge very candidly that their scepticism on this score had gone too far; and nobody paid any attention to the other points they had made. Now, we may well recognise, retrospectively, that Mach's rejection of mechanical pictures of atomic phenomena and plea for a more abstract mode of description (although put in too general terms to have really constructive value) was at any rate not so far off the mark. One would fancy that our Schrödinger equations in multidimensional configuration space, involving further strange variables like spin, would have been much to his liking. As to Ostwald, he would hail the fundamental role played in quantum theory by the consideration of groups of transformations of variables and the resulting conservation laws; indeed, he would be entitled to point out that out of the wreckage of the edifice of nineteenth century physics the only construction emerging intact is the principle of conservation of energy.

Thus reminded that scepticism is sometimes not out of place, let us then ask what the future has in stock for us. Well, probably new revolutions in our views which may be just as sweeping and momentous as the one I have tried to describe. In the new domain of the so-called elementary particles which we are now beginning to explore it seems that the principles of quantum theory will meet limits of their validity. It is true that the fundamental postulates of the existence of stationary states and quantal transitions between them give us here continued and unfailing guidance; but with regard to the complementary aspect, the possibility of localising the processes in space and time, there are indications that we may expect some fundamental limitation. Which form it will take is impossible to say, but we may be sure that within this still wider framework the insight in the laws of atomic phenomena gained in these fifty years of struggle will retain its validity. What this long struggle, not only for the unravelling of the phenomena, but also for the elucidation of the nature of our own thinking has achieved, is greater harmony between the two sides of our existence: our multiple interactions with the external world and our endeavour to give a rational and objective account of this experience.

3
Controlled Thermonuclear Fusion

SIR JOHN COCKCROFT

The objective of research on controlled thermonuclear fusion is to produce energy from the fusion of nuclei of deuterium or of deuterium and tritium nuclei. To achieve this temperatures of 400 million degrees must be attained in deuterium or 50 million degrees in deuterium-tritium.

British research on controlled fusion began in the late 1940s at Liverpool. Craggs and his group produced a high-current spark discharge in deuterium and looked hopefully for neutrons but were disappointed. Working under G. P. Thomson, Cousins and Ware passed currents of up to 15,000 amperes in a toroidal discharge and observed the pinch effect which had in argon and hydrogen been studied ten years earlier by Tonks. They found that the pinch discharge wandered backwards and forwards across the torus, thus demonstrating one of the modes of instability of pinched discharges. At Oxford, Thonemann and his colleagues started work first on linear discharge and later on the toroidal discharges and also observed the pinch effect. Most of this work was published in 1951-52.

After this, there is a gap in publication until April 1956 when Kurchatov broke the spell by his celebrated lecture at Harwell.

During this period the work at Imperial College was moved to A.E.I. Aldermaston to enable its scope to expand and the Oxford work was moved to Harwell and the era of larger scale torus construction began, starting with a 30 cm diameter metal torus built to study the pinch effect with much higher currents. In this torus currents of about 10 kA were passed for about 10^{-3} sec. Spectroscopic measurements were started and from the

Doppler broadening of spectral lines, ion temperatures of about 10^{6}°K were estimated. At the same time the kink instability observed by Ware was found in this torus and it was found necessary to apply an axial field to stabilise the discharge and prevent it whipping about and touching the walls.

These results were sufficiently promising to begin work in 1955 on the construction of a 1-metre diameter torus later called ZETA with the objective of increasing the gas currents to about 100,000 amperes. One of the reasons for going to a larger diameter torus was to reduce the influence of impurities from the metal walls in the discharge. At the same time the A.E.I. group with Ware started the construction of a torus of 30 cm bore at Aldermaston.

Between 1952 and 1956 publication of our work stopped because we thought that these devices might be powerful neutron sources for the production of fissile materials, which could have been used for military purposes. By 1956, however, we had concluded that this was not likely to happen and our group decided that it was time to remove restrictions on publications. In April 1956 one of the periodic declassification Conferences was arranged in America to discuss this.

Just before I left for the U.S. we had the famous visit from Bulganin and Krushchev accompanied by Ivor Kurchatov. I had not met Kurchatov before but was greatly impressed by his intelligence and by his eagerness to talk about collaboration in Atomic Energy work. We had a very animated discussion at the top of the Athenaeum staircase where he was able to go much further than I could reciprocate, having had no idea of the way the discussion would go. He suggested that he should deliver a lecture at Harwell and I agreed to arrange this. I told Kurchatov in the course of a banquet at the Guildhall that I was leaving next day for the United States to discuss the declassification of this field of work. On 25th April after I had left, Kurchatov delivered his lecture in the Harwell Conference Room describing work at the Atomic Energy Institute in Moscow on a linear unstabilised pinch discharge. Peak currents ranging from 100 k.a. to 2 million amperes had been passed through straight tubes of diameters up to 60 cm in a variety of gases from hydrogen up to xenon. Theoretical calculations prior to the experimental work had suggested that the increase of temperature due to the compression in the pinch effect should take temperatures up to a level where the D-D reaction should produce powerful bursts of neutrons. High-speed photography produced pictures showed the rapid contraction of the discharge and subsequent oscillations and in 1952, soon after the experiments with the pinch discharge were started, it was found that the discharge in deuterium became a source of neutrons. Neutrons were always observed in short pulses with a steep front. Although at first it was thought that the behaviour of the neutrons could be explained by a controlled thermonuclear mechanism, serious doubts began to appear when it was found that the neutrons appeared at currents well below that calculated to produce a thermonuclear reaction. It was found that no neutrons appeared during the first contraction. They appeared at the second contraction and it was thought that instabilities were producing electric fields along the axis of the discharge tube, which could speed up deuterons by a direct process and so lead to neutron production by the D-D reaction in the classical, non-thermonuclear way.

By August 1957 ZETA had been built and operated for a month or so and a Conference was held at Princeton at which our results as well as U.S. results in this field were described. With currents of 200,000 amperes, Doppler width measurements of impurity ion spectra suggested ion temperatures of up to 5 million degrees and neutron production was observed for the first time in stabilised pinch discharges.

Soon after this, formal agreement to declassify controlled thermonuclear research was obtained and the Harwell results and some U.S. results were published in a note in Nature. This first lifting of the veil aroused tremendous interest in the Press and led to undue optimism about prospects of fusion power in the near future. The point of greatest popular interest was whether the neutrons observed were due to a "true controlled thermonuclear reaction". The note published in Nature stated specifically that "the neutron flux so far obtained was insufficient to obtain a sufficient accuracy of measurement to show that a thermonuclear process was responsible for the neutrons". We emphasised time and time again that many years of intensive work would be required to obtain a laboratory thermonuclear device which would produce more energy than it consumed and that after this it would take many more years to develop a full-scale power producing unit. We were not believed.

All of this was a precursor to the very exciting 1958 Geneva Conference on the Peaceful Uses of Atomic Energy, when for the first time C.T.R. work was fully discussed between East and West and a dazzling display of model C.T.R. devices actually working was produced by the Americans. They unveiled for the first time the Princeton Stellarator, the Livermore Mirror Machine, the Los Alamos Scylla, and the Oakridge DCX; whilst the Russians had models of their OGRA, a parallel to DCX, and ALPHA, a copy of ZETA made in the remarkably short time of six months—a terrific feat. In spite of this wealth of revelation the elusive "true thermonuclear reaction" was not claimed or produced. By this time we had become much more aware of the ways in which DD fusion reactions could be produced by other mechanisms.

Three years after the Geneva Conference there was another stocktaking, at the Salzburg Conference. During the preceding three years there had been a marked development of techniques for finding out what was going on in the high temperature discharges. Radiation losses in the infra-red from plasma in ZETA are measured by an infra-red spectrometer viewing the plasma from 10 cm away and later using a semi conducting detector of n type indium antimonide making use of cyclotron resonance. This has the advantage of a microsecond response time and enables time resolved measurements of radiation to be made in the wavelength region 0·1-2 mm. Electrostatic analysers measure the spectrum of electrons hitting the walls of the discharge tube. Vacuum ultra-violet spectrometers are used to observe the energy loss in the ultra-violet region and the time variation of the different states of impurity ions such as N(V), O(V), and O(VI), showing the consecutive excitations.

During the same period ZETA had been reconstituted. To get rid of troublesome power arcs at the walls a corrugated stainless-steel liner had been fitted, the stainless steel being so thin that the liner did not short-circuit the discharge appreciably. This also enabled the interior to be baked out and a better vacuum of the order of 10^{-6} mm to be obtained. A large condenser bank was installed allowing currents of up to 900,000 amperes to be passed and results were given at Saltzburg for several hundred thousand discharges of between 200 and 600 k.a. with B_z fields of 1,800 gauss with no electrical breakdowns of the torus.

As a result of the improvement in diagnostic techniques and better behaviour of ZETA many more measurements could be made and two main sources of energy loss were identified. One mechanism of loss is through radiation loss from impurity ions. This radiation is almost all in the ultra-violet at 1,600 Å. Figure 1 shows that the radiation loss varies between 10 per cent and 100 per cent of the input energy as the pressure increases.

At low pressures of less than about 1 m. torr, energy is lost through plasma crossing the lines of force which, on a simple classical picture, ought to contain the plasma, since ions should spiral round the lines of force. Gibson and Mason detected the plasma close to the walls by probe measurements. The electron density close to the walls was shown to be about 3.10^{13}. The energy of electrons reaching the walls was measured by an electrostatic analyser. They had a Maxwellian Temperature distribution with energies ranging from 15-50 e.v. depending on pressure and contamination. High electron temperatures were observed at low pressures. At 5 m. torr they had only a few e.v. and only a small proportion of the energy was lost in this way. The plasma diffusion was explained by electrical field fluctuations of a magnitude and frequency which could explain the diffusion across the lines of magnetic force. The fields are of the order of 50 volts/cm and are principally transverse to B. The well-known $E \times B$ diffusion of plasma as a whole takes place.

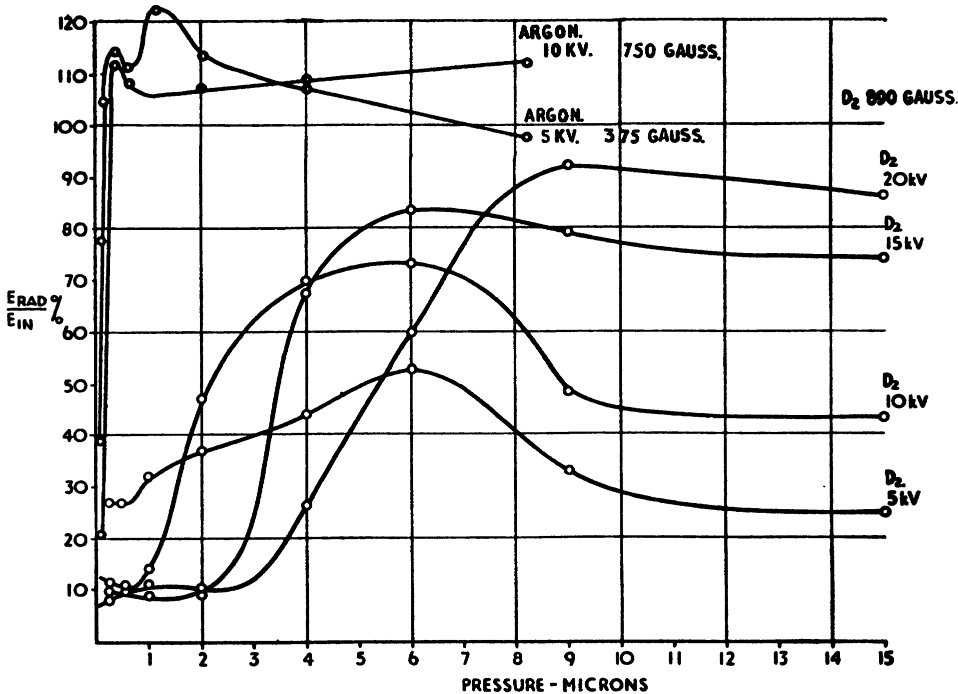


FIG. 1. Radiation loss as a proportion of input energy.

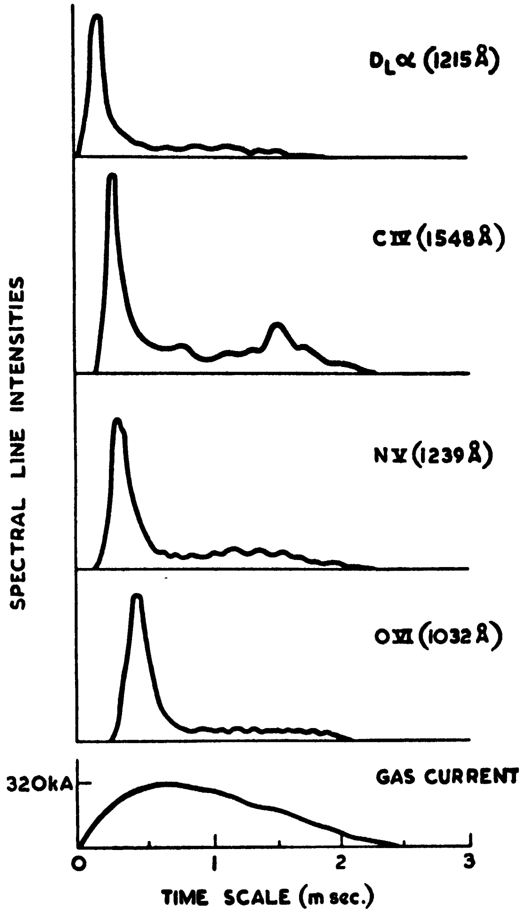


FIG. 2. Spectrum line intensity transients of Deuterium and the Lithium-like impurity ions.

Still more striking evidence was proved by spectroscopic analysis of the life history of impurity ions in plasma. Fig. 2 shows the successive excitation of impurity lines. The intensity rises to a maximum and then decreases as the next ion species is produced. These measurements provide evidence for the variation of plasma density and show that in addition to the so-called pump-out effect (loss of plasma to the walls) there is an injection of cold gas from the walls through evaporation and unipolar arcs, so that the total amount of gas in the plasma rises to a maximum and then falls again. When the plasma line density falls to $6 \cdot 10^{16}$ cm^{-1} there is an abrupt termination of current and simultaneously a runaway of electrons leading to X-ray emission and a large part of the total energy in the torus at the end of the pulse is dissipated by this mechanism. Electron temperatures in the plasma have been measured by microwave, infra-red and ultra-violet spectroscopic techniques and are between 2 and $4 \cdot 10^{5}$° C. The general result of 2 years work with ZETA was therefore a marked growth of understanding of plasma physics but little progress towards higher temperatures.

Further progress was reported at Salzburg on the Livermore multi-compression experiment of R. F. Post. In these experiments plasma is contained in a mirror field and moves from one stage to another gradually being compressed by increasing the magnetic field and so heated, Fig. 3. After two stages of compression, ion temperatures of the order of 30 million and densities of the order of 10^{13} were reached. A third compression increased the temperature but was not as successful as had been hoped, plasma instabilities leading to loss of the plasma in less than 100 microseconds.

In a recent article by Colgate and Furth the various kinds of plasma instabilities have been discussed. Plasma instabilities are of two major kinds: hydromagnetic and microinstabilities. Hydromagnetic instabilities do not depend on the particle nature of the plasma. "The plasma can be treated like a conducting fluid using the well known method of hydrodynamics modified by the electromagnetic forces. The instabilities depend on the gross configuration of the plasma and of the magnetic field which confines it".

One particular kind of hydromagnetic instability is illustrated in Fig. 4 taken from the paper by Colgate; it permits the plasma to escape from a mirror field without much distortion of the magnetic field. The opposite drift of ions and electrons produces a current which interferes with the field so as to push the whole

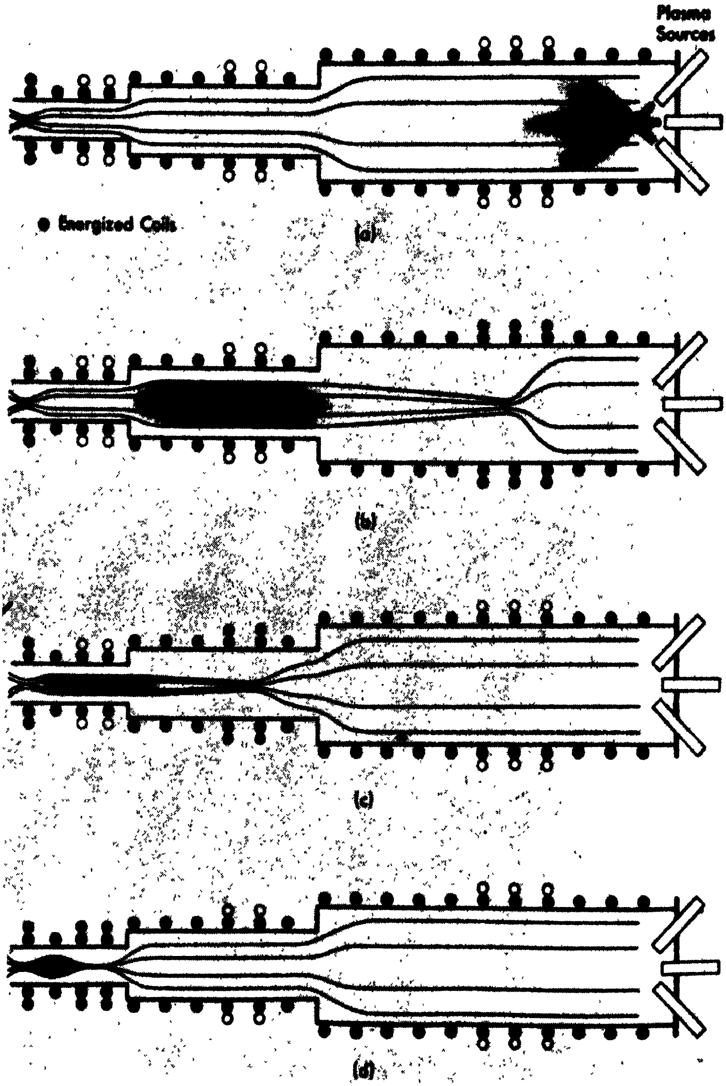
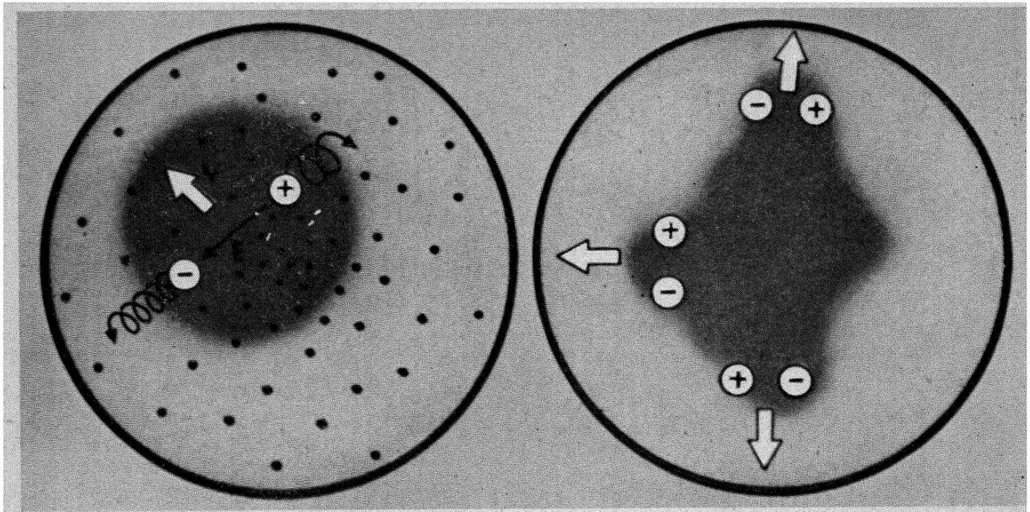


FIG. 3. Multicompression plasma experiment of R. F. Post.



(a) (b)

Fig. 4. (a) Hydromagnetic instability.
(b) Flute instability.

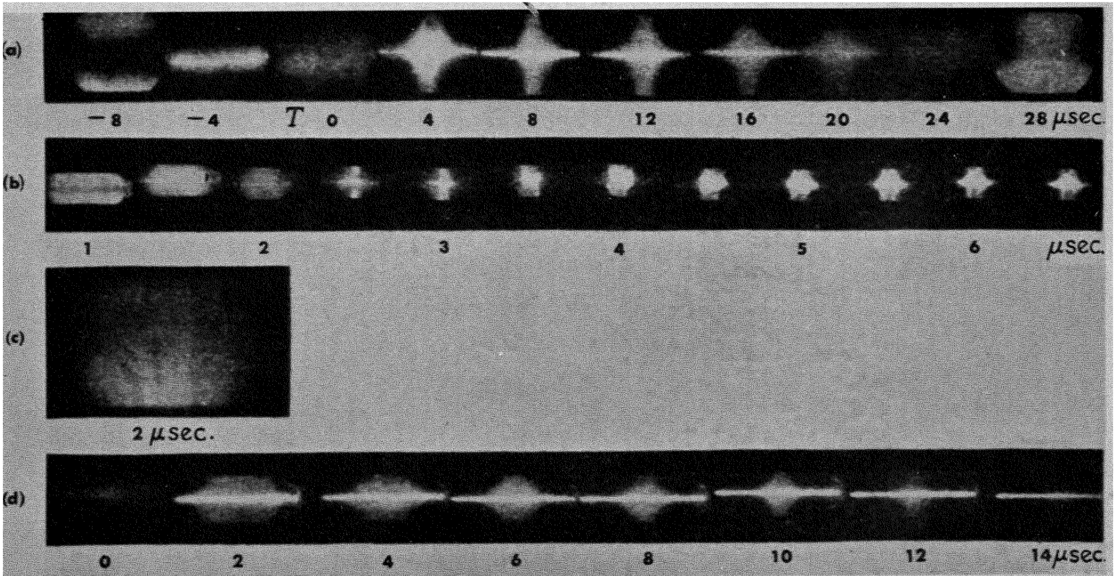


FIG. 5. Plasma compression in Cusp field.

plasma across to the wall. A similar mechanism applied to smaller sections of the plasma gives rise to the so-called flute instability.

"Microinstabilities depend on the detailed distribution of particle velocities in the plasma. They derive the energy building them up from the energy of directed motion inside the plasma. For example when a current flows through a plasma the kinetic energy of the current carriers can give rise to fluctuating high frequency fields." This is made use of in travelling-wave tubes.

"Unstable modes have been found in such abundance that it appears that whenever an appreciable energy source is available —in the thermodynamic sense—the plasma will find a way to couple instabilities to this source."

To minimise instabilities the pressure of the plasma should be low in relation to the magnetic field pressure and the departure of the particle velocity distribution from thermal equilibrium should be minimised. One should also try to choose a configuration which is in theory hydromagnetically stable.

Another attack on the problem of producing a hot plasma is by injecting molecular ions into a mirror field and then dissociating the ions and trapping the atomic ions. The dissociation was carried out in DCX by passing the injection ion beams through a powerful axial carbon arc. In the Russian OGRA the molecular ions are dissociated by collision with molecules of residual gas in the chamber. The plasma formed in this way has been found to be subject to instabilities. The low concentration plasma of OGRA was subject to "flute instability". However, it was found that the instability could be reduced by means of an additional magnetic field which increased from the centre outwards. In spite of this the ion concentration had not been raised beyond $10^7/cm^3$. In the DCX machine the bun-shaped plasma region was shown to expand and to be subject also to instabilities.

A.W.R.E. have developed an interesting variation of the injection mirror machine which they call PHOENIX. In this machine neutral H atoms, produced by dissociation of 90 Kev H_2^+ ions, are fired into the machine. In the process of molecular dissociations a small proportion of neutral atoms are formed at

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higher excited levels and these atoms are then ionised by entering the mirror magnetic field. The probability of ionisation depends on the vector product of the speed of the particles and the intensity of the magnetic field. The effect of this Lorentz ionisation should be to increase by two orders of magnitude the plasma density which can be built up initially. The fourth line of attack reported at Salzburg was the so-called theta pinch developed from the 1958 Scylla of Los Alamos. In the theta pinch devices, a rapid compression of the plasma is obtained under the action of a rising solenoidal external magnetic field, produced by a fast condenser discharge. The apparatus has the merit of simplicity. In processes of this kind the trapped magnetic field is maintained in a direction opposite to that of the magnetic forces pressing against the external surface of the plasma. The energy of compression appears in the kinetic energy of the ions. Plasma densities of the order of $10^{17}/\text{cm}^3$ and electron temperatures in the region of $10^{7\circ}$ have been measured for periods of a few microseconds. The plasma was reported to be subject to flute instabilities, particularly during compression, when under some circumstances the lump of plasma looks like an ink blot or a "resting octopus with a hole in the middle", according to Artsimovich.

Another approach to the problem of the confinement of a plasma is to use the magnetic field configuration which has a hyperbolic shape—the so-called cusp geometry, Fig. 6. In this geometry the magnetic field strength increases with distance from the centre, where it is zero. According to simple theory this should be hydromagnetically stable. This theoretical advantage is however offset to some extent by leakage of charged particles out of the magnetic trap. Experimental work is directed towards seeing whether in fact the plasma is stable and to determine the importance of the loss. T. K. Allen and his collaborators have experimented at Harwell. The initial neutral gas is ionised and preheated by a condenser discharge. The plasma is then compressed and heated by currents flowing in opposite directions in two single turn coils. Fig. 5 shows the course of events. The approach of the shock fronts is seen followed by the first half cycle and part of the second cycle of the cusp field. In (b) the growth of an instability is shown after 2-3 microseconds in a discharge where the speed of contraction exceeded the speed of sound in the gas, (c) shows a flute instability in helium. (d) shows a discharge in hydrogen in which no instability is observed and the full course of adiabatic compression. Spectroscopic measure-

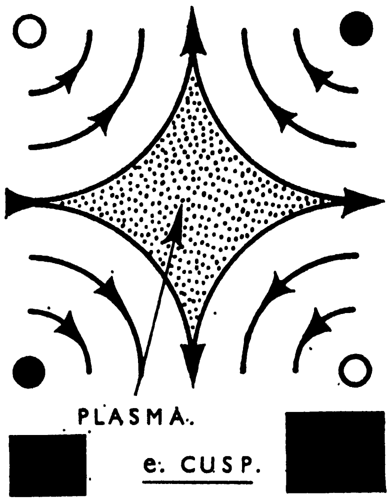


FIG. 6. Cusp Geometry.

ments showed that the plasma density of the order of 10^{16}/cu. cm had been reached with electron temperatures of about 100,000.

The progress towards the goal of fusion power is shown in Fig. 7 (Colgate et al). To achieve our objective we have to reach high temperatures, high densities, and a long plasma containment time.

Artsimovich summarised the Salzburg discussion by saying that in evaluating future prospects "the point that gives rise to the greatest uncertainty is the nature of the danger connected with various forms of plasma instability". He said that "all our original beliefs that the door entering the desired region of ultra high temperatures would open smoothly at the first powerful

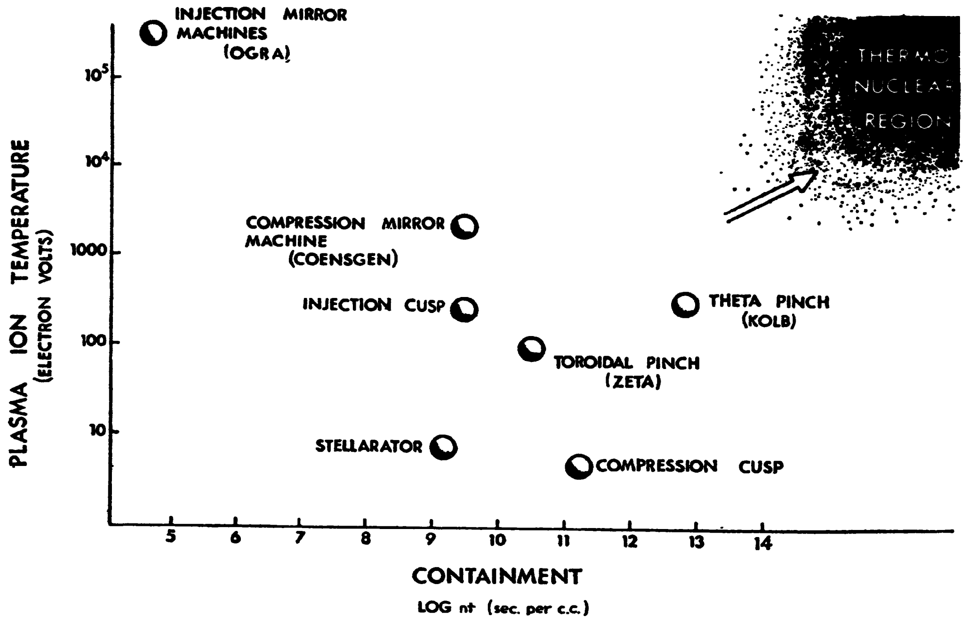


FIG. 7. Progress towards Fusion Power. The successful machine must be able to produce a plasma ion temperature of about 10^5 electron volts and a containment time of about 10^{14} sec/cc as shown in the dark area on the right.

pressure exerted by the creative energy of physicists had proved as unfounded as the sinners hope of entering paradise without passing through purgatory" and "yet there can be hardly any doubt that the problems of controlled fusion will eventually be solved, but we do not know how long we will have to remain in purgatory".

Since Salzburg there has been a period of rebuilding apparatus to take account of the lessons learnt in the preceding 3 years. The British group is building a variety of medium scale devices for installation at Culham, which is being planned as a research centre in plasma physics willing to open its doors to all comers from East and West. The installations will include PHOENIX, a cusp device and a theta-pitch apparatus.

At Princeton a large-scale Stellarator C3 is now starting operation, Fig. 8. The Stellarator has an ideal geometry from the point of view of magnetohydrodynamic stability. Ohmic heating is used to heat the plasma to temperatures of the order of 1 million degrees. Beyond that radio frequency heating known as ion cyclotron heating is to be used. Already some instability difficulties have appeared during the ohmic heating.

There has also been a development in numerous laboratories of practical application of plasmas; in particular, the attempt to convert heat energy to electrical energy at high efficiency by the so-called magnetic hydrodynamic generator. This generator is simple in principle. A high temperature electrically conducting plasma is formed by heating a gas and injecting an impurity such as caesium or potassium which increases the electrical conductivity. The hot moving plasma is passed through a strong magnetic field and electric currents are produced at right angles to the velocity and field. One of the problems seems to be to produce electrodes which will stand up to the high temperatures. The Imperial College group last year extracted powers of 300 KW for a period of 100 secs from a plasma of shock ionised argon travelling up to 400,000 cm/sec through a field of 10,000 gauss. Currents of more than 10,000 amperes were drawn. There are, also, the very interesting applications of plasma physics to solar and ionospheric studies and the Culham group are preparing

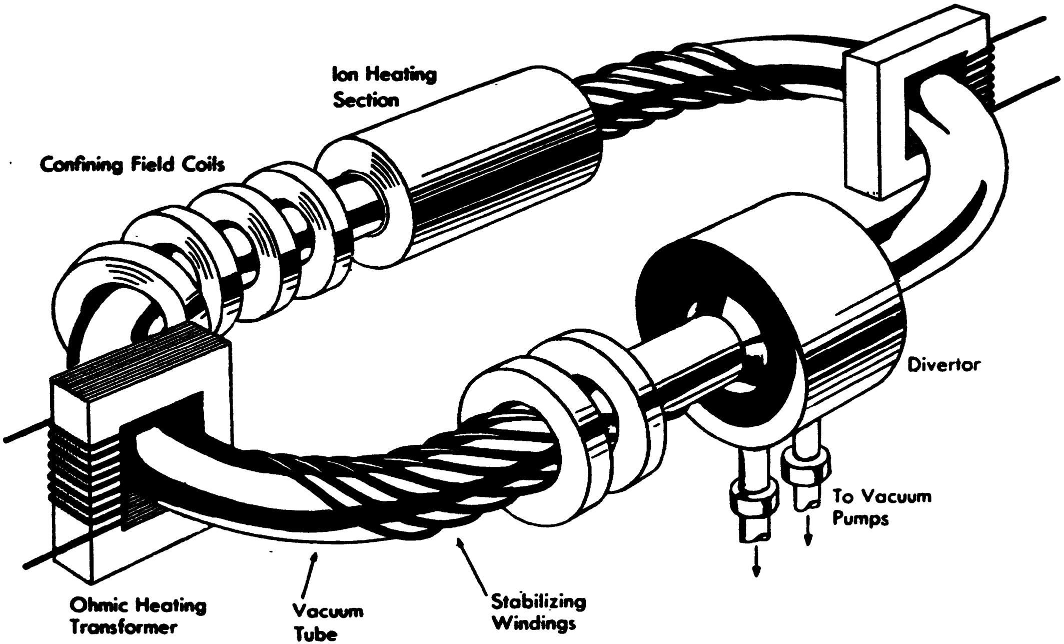


FIG. 8. Model C Stellarator.

to use Skylark rockets to measure the spectra from plasma ions in the soft ultra-violet region and to measure soft x-ray emission from the sun. These experiments may throw light on the mechanism of plasma excitation. We have, therefore, developed a very interesting new field of physics. Whether or not it will lead to large-scale fusion power is still as uncertain as it was five years ago.

March, 1962.

4
Nucleosynthesis and the Origin of the Solar System

PROFESSOR HOYLE

This report deals with a piece of work carried out with two collaborators, W. A. Fowler and J. L. Greenstein. Fowler always refers to this subject as "Deuteronomy" because it concerns the origin of deuterons, and he frequently quotes from the book of Deuteronomy the words "and the Lord showed signs and wonders". The signs and wonders in question are written in Table I, and what I wish to convince you of is that a great deal of the history of the solar system is actually written in these numbers. They are the relative abundances of a group of the light elements. This group of light nuclei has always presented a great difficulty in understanding the genesis of the chemical elements. One of the main processes now going on in the sun is a complex of reactions that have the effect of producing He^4 from hydrogen. In the course of this complex of reactions deuterium is indeed produced, but only in a concentration at any given moment of about 1 part in 10^{17}. This is very far removed from the 1 part in 10^4 that we find in terrestrial water, as shown in Table I.

TABLE I
Relative terrestrial meteoritic abundances

	$Si = 10^6$
D^2	0.6 (1.5×10^{-4} H^1)
Li^6	7.4
Li^7	92.6
Be^9	20
B^{10}	4.5
B^{11}	19.5

In the next stage of synthesis inside stars three alpha particles are turned into C^{12}. This is a "one bite" process; it is a triple collision in which carbon is produced. And so the elements of the group in Table I are entirely by-passed in this reaction. Yet of course we know that they exist on the earth. The difficulty in any theory of the internal stellar origin of deuterium, lithium, beryllium and boron, is that all these nuclei are very weakly bound, whereas it is a characteristic of reactions inside stars that they always tend to favour nuclei that are very strongly bound. So it is quite clear that one cannot appeal to internal reactions for support in such a theory.

Some years ago, in a paper by the Burbidges, Fowler and myself, we discussed an explanation in terms of spallation reactions, namely reactions in which an energetic incident particle hits a nucleus and produces a spray of other particles. The point about this is that the energy of the incident particle is large compared with the nuclear binding energy and therefore it does not matter too much whether a bit of the spray that comes out is strongly bound or not. One is describing a process that does not particularly favour strong nuclear binding because the energies concerned are so large. One might expect that when a nucleus is struck with a large amount of energy it should split entirely into its component parts. This does not happen, however.

When we wrote our paper, we referred to this spallation process as the X-process. There were several possibilities; the reaction could occur on the surfaces of the stars or in super-novae. Another possibility was that the reactions were purely local, occurring at the time when the planetary system was formed, and that is the point of view I am going to take here.

The reason why we believe that this point of view is right is that Greenstein and Bonsack have recently shown that in stars that are newly forming there are unusually heavy concentrations of lithium. For spectroscopic reasons they do not examine beryllium and boron; but during the formation of stars the lithium does exceed, by factors of 10 or more, the normal abundances. (See Table II.) Some stars known as T-Tauri stars, named after the star T-Tauri which is a prototype, have been judged by

George Herbig to be newly forming; and it was in these stars that Greenstein and Bonsack found an unusually high concentration of lithium.

TABLE II

Logarithmic abundance ratios of Li for various stars compared with hydrogen

Star type	Log [Li/H]
Sun	-11.0 to -11.15
T-Tauri stars	-8.4 to -9.5
Magnetic A Star	Weak or absent
G—K dwarfs	< -12
A dwarfs	Weak or absent

Now, if spallation reactions are the source of the group of nuclei that we are discussing, the argument I have mentioned, that nuclear binding does not matter very much, would immediately lead one to think that there really ought not to be very much difference in the abundances of these two nuclei. Indeed the calculations that have been made suggest that the abundance would be 1 part Li^6 to 1 part Li^7. You can see immediately, however, that this is not what we find in Nature. Table I shows their relative abundances on a scale in which silicon is rated at a million. The Li^7/Li^6 ratio is about 12 to 1. So there is something very queer here and it is the explanation of this discrepancy that I am now going to attempt.

TABLE III

Spallation σ_s and thermal neutron σ_n cross-section

	σ_s	σ_n
D^2	100	0·57
Li^6	15	945×10^3
Li^7	15	33
Be^9	11	10
B^{10}	38	3813×10^3
B^{11}	11	<50

All cross-sections in millibarns.

Table III shows the spallation cross-sections, estimated in millibarns for spallation reactions with protons of 500 million volts. The boron spallation cross-sections are not known very well, but we should expect approximately equal abundances to be produced, whereas the numbers in Table I do not show this at all. Now the explanation seems to stand out very clearly as soon as one looks at the cross-sections for thermal neutron processes; one sees immediately that the two cases where abundance is low, B^{10} and Li^{6}, are precisely those for which there is a very high cross-section.

It is true, I might mention in parenthesis, that one can find a rather similar explanation for Li^{6}. If, instead of using neutron reaction, one uses a proton ($p\alpha$) reaction and a temperature about the same as that of the centre of the sun, one can destroy Li^{6}, one can scour it out; but what then emerges is that it is B^{11} you scour out, not the B^{10}. The use of a proton produces quite the wrong effect on the boron. It is only the neutron reactions that give the explanation of both. It is seen moreover that the unusually large amount of Li^{7} is also explained; for the very reaction which destroys the B^{10} and gives it a lower value increases the abundance of Li^{7} by the formation of still more Li^{7}.

This then is a beginning. Already, however, the explanation requires the presence of thermalised neutrons, and this is the big surprise. There does not seem to be any other nuclear explanation of these very strange abundance ratios. Now the presence of thermal neutrons for these reactions requires that the hydrogen concentration must have been low at the time these processes occurred; most of the original solar hydrogen must have been taken away from the materials that now form the earth and the meteorites. Why this? Thermalised neutrons could be present even if there were a lot of hydrogen, because the fast neutrons could be produced by the spallation and they would certainly be thermalised by the hydrogen. In the presence of a solar concentration of hydrogen all the neutrons are taken up by the hydrogen, and in effect there is nothing left over to react on a constituent such as Li^{6}, which is in very minute quantities compared with hydrogen in ordinary solar material. So it is quite clear that one cannot expect any such reaction in the presence of a normal solar proportion of material, and this immediately leads to the prediction that lithium in the sun would not have these strange proportions. This is a clear-cut prediction. If one accepts this argument, and I think it is a strong one, then any theory of the origin of the planets that conceives a primitive proto-planet forming with the solar concentrations of all the materials, i.e., with big hydrogen excess, is certainly wrong.

What this argument supports is the following picture. The original hydrogen is pushed by an outward motion away from the sun, and as it moves outwards it leaves behind, in the form of solid particles, the materials that have subsequently gone to form the earth, the meteorites and the other inner planets. In other words, magnesium, silicon, and iron are largely left behind in the form of solid particles. These drop out of the gas simply because they have low saturation vapour pressures at the temperatures in question, which in this case are only a few hundred degrees; and any solids that form and become sufficiently big not to be carried by the gas tend to be left behind. Then the planets subsequently aggregate from these smaller particles. Later I shall show how big these particles have to be.

The picture, then, is that the hydrogen, the gases, the things that do not condense at temperatures of two or three hundred degrees, continue to move outwards, leaving the solids behind. Then these solids are subjected to bombardment by flare activity at the surface of the primitive sun, i.e., by high-speed particles which strike them after the hydrogen has gone. Now there is another reason why there must be solids, why all this must happen in the solid phase rather than in the vapour phase, and it is this: if it happened in the vapour phase, at the low densities that would then exist, the neutrons would decay before being absorbed. What is required on the other hand is that a particle should hit some solid and produce spallation neutrons, which are then thermalised in the solid within a short time and are captured long before they decay. This is the general picture.

Now, I shall say a little more of why we think the gases move outwards. It is known from considerations of star formation, that the original angular momentum of the primitive sun must have been of the order of a thousand times the angular momentum of the present-day sun. The factor of discrepancy is so large that I do not think there can be any question that there is a significant problem here. We have to explain why the present-day sun rotates so slowly and has so little angular momentum. The explanation seems to lie very close to hand when we observe that the calculated angular momentum of the planetary material, allowing for gases that seem to have escaped from the solar system, is, within a factor of 2 or 3, in agreement with the calculated original angular momentum obtained from star formation considerations. So it seems one can say that the angular

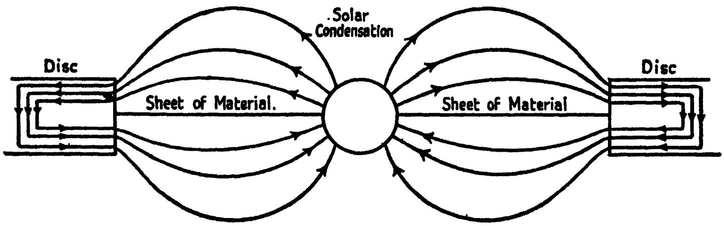


FIG. 1

momentum of the original solar condensation was stored in some fashion in the planetary material, not in the sun. This seems to demand the existence of a torque coupling between the sun and the planetary material. Some thirty or forty years ago there seemed no possibility of understanding how torque couplings of the required type could exist. Soon after the war Weizäcker tried to prove its existence by a theory of turbulence, but again the attempt failed because establishing communication from an inner body to outer material by means of turbulence is not satisfactory. Yet it is perfectly possible for magnetic fields to cross space from a given massive body to material that lies at some distance; and these fields could carry stress through space and transmit angular momentum. In Fig. 1 are represented lines of force extending to planetary material and returning again to the Sun; a much less symmetrical picture than this one could well be correct without really affecting the nature of the problem. The only important physical point which has to be noticed is that there must be a sheet of gaeous material to supply a normal pressure, and to keep the two fields apart. If there were a complete vacuum, then the lines of force would "bite off". A thin sheet of material along the plane of the solar system is needed to prevent that.

If one pictured this "face-on" it would appear as in Fig. 2.

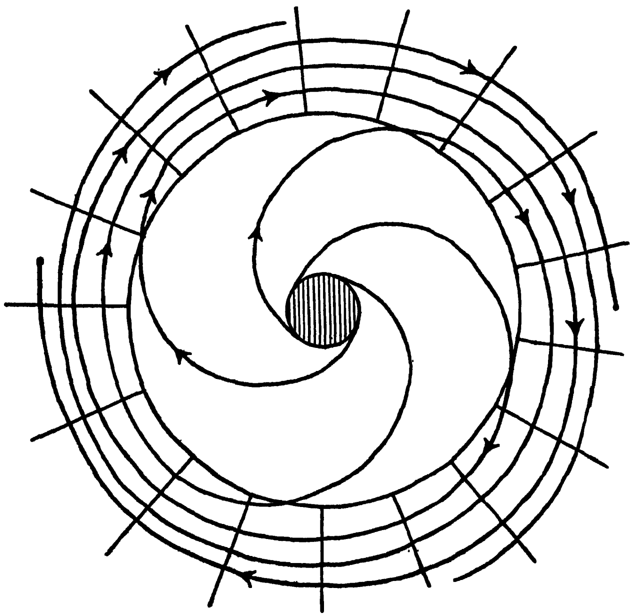


FIG. 2

Now, since the sun revolves more quickly because of its smaller size, it twists the field as shown in the diagram. The lines of force do not really end; we are looking at only one hemisphere and schematically there is a symmetrical picture for the other hemisphere. Any structure such as this carries a Maxwell-Faraday stress, and one can easily calculate orders of magnitude and show that the magnetic intensity at the surface of the primitive sun needs to be of the order of one gauss. This could be effective in transmitting the angular momentum from the sun to the planetary material, if the sun has been spinning fast enough to develop an outer disc of planetary material in the Laplacian sense. Now, knowing something that Laplace did not know, namely the existence of magnetic torque coupling, we see that it is perfectly possible to transmit the angular momentum.

That is the story as far as angular momentum is concerned. What happens as regards energy? The torque coupling, of course, does not at all mean that the original energy of rotation of the sun must be transmitted to the planetary gas. The conservation between the planetary material and the sun is a conservation of angular momentum, not of energy.

The situation for energy is this. We start with the energy of rotation. The rotation does work against the magnetic field and this goes first into twisting the magnetic field, and then what one hopes to find is that the energy which has been stored in the field now appears in flare activity of the type that occurs at the surface of the sun at the present day: electromagnetic processes operate, in which accelerated particles are produced and light is emitted. Now we know how much energy is involved. It is about 10^{45} ergs, and it is entirely possible that as much as 10% or even 20% of that finally emerges in the flares. One of the big surprises about solar flares is that they are astonishingly efficient in their production of high speed particles. So it is not unreasonable to suppose that something of the order of 10^{45} ergs appears here as accelerated particles. The time scale for this is just the time scale for the contraction of the primitive sun, which has been known since the time of Helmholtz and is of the order of $T=3\times10^{14}$ sec., about ten million years.

Such, then, is the astronomical background of the nuclear physics to which I now wish to return. I have given a sort of nuclear introduction and an astronomical one. I should like now to turn to a few details and consider the following formula:

$$\frac{dLi^6}{dn}=\alpha-\frac{\sigma\,Li^6}{\Sigma}$$

The quantity n that appears refers to the number of neutrons produced; so it does not really matter what particular volume is referred to; it can be regarded as unit volume. The equation represents the rate of increase of the number of Li^6 nuclei expressed in terms of the production of neutrons as the variable. Let us consider the nucleus Li^6 produced by Li^6 spallation and destroyed by the Li^6 (n, α) T reaction. The quantity α is the ratio of Li^6 nuclei to the number of neutrons produced by spallation, which is approximately constant and independent of n. Of course it would be wrong to write the equation simply as

$$\frac{dLi^6}{dn} = \alpha$$

because we have seen that as you make the Li^6 you also destroy it by means of the neutron reactions. Some of the neutrons hit Li^6 and are absorbed. One has to subtract from α the second term on the right hand of the equation. That term consists of σ (the absorption cross-section for thermal neutrons) multiplied by Li^6 and finally divided by Σ (which represents the total effect of the thermal neutron cross-sections for all the various atomic species present). The quantity Σ does not change during the whole process because the number of neutrons produced is not sufficient to induce major changes in the abundant nuclear species like silicon. So Σ is effectively a constant. The Li^6 is the only variable in the above equation which therefore integrates to give

$$Li^6 = \frac{Li^6_s}{\sigma}\left(\frac{\Sigma}{n}\right)\left[1 - \exp - \frac{\sigma n}{\Sigma}\right] \approx \frac{Li^6_s}{\sigma}\left(\frac{\Sigma}{n}\right)$$

The Symbol Li^6_s has been written for the total number of Li^6 nuclei produced in the synthesis, i.e. σn. This is the number of lithium nuclei that would have been made if there had been no destructive reaction. What you would have made must be something like the Be^9—quite close to it, because Be^9 is not destroyed by any reactions and the spallation cross-section for the production of Be^9 is quite close to that for the production of Li^6. From Table I we see that the ratio Be^9 to

Li^6 is 20 : 7, a factor of approximately 3. In Table III we find that the cross-section for thermal neutrons on Li^6 is approximately 950 barns (or 945×10^3 millibarns). Therefore, if we measure all cross-sections in barns as units, we find that $n/\Sigma = 4 \times 10^{-3} = 1/250$ barns for terrestrial matter. What has emerged from this is that the ratio of n (the total number of neutrons per unit volume) to Σ is of the order of 1 : 250. The unit of volume clearly does not matter because it does not affect the problem. One has to remember that in using explicit numbers the cross-sections that are contained in the definition of Σ are in *barns*.

If we now take account of the neutrons produced in dense material and so completely absorbed, we instantly have an equation showing that the total number of neutrons which have been made and absorbed is the product of the flux ϕ_n and the time during which the process operates, multiplied by Σ. Writing an equation for ϕ_n we get $\phi_n = 10^{24} n/T\Sigma$. Using $n/\Sigma = 4 \times 10^{-3}$ and $T = 3 \cdot 10^{14}$ seconds or 10 million years, we can calculate the neutron flux that must be required to give the lithium. This comes to about 10^7 neutrons per sq. cm. per sec., or about 10^7 times the present activity of the sun. If energy of the order of 10^{45} ergs is being dissipated in flare processes in a time scale of 10 million years, then it turns out that it is just about 10^7 times the present solar rate. In other words, energy considerations are just about consistent with this number of neutrons, and in relation to the present day they fit very well. Incidentally, this may clear up a point about which you may have wondered concerning T-Tauri stars. How does one pick those out as stars that have newly formed? They are picked out because there is abnormally great non-thermodynamic activity at their surface. Of course, they were suspected in the first place of being newly formed stars from the observation that they are found only in very intimate connection with clouds of gas. There have been many such observations, and these stars are all identified by the large measure of non-thermodynamic activity.

How much deuterium is formed? This is now a simple calculation. I am going to use the symbol D for the number of deuterons per unit volume. The number that is going to be produced will be of order $n\sigma_1 H^1/\Sigma$, where $\sigma_1 = 0.33$ barns is the thermal neutron capture cross-section for protons. Using $n/\Sigma = 4 \times 10^{-3}$, one immediately gets $D/H^1 = 1.5 \times 10^{-3}$ in terrestrial matter. A slight correction is made to allow for some spallation, because deuterons are formed or partially produced by spallation. (Roughly a 10% effect is due to spallation.) We calculate, then, that the ratio of D to H^1 in this material was 1.5×10^{-3} and for the first time we were in a position to say we had more deuterium than there is in the ocean waters. The real trouble always had been to understand how to obtain anything like a factor of 1.5×10^{-4} (the observed terrestrial abundance factor). Now we had a factor ten times as great. What does this mean? It must surely mean that only 10% of the material was irradiated by the solar protons; and this is a necessary condition, as I will now show. There are other arguments that make it absolutely certain that not all the material was irradiated by the solar protons.

Suppose I now make an exactly similar calculation but for a general nuclear species. The change of that nuclear species is going to be the number of neutrons times the cross-section times the number of absorbing nuclei, but divided by Σ; and if one puts in the value deduced for n/Σ one may find a situation where the nuclear species has been modified by as much as itself if σ is very large, and the nuclear species has been scoured out altogether. Such is the result if the cross-section becomes large; it applies for instance to Gd^{157} and to U^{235}. Unless part of the planetary material were protected from the protons, all the U^{235} and Gd^{157} would simply be scoured out; whereas in fact they show normal abundance. What can this mean physically? If you have a planetesimal mass of the order of 10 metres in radius, and you consider protons coming into it at 500 million volts, then these penetrate about 40 centimetres. According to the calculations the neutrons become thermalised in about half that distance. So the nuclear activity happens in the skin, and if that skin forms 10% of the mass then the rest inside (the 9/10ths) is shielded. This allows one to calculate the size of the mass. It turns out that one needs a mean size of about 10 metres in radius. If you make a mass any bigger, then the effective cross-section that it presents to particles that come from the sun becomes too small, and the process becomes very inefficient. It turns out that about 10 metres in radius is just about the right size to make sure that an appreciable fraction of the solar flux hits something and does not just go away into space. This also fits into the picture that I drew of a gas coming out of the sun and material being left behind, because it is possible by a hydrodynamic calculation to decide how big those particles have to be in order to be left behind. It is clear that if they were too small they would be carried by the gas flow; and it is very interesting to find hydrodynamically that particles are left behind when they are about a metre in radius. So once again this fact seems to provide a fairly effective corroboration.

Only a few points remain to be tidied up before I finish; I have spoken about Gd^{157}, U^{235}, D and so forth, but it will be clear to you that all this will apply generally to all the nuclear species that are present. Some of them may be very interesting. For instance, the detection by Reynolds of xenon with abnormal isotope abundances in some meteorites, turns out to be very beautifully explicable. What one does here is to make I^{129} from I^{128} by adding a neutron, and then this decays and gives the anomalous abundance of Xe^{129} which Reynolds found trapped inside the meteorite. Details of this sort emerge.

Well, that disposes of almost my final point. I should just like to clear up one final question about the cosmogonic side. How much mass altogether does one expect to be left behind? The material of the inner planets has got to consist mostly of materials of low saturation vapour pressure, and that means mainly Mg, Si, Fe, perhaps in oxidized form. Now the mass of these in ordinary solar material is about ¼%, something of the order of 400 grams of solar material yielding one gram of this stuff. This immediately suggests that the mass that was left behind was 1 part in 400 of the total planetary material. Now Jupiter for instance is 300 times the earth in mass, Saturn is approximately 100, totalling approximately 400; and actually one has to allow for some gases that escape and leave the solar system. When one tidies up the calculation one finds that one expects two or three times the earth mass to be left, just about right for the inner planets. So one has a very nice explanation of why the masses of the inner planets—those that are composed mainly of this group of materials—are low. It is just because that was the amount in the original planetary material.

Finally, let me refer again to stars of the T-Tauri type. These suggest that this type of process is not by any means something which operated uniquely in our own system. It seems to be quite general; and one can estimate the number of T-Tauri stars that have been formed in the history of the galaxy to be of the order of 10^9 or more. One might therefore suspect that something analogous to all this has happened in the other cases. The interesting point that I want to end with, is that something that looks at first sight purely a matter of chance about the solar system, viz. that the small planets are in the middle, suddenly is seen to be no mere chance at all.

5
The Upper Mantle Project

G. D. GARLAND

International co-operation in science is vital when one is studying the Earth on which we live. One cannot stop a particular study of the Earth at national boundaries; one has to study the Earth as a whole and for that reason there is an active body of geophysicists from all countries who organise geophysical science on an international basis. This organisation is called the International Union of Geodesy and Geophysics. Geophysics is a borderline subject and uses methods of physics to study the Earth. The methods of geology, which is perhaps a better known subject, are largely limited to those parts of the Earth we can see. When one wants to study other parts of the Earth, either the interior or the outer envelopes, one has to use the methods of physics. The International Union of Geodesy and Geophysics was the organisation responsible for the International Geophysical Year which took place in 1957 and 1958, and was a very successful venture in international co-operation directed to the study of the Earth. Now another international geophysical project has been proposed; this is the Upper Mantle project.

The International Geophysical Year, or "I.G.Y.", showed what could be done by co-operation between scientists in different countries, but the emphasis on this occasion was very much on the fluid parts of the Earth – the oceans and the atmosphere. It was realised after the conclusion of this project that there was much still to be learnt about the Earth's interior and that possibly a second project to deal more specifically with the solid Earth would be in order. For many years we have known that the solid Earth can be divided into three major regions. There is a thin outer crust, a matter of a few tens of miles thick, a central core extending for rather more than half the radius, and a portion between the core and the crust which is solid but rather different in properties from the crustal rocks. This part we call the mantle. The situation is not unlike that of an egg. If you take a hard-boiled egg and cut it in half you have shell, white and yolk, which represents fairly well to scale the crust, the mantle and the central core of the Earth. The analogy is rather good if the egg is cooked for just the right time so that the yolk is fluid. It is the white of the egg, the mantle, that solid part of the earth which lies underneath the crust, which is the subject of study in the current project. The Upper Mantle Project began on January 1st 1962 and will last for three years. The idea is to study the Earth's interior for its own sake. It is somewhat unfortunate that there have been other issues which have detracted from this study. For example, in the problems of the detection of nuclear explosions by seismic and other methods a knowledge of the Earth's interior, through which the seismic waves travel, is essential. There is, therefore, tremendous interest in the military aspects of this study, although the project as an international experiment was really proposed for reasons of pure science.

I should like to begin by summarising the knowledge that has been available for a few years on the outer part of the solid Earth under the crust, and then mention a few of the outstanding unanswered questions, suggesting lines of research which might be undertaken to clarify some of these problems.

There is no doubt that the most definite information we have regarding the Earth's interior comes from seismology. This is the study not only of earthquakes themselves but of the velocities of the elastic waves which are produced by earthquakes and which travel through the Earth. On the graph (Fig. 1) we have the velocities of longitudinal or sound waves, and transverse elastic waves, plotted as functions of depth in the Earth. While there are slight differences depending on different interpretations, these velocities are really very well determined. They represent one of the best known parameters of the Earth's interior, and the graph clearly reveals the division between mantle and core, the core being a region where there are no "S" or transverse waves. There is a sharp drop in the velocity of the longitudinal sound wave at the core boundary. The region we call the mantle is that solid region extending from the base of the crust to the core boundary, the region in which the velocity of the sound waves increases with depth.

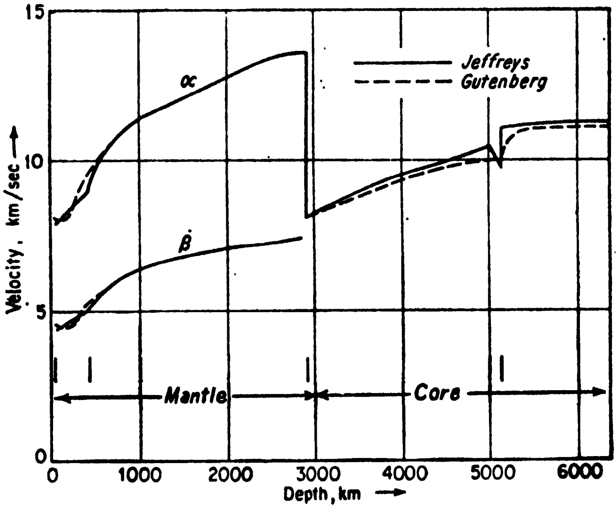


FIG. 1. Velocities of longitudinal and transverse waves as a function of depth in the earth (after Birch).

The study of the interior of the Earth is rather interesting historically, in that up to a few years ago it was generally believed that the mantle was extremely uniform, most of the complexities of geology occurring in the crust. We now believe this is not so and some of the evidence to be presented will indicate that there are regional differences in the Earth's mantle. If one looks at things on a rather smaller scale and works first of all on the continents, studying the reflections and refractions of waves from earthquakes or explosions, one can find the variation of seismic velocity with depth. At the base of the crust, the seismic velocity changes rather abruptly from values which are typical of crustal rocks, to a higher value. This boundary was determined some fifty years ago by the seismologist, Mohorovičić, and has become known as the Mohorovičić discontinuity. In the last few years this has been shortened to "Moho".

One can do similar experiments at sea, studying by seismic means the sedimentary and other layers beneath the floor of the ocean. Again there is the jump in the velocity of seismic waves, and again we have the Moho representing the base of

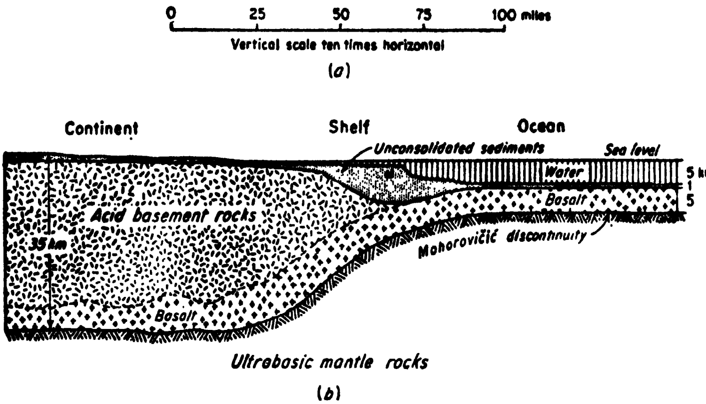


FIG. 2. Generalized section through the crust at a continental margin (after Jacobs, Russell and Wilson).

the crust (Fig 2). The crust under the oceans is considerably thinner than that under the continents, so that continents are like thickened rafts of crustal material. The question is whether the material under the crust (i.e., the upper part of the mantle),, is the same under the oceans as it is under the continents.

In recent years a tremendous amount of information has been gained on the study of waves which are propagated around or near the Earth's surface. These waves travel with a velocity which varies depending on the frequency of the wave, that is, there is dispersion, and the nature of the dispersion provides a very sensitive method of studying velocity variations. A particular type, known as mantle Rayleigh waves, travel under the crust and around the upper part of the mantle, giving a detailed measure of the local variations of velocity in the upper part of the mantle. This velocity is everywhere about 8 km per sec., and until a few years ago this was taken as a rather magic number, but recent work such as that illustrated here (Fig. 3) has shown that there are differences in velocity from place to place. In

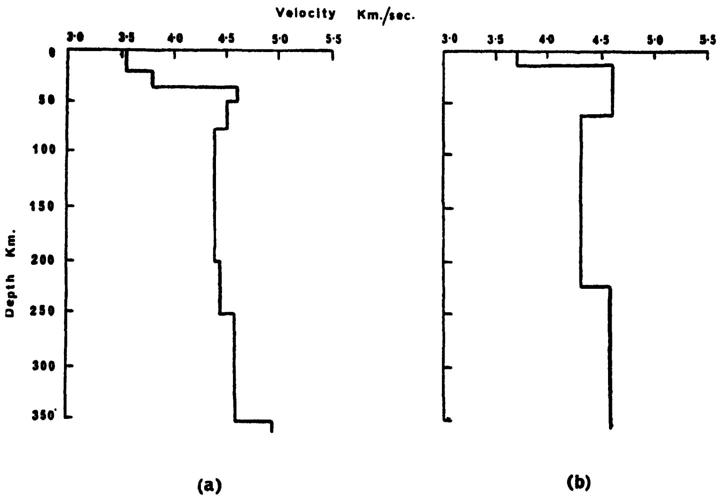


FIG. 3. Examples of transverse wave velocity profiles under continents (a) and under oceans (b). (After Aki and Press).

fact it appears that there is a region in which the velocity decreases with depth, forming a low velocity layer under the crust. The important thing is that, when many cases are analysed for different parts of the Earth, there appears to be a consistent difference between the exact nature of the velocity layers under the ocean compared with that under the continents. The position and character of this minimum under the oceans is quite different from that under the continents; here then is some of the evidence that this part of the Earth is not so uniform as was formerly thought.

The continents are so thin relative to the scale of the Earth that the forces which produce effects on the surface – which build mountains, produce volcanoes and earthquakes – must have their origin in the mantle. In fact one of the main purposes of the Upper Mantle Project is to try to understand the processes in the mantle and to use them as a method of interpreting the geological effects we see at the surface. We are living on very thin rafts, and these rafts are really at the mercy of forces or motions which exist in the mantle. There are many theories of the distortion of rocks at the Earth's surface and of the motion of continents which suppose that the material of the mantle is in a state of convection. We have said that this material is solid, which it is as far as the passage of elastic waves is concerned, but it seems possible or even probable now, if one looks at it from a longer term point of view, that it is capable of undergoing convection.

Whether convection is or is not a main cause of mountain building and geological activity at the Earth's surface it seems very probable that the thermal state of the mantle is a critical factor. If it is not convection which is producing the topography that we see on the continents, then it is probably an effect of the contraction of the Earth during cooling or an effect of the expansion of the Earth during heating – we do not really know which is happening. In any case a study of the distribution of temperature in the outer part of the mantle in particular, and of the way in which heat is brought to the Earth's surface seems to be of very great importance. There is no doubt that during the Upper Mantle Project thermal studies will have to play an important part. If there are convection cells in the mantle, one would expect greater quantities of heat to be brought to the surface over the rising convection currents than over the sinking convection currents. If one is to base a theory of disturbance at the Earth's surface on a contracting Earth, one has to show that the Earth is cooling. In either case it is important to know something of the flow of heat from the interior and something of the internal distribution of temperature. Figure 4 indicates what may be happening above a mantle convection cell. Where material in the mantle is moving in and going down, the crust tends to be dragged together to produce deformation, while over the rising part there is a tendency to tear the crust apart and allow material from the mantle to rise. It has been suggested that this sort of pattern is typical of the oceanic ridges – the mid-Atlantic ridge, and other ridges that have been discovered in recent oceanographic work.

I mentioned the importance of measuring the flow of heat from the interior of the Earth. This is certainly a subject of very great geophysical interest, and yet until very recently it was a subject of which not much was known. Only a few years ago

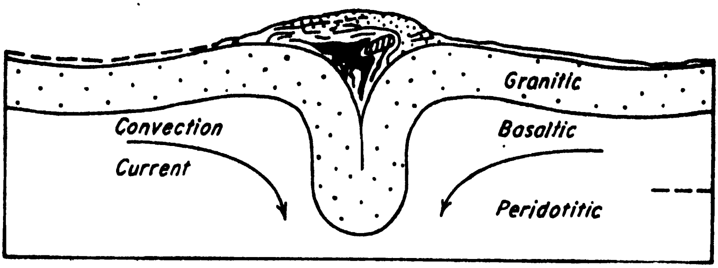


FIG. 4. Possible distortion of the crust by mantle convection currents. (after Hess).

there were but a few determinations scattered over North America, the British Isles, the Middle East and South Africa. For vast areas of the continents and for all of the oceans there was no determination of the rate of outflow of heat. The situation has changed in that there are now many more determinations available for the oceans than for the continents, for it turns out that it is now very much easier to make determinations at sea than it is on land. There have been only a few recent additions to the continental values.

On land one determines heat flow by measuring the temperature gradient and the thermal conductivity which is the ability of the rocks to conduct heat. The number of holes which have been drilled in different parts of the world might cause you to think that this process would be easy. Unfortunately, it turns out that in the holes in which it is possible to measure temperature no one has thought to save samples of the rock for conductivity measurements; and conversely the holes for which there are rock samples available for conductivity measurements are not very convenient for measuring temperature.

On the other hand the floors of the oceans are covered almost everywhere by layers of unconsolidated muds, and Bullard showed that it was possible to measure the gradient in this unconsolidated material by lowering a probe, consisting of a long pipe with thermocouples and recording apparatus. This probe penetrates the muds and after a very few minutes comes to temperature equilibrium, allowing the gradient to be measured. Of course one is measuring the temperature gradient over a very small interval and the temperature must be measured accurately. The material – the unconsolidated muds – can be cored and brought to the ship in cylinders or core-barrels, for immediate measurement of conductivity. The result is that a complete determination of heat flow can be made in quite a short time. This is very different from the situation on land. What we really need, and what would be a very worthwhile research project as part of the Upper Mantle Project, would be a more rapid method of measuring heat flow on land. Of course the oceanographers have a tremendous advantage. It is not merely the fact that there is unconsolidated material into which it is easy to push a probe, but also the circumstance that the floor of the oceans represents a region of very great thermal stability. From season to season there is virtually no change in temperature, whereas on land we are faced with the seasonal variation in temperature at the Earth's surface, which disturbs the temperature down to a depth of a few hundred feet, making it necessary to go below that depth to measure an undisturbed temperature gradient. Furthermore, the material on the ocean floor is fairly uniform and it is comparatively easy to measure the average thermal conductivity. The classical method of determining the thermal conductivity of rocks at continental sites gives a value for a very small piece of rock, which may be far from representative. This has led to the attempt to measure thermal conductivity *in situ*, by putting heating elements into bore holes and measuring the change of temperature in the bore hole as a result of heating. A completely satisfactory instrument of this type is not yet available.

In Fig. 5 are shown some of the heat-flow determinations made in the last few years in the North Atlantic Ocean. The

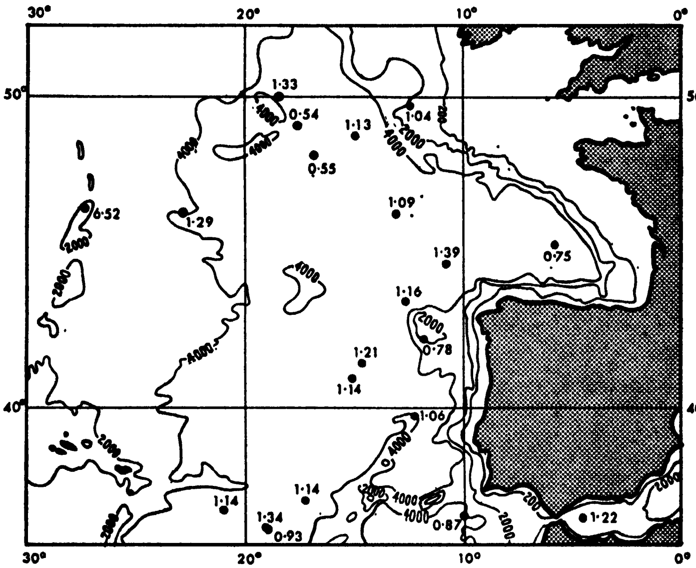


Fig. 5. Heat flow determinations in the North Atlantic ocean (after Bullard and Day). Values in microcalories per cm.² sec. Depth contours in metres.

number is considerable, and there are as many determinations here as there are for probably all the continents of the Earth. The values are in microcalories per cm² of surface area per second. Typically they cluster around 1, although you will see a range from a fraction of 1 to at least 6.5. The heat-flow from the Earth's interior of one millionth of a calorie per cm² of area per sec. is a very small fraction of the heat we receive from the sun, but it is the variations in this quantity which are of special interest in studies of the Earth's interior. For example, are certain values high because the observations are made over rising convection currents? Are others in the deeper parts of the ocean low because they are made over sinking convection currents? What does the scatter in these values tell us about the nature of the earth's interior? An interesting point is that 1 microcalorie in these units is the typical continental value even though, as I mentioned, there are not many continental determinations. Thus, the mean of the values at sea is very close to the continental values. This had not been predicted: until a few years ago it was felt that most of the heat which was observed to flow out through the continents came from heat produced by radio-active disintegrations in the continental rocks. It is known that there is a very much higher concentration of radio-activity in the continental rocks than in the rocks found in the ocean floors, and it was felt, therefore, that the heat flow through the oceans should be less. This discrepancy is still not completely explained, but the fact that the oceanic values on the average are very close to the continental values suggests that the heat flow we observe through the continents may be coming, to a larger degree than previously supposed, through the continents from the mantle. In other words, our estimates of continental radio-activity may be too high.

While we are on this subject I might mention that certain very simple parameters remain to be determined – for example the average composition of crustal rocks is by no means well established. It is hoped that in the next few years, while studies of the Earth's interior are going on, the surface will not be entirely neglected. Many geological surveys formerly restricted chemical analyses to rock samples of unusual interest, thereby neglecting the analyses of rocks closer to average chemical composition.

We suggested earlier that because the seismic velocity of the material beneath the Moho has been measured in many places, we have some idea of the probable nature of the material of the upper mantle. In fact the seismic velocities and its other properties are not very different from those of a rock called dunite, which consists largely of the mineral olivine, an iron magnesium silicate. We have no direct proof that this rock is actually the material of the upper mantle, it simply has certain suitable properties. However the mantle is probably not so simple, mineralogically or chemically; it may be more similar to the rock eclogite, which in addition contains the mineral garnet.

One of the problems we would like to answer during the Upper Mantle Project is whether there is at the Moho, a change in chemical composition. The materials of the upper mantle may be similar chemically to the crust, but have different physical properties because of the increase in temperature and pressure. Now as a result of heat flow measurements it is possible to predict what the temperature is at the Moho under both continents and oceans; it is also possible to estimate the pressure at the depth of the Moho under continents and oceans. If the material of the upper mantle represents a phase change, the point at which this phase change takes place must have a characteristic temperature and pressure. In other words, the change from material with the properties of the crust to the higher density material of the mantle must take place at some particular combination of temperature and pressure. However Bullard and Griggs have shown that it is difficult to construct a transition curve for crustal material which would be consistent with the combinations of temperature and pressure found beneath continents and oceans.

It is obviously desirable to have some more definite information on the material of the upper mantle. This has led to the suggestion of drilling from some point on the Earth's surface to the upper mantle and getting a piece of this material, to see what it looks like. Now if one is going to drill to the "Moho", the hole that one drills is, of course, the "Mohole". The "Mohole" was actually proposed by a group in the United States before the International Upper Mantle Project, but now will be considered part of this Project. The easiest place to drill holes is on land, but the depth of the Mohorovičić discontinuity is about 35 kilometres under the continents. Temperature increases in the continents at a rate of about 30° C per kilometre depth, and a very simple calculation suggests that at a depth of 35 kilometres one reaches the melting point of most materials of which we would like to make drilling bits. Therefore we must drill at some place where the Mohorovičić discontinuity is not so deep, such as under the oceans. In fact, one has to go to the deepest oceans in order to reach the Mohorovičić discontinuity after drilling through the shortest thickness of crust. This Project has been fairly well covered in the press; you will probably have read of the American experiments. It is necessary to drill from a barge in deep ocean, too deep to anchor conveniently on the bottom. The barge has to be positioned dynamically by having engines on it, maintained over the correct point by radar. The drilling bits are lowered through the ocean, which is possibly 5 kilometres deep, and they must start drilling on the ocean floor and penetrate a similar depth, perhaps 5 kilometres, of sub-oceanic crust. The early experiments which have been carried out, and which have been mentioned in the press, have shown that it is possible to operate the barges, and that it is possible to start drilling into the oceanic floor. Pieces of basalt have actually been recovered from the top of the oceanic crust, but of course drilling through 5 kilometres of basalt will take many months. What we hope will be the outcome of this project is the recovery of a piece of material from beneath the Mohorovičić discontinuity. It will also give a deep hole, a hole in which temperature, radioactivity, and many other things can be measured. It is going to be an extremely expensive experiment, and for that reason most of the other countries taking part in the Upper Mantle Project will probably not undertake Moholes.

Most of the other nations will study the upper mantle by means of seismic methods, the measurement of heat flow and other physical techniques. While the measurement of heat flow at the surface of the Earth on either the continents or the oceans is extremely important, it does not tell us the present temperature of the Earth's interior. Because the flow of heat obeys the equation, known to physicists as the diffusion equation, there is a time lag factor, and the present rate of outflow of heat tells us about the thermal state of the Earth's interior some years ago. To measure temperatures which exist now, in the deep interior of the Earth, we have to use some indirect method and one of the more promising indirect methods for measuring the present temperature distribution in the Earth's mantle appears to be in the measurement of electrical conduc-

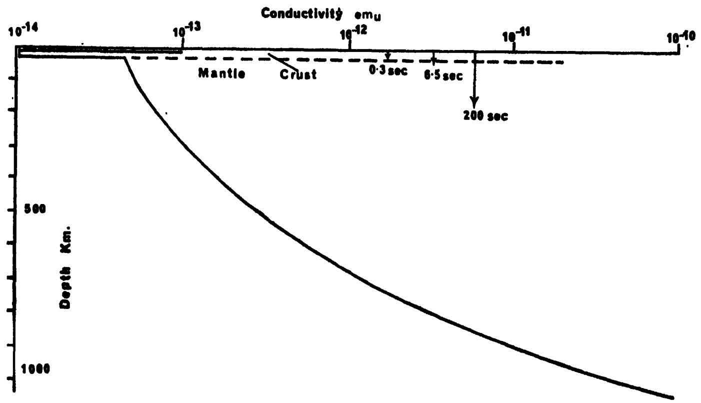


FIG. 6. Generalized variation with depth of electrical conductivity within the earth. The arrows at the upper right indicate the depths of penetration for external variations of various periods.

tivity. The material of the mantle, whether eclogite or something else, has the properties of a semiconductor, in that when the temperature is increased, the electrical conductivity is increased. Figure 6 indicates the probable variation with depth of the electrical conductivity in the Earth. Near the Earth's surface where the rocks are wet, the conductivity is reasonably high. As one goes down through the crust into the solid granitic rocks, there is a high percentage of quartz, which is a very good insulator, and the conductivity becomes very much lower. Probably at or near the base of the crust as one comes into the upper mantle the conductivity increases, although this is rather speculative. But certainly as one moves down in depth into the mantle, the conductivity increases. This increase has been known for many years and was found from a study of magnetic variations of various periods, chiefly the daily variation. Analyses of this variation have shown that there must be electric currents induced in the interior of the Earth at a considerable depth. Certainly the conductivity must increase, but as far as the present Project is concerned our main interest is whether there are anomalies in this curve. In other words, are there local areas in the Earth in which the temperature in the upper parts of the mantle is very different from normal? If there are, then this again might suggest the presence of convection currents.

If one is going to look at rather local areas in the mantle at depths of a few hundred kilometres, one probably will look at magnetic variations of rather shorter period. Fortunately, the Earth's magnetic field is subject to a very wide range of variations in time. We have the very long-period secular change of the Earth's field; I have mentioned the daily variations which were used to establish the general nature of this curve; and there is a complete range of shorter period variations from periods of hours, to minutes, to seconds, to cycles per second and up into audio and radio frequencies. By picking the right portion of this spectrum of changes, one can examine different depths in the Earth, since the effects for a given frequency are influenced by material at different depths. The higher the frequency of the variation, the shallower does it penetrate into the Earth, and the more information does one get on near surface effects. If one studies a variation with a period of 200 seconds, or say 3 or 4 minutes, it provides information mostly about the conductivity of the crust and the very upper part of the mantle.

If one looks at variations of a still shorter period, in the order of a fraction of a second, one gets information only about the Earth's crust. Such a variation is damped out before it reaches the base of the crust, thus any effects that one can measure at the surface as a result of induced currents must come from the crust itself.

Some rather remarkable work has been carried out on the study of variations with periods of several minutes, suggesting that there are indeed regions of anomalous electrical conductivity in the upper part of the mantle. In both Japan and

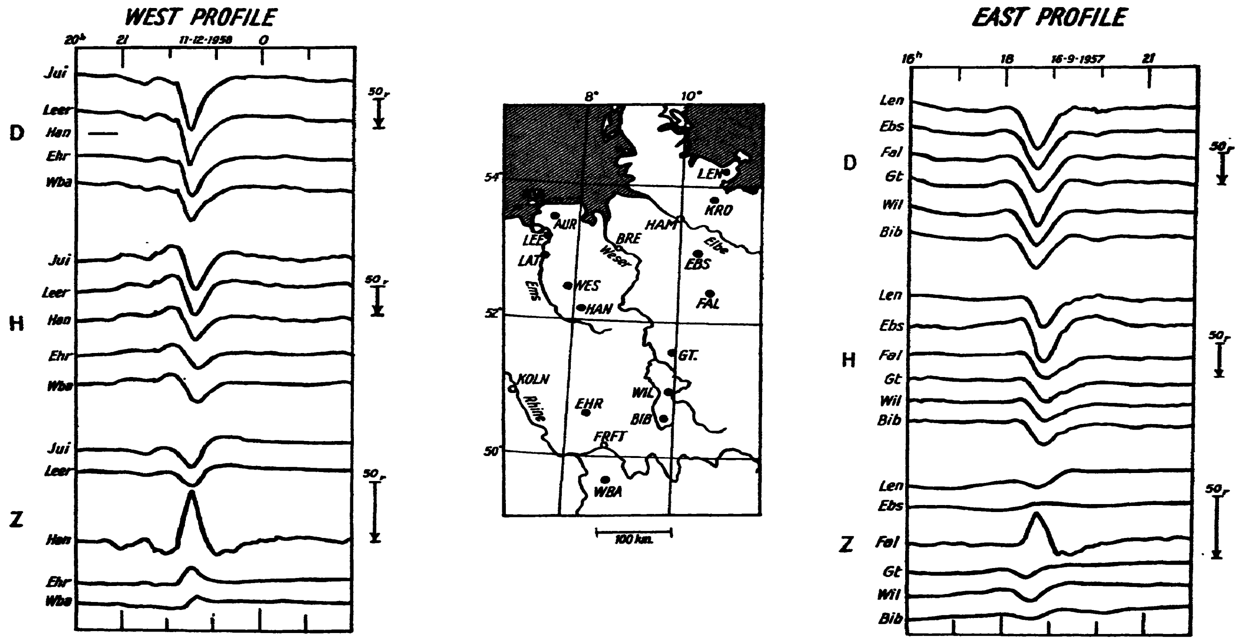


Fig. 7. Comparison of magnetic variations observed at a network of observatories in north Germany (after Schmucker).

north Germany, there are striking differences from place to place in the pattern of magnetic changes. Figure 7 shows the differences in the character of a disturbance as recorded at a line of stations in Germany. Now disturbances of about this period, and shorter periods, in the Earth's field, are due primarily to processes outside the Earth – to electric currents flowing in the upper atmosphere. But it is possible to use mathematical methods, if one has simultaneous readings from a number of stations, to separate contributions from the primary cause, and the contribution from any electric currents induced in the interior. In such an analysis, it turns out that much of this very large change must be internal in nature. There must be in the vicinity of the central part of this line of stations, inside the Earth, some highly conductive material, in which electric currents are being induced. The secondary magnetic effect of these electric currents is responsible for this very large local enhancement of the disturbance. It will be noted that it is the vertical component which characteristically shows the strong effect. One may think of a line of current flowing at some depth below the Earth's surface. The magnetic field lines around that line of current are circular. If you were immediately over the current line, there would be no effect on the vertical component of the Earth's field. If you were placed to one side, the vertical component would be up on one side and down on the other. Hence the reversal of sign and the disappearance of the vertical effect, if you happen to have a station immediately over the line of current flow. This suggests that beneath the station which shows the smallest vertical effect there is an enhancement of electrical conductivity in the Earth. Workers at Göttingen have tried to interpret this in detail. They suggest that in this region the surfaces of equal electrical conductivity are warped so that better conducting material is brought closer to the Earth's surface. If it is assumed that all of the variation in conductivity is due to variation in temperature, then the surfaces of equal temperature must be warped also. If this is the case, and if more anomalous regions of this type can be found over the Earth, it means that our whole concept of the temperature distribution in the upper part of the mantle will have to be revised. The determination of a consistent pattern of temperature variations might be very useful in indicating whether or not there are convection currents in the mantle. The remarkable thing about the north German anomaly is that the region is not an active one tectonically – there are no earthquakes, no volcanoes, no evidence of current geological activity in this area. There is the similar anomaly underneath Japan, but one might almost expect it.

We looked for such a region under North America, using a line of magnetic variation stations operated through the mid-Western United States, during the International Geophysical Year. There were similar patterns of stations in Canada also, but these were formed really to study the auroral zone, and were placed largely in the regions of most intense magnetic disturbance. Of course, if one is looking for an effect due to internal variations in conductivity, one would like to make observations away from the auroral zone where there is so much disturbance due to external sources. There is some evidence that near the Continental divide, through Colorado and Wyoming, there is an anomalous region. This is suggested partly by the spectra, (that is the energy as a function of the frequency of disturbance) which is found to be anomalous in this region. If one station has characteristically a very much lower energy at one particular frequency, there is a suggestion that it might be located immediately over the conductor at which the effect on the vertical component is very small. We hope to do some work of this nature in Canada, operating eight portable magnetic variation stations to look for anomalies of this type. I would think that this study, which can be done without too great an expense, would be rather attractive to a number of countries around the world.

I have suggested lines of approach for studying the mantle apart from actual deep drilling, which I think very few countries will undertake. In summary, these include: the detailed study by seismology of variations in elastic wave velocity, an increase in the number of heat flow measurements and research on more rapid methods of measuring heat flow through the continents, investigations of the electrical conductivity in the upper part of the mantle for the study of variations in magnetic disturbance. The aim in each case is try to find out what processes are going on in the mantle, and their influence on the development of the crust. That is the background of the Upper Mantle Project. It was proposed as an international scientific experiment, primarily for reasons of pure science, to try to find out more about the Earth's interior; to use the experiences of the I.G.Y. in international co-operation and scientific effort, but to find out more about the solid Earth, rather than the fluid envelopes which were the special study of the International Geophysical Year.

6

History and Future Applications of Superconductivity

C. J. GORTER

Fifty-one years ago, in the spring of 1911, Kamerlingh Onnes baffled the scientific world by the announcement that near the temperature of the boiling point of liquid helium, about 4.2°K, the electrical resistance of solid mercury makes a large jump downward and becomes practically zero. Two years later when he was awarded the Nobel prize of physics, he had proved already that superconductivity was not confined to mercury alone. However, one would feel now that Kamerlingh Onnes' main achievement was not so much the astonishing discovery of superconductivity, as the first liquefaction of helium three years earlier (on June 10, 1908), which made this discovery possible. In fact on that day modern low temperature research really started by enabling physicists to carry out conveniently all sorts of investigations in an easily adjustable temperature bath that is usable from 4.2°K down to about 1°K.

Till that day the lowest temperature obtainable for practical purposes was about 12°K in solidified hydrogen. In the following years Kamerlingh Onnes and his successors carried out the most varied researches in his newly-opened temperature interval. Several new phenomena were thus discovered and many earlier extrapolations from higher temperatures about the behaviour of matter were confirmed, corrected or rejected, when the so-called absolute zero of temperature was approached.

The first discovery, superconductivity, was perhaps the most important of them all and Kamerlingh Onnes himself apparently attributed the choice of the resistance of mercury to his scientific intuition. In order to see whether intuition really deserves this credit, let us consider for a moment the state of scientific knowledge of the resistance of metals at the beginning of this century.

The conductivity of metals was known to be due to electrons—Lord Kelvin called them electrions—which could move more or less freely between the metal atoms. The electrons are accelerated by an applied electric field, but they lose the extra velocity acquired in this way by interaction with the metal atoms. In the irreversible processes of this interaction heat is dissipated, Joule Heat, and the resulting current decreases with increasing resistance due to this scattering of the electrons. This is nothing but Ohm's Law. It was considered to be natural that, upon increasing the temperature, which was known to intensify the heat dissipation, the resistance would increase. At high temperatures this resistance increases proportionately to T. However, what would happen when the temperature fell to very low values? Lord Kelvin proposed in 1902 that at very low temperatures metals might behave like glass or a Nernst filament already at room temperature: "There is no difficulty in believing that the electrons rest in stable equilibrium within the atoms, closely packed to constitute the solid metal at 0° absolute and may move about within the atoms with their wildly irregular thermal motions at 1° of absolute temperature, they may between 1° and 2° begin to spill from atom to atom." This was written not long after a lecture by Dewar, who had announced that at 16°K metals had a small resistance.

Kamerlingh Onnes and his collaborators carried out more experiments down to liquid hydrogen temperatures and found that at the lowest temperatures the decrease of the resistance of gold and platinum wires stops and approaches a residual resistance which decreases as the purity of the metal increases (Fig. 1, curves 1 and 2). It was only in the late twenties that this was explained on a quantum mechanical basis: the electrons keep their freedom even at very low temperatures but their scattering, leading to Joule's heat and Ohm's resistance, has diminished because of the smaller heat vibrations and temperature—independent scattering occurs only at the impurities and irregularities of the crystal lattice.

But, at the time, Kamerlingh Onnes interpreted his result as a partial confirmation of Lord Kelvin's prediction that the resistance would not continue to decrease but would increase again when approaching the absolute zero more closely. Kamerlingh

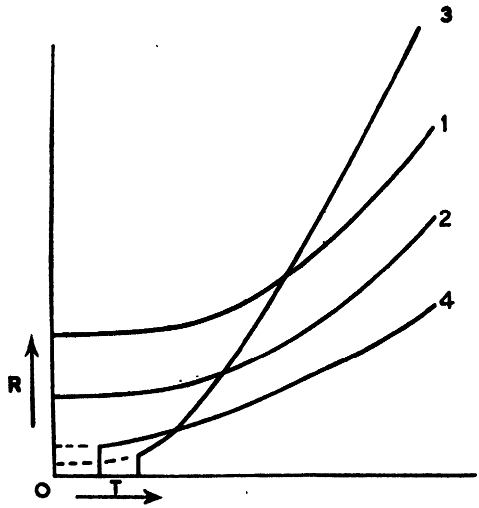


FIG. 1. Specific resistances, R, of some metals for different absolute temperatures, T.

Curve 1: metal which does not become superconductive.

Curve 2: specimen of the same metal, but of greater purity.

Curves 3 and 4: two superconductors; the dotted curves correspond to the state in which superconductivity is suppressed by a large magnetic field.

Onnes however remarked that his mercury behaved in a different way, its resistance continued to decrease at liquid hydrogen temperatures and he wondered whether it might even become zero at the absolute zero and so belie, for this case, Lord

Kelvin's expectations. When he found the complete disappearance of the resistance even at 4°K, he considered this as a confirmation of the value of his scientific intuition. But he lived long enough to see that this was wrong; the steeper temperature dependence of the resistance of his mercury was due to a greater purity, leading to a lower residual resistance (Fig. 1, curve 3). He later found that superconductivity may also very well occur in

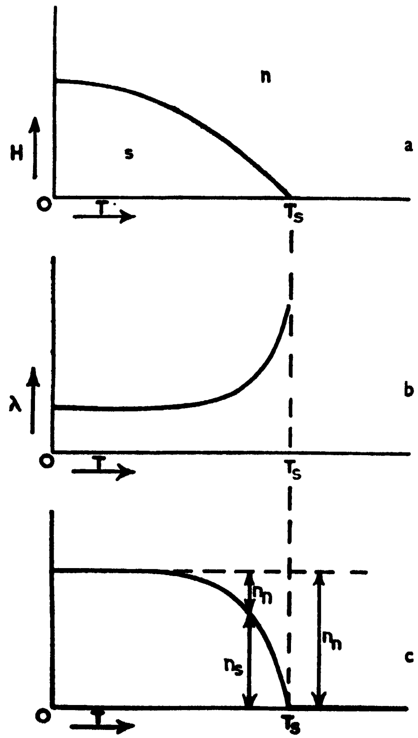


FIG. 2.

(a) T is the absolute temperature and H the magnetic field. T_s is the transition temperature. The line separating the superconducting (s) and normal (n) regions gives the critical field H_0 for different temperatures.

(b) The penetration depth λ for different temperatures.

(c) Number of normal electrons (n_n) and superconducting electrons (n_s) for different temperatures according to the two-fluid model (1934).

impure metals and alloys. So the intuition, though it led to a fine result, was scientifically without a deeper value.

Superconductivity having been discovered, Kamerlingh Onnes rapidly investigated several of its properties; lead and tin were the other metals which first were found to be superconductive.

But it was a great disappointment to Kamerlingh Onnes that the practical applicability for transmitting large currents without Joule losses and for obtaining high magnetic fields without consumption of electrical energy was frustrated as a consequence of the disturbance of superconductivity by large fields and by strong electric currents. Immediately below the transition temperature T_s even very small fields and currents are able to destroy superconductivity, while at temperatures which are small

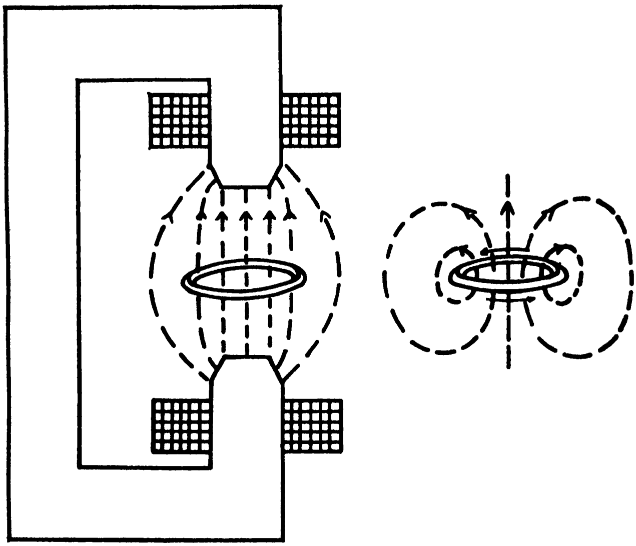


FIG. 3. The generation of a persistent current in a superconducting ring. At the left the ring is cooled down, while it encloses a magnetic flux. At the right the superconducting ring has been taken out of the magnet. The flux is maintained by a persistent current.

compared to T_s the critical field becomes constant at a value of a few hundred oersteds (Fig. 2a). When superconductivity is disturbed by a large field the sample goes over into the normal state, which is a natural continuation of the state above T_s. Silsbee already remarked in 1916 that superconductivity in a wire was apparently disturbed as soon as the magnetic field at the surface of the wire reaches the value of the critical field. In the centre of the wire it can never be completely disturbed since the field is zero there and the metal must be split up into an intermediate state, which is a mixture of superconducting and normal matter.

Zero resistance means infinite electric conductivity and consequently the magnetic flux through a closed curve which is completely embedded in a superconductor cannot change. If it should change, the inducted voltage would give rise to an infinite electric current density along the curve; this is impossible. If therefore a current circulates in a superconductive ring it will not decrease in strength. Such a persistent current can easily be set up by cooling down a ring in the presence of not too large a magnetic field perpendicular to its plane (Fig. 3). If the ring then passes into the superconductive state, a certain magnetic flux is enclosed by the ring. If subsequently the magnetic field is switched off or the ring is taken out of the field, a current must be set up in the ring in order to keep the enclosed flux constant. In a normal metal such a current will also be set up but it will die out in a short time, its energy being dissipated as Joule heat in the metal ring. In the superconducting state Joule's mechanism is absent and the current persists. Collins in the U.S.A. has kept a large current circulating in such a ring for more than a year.

It is clear that if a superconductor is cooled down in zero field, subsequent application of an external magnetic field cannot lead to penetration of the field into the body of the superconductor. Thus the external field must be screened off by surface currents. Similar currents must run along the surface of our ring that carries a persistent current in particular along its inside surface. The surface currents may be quite large, of the order of a few hundred amperes per cm. Since they cannot be surface currents in the strict sense of the word (the current density would be infinite) they must run in a thin surface layer. Also the magnetic field will penetrate this layer and gradually diminish if one moves into the body of the superconductor. This *penetration depth* λ is found to be of the order of 500 Å: a few hundred atomic distances or about $\frac{1}{20}$ *of a micron.* This means that the current density in the surface layer may be of the order of 10^8 A/cm^2. We shall come back to this large value when we discuss applications. These values diminish considerably and also λ becomes larger when the critical temperature T_s is approached (Fig. 2b).

Meissner discovered in 1933 that a magnetic field can never be present in superconductive bodies, apart from surface layers. Thus if we cool down a body, surface currents (currents in a surface layer) appear as soon as superconductivity sets in, which screen off the field from the inside of the body.

Caloric measurements carried out by Keesom and others have proved that at T_s at zero field there is no heat of transition between the superconductor and normal state, only a jump in the specific heat. Thus energy and entropy of the superconductor approach the values of these quantities in the normal state when T_s is approached. The caloric quantities being known, the disturbance of superconductivity by a magnetic field can now be understood as a direct consequence of thermodynamics. As soon as the magnetic energy of the screening currents in the surface layer exceeds the difference in free energy between the phases, the boundary of the superconductive phase is pressed inward and superconductivity is suppressed, at least partially. One may use the classical value of the Maxwell pressure $H^2/8\pi$, accompanying a magnetic field, to describe the pressure on the boundary between the phases. This of course assumes that inside the superconductor the magnetic field is zero. This simple reasoning, which is not of a microscopic nature, and has nothing to do with the fundamentals of superconductivity, completely explains the s-n line in the H-T diagram.

After this success of thermodynamics we proposed in Leiden in 1934 the so-called *two-fluid model* of superconductivity, which likewise had a thermodynamical character. The prototype of this model is the well-known case of a liquid plus vapour in a closed vessel of constant volume. When T increases the liquid gradually disappears and there is an extra contribution to the specific heat of the whole system. The critical temperature is that at which the last drop of liquid disappears, and above T_s the contribution to the specific heat has suddenly vanished. But the total entropy and energy of the (liquid + vapour) combination approaches that of the mere vapour when T_s is approached from below. Thus it is supposed that in a superconductor the electrons split up in two fluids: the normal electrons and the superconductive ones (Fig. 2c). The latter maintain an electric short circuit until the superconductor has become energetically identical with the normal metal. The two-fluid model became quite fashionable when the presence of normal electrons in the superconductor was confirmed by high frequency fields and when the increase of the penetration depth (Fig. 2b) could be understood from the decreased concentration of superconductive electrons. Moreover the superfluidity of liquid helium II can be treated by a similar two-fluid model.

Diagram 4 shows how superconductivity is distributed in the *periodic system*. There are two groups of superconducting elements, the 9 soft elements in columns IIb, IIIb and IVb and 13 so-called hard superconductors in the region of transition elements.[1] The 10 metals with T_s larger than 2°K have been underlined. It is remarkable that three groups of very common metals are not superconducting: the mono- and divalent alkalis and alkaline-earth metals, the noble metals and the magnetic transition metals. This is particularly marked in the magnetic rare earths which, in contrast with the non-magnetic lanthanum, are not superconducting. In addition to these 22 pure metals many alloys are superconducting, several consisting of non-superconducting elements (Au_2Bi_1, MoN, $CoSi_2$, NiAs, CuS) which are situated at opposite sides of the superconducting groups in the periodic system.

The so-called isotope effect according to which T_s is proportional to $\bar{M}^{-\frac{1}{2}}$, where $\bar{M}$ is the average atomic weight of an isotope mixture, has played a great role in the development of the theory of superconductivity. Fröhlich suggested that an interaction between pairs of electrons through the intermediary

[1] Recently Mo and Ir have also been found to be superconductors.

of quantised elastic waves, the so-called phonons, might be responsible for superconductivity. Since the frequency of such phonons are in general proportional to $\bar{M}^{-\frac{1}{2}}$, he predicted the isotope effect. On the same basis Bardeen and Boguljubow, each with their collaborators, developed an apparently quite successful theory of superconductivity. This theory cannot be repre-

Ia	IIa	IIIa	IVa	Va	VIa	VIIa		Ib	IIb	IIIb	IVb	Vb	VIb	VIIb	VIII
H															He
Li	Be									B	C	N	O	F	Ne
Na	Mg									Al	Si	P	S	Cl	A
K	Ca	Sc	Ti	V	Cr	Mn	Fe Co Ni	Cu	Zn	Ga	Ge	As	Se	Br	K
Rb	Sr	Y	Zr	Nb	Mo	Tc	Ru Rh Pd	Ag	Cd	In	Sn	Sb	Te	J	Xe
Cs	Ba	La	Hf	Ta	W	Re	Os Ir Pt	Au	Hg	Tl	Pb	Bi	Po	At	Rn
Fr	Ra	Ac	Th	Pa	U										

FIG. 4. Periodic system of chemical elements. Those which become superconducting elements are shown inside the heavy line. The 10 elements having transition temperatures above 2°K have been underlined.

sented by the original two-fluid model but leads to an *energy gap model.* At zero temperature an energy gap of the order of 10^{-3} electron volt, due to the interaction, would separate the non-occupied conduction electron states from the occupied ones. The latter electron states, those below the energy gap, are responsible for superconductivity; in the presence of a local magnetic field they transport a frictionless current. The difference between this and the original two-fluid model is that the gap gets narrower upon increasing the temperature. T_s is not the temperature at which the last superconducting electron disappears, but that at which the energy gap disappears. So T_s is a kind of Van der Waals critical temperature.

Research carried out in the U.S.A. with micro-waves in the millimetre region and with the tunnel effect leading to electron passage through a thin insulating layer on a superconductor, has confirmed the model of the self-closing gap.

On the basis of the Bardeen theory Pines, has proposed the formula

$$T_s = 0.85\theta \exp(-1/N(E)V)$$

where θ is the Debye temperature, N(E) is the level density and V the matrix element of the interaction energy. When applied to the data, the order of magnitude of V varies remarkably little.

Quite recently it has been observed that Ru, belonging to the group of hard superconductors, of which it is the one with the most reproducible properties, has no isotope effect.† There are also a few indications that the interactions between the electrons may essentially differ in nature in different groups of metals (Matthias).

As to possible applications, it is clear that, because superconductivity so far is confined to temperatures below 20°K, complicated, expensive and fragile thermal insulation and cryogenic apparatus would be necessary. Thus it is clear that, however attractive it may appear to suppress the Joule losses connected with the transport of electric energy, one cannot expect that our main electric transmission cables from the power stations to the population or industrial centres will soon be refrigerated. To achieve this with cables within the grid system would be even less feasible. For the time being therefore only applications in scientific or industrial laboratories will be considered and perhaps in some special production processes. Economically this may not sound impressive but the funds used in research and development, which at present are about 2% of the national income in technically developed countries, are steadily increasing. In some production processes, which involve strong electric currents, considerable savings might be feasible.

But let us mention first a pure laboratory application. It is

† It has been found recently that there are other transition elements with unexpectedly small isotope effects.

well known that the spontaneous voltage fluctuations across the resistance of every electric measuring device impose a restriction on its sensitivity. The lower limit of the electrical energy to be measured (product of power and measuring time) is $2kT \approx 10^{-20}$ joule at room temperature. If electric voltage is measured in a low temperature circuit, this limit lies considerably lower. Mak-

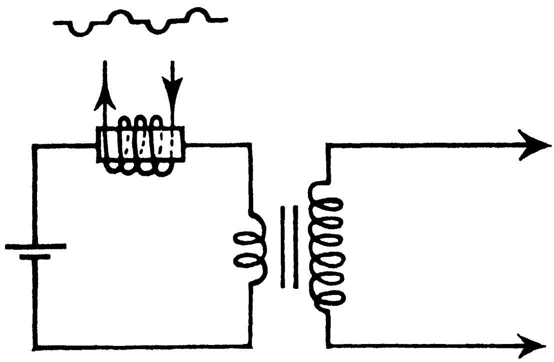


FIG. 5. Scheme indicating De Vroomen's device to measure voltages down to 10^{-12} volts. The current excited by the small voltage to be measured is modulated by periodically suppressing superconductivity in part of the circuit. The varying part of the current is fed into an electronic voltmeter by way of a transformer with a very high turns ratio.

ing use of a circuit that is almost completely superconducting Pippard and Pullan have reduced the detectable voltage limit to about 10^{-12} volt in a 10^{-7} ohm circuit with a superconducting galvanometer. De Vroomen even reached approximately the same limit at 10^{-5} ohm. By periodically suppressing superconductivity in a wire and feeding the modulated current through a transformer with superconducting coils to a chopper-amplifier, the theoretical limit placed by the room temperature chopper has been approached but not yet reached (Fig. 5). The device is used for measuring thermoelectric voltages, which are often of the order of 10^{-9} volt-degree, but it could also be used for several other purposes.

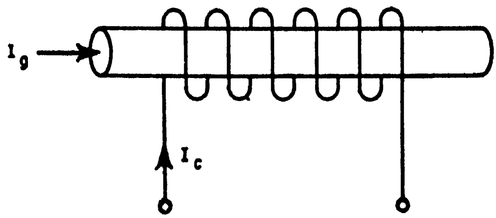
Superconducting elements could possibly compete with the low noise level reached by microwave amplifiers using stimulated emission of radiation (masers) although, as was mentioned, superconductivity disappears at very high frequencies.

Another possible application which apparently may soon be put into practice is the use of superconducting elements in computers. Buck's so-called cryotron is sketched in Fig. 6. It consists of a central wire and a coil. If a sufficiently strong current passes through the coil, superconductivity in the wire is suppressed. A bistable element consists of two cryotrons: the central wires are each in series with the coil of the other. If the current passes through one of the two paths, the other one is blocked.

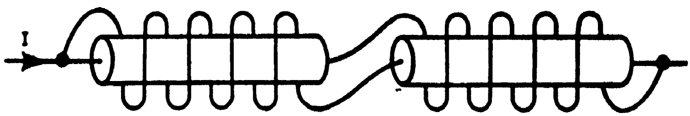
Fig. 7 shows how the flip-flop can be switched and how its state can be read out by two other pairs of cryotrons. Fast switching can be achieved by using thin films and thus reducing the joule losses in the phase transition. Other devices make use of frozen-in flux in superconducting rings (or plates) which can serve as elements of a memory. For them names have been coined such as persistatron or persistor.

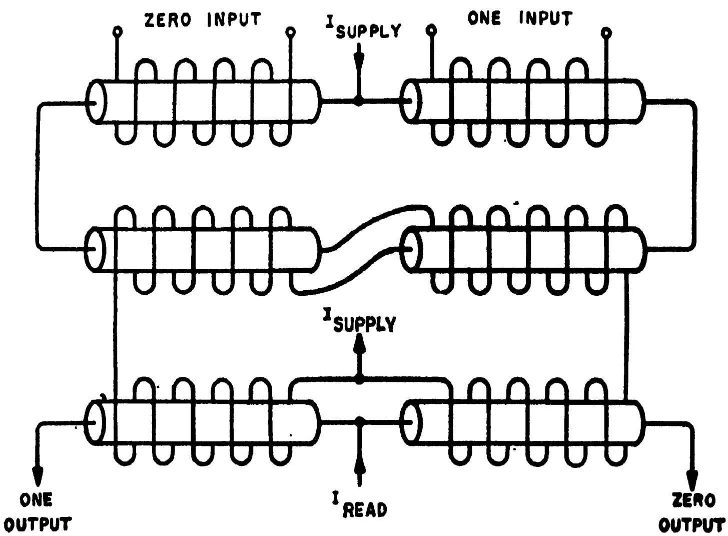
A very important use of strong electric currents is the generation of high magnetic fields. In conventional high power engineering that is used in generators, convertors, transformers, motors, etc. Above about 20,000 Oersteds the production of magnetic fields for laboratory purposes requires considerable power. In Leiden we use a 4 megawatt installation and in Berkeley there is a 7-megawatt magnet. Nuclear and elementary particle physicists tend to use even more powerful installations and a drastic reduction of the power consumption would be welcome.

We mentioned Kamerlingh Onnes' early disappointment about the suppression of superconductivity by rather strong currents and magnetic fields. This is due to the Maxwell pressure $H^2/8\pi$ acting on a boundary of a superconducting body. On the other hand we mentioned that the currents near this boundary are concentrated in a thin layer and may be of the order of $10^8 A/cm^2$ which is 10^4 times higher than one can obtain in very well cooled copper wire. This suggests that superconducting *films* parallel to the field might be used which are considerably thinner



Single Cryotron





FIGS. 6 and 7. Single cryotron and cryotron bistable element: flip-flop (from Proc. Inst. of Radio Engineers, U.S.A., *44*, 482, 1956). Cryotron flip-flop in the read-in cryotron and read-out cryotron.

than λ. The usual condition for the equilibrium position of the s-n boundary gives, according to F. and H. London that, if $d \ll \lambda$, the maximum magnetic field with zero resulting current is (1) $H_m \approx H_0\lambda/d$ and the maximum current without external field is (3) $I_m \approx H_0 d/4\pi\lambda$; in intermediate cases the linear interpolation (2) applies. Fig. 8a displays H_0 and the current density I_m as a function of x for the maximum condition.

On the basis of the original thermodynamic considerations one should expect that, if at the flat boundary of a normal and a superconductive region the current density is lower or higher than $H_0/4\pi\lambda$, then the boundary will be displaced either to the normal or to the superconducting side. The films, however, are too thin to permit the formation of an internal boundary. The high surface-free energy excludes possibilities other than a completely normal or completely superconducting state. Different attempts have been made to formulate other criteria. This is difficult in the case of a current, since then irreversible processes cannot be avoided.

Landau and Ginzburg have developed a rather different picture according to which, under the influence of high fields and strong currents, superconductivity (the superconducting electrons or the energy gap) in thin films gradually disappears. The general character of the theoretical prediction about the critical fields and currents hardly changes. Only the effective λ increases with decreasing d (resulting in higher critical fields and lower critical currents), while according to Landau and Ginzburg a critical current is hardly depressed by a relatively small transversal field.

Diagram 8b also shows what happens in the $I_m - H_m$ diagram if films of different thickness are placed in series or in parallel. The first picture has the general convex appearance observed for films. In drawing the second diagram it is supposed that the flux enclosed between the two parallel leads is adjusted so as to have the most favourable value; if the adjustment is not made, the resulting maximum current is lower.

The fact that alloys and some hard superconducting metals lose their last traces of superconductivity only in external fields

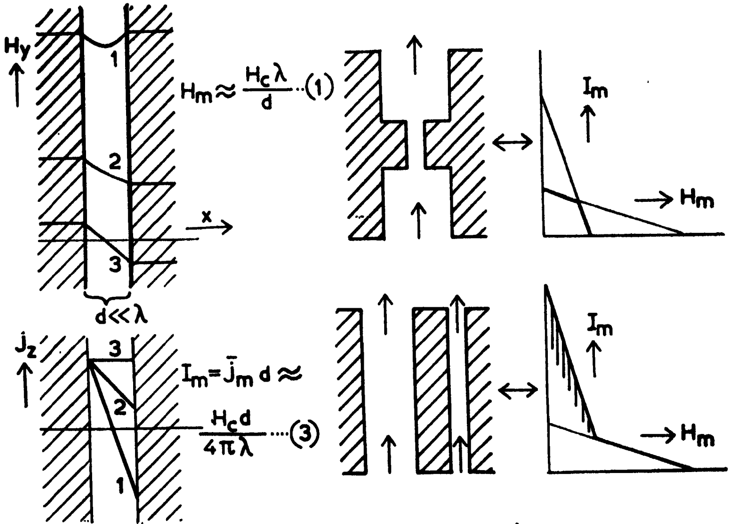


FIG. 8 (a) Magnetic field H and current density j_z in a superconducting film at approximately critical conditions (1) external field only; (2) intermediate case, (3) current only.
(b) Maximum field H_m and maximum current I_m if two films of different thicknesses placed in series or parallel. In the last case the maximum values are only reached if the enclosed flux has the most favourable value.

that are much higher than H_0 had been ascribed in 1935 to a splitting up of the substance into normal and superconductive films or filaments of thickness smaller than λ.

Mendelsohn proposed a sponge structure due to varying composition of the alloy from point to point. We emphasized that for some reason thin films or filaments must be possible in an alloy. So apparently the *n-s* surface energy is of relatively smaller importance than in a pure soft metal. In the framework of his non-local theory Pippard stressed that a short mean free path of the electrons leads to a small surface energy and a large λ. In 1957 Abrikosov, on the basis of the Landau-Ginzburg treatment, and in 1961

Goodman, on the basis of the above line of thought, developed rather similar pictures according to which, under appropriate conditions, the splitting up of the superconducting state sets in at a well-defined field below H_0, while the last traces of superconductivity disappear at a field much higher than H_0 in a second order transition. It has to be noticed that, as mentioned in connection with diagram 8b, the maximum values of field and current are only reached if the enclosed fluxes in non-simply connected superconductive structures have their most favourable (quantised) values. If these fluxes are zero the sponge reacts as a massive wire. It has to be expected that the adjustment of these fluxes will give rise to flux hysteresis and "dragging phenomena" as observed by Le Blanc and others. Concave $I_m - H_m$ diagrams (see diagram 8b) are also often found.

It is understandable therefore that many attempts have been made to see if alloys can stand a combination of high currents and strong fields. The first attempts, which met with little success, were made by Keesom in 1933 but quite recently rather sensational successes have been obtained with niobium-tin and niobium-zirconium and similar alloys in the laboratories of the Bell Telephone, General Electric, and other scientific and industrial laboratories in the U.S.A. Fields of the order of 150,000 oersteds at current densities of 10^5 ampere/cm^2 have been reported not to suppress superconductivity. It is clear that very strong Maxwell pressures (or Lorentz forces) have to be endured by the superconductive filaments in the alloy and to be transmitted to the metal lattice as a whole. Metals are required which are strained and even cold-worked, so that it becomes difficult to make the electrical connections by welding or soldering.

Several superconducting magnets producing fields over 60,000 oersteds have already been constructed making use of these alloys and one can even obtain them commercially. In some cases powerful magnets are excited by simple storage batteries and even persistent currents have been used. Of course the energy in such a magnet is considerable. If we have 10^5 oersteds, the magnetic energy $H^2/8\pi$ is 40 joule per cm^3. So if one wishes to excite a small laboratory magnet with an effective volume of

100 cm^2 with a battery of 4 volt delivering 20 ampere, the loading time is considerably longer than $40 \times 100/4 \times 20 = 50$ seconds, of the order of several minutes. Refined devices have been proposed to "load" a persistent current circuit. The Maxwell pressure at 10^5 oersteds is 400 atmospheres and the resulting forces have to be taken up by the current leads. Mechanically this may not be easy when one proceeds to stronger and stronger fields.

It may be remarked that the current densities obtained so far are much below the current densities in the surface layer of any superconductor. This indicates that only a few per cent of the volume is superconducting in the alloys concerned. Thus in principle much higher current densities might be reached. This appears to be a matter of metallurgy and of trial and error. It may also be that in the long run coils made from evaporated films, consisting alternatively of pure metal and an insulator are promising. When one uses pure metals, the large *n-s* surface energy is an advantage. For mechanical reasons the "hard" superconductors might be preferable. For the time being, however, attention is focussed on the Niobium alloys, which seem to be promising indeed.

The advantages of producing large magnetic fields lie not only in the very small- or zero-power consumption, but also in the very small space required for the windings because of the high current density. A sphere of interest is, of course, plasma physics in which strong and extensive magnetic fields are required for confining discharges in a limited space by coiling up the motion of high energy particles so that they cannot hit the wall of the container and so lose their energy. Perhaps superconducting magnets may well be the necessary tool for obtaining energy from the fusion of atomic nuclei. The same may apply for the magneto-hydrodynamical generation of electric power from heat.

The nuclear physicists too, and in particular those studying elementary particles, are most interested in compact and cheap magnets for their accelerators. There are many more fields in science and in engineering which might soon wish to profit from these new possibilities.

7
The State of Physics

R. E. PEIERLS

I am not a very good prophet, and I do not want to forecast the future of physics, though I had the pleasure recently of taking part in a discussion at M.I.T. about the future of physics, where a number of people expressed their views. The interpretation of the term "future" then ranged from the next ten years to (in the case of Feynman) the next thousand years. I shall not try to get involved with such a long range of forecast as Feynman, though I shall steal perhaps some of his ideas. But one cannot discuss the present state of physics entirely within itself; after all the present is, mathematically speaking, of measure zero and is a pause between the past and the future.

We must realise that a subject like physics changes all the time. It is not like what it has ever been and it is never going to be exactly like it is. Even what we mean by physics, our view of what is the proper domain of the physicist, also keeps changing.

We tend to forget too easily that at the end of the last century physics as then understood was practically finished. I am exaggerating, because there were then of course people with vision who could see further ahead, but according to the conventional view of the time, the proper job of the physicist was to discuss the macroscopic laws relating the behaviours of various substances in various circumstances. Ohm's Law was a proper part of physics, but the value of the conductivity of copper, and why copper conducted and quartz did not, was not a question of which the physicist was aware. There were only a few people in those days who realised that one could apply to these problems of the structure of matter the same consideration that the physicist applied to his apparatus in the laboratory, and that in that way one could understand what made atoms and molecules and matter operate, in the same way as one could gain an understanding of large-scale apparatus. For this reason most of the present problems which the physicist has solved or is solving were not even regarded as problems in physics a few generationss ago.

There has been a complete change, and we have gone, as you all know, through a very rapid stage of development in the course of which one, though by no means the only, interesting and important preoccupation of the physicist has been to explore the fundamental laws of physics. A key-note in modern physics has been the desire to establish the most fundamental laws, trying to start from an investigation of the behaviour of atoms, and the realisation that the laws of classical physics, as they were then known, were not adequate to describe what went on in the atomic scale, and that something new was required. As you know, the study of the atom was at first dominated by Rutherford's discovery of its structure and by Niels Bohr's application of the hypothesis of Planck to the atomic phenomena—an application which at that time was completely unexpected. This got us involved with such revolutionary concepts that the quantum theory, which represents the formulation and full description of these concepts, took some twenty-five years to complete and understand. And then we were faced with a really wonderful period of physics in which suddenly everything, or nearly everything, about atoms and about combinations of atoms such as molecules or solids, seemed to fall into place.

I had the good fortune to be starting in physics at just about that time, and it was a time when you could choose almost any problem that nobody else had yet had time to look at in the new light and you could suddenly see an explanation for things that had appeared mysteries or that had appeared contradictory up to then. Physics in those days, in the early nineteen-thirties, had a tremendous note of optimism. Everything suddenly seemed to find an explanation.

Of course, the optimism was sometimes overdone. I remember a meeting in Copenhagen when, not at the meeting, but over meals in the intervals, some people discussed the question of whether physics would soon be finished, because practically everything about the atom and about matter was understood. There were some odd things left, but they would sooner or later all come right. There were at that time only two numbers in physics which had not yet been explained. One was the mass ratio of the two particles which then existed, the proton and the electron (the neutron had not yet been discovered), and the other was the fine structure constant which measured the strength of the electric charge of the electron in quantum units; there seemed to be some relation between the two so that probably there might be a connection.

It is true we could not do much about atomic nuclei. At that time we did not know what was in them, because the neutron was not known; instead it was then believed that there were electrons inside nuclei. They would have to be confined to a very small space. Because of the uncertainty principle, these electrons would therefore have to move with very high energies, that means highly relativistically. There were also some mathematical troubles with the relativistic electron theory. So it was a plausible idea that these things were connected, and once one could remove the difficulties that were in the way of combining relativity and quantum theory, one would probably also understand the nucleus. It seemed quite probable that these difficulties could resolve themselves only for definite values of these two numbers, the mass ratio and the fine structure constant. So, at least to some people, it seemed that physics might soon be finished, and what should we do then? The prevailing view was that one should perhaps turn then to biology where many new things were still to be done. At least one of that group actually turned over to biology soon after that, whether for quite that reason or not I could not say.

As it happened, things went very differently. The discovery of the neutron had taught us that there were new things in nuclei which we had not suspected. The discovery of the neutron had become possible because experimentalists developed techniques and equipment for probing nuclei more effectively. As a result a whole new field of physics opened—the study of the nucleus; we had previously not worried about the problems of nuclear physics because we had not known of them. Now there were new problems, one of them in particular being the problem of the forces that hold the nucleus together, which were immediately realised to be completely different from the kind of forces we had met in physics before. And this led then to the bold hypothesis of Yukawa, which linked the field which transmitted these nuclear forces over small but finite distances with a new kind of particle, now called the π meson. This particle was duly discovered, after the discovery of another particle, the μ meson, at first confused with it, which as far as we know has only a slight family relation with it, and whose function in physics I think remains as yet a mystery.

Up to this point it could be said that all the things we found were really essential parts of the same story, because without protons and neutrons and electrons, matter could not exist in the form in which we know it. Without the mesons and the phenomena associated with them the nuclei could not hold together. So all that was essential and we could not do without any part of it. But physics did not stop there. It was discovered shortly afterwards that there exist other particles, usually of shorter life than the ones seen up to that point, but of sufficiently long life to be studied.

This field of elementary particles, which developed from the discovery by Rochester and Butler of the first of the particles now called "strange particles", is by now a field of fascinating interest and enormous size. I would not attempt to present those of you who are not familiar with this, with a list of all the elementary particles now known to the physicist. The important thing is that a vast new field has been opened, and a field which was not necessarily implied by the phenomena we had seen before. It is to some extent gratuitous; physics could for all we know have stopped at the point reached before, in the sense of no new objects being found, but it did not.

These elementary particles, if one includes neutrons and protons, and mesons amongst them, are now the front line of physics, in the sense that they get us closest to the basic laws, and their study is the field in which we are likely to encounter new refinements of the basic laws, which might be quite as revolutionary as the development of quantum theory. From this point of view it is of interest to speculate whether this growing list of particles will ever end, whether we are going to go on discovering new ones, or whether the list will stop, whether perhaps we have already found all of them.

There is a simple argument which is relevant to this: One normally finds that all these particles can decay and transform into others. There are plenty of examples of this type of break-up amongst all the new particles. In general, one would expect that the life times of particles against such decays should be exceedingly short, because the natural scale of time in that region is short; we would expect perhaps times of the order of 10^{-20} seconds or less. But there are exceptions, fortunately, because otherwise it would have been rather hard to piece together even the partial story that we now know. The exceptions are due to the fact that there are certain rules in this game which say that the particles can be classified in such a way that they cannot go from one group to the other easily. They go only by means of the so-called weak interactions, of which the well-known beta decay is the oldest example, and which are by many orders of magnitude weaker than for example the forces involved in collisions or the forces holding the nucleus together.

Therefore, particles which can decay only through those weak interactions have longer life-times and become reasonable objects for study by the physicist, who can detect their tracks or their effects in other ways, and make them travel some distance. If a particle is created at some point in a bubble chamber or in a photographic plate and decays somewhere else, then one can see easily that the link between the two is associated with a physical particle. If on the other hand, a particle is so short-lived that one cannot detect the distance it has moved between birth and death then the direct description of the particle becomes much harder.

We think we know the rules of this game and we have already found particles of each of the possible types of classification that there are under those rules. Therefore, any new particles which might still be found would be of the same class as some already existing and, therefore, could transform by getting rid of some of their intrinsic energy in one of the known radiations without involving weak interactions; they would therefore all be exceeding short-lived, so that we must expect to look for them not directly as particles, but by circumstantial evidence through their decay products.

This, I think, was clear some years ago and it is now happening. Just in the last year or so a large number of phenomena have been observed which are usually called resonances. This is just another name for very short-lived particles, objects which decay so quickly that one cannot see them directly. They decay in characteristic ways and, if one detects the decay products and watches their motion relative to each other, one can deduce that they have been created through an intermediate state of some new and very short-lived particles. In this sense our table of new objects is not closed but is growing, and I do not think there will ever be any end to it.

What is going to happen is that as we probe further and further the life-times of these particles will get even shorter and shorter, and the properties will get less and less distinct, so that there will be no precise point beyond which we should stop saying "this is a particle".

This means that physics has again, in this fundamental field, gone through an expanding phase. It seems that physics always contracts or expands. From the stage where everything seemed to be understood except for some subtle points about relativistic electrons and except for the two numbers, we have now again reached a stage in which we have a vast list of particles and we have added to it a further list of resonance states. All this we can describe but not yet interpret. We are almost back to the stage in which Chemistry was at, say, the beginning of the century, when there were some 92 elements; and when we knew their names and could make them react with each other. But the question why one of them had certain properties and another others, or how one would react with another, was not explained, but had to be answered by looking in a book of reference, or by doing the experiment. We have not yet, I think, 92 elementary particles, but we are not so far from that number, and it is clear that, in this respect certainly, the story is very far from finished. We have not even started to have any idea of how to account for this variety.

It is quite clear that in some way physics must find a more basic thing of which all these different objects are different manifestations. I do not believe that the story will repeat itself in the sense in which the story of the atom developed, so that these particles would be found composite and built of smaller units. The way the facts look makes this exceedingly unlikely, and in any case it would be too simple to expect the history of physics to repeat itself in such a literal way. (As you know history does not repeat itself—only historians repeat each other!)

But there must be something else, and probably the hardest thing is to find the right concepts to use. A more likely answer is that all these particles are not built up from each other or from smaller units, but that they are different aspects of the same thing, and that maybe in some sense they represent different states of one field or of a smaller number of fields in nature than we have now, although it may be that such a statement is only playing with words. Some attempts have been made (particularly by Heisenberg) to set us a tentative mathematical description which would have such results and bring several particles out of the same basic field structure, but I do not think that one could claim that these have as yet proved themselves, or have achieved success.

We must leave the story there, because that is all we know of the elementary particles. While they do not form part of Nature in the ordinary way, and do not exist in stable matter (they exist around us as a result of bombardment by cosmic rays, but in rather small numbers), they are, nevertheless, essential pieces of evidence which we require to unravel the basic story of physics.

In starting off my description of the present state of physics with the most fundamental side, I certainly do not wish to imply that it is necessarily the most important or the only interesting part of physics. It may be of some interest to take a look at those parts of physics which once were the front line. That is true of the problems of atoms, and it is true now also to a large extent of the problems of atomic nuclei. We may ask what is going on in those fields. In the case of the atom, as I have already said, we are now confident that we know all the basic laws as far as they are relevant. We can write down the Schrödinger equation, which will give us the behaviour of any atom in all conceivable circumstances. You might say that there are perhaps some elements of uncertainty left in that in the centre of the atom there is the nucleus, the structure of which is not fully understood, and which interacts in some measure with the electrons. However the detailed dynamical effects involving the nucleus are so minute that they will never – or only in the most exceptional circumstances – be of importance.

So we really can say we have under our control the equations of the atom to the same extent to which the electrical engineer knows Maxwell's equations as they apply to an electric machine or to a radio transmitter. But that does not mean that the field has become uninteresting, or that it has ceased to be part of physics.

That it is interesting follows from the fact that knowing the equations is not the whole story, because one cannot in practice get a complete mathematical solution of these equations except in the simplest possible circumstances. In the case of the hydrogen atom one can write down the whole spectrum in exact form with all sorts of corrections. For helium one can almost do that, but it gets a little harder. Beyond that, one understands things qualitatively, but one cannot work out everything that is going to happen by dead reckoning, just as the job of the electrical engineer who designs a machine is not made unnecessary because we have equations from which we could in principle work out the complete behaviour of a machine of any specification. Firstly we must still select what kind of machine we want to use, what we want it to do and how to achieve it, and, secondly, very often it will be quicker to build the machine, or build a model of it, and find out what is going to happen, than to rely on pure calculation.

Now, we have both situations in the atomic field. We have really what one almost might call atomic engineering, where one makes atoms and electrons perform certain jobs. In this respect the field has acquired features similar to engineering and technology, and of course it plays a great part in technology.

We might then ask whether this field is really ceasing to be physics and becoming engineering. To some extent this might be a question of terminology. It is true today that research projects which are being carried out in the engineering departments of some Universities, are to be found in the physics departments of others, and there is an overlap in the definitions – it is a good thing that there should be. But I think there is more to it than that, because, although many of these problems require a motivation not very different from that of the engineer, they still require a man of the outlook of a physicist.

The experimental techniques for handling the problems, the ways one thinks about them, the ways in which one simplifies a problem in trying to get a theoretical understanding and works one's way up from a simple situation which can be completely understood to the more complex situation that might correspond to the practical problem, all these are techniques which traditionally the physicist has been taught. I think we shall always continue to need physicists in these fields.

It is also true that there is a very close interplay between the different fields of physics, in that for example the study of atomic problems is required as a background to a nuclear experiment. The nuclear physicist must know what his particles are doing in their passage through matter, and this leads him into atomic problems. Conversely, discoveries of nuclear physics may assist with our study of matter in the aggregate state.

Nuclear magnetic resonance is a beautiful example of this kind of interplay. If the problems of solid-state physics, to which this technique can be applied, were regarded as engineering problems, divorced from physics, our progress might have been very much less rapid.

I might here, perhaps, quote another interesting example more recent than nuclear resonance. This is the so-called Mössbauer effect, which makes it possible to obtain fantastically sharp resonance lines through nuclear excitation, using the resonant absorption of gamma rays by nuclei. In demonstrating these sharp resonances, one is exploiting the properties of solids and the nature of the limitations that quantum theory imposes on the motion of atoms in a solid. There are many interesting lessons to be learnt from this discovery.

If I may inject a personal note—it has taught me an important lesson because I was in very close touch with experimentalists who were interested in the scattering of gamma rays, and who in fact asked questions, for example, about the reason for the shapes of the resonant absorption line in a solid absorber and in a liquid absorber being different. The reason for the line breadth is partly the Doppler effect due to the thermal motion of the atoms. Classical physics would say that the thermal motion of the atoms is precisely the same in the solid and in the liquid, and there should be no difference. The experimentalists were surprised that there was a small difference, but I pointed out that there was no reason to be surprised because quantum theory had to be applied to this problem, and the details of the motion of the atoms in the solid were to be thought of, for example, in terms of Debye's theory of the specific heat of crystals, and this therefore would predict a difference between solid and liquid. Now it was only one step from saying that to seeing that in suitable circumstances one could in fact get away completely from the effects of the motion of the atoms and obtain sharp lines; but I failed to take that step, and had to wait until the ingenious work of Mössbauer was published.

But I am referring to the Mössbauer effect not to ask you to commiserate with my regrets at having missed the obvious, but because of another thing that happened. Naturally this work was done by nuclear physicists who were interested and experienced in the use of gamma rays; and then for a year or two people did not take much notice of the new possibilities, largely because in order to understand what was going on, one had to know something about gamma rays and one also had to know something about solids. One was really exploiting solid-state physics, and even the important applications of the new discovery, apart from demonstrating a fascinating and curious phenomenon, was that it could be used as a tool for tackling certain problems in solid-state physics.

But nowadays, physicists are either solid-state physicists or they are nuclear physicists, or something else; in the first case they do not know anything about gamma rays, and in the second case they do not know anything about solids. In fact quite a few papers were written about the theory of this effect, some right, some wrong, in which the authors expounded with great enthusiasm some nice equations relevant to the new effect, without realising that the formulae they had written down could be found in any text-book about X-ray crystallography, where the same problem had been solved completely.

I think there is a lesson in this, and that is that one must take great trouble to resist excessive fragmentation in a science like physics. Much of the best work has been done and always will be done by people who cut across the various conventional divisions and who are willing to use in one field the ideas and concepts and techniques developed in another. It was for this reason that I was a little apprehensive when Professor Blackett suggested the organisation of a large physics department in terms of joint undergraduate teaching, but autonomous and independent research schools, each with its own separate group of research students. Of course, in one way this is how one has to proceed because everyone has to work on a specific problem. One cannot today be involved simultaneously with all parts of physics; this would just be impossible.

But at the same time I think one has to watch carefully that these groups of research students and senior research workers do not retire into their little corners and talk only about the problems involved in their own research work; or else the further progress of all parts of physics might be considerably delayed. I think this applies particularly in theoretical physics where the adaptation of an idea from one field to another can proceed more quickly and more easily.

I would advocate that the training of theoreticians should be as broad as possible. In practice, I think, breadth is possible only if they work in an atmosphere in which a large number of very different problems are under investigation by different people. This can sometimes lead to another curious phenomenon. In the fundamental problems of which I was talking first, the theoretical techniques used are usually called "field theory". This subject, because it is more fundamental in the sense of coming closer to the fundamental laws, has a certain amount of glamour attached to it, which extends beyond what is necessarily the value of any particular piece of work. It is an abstract and difficult field in which progress is necessarily slow, and many papers have to be produced which are of rather ephemeral value. I think a sound and novel piece of original work in one of the more established parts of physics may well be of much more lasting value than a minor contribution to a large calculation on field theory.

In field theory it has become customary to use certain very elegant and ingenious techniques and it was discovered that some of these techniques could also play a useful part in other topics, for example in solid-state physics, and so, quite rightly, a number of papers came out discussing, for example, the motion of electrons in polar crystals, the so-called polaron problem, in the language of field theory. One then finds the curious phenomenon that people develop a particular pride in re-writing in terms of the language of field theory, work which could perfectly simply and adequately be done by quite low-brow techniques. Sometimes, even now, it is considered particularly worthy of merit to re-write a standard treatment of a problem in the language of field theory. In my opinion, there should be more contact between the different specialists, so that research workers become more familiar with all the important techniques. Or at least they might see them in operation even if they could not manage during their research training to have actual experience of them. The result might be that it would be easier to call on these techniques when they were really appropriate, and there would be less temptation to employ them for their own sake, or to derive special pleasure from doing so.

I have of course exaggerated, because even in the fields of solid-state physics and atomic physics by no means all the basic fundamental problems have been solved. It is certain that we know the fundamental laws, but we have not succeeded in all cases in extracting from these fundamental laws an explanation for certain interesting phenomena. Perhaps one of the latest achievements in that direction is the understanding of superconductivity, one of the most fascinating phenomena, which was discovered as a complete surprise, and which for many years resisted attempts to link it with the fundamental laws of electrons in solids.

In the early days of the quantum theory of solids, whenever one saw a new facet of the equations suggesting a reason for conductivity to vary rapidly, one asked oneself the question "could this conceivably be an explanation for superconductivity?" But it usually was not. In fact Felix Bloch, one of the men who helped to develop the theory of solids, in those days pronounced what was then, I think, called the Bloch Theorem, which stated that "theories of superconductivity can be disproved". We have lately made considerable progress with this problem through the work of people like Fröhlich and particularly Bardeen, Cooper and Schrieffer, who really have helped to give us an understanding of superconductivity.

There are still considerable difficulties, mainly because one is dealing with extremely subtle effects, as evidenced by the fact that the phenomena take place at temperatures of a few degrees from absolute zero. They, therefore, obviously involve some very weak interactions of a magnitude corresponding to the thermal energy at that temperature. Hence a theoretical foundation for such a phenomenon is extremely difficult because one can never quite prove that the equations are accurate enough to establish such small effects. One would have to make sure that one had neglected no small effect of that magnitude. This is quite impossible since these energies are perhaps one part in ten thousand or less of the total energies involved in the motion of an electron in the metal. We would never dream of working out the whole problem to that kind of accuracy. Hence to some extent one has to have faith that one has picked out the things that matter, and that what is rejected does not matter.

We are, at the moment, in a very interesting situation, because very similar concepts can be applied to the superfluidity of liquid helium in which very similar surprising and interesting things happen at low enough temperatures. There again we have a theory which we think can be relied upon, and accounts for the facts.

Here a new problem arises because there is the light helium isotope, He^3, also as a liquid, and one is tempted to speculate about its behaviour. The theory was in fact applied to this case before the answer was known experimentally, and that of course is always the best way of testing a theory, whether it can predict something which is not already known.

At one time the best calculations predicted that at a certain temperature there should also be a phase transition in He^3. But there was not, and people looked at their calculations again and found some possible complications indicating that the temperature of the anomaly might be lower. If one predicts a phenomenon at a low enough temperature one can always make sure that there will be no contradiction. But experiment is catching up rapidly, and it is now well on the way of getting down to temperatures where the theories fairly confidently predict that something interesting will happen. We do not yet know the answer. It may be that the expected will happen, and this will strengthen our confidence in that particular theoretical picture. If not, there will have to be some second thoughts.

I have picked my illustration, naturally, very largely from the parts of physics with which I am familiar, and have given therefore a somewhat one-sided picture, but in any case there would not have been time to comment on the whole of physics. But I think there is always the common feature that the way we treat physics and look at problems is much the same in spite of a very wide variety of motivations. Very possibly different people might have different motives and reasons for thinking some problems interesting.

We have on the one hand the limit of the front-line work which will enlarge our knowledge of the fundamental laws, but there are many other parts of physics which are now very clearly removed from that frontier. We have parts which are obviously of importance because of their applications, but most of the physicists who study them do so not because they are conscious of the importance of the applications, but because they are fascinated by the challenge of the problems themselves.

I am not sure, for example, whether one would ever expect to find a practical technological application of the superfluidity of liquid helium. I would not rule out the possibility; certainly we already know of suggestions for exploiting technologically the phenomena of superconductivity, and maybe some day superfluidity will have its applications. But that is not the reason why we think it is interesting. It may well be the reason that one asks for Government support for some of these researches, but it should never be the only motivation.

Nuclear physics is an intermediate case in that it is no longer mainly in the front line, and it is also not being studied principally for the sake of the applications. There is, of course, a considerable nuclear technology through nuclear power and nuclear weapons, but this is concerned really with a very limited range of phenomena in nuclear physics which are already fairly well understood. I do not think that much of the present work on nuclear physics really contributes, or is likely to make contributions to that. One might therefore well question whether one should continue to do nuclear physics. But firstly, I am sure it will go on, and secondly, I am sure it is right that it should go on because of its interplay with other parts of physics which we have not yet adequately explored. Of course the nuclear physicist carries on because he is challenged by the problem.

One might in abstract logic see a danger here. If we just follow what looks interesting, what attracts our fancy at the moment, then indeed some branch of physics might not be so different from stamp collecting. But I think this is academic, because the atmosphere of physics leads to a value judgement, a feeling for what is important, which in general guides the physicist to something that is worthwhile. This must be taken in a broad sense, because it can never be claimed that any particular experiment or theoretical study is necessarily worthwhile. We cannot escape trying unsuccessful ideas. In a research field of a live science, one has to try many things before one of them is successful.

So in spite of not always being able to give a very conscious logical explanation why we are studying a particular problem, we are yet very far removed from the attitude of the famous nineteenth-century physicist (I have forgotten who made this remark but you are no doubt familiar with it) who said, "I would certainly measure the velocity of the rainwater in the gutter if only I could measure it accurately enough". This, I think, is not the present attitude of physics. Why we are doing what we are doing is as hard to formulate in a specific case as our reasons for what we do in our everyday lives, where it is notoriously difficult to decide why we do what we do. But, in spite of its danger of fragmentation, and in spite of its broad spread over specialized fields of study, physics is one subject with one prevailing attitude running through it, and taken as a whole it is I think moving in directions which are sound. They are always changing, and before the end of the useful life of the new building that we are here to admire today, they will no doubt have changed in many new ways and will bring out new problems whose existence we do not even suspect at the present.