SOLID

by Frederick Seitz

Physicists often leave the new ground they break for others to till and to cultivate. Solid state physics, the author finds, is unique because physicists are continuing its exploitation. This, he thinks, may be a new pattern for other fields from which the frontier has withdrawn. This article is taken from a speech given at the dinner meeting of the American Physical Society's Division of Solid State Physics in Cleveland on March 11, 1949.

Solid state physics is a field which has been in a somewhat unique position for about fifteen years. Although it probably will not continue to be unique very much longer, it may set a somewhat new pattern which other fields will follow. That pattern is the secondary exploitation by physicists of a field which physicists themselves have opened.

The primary goal of physics has been of course to discover the fundamental laws underlying the operation of the material world about us. Prior to 1900 the discovery of new laws was sufficiently infrequent that most contributors spent their entire lives in a period during which only a fraction of the fundamental laws relating to a given field were uncovered. Consider electromagnetic phenomena, for example. The complete expression of the laws relating to this subject is contained in Maxwell's four equations which were first formulated in about 1870 and not fully understood for another generation. Actually the constituent laws were discovered over a period of about two hundred and seventy years extending from the work of Gilbert in 1600. Most of classical physics was developed at a sufficiently slow pace that the individual investigator experienced perhaps one or two revolutionary discoveries during his lifetime. The facts which he learned in his twenties were still moderately fash-

Frederick Seitz, head of the department of physics, Carnegie Institute of Technology since 1942, has recently accepted an appointment for the coming year as research professor at the University of Illinois where he will head a group doing research in the physics of solids. He is best known for his work in this field and is the author of two books on the subject. Both during and following the war he was involved in atomic energy work and was director of the training program at Oak Ridge during 1946–47.

ionable by the time he was sixty, and he had ample time to digest the innovations in the entire field, provided he possessed that essential ingredient of a good scientific life—leisure.

Since 1900 the pace has greatly accelerated. Non-relativistic quantum mechanics, which is fully as intricate as electromagnetic theory, was developed essentially from scratch in about thirty years. A great physicist such as Bohr, who came into the scene of physics when the development of quantum theory was in its infancy, found a unified part of this field brought to an essentially final stage of development when he was still in his middle years. A great pioneer of this type is able to witness and participate in the passage of the frontier of physics across several mountain ranges and to see many of the valleys left behind become completely urbanized.

In this state of affairs, in which the frontier moves so rapidly, it is entirely likely that relatively unexplored regions will be left behind which still contain primary mysteries and which merit further exploration. The field of solids is of this nature, and in many important respects provides a unique example. The most unique feature of this case lies in the fact that this secondary exploration is being carried out primarily by physicists.

By-Pass

To emphasize this point let us consider one or two other cases in which important pockets have been left behind in the development of physics. After Kelvin and his associates had formulated the basic laws of thermodynamics it was evident that there was a vast field of science which could be further explored with the use of this tool. For the most part this secondary exploration was left in the hands of the chemists, whereas physicists devoted themselves primarily to discovering the connection between thermodynamics and the laws of mechanics—a task which met its climax in the work of Boltzmann and Gibbs and provided one approach to quantum theory. At this stage in the development of physical science, the products of the physicist were handed on so rapidly to the chemist that the chemists were able to assimilate and enjoy all of the more delicate elements of logic of the field. It is evident if one reads the works of such chemists as Ostwald, Nernst, and Eucken that they took over these fields from the physicists as if they were entrusted with a rare religious relic whose possession they prized as highly as anything else in their lives.

Similarly, after the formulation of electromagnetic theory, the physicist employed the fundamental laws primarily as a tool for expanding the main frontier of physics. As a result the field of electrical engineering was born. Although the physicist has furthered this field of engineering enormously since the days of Maxwell and Hertz, his contribution has almost always been of an incidental nature. He has either been trying to develop a new item of apparatus for his own research to further the fields of electron physics or nuclear physics, or to win a war.

It is true that the physicist still has strong claims in the fields of optics and spectroscopy. However, much of the frontier has clearly been taken over by chemists and astronomers, so that the physicist can at most claim partnership in these fields. Perhaps the clearest indication of this is the fact that many of the professional activities in the fields are coordinated through the American Optical Society which permits relatively broad membership.

With this pattern of history clearly in mind, it seemed to me in the late thirties that the field of solids would soon cease to be of more than passing interest to any large number of physicists. In the main, I felt that the physical chemists would absorb it as a natural extension of their own interests and that the few physicists who decided to continue in the field would soon find themselves members of the American Chemical Society. A transition of this type is certainly no less logical than the transition which centered about the field of thermodynamics and statistical mechanics. In the last analysis the present goal of the chemist is presumably to explore the properties of complex atomic aggregates. The great simplicity which solid systems derive from their crystalline structure makes them a natural field for exploitation.

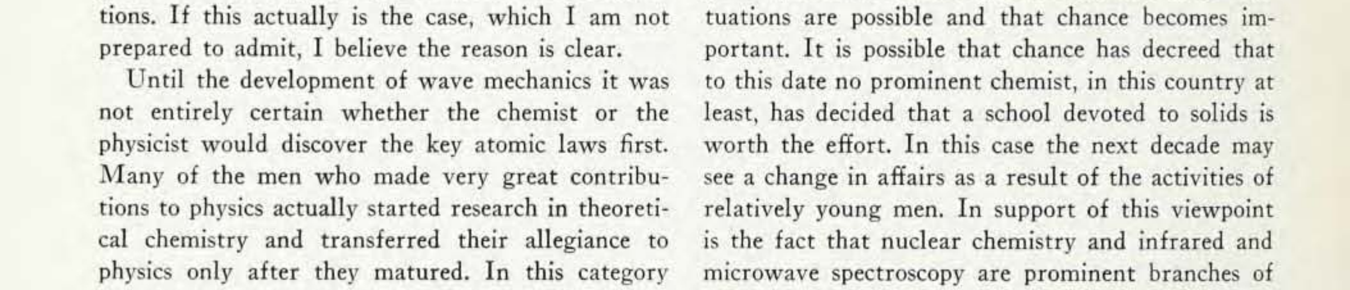
The Second Assault

Surprisingly enough there does not seem to be any strong trend of this type, at least at the present moment. Last year I had the privilege of spending a month in Washington as a guest of the Office of Naval Research in order to aid in the review of its research program. The importance of this agency as a clearing house for American research does not require elaboration here. I found to my surprise that essentially all of the research in solids which stresses the atomic or electronic point of view was being undertaken by physicists in a completely traditional manner. Practically no chemists in this country seem interested in taking over the frontier of the field to exploit it along the lines which the physicists have opened. Naturally one must be careful of generalizations, but I believe it is safe to say that the interest of chemists is purely peripheral at this moment.

This leads us to the following very engaging question: how does this situation come about? I must admit that I do not have a very clear answer. There are two possible explanations which come to mind: the most unhappy one, which I find difficulty in believing, rests on the viewpoint that the golden age of theoretical chemistry is passing and that henceforth chemistry will be primarily an empirical science which employs those theoretical principles and viewpoints which it has already assimilated, and depends otherwise upon empirical research for innovations. If this actually is the case, which I am not prepared to admit, I believe the reason is clear.

Until the development of wave mechanics it was not entirely certain whether the chemist or the physicist would discover the key atomic laws first. Many of the men who made very great contributions to physics actually started research in theoretical chemistry and transferred their allegiance to physics only after they matured. In this category are, for example, men such as Stern and Franck. Many important figures such as Langmuir and G. N. Lewis remained in chemistry. In any case these men were probably lured into the field of chemistry initially because of the great mystery which surrounded atomic phenomena. Whether they remained chemists professionally or became physicists was entirely incidental. Their most important interest was to be at the frontier. Once wave mechanics was developed the primary frontier moved on and one could say, as Dirac did, that the fundamental laws of chemistry were known. Once this stage had been reached it is quite possible that chemistry ceased to hold its lure for men of the type who prefer the rigors of the frontier to an urban scientific life. Henceforth their younger counterparts will be found in nuclear physics.

A second explanation, which is less radical, rests upon the elements of chance. Most innovations in a field depend upon the work of a very few individuals who establish schools, train younger men, and thereby magnify their contributions. For example, in the early thirties Wigner, Slater, and Mott established schools devoted to the application of quantum mechanics to solids, which acted as the source points of much of the theoretical work which has been produced since. Parallel cases can be found in all fields. Granting that critical developments depend upon small numbers, it follows that large fluctuations are possible and that chance becomes important. It is possible that chance has decreed that to this date no prominent chemist, in this country at least, has decided that a school devoted to solids is worth the effort. In this case the next decade may see a change in affairs as a result of the activities of relatively young men. In support of this viewpoint is the fact that nuclear chemistry and infrared and microwave spectroscopy are prominent branches of chemistry which have taken over a great deal from physics in the recent past. In cases such as this a small number of individuals, such as Seaborg and E. Bright Wilson, have played a very important role. This viewpoint is also supported by the fact that one or two European physical chemists, such as C. Wagner, have taken a strong interest in the modern developments of solids.



Here to Stay

In any event, I believe it is safe to say that those physicists who decide to continue with or to enter into the study of solids will have undisputed leadership in the domain for some time to come. I am inclined to believe that this field will offer great freedom for continued research for at least another generation under conditions that permit application of that type of logical discipline which is one of the essential features of physics. It should not be forgotten that our appreciation of the properties of materials such as silicon and germanium dates only from about 1942. We are only beginning to understand why free electrons have a long range of travel in some materials and not in others. Before us lies a whole host of almost unsolved problems of great fundamental interest. There are, for example, at least a half dozen first-rate mysteries associated with the properties of solids in the immediate vicinity of absolute zero. The understanding of the properties of organic crystals is still very small. I note also that an English investigator has only recently observed that copper phthalocyanine is a semiconductor. We still have only the most indirect knowledge of the mechanism of plastic flow in solids and the factors which influence it. Moreover, all of these fields have many practical aspects which are of great use for modern technology.

All in all, I see no reason for hesitating to encourage bright graduate students to enter the field. The portion associated with low temperatures alone should prove fascinating for a number of years.

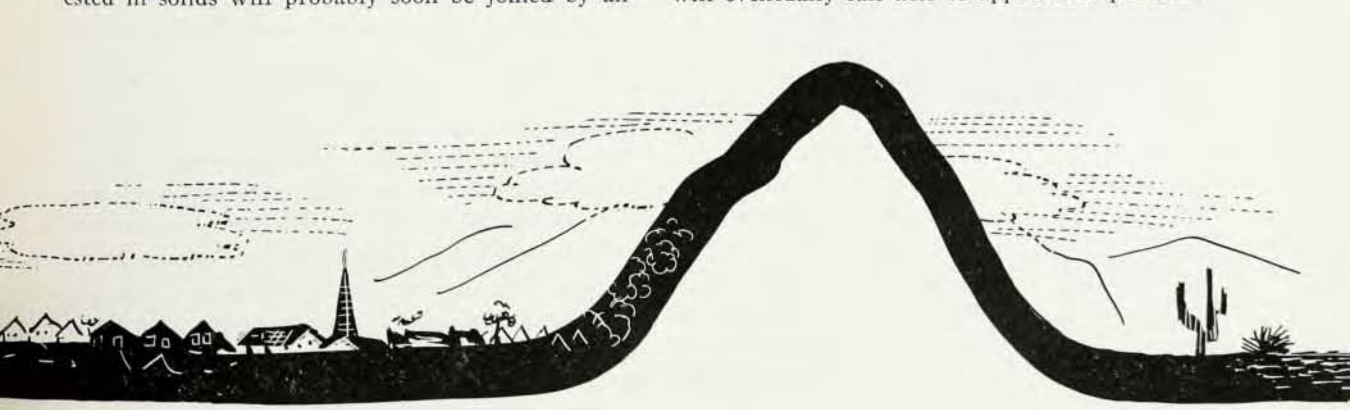
Other "Pockets"

It is interesting to note that the physicists interested in solids will probably soon be joined by another group in living the semi-urban life which follows soon after the passing of the frontier. This is the group which is engaged in what is now coming to be called classical nuclear physics, that is the domain which can be developed and discussed without direct knowledge of the properties of the meson.

Nuclear physics has developed so rapidly during the past years that the outlook has undergone considerable evolution. Immediately after the discovery of the neutron in 1932 it was felt that the properties of nuclei might be explained by treating neutron and proton (nucleons) as fundamental particles which interact in pairs, much like the electrons and nuclei in atomic systems. It became clear, well before the war, that the nucleons actually are complex particles whose interaction probably can be understood quantitatively only after a great deal more is understood about another particle or group of particles now known as mesons.

This growth of outlook has caused a bifurcation of the field into what may be termed meson physics, which can be explored experimentally only with the aid of accelerators which produce particles with energy in excess of one hundred million electron volts, or with cosmic rays, and into what is called nucleonics or classical nuclear physics. Apparently the latter field alone is accessible to the prewar accelerators and the chain reacting piles developed during the war.

In a broad and general way the field of classical nuclear physics appears now to be one valley removed from that in which the main frontier lies. On the whole it seems to occupy a position closely similar to that which solid state physics occupied just before the war. Here too it seems probable that future development will lie mainly in the hands of physicists, although it is possible that the chemists will eventually fall heir to appreciable portions.



In the meantime meson physics is in the midst of an exceedingly active state of development highly reminiscent of that which existed in classical nuclear physics in the stage at which the neutron and artificial radioactivity were discovered. It would be unjustifiably bold to suggest that no startling new discoveries will be made in classical range; however the same situation is valid for the field of solids, particularly if low temperature phenomena are kept in mind.

Solid

It is interesting to speculate on the future of the physics of solids on a somewhat coarser scale of time than that which normally is of practical personal interest, but which is of philosophical interest—say over the course of fifty years. I select this period of time with intent because, as you may know, Gamow has recently estimated that the present program of physics may well have run its course by that time. (This is all based on the assumption that our culture survives that long.) Gamow suggests that by that time particle physics may have been unfolded and that the running frontier may have attained its rational limit.

Until this time is reached, I believe that training in physics will be much the same as at present, although there will be an increase in the subject matter to be covered in graduate study. It will still be necessary to train students in fundamentals with the primary purpose of preparing a very important fraction for the arduous but enjoyable task of expanding the frontier in its present direction. This will require great economy of effort if formal education is not to extend beyond the present PhD level. During this period the main line of fundamental training will probably place a minimum of emphasis on electron physics since the electron will be regarded as a relatively unimportant fundamental particle. The hydrogen atom and a few similar simple electronic systems will be considered in the basic courses such as modern physics and quantum mechanics, but, on the whole, emphasis will be placed on the properties of the nucleons in looking for good examples to illustrate the applications of wave mechanics.

It is highly doubtful if the basic ideas of the band theory of solids will be common knowledge to all new PhD's. This trend is in fact clearly evident in recent books on quantum mechanics such as Schiff's, which will become the standard texts for training graduate physicists. Here and there one will probably find exceptional physics departments. In fact there may be an interesting competition between academic institutions in the selection of special "classical" fields in which research is done; however these fields will not be identical in all schools. The physics of solids and nucleonics stand a good chance of surviving well into this period of time as fields of physics because of their broad interest, although there is still a chance that the field of solid state physics will be claimed by chemistry and electrical engineering. Similarly, classical nuclear physics may be claimed by a new engineering field called "nuclear engineering" which is discussed frequently.

Should particle physics meet a natural terminus in about fifty years we may expect a lateral expansion of physical science in many diffrent directions, assuming society still maintains its interest in science. Since the electron has tremendous human interest because of its importance in chemical binding, without which there would be no human beings, I would expect our field to continue its life. Perhaps the development of rapid computing machines will make it easy to carry out manipulations that are now difficult so that it will be possible greatly to extend the treatment of complex atomic systems on the basis of wave mechanics. In such a case the facts concerning solids which are now being accumulated probably will occupy an important position in the cornerstone of the new structure.