TIME SCALES:

Speculations about the Fut

By ***M. Dresden***

FOR the past five years the American Institute of Physics has sponsored a series of visits by various physicists to a number of universities and colleges. One of the purposes of these visits is to acquaint the students and faculty of these institutions with some of the results of current researches. While discussing the present, questions are inevitably raised about the future of physics. Faculty and students, for different reasons and from different vantage points, appear extremely interested in the subject. The following text was written with this subject and this audience in mind. The lecture was never delivered as such, although many of the ideas expressed here were discussed in several of the lectures actually given.

Motivation

There is always something tantalizing about the speculations which deal with the future of a field. It is interesting for the speaker, for he can be utterly irresponsible; having stated once and for all that nobody knows anything definite about the future, he can speculate to his heart's content. In this process he can only win, for, if developments mentioned by him do take place, he is a man of profound insight and broad vision; if they do not, this only verifies his initial statement that, in fact, nobody knows the future. The listener too can share the interest, for he can speculate in the same vague manner as the speaker, and, where the speaker does not have a compelling responsibility to be correct, the listener need not feel any strong responsibility to understand. Both the speaker and the listener can enjoy their irresponsible and wild speculations.

Apart from these general speculative considerations about the character of physics in the future, every physicist has to deal with matters which in some way express his personal guesses as to the likely future of the field. These guesses may show up in research problems he tackles, in the textbooks he chooses for his courses, the laboratory equipment he buys, the papers he reads or does not read. Even without explicit reference to the future of physics, such decisions are likely to represent a person's best estimate of what may be important in times to come. In physics (as in many other areas) the future is always with us. The most overpowering manifestation of this presence is in the deluge of research papers, reports, conference proceedings, etc. Each one of these *could* contain results which would profoundly affect any one area of the field. To be sure, it is not likely. In fact, some of the results published are wrong, a good deal of the information is irrelevant, much of it is transitory. But even so, the possibility of a significant change is ever present; the problem of keeping "up to date", of noticing the potentially important advances even in a limited area of physics, is of continual concern to most physicists. This is not just a problem for researchers; it is of equal concern to those who teach, for graduate courses and certainly thesis research should reflect current trends, requiring frequent change and shifting emphasis within the graduate curriculum. The undergraduate curriculum is generally more stable, but not immune to change.

Even though the changes in the undergraduate curriculum take place more slowly, there one has the additional difficulty of deciding what new material to present and the problem of exposing relatively unsophisticated (if not unwilling) students to strange and unfamiliar concepts. This requires profound thought and a deep understanding of the subject.

It is hard to know what attitude to adopt towards these changes. It is certainly wise to exercise a healthy skepticism towards all claims of radical innovations and revolutionary changes. Yet it is clear that one cannot

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Physics

dismiss all the work done as pointless, all the activity as useless, and oppose or ignore any and all changes.

It is the purpose of these comments to discuss and analyze the various changes in physics, to study the likely effect of those changes which can now be anticipated, and to suggest possible approaches for living in peace with the future. It is perhaps well to caution you again: physicists usually do not presume to know what lies ahead, but, when they have engaged in such speculations, they have often been badly mistaken. Worse than that, they have frequently shown little appreciation of the significance of discoveries made in their own times. Frightening examples are afforded by the slight significance Rutherford attributed to his own model of the atom and by the unwillingness of Planck to accept Einstein's quantum interpretation of the photoelectric effect. And so it may well be that while speculating about discoveries in the future, we are actually overlooking significant discoveries already made, but unappreciated.

Why Does Physics Change?

To arrive at some basis from which we may judge the significance of the many changes in physics, it is useful to outline some of the main factors which produce such changes. (Even though these factors are listed independently, it is probably obvious that they all influence one another.)

1. The changing technical status.
2. The shortened time between the discovery of effects and their technical applications.
3. The changing fashions.
4. The varying outside demands.

It is well known that physics starts with experimentation. The character of the experiments done is to a large extent dependent on the equipment available. In the course of the interpretation and description of experiments, certain concepts and principles evolve as particularly fundamental ones. Using such principles, large parts of physics can be unified in theoretical structures of broad scope and wide applicability. From such an increased understanding of the physical world flows a variety of technological applications. The increased technological skills can be used to construct better and more accurate measuring apparatus. This, in turn, allows a more detailed examination of the physical world, yielding greater understanding—and so the process continues. We have here one of the most significant causes for changes in physics. A knowledge of physics always produces the possibility of doing more and better physics, and the presence of a highly developed technology is an essential intermediary in this process. The reason physics changes so fast is, in part, because the time interval between the discovery of a principle and its technical utilization has become extremely short. Discovery, interpretation, and applications follow one another in rapid succession, so that experiments which at one time could not be done at all can now be done with relative ease. Experiments which at the beginning of this century marked the frontier of physics are now commonly performed in undergraduate physics courses. This development has an important, and in some senses unfortunate, consequence. From what has been already said, it follows that the performance of most significant experiments requires delicate apparatus. This apparatus is expensive, elaborate, and indispensable, and a tremendous amount of time and effort is spent in its design, construction, and maintenance. This technical aspect of physics—which is not itself physics—demands an increasing amount of time on the part of most physicists. It can again be seen that these technical improvements necessitate frequent changes. If, say, a particularly effective particle-detection device is discovered, it becomes imperative for all workers in the field either to use it or to develop other methods yielding comparable or better results. The "technical encumbrances" of the physicist vary from field to field; they are most extreme in high-energy physics and "space" physics, but they are important in all phases of physics. Thus you may not think that a person is doing physics when he is reading a new catalogue of a vacuum-tube manufacturer. Very likely, however, he is checking to see whether or not there exist vacuum tubes having special characteristics which he needs to do a particular experiment. And even though reading the catalogue does not make him a physicist, not reading it may well keep him from doing physics.

The continual interplay of technical advances, the increasing significance of technical apparatus, the frequent changes in this apparatus, these are all characteristic of the present epoch in physics; they reflect the changing, restless character of the field.

Apart from the changing techniques associated with experimentation, physicists have also to contend with profound changes in the mathematical techniques employed in physics. Different aspects of mathematics are applied in different branches of physics; classical analysis and complex-variable theory are applied in mechanics and electrodynamics, matrices and operator theory in quantum mechanics, differential geometry in relativity, and group theory in spectroscopy and fundamental-particle theory. Whether or not new understanding, new physical insights, are ever obtained as a result of using more powerful and appropriate mathematical methods is a much debated question. Be that as it may, it is certain that a lack of mathematical technique can stop a person from participating in certain areas of physics as effectively as can a lack of experimental equipment. It is not at all uncommon that new areas of physics (relativity, quantum theory, fundamental-particle theory) require their own special uses of different mathematical disciplines. Thus the changes in physics are accompanied by a different use of mathematics. This combination contributes substantially to the rapidly changing character of physics.

All of the changes referred to so far originated in new uses of apparatus or in the development of new procedures. Such changes can be described rather easily; they can, after all, be observed directly. Physics is also subject to changing fashions. These are more difficult to describe. Although some people might not even agree that there are such things as fashions in physics at all, it is my opinion that they do exist. They are expressed perhaps most importantly in the decisions as to what problems are significant; but they also are evident in the manner in which problems are approached, in the type of mathematical techniques employed. The fashions themselves are of course strongly influenced by the experimental status of the field. For example, there is really no reason why the tremendous amount of theoretical work done in quantum electrodynamics from 1946 to 1950 could not have been done ten years earlier. The theoretical framework had been set up, the divergence problems were all present. In fact, Kramers, in his famous book on quantum mechanics, approached the problem in the fashion which became commonplace later, but at that time his work was largely ignored. In 1946, the magnificent experiments of Lamb provided the additional impetus for the development of quantum electrodynamics. Here is an instance where the new microwave techniques (developed during the war) allowed an experimental accuracy yielding results which required a careful reconsideration of the basic ideas of quantum electrodynamics. This field was very fashionable from 1946 to 1950, and many of the concepts introduced (the renormalization notion) have been transcribed to other fields such as superconductivity and solid-state theory. In the mathematical description of quantum electrodynamics, Feynman introduced a very ingenious diagrammatic method which turned out to be extremely powerful. This method, which has the character of a technical improvement, has been carried over to many different parts of physics, with major success. There can be no doubt that the analysis by means of diagrams has become an essential tool for the theoretical physicist. The method is currently quite fashionable, not only in field theory, but also in solid-state physics, statistical mechanics, and many other areas.

The most profound way in which fashion influences physics is in the choice and setting of the "fundamental" problems—those where new situations are described and where one feels there is a good chance to discover new basic principles of physics. As examples, one might mention the existence of the μ meson, or the validity of parity inversion invariance in strong and not in weak interactions. Other problems, in contrast, offer a complicated and unfamiliar juxtaposition of known circumstances. The latter would be exemplified by a calculation of the coefficient of sliding friction between lead and gold based on the Schrödinger equation. I feel that I gage the taste and current fashion in physics correctly if I say that a complete and accurate calculation of the friction coefficient would cause considerably less excitement than a partial and incomplete calculation of the mass of the μ meson.

Still, the fundamental character of a problem, of and by itself, does not guarantee that it will belong to the group of fashionable problems. The problems of general relativity certainly are as fundamental as one can wish; yet this subject has not been in the mainstream of physics for some years, although profound and significant contributions have been and are being made. In a way, the subject dips in and out of the mainstream. It is important to note that what is fundamental can change overnight. Before the work of Fermi, the subject of β decay consisted of a number of interesting, but loosely connected experiments. After Fermi's work (1934) more systematic and more detailed experimental material became available which could be correlated with the theory in some fashion. The measurement of β-ray spectra became more precise; the interaction responsible for the decay process appeared to have a fairly involved structure. Until 1957, β decay was an interesting subject, certainly not a "hot" subject, but quietly respectable. Actually, one major university attempted to dispose of its β-ray spectrometers; clearly they did not anticipate any major developments in that area. Then, with the discovery that parity is not conserved in β decay (or generally in weak interactions), a new wave of activity swept over the field of β decay. This, by now, has once more leveled off. (Perhaps the β-ray spectrometers are for sale again!) In general, experimental discoveries and theoretical suggestions may transfigure any part of physics, no matter how pedestrian, into a new and vital branch, full of frantic activity—for a while in any case.

One of the factors which has contributed directly to the rapidly changing character of physics is large-scale research and group research. This type of activity reached a major peak during the war, when physicists were called upon to perform special tasks of military importance. Since the war this same kind of costly, large-scale research has expanded in a variety of directions, with the active support of the military establishments, other agencies of the government, and industry. These efforts do not change the basic problems in physics, but they do affect the way in which physics is done. Individual effort is in some sense submerged in group effort; the isolation necessary for quiet contemplation is replaced by an intricate system of advanced, prepublication notices; the well-written, carefully weighed scholarly study is replaced by a terse, cryptic announcement of papers to come. The very frequent national meetings, international conferences, all serve to spread information widely and rapidly. Special "letters journals" are specifically devoted to the rapid publication of short notes. Even though the large-scale research efforts do not basically change physics, their existence is likely to change the selection mechanism of persons entering the field. Another significant factor influencing the character of physics (and many other fields as well) is the availability of high-speed computing machines. There is a large class of physical problems in solid-state physics, nuclear physics, high-energy physics, space physics, and other fields where the importance of computing techniques is becoming more and more pronounced. This is true for the organization of experimental material, as well as for the comparison of data with experiment. It is clear that, if a physical problem can be reduced to a problem which can be handled effectively by a machine, it is thereby soluble, but to know whether or not certain problems are soluble, we must know something about the capabilities of computing machines. Thus, certain problems in physics must be thought through anew, keeping in mind the possibility of using computers. With the rapid changes in computer technology, the applicability of computers to physical problems can be expected to change rapidly.

The Time Scales

The various changes referred to all take time. It may help in appreciating the changes taking place if a comparison of these various times is made. Let t_T be the time it takes to train a physicist from graduation from high school to a PhD degree; thus t_T is of the order of ten years. Similarly, let t_c be the time a person can be expected to be active in his career; t_c is about 30–40 years. And let t_i be the time in which major changes in instrumentation occur, let t_Δ be the time in which major conceptual changes occur, and let t_F be the time during which a given part of physics is unquestionably the major concern of a large number of physicists. Whereas the times t_T and t_c have changed relatively slight amounts over many centuries, t_i (the instrumentation time) has changed markedly. In the nineteenth century, t_i was of the order of thirty years; in the twentieth century, t_i has decreased quite rapidly. Of course t_i depends upon the field in question, but nowadays most fields of experimental study require some extensive retooling every four or five years, so that now t_i is about five years.

The time t_Δ, the time it takes for a major conceptual change to be assimilated by the community of physicists, is rather harder to guess. It probably takes at least one generation of physicists to accomplish such an assimilation. The opponents of a set of ideas are more likely to die than to change their minds. This would make t_Δ of the order of thirty years. Dyson ("Innovation in Physics", *Scientific American,* September 1958), looking at the evolution of ideas in physics, guesses a value of about thirty years for something like t_Δ. Finally, the time t_F, the time during which a subject is fashionable or first, has also decreased markedly.* Since the basic ideas in different fields are frequently so very different, physicists, if they want to move with fashion, may be compelled to learn new techniques, new methods, every t_F years or so. In terms of these times, the situation in the nineteenth and early twentieth centuries could be expressed by the inequalities:

$$t_T < t_c < t_F < t_\Delta$$

This indicates that the training obtained was sufficient for a productive career, but during that period the field changed relatively little. Nowadays one certainly has $t_c > t_F$, $t_c > t_i$, perhaps $t_c \geqslant t_\Delta$.

Since $t_c > t_i$, $t_c > t_F$, one may look forward, during an active career, to frequent changes in instrumentation, and there will most likely be a variety of different fields in the forefront of physics. These "fashionable" fields may be very different in the instrumentation they require, the mathematics they employ, the concepts used. Since $t_c \sim t_\Delta$, there is even an excellent chance that during that time (t_c), a number of basic notions of physics will have to be reconsidered, or abandoned in the light of new experiences in new fields. There is a very good chance the new fields will use their own (probably unfamiliar) mathematical methods. This appears to be the unsettled picture one may reasonably anticipate.

Speculations About Future Fields

From the present viewpoint a number of fields can be expected to increase in relative importance. Some of these are listed, together with a brief discussion of their claim to prominence.

High-energy physics

It would appear a fair statement that some of the most spectacular results obtained in physics in the last few years have been obtained in connection with high-energy physics. The existence of π and μ mesons, the pseudo-scalar character of the π meson, the various new particles, the nonconservation of parity in weak interactions, all are examples. Whether or not results of a similar unexpected character can be anticipated at higher energies is anybody's guess. However, there are a number of theoretical predictions regarding behavior at very high energies which certainly need to be checked. It is certainly true that one deals here with completely new and unexplored regions of physics, and I, with an admitted personal bias, expect that results

* The time t_F refers to the time the subject is of primary interest to physicists, t_F for atomic spectra was about 35 years; the subject is still of great technical and applied interest, but it is not the main one in contemporary physics; t_F for nuclear physics appears to be about 25–30 years.

requiring major modifications in our thinking will be forthcoming. I do not know whether the modifications themselves will be here soon, but I think that the experimental results in these new regions will at least show us the inadequacies of some of our present notions. The situation can be expected to be extremely confused and extremely interesting for some time to come.

Space physics

The particles, the radiation, and the general physical make-up of outer space are currently under extensive investigation using artificial satellites. It is clear that one is investigating—as in high-energy physics—a hitherto unexplored area and that much detailed information will become available. It is much less clear whether the framework in which the description of outer space is to be expressed will be that typical of classical or quantum physics, or whether this physical situation will have peculiarities all its own. There is a belief (especially among high-energy physicists) that in outer space one has a very complicated system, but that no "new" principles should be expected to emerge from its study. It does not seem profitable to insist on the distinction between known principles applied to altogether new situations, and situations which require an altogether new mode of description. Given an experimental situation which can be described both qualitatively and quantitatively, starting from certain well-defined physical ideas, one can have a reasonable understanding of the situation. As long as this is *not* so (and this is certainly the present status in space physics), the physical situation is not understood and it needs to be investigated. What will emerge in the way of new principles, or old principles and new applications, is less relevant than the general increase in understanding which results from the study. In space physics, a tremendous amount of data needs to be gathered before one can start to think seriously about a theoretical framework. It is my personal guess that these investigations will provide results as striking as those in high-energy physics.

Plasma physics, astrophysics, general relativity

The study of assemblies of charged particles in strong magnetic fields under various extreme conditions of temperature and pressure is currently of considerable interest. It had been known for many years that the study of this kind of system is pertinent for astrophysics; the recent growth in this field of plasma physics stems directly from its relevance for controlled thermonuclear reactions. In this study one starts out from quite well-known physical principles (electrodynamics and statistical mechanics). The extreme values of the physical parameters involved make this study still an investigation of a basically new area in physics. Whether essentially new ideas and concepts are needed to describe the phenomena remains to be seen, but it is already clear that a large variety of new and unfamiliar effects do occur. The understanding of many of them is

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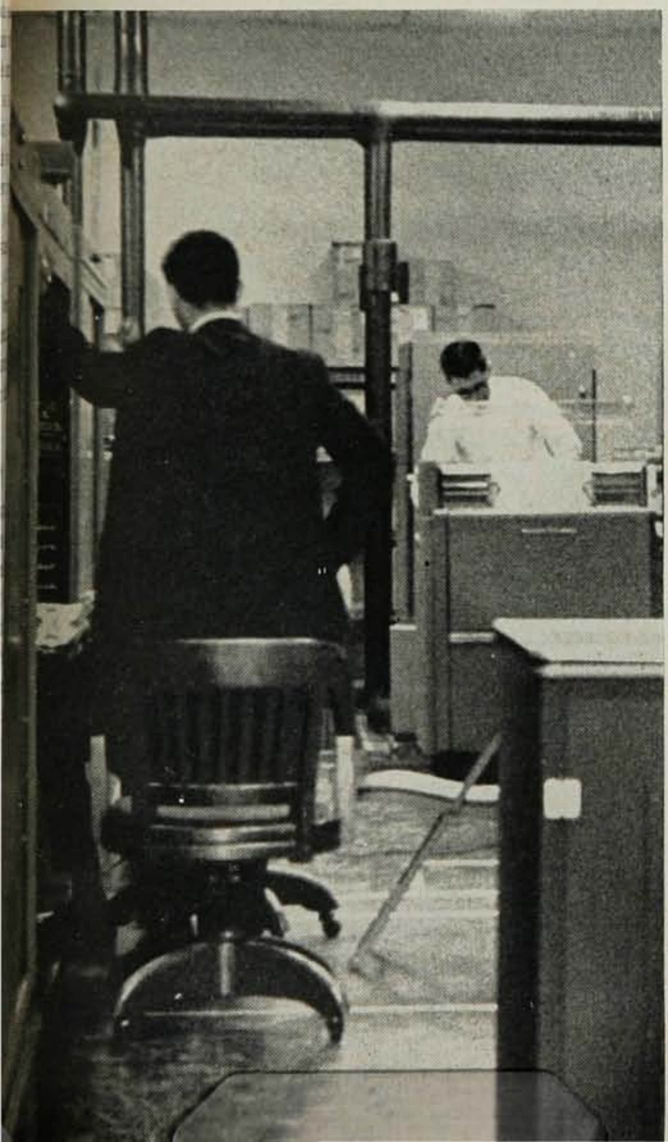

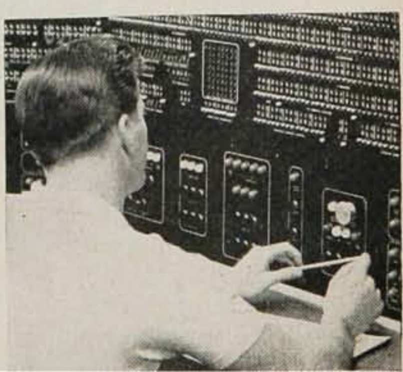

still very incomplete. It would also seem likely that the extreme states of matter as they occur in the interior of stars would become more amenable to discussion. New astronomical information will be forthcoming at a rapid rate in any case, through radio astronomy and through observations from satellites and the moon. It would seem an excellent bet that unusual results await us in this area. One can hope that further advances in astronomy may make contact with the gravitational theories of physics. It seems, at times, as if the general theory of relativity is just waiting to be integrated in some kind of a larger scheme. Experimental confirmation of such general schemes very likely has to be of an astronomical character. I would not at all be surprised to see major innovations result from the combination of astrophysics and space physics.

Biophysics

Many physicists (and I am one of them) suffer from an abysmal ignorance of biology. This is unfortunate, for there seems to be a very good chance that within a decade or so the study of macromolecules, the study of the molecular basis of biology generally, will be of major concern to physicists. This field is already expanding at a phenomenal rate.

It is perhaps pertinent to call attention to the incredible subtlety of biological phenomena. Stereoisomers can have widely different biological effects, even though their physical and chemical properties are generally quite similar. A "fundamental" explanation of such biological differences would require extremely precise and delicate theories.

Applied physics, political physics

Although fashions in physics change rapidly, one should not believe that all the problems in a field are solved, or that the remaining problems are easy, simply because the field is no longer fashionable (or the *most* fashionable). Quite the opposite is true. Once the principles are more or less understood, there remain many specific, detailed questions to straighten out before the new knowledge can be put to practical use. It is actually rare that in applications one can effectively utilize an approach starting from first principles. If one wants to *use* a certain alloy of aluminum to build an airplane, one will measure some of its properties and compute others from the measured information, but it is unlikely that the Schrödinger equation will ever make its appearance.

I feel that the area of applied physics offers a tremendous number of interesting and challenging problems. It may even be that the "return" on one's "intellectual investment" is greater in applied physics than it is in pure physics. This is probably a good thing for prospective physicists to keep in mind. With all the shifting emphasis in physics, the change from pure to applied physics can come about with dramatic suddenness. The phenomenon of superconductivity was still somewhat of a scientific curiosity as late as 1935. Now, not quite thirty years later, this major problem in low-temperature physics seems to be well on its way towards solution. Technical applications of various kinds (superconductive magnets, superconductive computer elements) can be expected in the near future. It is a sobering thought that the purest of research may be turned into a practical development in very short order. It is perhaps even more sobering to recognize that there are many experimental situations in applied physics where everybody knows what principles should apply, but no one knows how to describe the situation accurately.

This enumeration of newly developing fields is incomplete. Quite recently a different type of physicist has come upon the scene, whom I will call a "political physicist". Suppose a physicist has some fine ideas in high-energy physics—specifically, a complete theory of all phenomena up to 50 BeV, at which energy he predicts dramatic new effects. All he needs is a 50-BeV proton beam, of 10^{15} particles/sec. Since this is just a fable, suppose he designs the machine, the detection equipment, the auxiliary apparatus—all perfect for the experiments he has in mind. But here the story stops; the number of millions of dollars needed for the facility is so large that even in a fable there are few places one can get that kind of money. This means the physicist must convince some government agency that the money is worth spending; he must also convince his fellow physicists. Suddenly his activities will no longer be in physics; but rather in trying to persuade other people to get him the tools with which he can do physics. If successful, he may still fail, for even if Congress eventually does appropriate the money, and if the machine, after n years is finally built, it most likely will be a young and vigorous PhD who will do all the experiments, while the initiating physicist serves as chairman of the board. Exaggerated as this picture may be, it has some familiar elements. Many physicists have spent large amounts of time on such arrangements. What is more, as long as physics needs substantial support from public funds, there must be physicists who are willing to devote their time and effort to explain the legitimate needs of physics to those who control public funds.

Conclusions

It would be sensible at this point to ask whether or not one can draw any conclusions from what has been said so far. I believe that in spite of the ever-changing and increasingly complex nature of physics, certain facts stand out and some morals can be drawn.

There is, first of all, the technical aspect of physics. It is impossible to do actual scientific work without mastering the necessary techniques. This is true for the research experimentalist, for the research theorist, and for the applied scientist; and it is equally true for the beginning student. A student must continually test his mastery of the techniques by doing problems, working out specific examples, filling in details. It is perfectly possible to say interesting things *about* physics, about the relation of physics to other aspects of culture, without explicit use of the techniques. It may be even proper to teach such material in liberal arts courses in physics. But the material in question is *not* physics. Physics on any level requires the ability to manipulate the techniques appropriate to that level. I do not mean to say that technical ability alone makes a physicist; I do mean to say that the absence of technical skills is a major, perhaps even a fatal, handicap in the pursuit of a scientific career. Thus, the first general conclusion is that prospective scientists should obtain and *maintain* sufficient technical competency to enable them to work independently.

The second general conclusion concerns the kind of problem one picks. Since, in fact, nobody knows for sure what problems or approaches may prove significant, every person possessing confidence in his own technical ability should be urged to make his own choice. The criterion for a good problem for a given person is his personal commitment to the problem; he must find it interesting and challenging. Of course, if a person goes off on his own, he must be prepared for difficulties. If his approach is unusual, not in the general pattern, his work may well be severely criticized, or ignored. In addition, one needs to recognize that new ideas and insights do not come with the clarity and precision of a finished, polished piece of work. More often one progresses painfully and slowly from mistake to misconception to understanding. To continue in spite of one's own mistakes and the criticisms of others takes persistence and courage—persistence to continue to work, courage to maintain or change the approach and to speculate and make mistakes. But then some of the great advances in physics followed right on the heels of serious mistakes. Bohr once said, "An expert in a field is a person who has made all possible mistakes in that field." That should give everybody courage.

The essential features of physics can only be appreciated through actual participation. At times one may be confused, bewildered, annoyed. Nothing makes sense, nothing seems to fit in a coherent scheme. One is embroiled in a maze of contradictions and paradoxes. The beauty and fascination of physics lies precisely in the gain of understanding and the glimpses of insight which one occasionally achieves after hard work. This is the reward of the student who finally can do a problem, of the teacher who hits upon a lucid and simple explanation, of the researcher who at last knows how to describe a phenomenon. And this must be the ultimate justification for doing physics; not the prestige attached to research, not the acclaim of students or colleagues, not the passing of a course, but the pleasure and joy experienced in the difficult and painful process of actual understanding. And in this process it does not really matter whether the problem is large or small, old or new, fashionable or not, whether it is acclaimed or ignored, for, as Lessing has said, "The search for the truth is more precious than its possession."