The Case Against The Nuclear Atom

Preface

One of the first things that a student in science or engineering acquires at the beginning of his college career is a sublime confidence in the objectivity of the scientific method and the unimpeachable status of the results thereof, along with a rather critical and condescending attitude toward other fields of learning which operate on a less exact basis. I still have a very vivid recollection of the amusement with which my classmates and I looked upon a statement in our economics textbook wherein the author commented on the theory of wages which he had just expounded at great length. This statement admitted that the theory did not produce the right results, but the author went on to say that he could not think of any better explanation, and consequently this one must be right anyway. Certainly, we students told ourselves, it was a pleasure to be identified with a branch of knowledge in which conclusions are reached by logical and mathematical processes rather than by any such ridiculous reasoning as this.

But those of us who have subsequently had occasion to leave the beaten path in the course of research work of one kind or another have been thoroughly disillusioned on this score. In spite of the high ideals to which the scientific world subscribes in theory, today's best guess is just as firmly enthroned in the field of science as it is in economics or any other of the less "exact" branches of knowledge, and the extent to which general acceptance is taken as the equivalent of proof in present-day scientific practice is nothing short of astounding. It is true that the areas in which the facts have been positively and unequivocally established are much larger in science than in these other fields, but outside of these fully explored areas the scientist is just as reluctant to admit ignorance as his counterparts in other disciplines, and just as prone to present his opinion or that of the "authorities" in his field as positive knowledge. There is, in fact, a very general tendency to elevate currently popular scientific theories and assumptions to the status of incontestable articles of faith whose validity must not be questioned, and the path of the innovator who dares to take issue with these cherished doctrines is thorny indeed.

The most serious aspect of this policy is that it tends to perpetuate basic errors when they are once made. Inevitably the theorists will take a wrong turn sooner or later, and present practice sets up an almost impassible roadblock in the way of getting back on the right track. This situation is greatly aggravated by what some observers have called the "epicyclical" character of much of present-day physical theory. When a theory encounters difficulties of a serious nature, it is no longer fashionable to abandon it, as would have been done in an earlier era. The present practice is to "save" the theory by adding the equivalent of one of the epicycles of Ptolemaic astronomy. Then when further trouble develops another epicycle is added, and so on. Each addition not only buries the errors of the original theory that much deeper and makes them that much harder to deal with, but also puts the originator of a new and better theory in a position where he cannot isolate the primary issue and meet it squarely; he must contend with all of the epicycles at the same time, however irrelevant they may actually be.

One of the most "epicyclical" of all physical theories is the nuclear theory of the atom. I am continually coming into conflict with this theory in my work, and while it has not been difficult to demonstrate the shortcomings of this theory in the particular applications with which I have been concerned, the theory and its coterie of epicycles are so firmly embedded in so much of present-day scientific thought that even the most glaring deficiencies make little impression on the general standing of the theory as long as they are exposed one by one in their separate areas. The usual reaction to a demonstration of the failure of the theory in any specific application is quite reminiscent of the attitude of the author of the economics textbook. "Perhaps I will have to admit that the theory gives the wrong answers in the particular case under consideration," the physicist says, "but it must be correct as a general proposition anyway, because everyone who knows anything about science accepts it." In view of this prevailing attitude which makes it impossible to deal with the situation on an item by item basis, it has seemed necessary to undertake a critical appraisal of the structure as a whole, to show how utterly untenable the cntire theory becomes when it is examined in the light of the immense amount of experimental knowledge now at our command. As the facts brought out in this work demonstrate, there never was any adequate experimental basis for the theory in the first place--the originators simply jumped to conclusions without considering the possible alternative explanations of the results of their experiments--and the advance of knowledge in the intervening half-century has completely destroyed the support which the theory originally derived from the scientific ideas and beliefs prevailing at the time it was originated. The conclusions of this work will no doubt be extremely distasteful to those who have been so confident of the validity of their atomic theory for so many years, but the facts are clear and unmistakable once anyone takes a good look at them. This situation must be faced eventually, and the longer the reckoning is postponed the greater the cost. However painful the necessary readjustment of thinking may be, the sooner it is accomplished the sooner it will be possible to get some tangible benefits out of the tremendous amount of time, money and effort that are now being wasted in futile attempts to find answers to meaningless problems and to establish the nature and properties of non-existent particles and forces.

D. B. Larson, August 1962

Chapter I
Introduction

A familiar American aphorism that has been attributed to practically everyone from Abraham Lincoln to Will Rogers asserts that "It's not what we don't know that hurts us, it's what we do know that isn't so." Like many another statement ostensibly uttered in a spirit of jest, this one contains a very large element of truth, and nowhere is that truth more evident than in the field of scientific theory.

In retrospect it is easy to recognize many glaring examples, and from the vantage point afforded us by the labors of the intervening centuries we are rather prone to underestimate the intellectual abilities of those who formulated and those who accepted these ideas that are now so thoroughly discredited. We smile indulgently at the egocentric astronomers of ancient Greece and the Arab countries who made man the focus of the physical universe and set up theories wherein the whole universe revolved around the tiny planet which the human race inhabits, but we are inclined to forget that the Ptolemaic theories of the universe met all of the demands upon them for more than a thousand years: a record that few of our modern theories are likely to equal. Then again, our present-day textbooks refer to the phlogiston theory in such terms as "a false, almost ludicrous, hypothesis," but they fail to bring out the fact that it is ludicrous only in the light of present-day knowledge; in the terms of reference provided by contemporary scientific knowledge it was a plausible and quite consistent explanation of the phenomena to which it applied, and it was accepted by the leading scientists of the era: such men as Priestley, Scheele, and Cavendish, whose intellectual stature does not suffer by comparison with that of the leaders of modern science. Much the same can be said about the caloric theory, the theory of the ether, and dozens of similar, though perhaps less striking, examples.

It might logically be expected that the principle of "once bit, twice shy" would apply in this case, and that the disastrous fate of so many presumably firmly-established scientific theories of the past would have a salutary effect in the way of discouraging over-confidence in the currently fashionable theories and concepts of science, but oddly enough, this is not true. If anything, present-day scientists are more cocksure than ever before. To be sure, they admit the existence of contradictions and weaknesses in existing theory, and they concede, at least in principle, that changes must take place, perhaps "radical changes," but almost to a man they stoutly contend that these changes must not alter the general framework of currently accepted theory; that they must be extensions or revisions of present-day ideas, not replacements for them. Here are some expressions of the prevailing viewpoint: From Pascual Jordan, "The author, therefore, is convinced that the new conceptions must be considered conclusive..."[1] From N. F. Mott, "...it now appears that we have in quantum mechanics a body of knowledge which in its proper field is likely to last just as long... as scientists and engineers have a place in civilized communities."[2] From Werner Heisenberg, "...we must assume that even the less palatable features of the laws of quantum mechanics will remain integral parts of theoretical science."<[3] From George Gamow, "In my opinion and in the opinion of many other theoretical physicists, the uncertainty principle will stand its ground indefinitely."[4] A. J. Hymans sums up the situation in these words, "The conventional view at the moment appears to be that the state of affairs revealed by Quantum Mechanics is final and ultimate."[5]

When viewed in the perspective of history this is a curious attitude. It is true, of course, that the areas in which knowledge is essentially complete and final are gradually expanding, and it is not unreasonable to envision the day when these completely-defined areas will embrace all or practically all of the physical universe. Some observers disagree, contending that the universe is qualitatively infinite, and that a complete understanding can never be attained, but even if we accept the more optimistic hypothesis that such an understanding is possible, it is obvious that we are still far from it. Consider the situation in elementary particle physics, for instance. As Heisenberg points out, "It is obvious that at the present state of our knowledge it would be hopeless to try to find the correct theory of the elementary particles,"[6] and it is freely conceded that we cannot even formulate the problem, to say nothing of finding the answer, since "we do not really know how to define an elementary particle."[7] H. Margenau says that the word "elementary" is now equivalent to perplexing, enigmatic, etc. Some theorists are beginning to doubt whether an adequate physical theory can *ever* be constructed. C. N. Yang, for example, was quoted in a recent news item as "expressing some doubts about the ability of the human brain in general, and his in particular, to accomplish this task."[8]

Against this background, the prevailing attitude that the currently popular basic theories of physical science are incontestable articles of faith not subject to challenge, an attitude which every innovator encounters, is nothing short of preposterous. There is every reason to believe that the historical pattern of scientific progress is still fully operative and that many, probably most, of the currently popular theories will ultimately fall as that progress continues. If a theory is solid and well-rounded it can resist attack successfully, and some of our modern theories will no doubt hold their own, but no theory should ever be exempt from the necessity of demonstrating its ability to meet whatever challenge is offered. Neither long years without question nor universal acceptance in present-day practice justifies any such exemption; on the contrary, theories of long standing are particularly vulnerable in that their original acceptance many years ago was necessarily based on information which, according to present standards, is very meager.

P. W. Bridgman once pointed out that there are important deficiencies in the type of examination to which scientific theories are usually subjected. The ordinary scientist does not normally feel that he can take the time to examine basic scientific concepts thoroughly. Many of the ideas to which he subscribes "have not been thought through carefully but are held in the comfortable belief... that some one must have examined them at some time."[9] This belief is not always justified, and even if such an examination has actually been made "at some time," this is not necessarily enough. Experimental knowledge is advancing so rapidly, in some areas at least, that it is not safe to place full trust in any theory unless it has had a thorough and critical examination *recently*.. According to Sir C. V. Raman, "The progress being made is so rapid that even the most eminent leaders of the science have had scarcely time to comprehend or understand, in its totality, the meaning of all the new knowledge. They can only just glimpse the general trends of progress and hope that they will live long enough to be able to understand it all a little better some day."[10]

One of the aspects of the "meaning of all the new knowledge" which is the most difficult to grasp, particularly under present-day conditions when all branches of science are so highly specialized, is the full effect of new discoveries on existing scientific thought, especially basic concepts and theories. It can easily happen, and indeed has happened, as will be demonstrated in the following pages, that new discoveries completely demolish the foundations of some accepted physical theory seemingly without anyone being aware of the fact, and the world of science moves along for the time being accepting both the new discovery and the totally incompatible idea of long standing.

In order to prevent this situation from getting completely out of hand it is obviously desirable to review the status of existing concepts and theories from time to time, paying special attention to the fields where the most rapid experimental progress is being made. An area that naturally suggests itself in this connection is the question of the structure of the atom. More than a half century has elapsed since Rutherford formulated his hypothesis of a nuclear atom: a period in which experimental science has made enormous strides. The physicist of today has at his command a huge store of knowledge of which Rutherford and his contemporaries had no inkling whatever. In the light of this situation it is no longer safe to assume that the conclusions reached in 1911 on the basis of the experimental knowledge which existed at that time–a very small fraction of that available today–are still valid, and it becomes pertinent to ask whether we might not arrive at some altogether different conclusions if we carried out a thorough reexamination of the subject with the benefit of all of the information now at our disposal. If the nuclear atom had been uniformly successful and if the present status of the theories of the atom and its structure were beyond reproach, such a question could be considered academic, but under existing conditions it can hardly be denied that it is very much to the point.

Actually we will find, when we examine present-day atomic theory carefully and critically, that it is a curious and contradictory mixture of half-century-old ideas with up-to-date conclusions based on the latest experimental evidence. We will find the textbook authors trustingly accepting the theories formulated by Rutherford and his contemporaries on the basis of the relatively few facts then available, and building a vast and complex theoretical structure on these highly imaginative basic concepts, then a page or a hundred pages later calmly and unblushingly stating conclusions derived from the immense body of experimental evidence now at hand which flatly contradict the previous statements and strike directly at the underpinnings of the basic theories so confidently expounded. We will find that the foundations of large and important portions of existing theory, originally thought to be secure against all attack, have been completely destroyed by the advances in the experimental field, leaving these sections of the theoretical structure suspended without any support; we will find assumption piled upon assumption in a manner unprecedented elsewhere in science; in short, we will find a theory that is inextricably enmeshed in difficulties of its own making, and hopelessly behind the times.

Perhaps the most surprising discovery that awaits anyone who turns the light of critical inquiry on the current theory of the atom is the extent to which the scientific profession has been willing to sacrifice logic and consistency in order to keep this cherished theory from being destroyed by the advance of knowledge. It is almost incredible that anyone would advance, in all seriousness, some of the arguments that are commonly presented in favor of the nuclear theory or particular aspects of that theory. A very common practice, for example, is to draw a conclusion favorable to the theory from an experiment or observation which actually has no relation at all to atomic theory. One contemporary physics textbook tells us, "...since the same value (of the ratio e/m) was obtained whatever gas was contained in the tube, the particle identified (the electron) was clearly a sub-atomic particle-that is, a constituent particle of atoms."[11] Now it is perfectly obvious that this experiment tells us nothing of the sort; it is evidence that all electrons are alike, but the further conclusion that they are constituents of atoms is wholly gratuitous. One might be inclined to think that the authors do not mean what they say, were it not for the fact that we find them saying exactly the same thing in slightly different words a few pages farther on, and we encounter the same statement over and over again in scientific literature.

Circular reasoning which bases the "proof" of a proposition on initial premises that assume the validity of the proposition is widespread. One text undertakes to prove the existence of ions in the solid state, and gives us a diagram of the NaCI crystal; then says, without further argument, "The only possible interpretation of such a structure... is that the atoms are charged and are therefore ions."[12] As it stands, this statement is utterly ridiculous. It can be justified only by first assuming the validity of the electrical theory of the cohesion of matter, and this, of course, is equivalent to assuming the point which is to be proved. Another text considers the relation of the positron to the atomic picture, and answers the question as to why the positron does not occur in nature as frequently as the electron in this manner, "The reason is that soon after a positron is created it disappears as a result of a collision with an electron."[13] In order to give this explanation any meaning at all, we have to assume that the universe is overpopulated with electrons to begin with: exactly the situation that the text is undertaking to explain.

Then again, we find in the textbooks a perfectly astounding number of assertions in support of the atomic theory which are completely without foundation, such as the following: "...later work, particularly that of H. Moseley in 1913... has shown that... the atomic number of an element represents the number of electrons outside the nucleus of the atom and also the number of protons in the nucleus of the same atom."[14] Even a minimum consideration of Moseley's work is sufficient to show that the only fact which he established is that the atomic number represents the number of units of some "fundamental quantity," as Moseley expressed it, which the atom contains, and to make it clear that this work does not give us the slightest indication as to the nature of that fundamental quantity. The identification of the atomic number with protons and electrons is pure hypothesis devised in an attempt to *explain* the findings of Moseley and other investigators, and the present-day tendency to twist the results of this and similar experimental work into a verification of the explanatory hypotheses is a barefaced distortion of the facts.

Surely the authors of the foregoing, and a great many other statements of a similarly indefensible character contained in modern textbooks, a considerable number of which will be discussed in subsequent chapters, know better. About the only possible explanation that comes readily to mind is that they are so thoroughly convinced of the validity of the theory–"Everybody knows that matter consists of nuclei and electrons,"[15] says another textbook–that they consider it unnecessary to exercise any particular care with respect to the validity of the arguments that are advanced to support it. It is high time, therefore, that we strip away the veneer of unsupported assumptions and worthless "proofs," and subject the underlying structure of theory to a close enough scrutiny to determine just how sound it actually is.

As a background for the discussion which follows, it will be desirable to review briefly the history of the various steps which ultimately culminated in the currently accepted theory of the nuclear atom. Since the existence of atoms, as such, is not being questioned in this presentation, it will not be necessary to follow the long and checkered career of the atomic theory itself, and we can begin with the situation as it existed in the middle of the nineteenth century, by which time the work in connection with the development of the kinetic theory of gases had placed the atomic theory on a firm footing. In this era the atom was regarded much as it was envisioned by Democritus: a hard spherical bit of matter, the indivisible ultimate unit of physical reality.

Although the possible existence of some kind of an internal structure within the atom was a subject of speculation much earlier (Prout's Hypothesis, for instance, was advanced in 1815), the first experimental indication that the "billiard ball" atom might be an oversimplification came with the discovery of the electron and the determination of its major properties in the closing years of the nineteenth century. Here, for the first time, a particle smaller than an atom was observed, and although there was as yet no good reason to believe that the electron could be identified as matter, or as a constituent of matter, there were obvious possibilities in this connection which led to a great deal of discussion and speculation. But only a few years later radioactivity was discovered, and in the burst of experimental activity that followed, it was soon determined that one of the "rays" that originated from the radioactive disintegrations was a stream of electrons. Subsequently the alpha particles, which also emanated from the radioactive materials, were identified as positively-charged helium atoms.

Even before the positive identification of the alpha particles, Rutherford and Soddy had demonstrated that atoms of a radioactive element are transformed into atoms of some other element lower in the atomic scale, and when it was established that electrons and helium atoms are ejected from the original atom in the radioactive disintegration process, this naturally led to the conclusion that the atoms are constructed of such particles. This conclusion was all the more plausible because the existence of oppositely directed charges on these two "atomic building blocks" also furnished an indication of the nature of the force that holds the building blocks together. With such points in its favor, this concept of an atom constructed of positively and negatively charged particles was almost immediately accepted, and has never been seriously challenged since.

The next question, that of the way in which the constituent particles are arranged in the atom, was resolved to the satisfaction of the scientific world almost as quickly. For a brief period the Thomson atom, which has been compared to a plum pudding, with the electrons corresponding to the raisins, occupied the center of the stage, but Rutherford's experiments around 1911 showed that this concept was untenable. His results on the scattering of alpha particles showed conclusively that if the atom is constructed of electrons and positively charged particles, the latter must be concentrated in an extremely small region. He therefore postulated an atom roughly analogous to the solar system, with a minute positively charged nucleus, around which electrons are distributed in some manner in sufficient numbers to create an equal and opposite charge, thus making the atom as a whole electrically neutral. Disregarding details, this Rutherford atom of 1911 is still the "official" concept of atomic structure: the nuclear atom of the present day.

But while we can thus disregard details in taking a birds-eye view of the situation, the question as to details must be faced sooner or later, and this has proved to be full of difficulties. It was quickly recognized that the simple picture originally conceived was not capable of representing all of the known facts, and that the nucleus must contain something more than the positively-charged particles. The first hypothesis that was proposed as a means of meeting this situation was that some electrons existed in the atomic nucleus in addition to the extra-nuclear electrons originally postulated, and this was the accepted view for the next twenty years or so. There are, however, some very serious objections to the idea of electrons inside the nucleus, and the theorists gave a sigh of relief in 1932, when the discovery of the neutron supplied a new building block that could be substituted for the nuclear electron. Since 1932 the atomic nucleus has been assumed to consist of protons and neutrons in the appropriate proportions for each element and isotope.

In the meantime, even greater trouble was encountered with the orbital electrons in the outer regions of the nuclear atom. As soon as detailed calculations were made on the Rutherford atom, it became apparent that this atom was not stable and could not even maintain itself if undisturbed, to say nothing of surviving thermal collisions. Niels Bohr met this problem in an unprecedented way by boldly postulating that the atomic electrons do not follow the usual laws of physics, conforming instead to certain unique behavior characteristics of their own, which he defined to fit the existing situation. In spite of the wide latitude afforded by this chance to write his own physical laws, Bohr found his atom enmeshed in constantly growing difficulties, and it ultimately had to be abandoned, or at least modified beyond all recognition. The present "official" view of the atom, of which more will be said later, regards it as something which, as Heisenberg says, does not "exist objectively"[16] and is "in a way, only a symbol."[17]

The strange and tortuous path which the revisions of the original Bohr theory have taken has left the scientific world somewhat bewildered, and as matters now stand the physicists are strung out all along the line of development. At one end are the educators, particularly those teaching elementary physics, who present the Bohr atom in all of its pristine glory as if every feature of the atomic structure were known specifically and in detail. At the other end are the theorists of the Copenhagen school, who deny the reality of the "elementary particles" and even of the atom itself, and tell us that anything other than a mathematical picture of the atom is impossible; that "...the atom of modern physics... has no *immediate and direct* physical properties at all, i.e., every type of visual conception we might wish to design is, *eo ipso*, faulty."[18] Somewhere in between are the majority of the individual physicists, who realize that the advance of knowledge has destroyed the original Bohr theory, but are nevertheless unwilling to go along with the extreme views of the Copenhagen group and concede that the ultimate units of the physical world are nothing but mathematical phantoms.

In the subsequent pages it will be necessary to discuss matters relevant to each of these points of view at one time or another, but in such cases the particular theory involved will be specifically indicated, and it should be understood that wherever reference is made to the "nuclear theory of the atom" without special qualification, this represents the general concept on which the physicists of all schools of thought are currently agreed; that is, an atom which consists of a nucleus, composed of protons and neutrons and hence positively charged, and an outer structure composed of negatively-charged electrons distributed around the nucleus in some manner.

Before beginning an examination of the observations and conclusions upon which the concept of the nuclear atom rests, it will be helpful to consider the general question as to how the validity of such a concept can be proved. Since science recognizes the observed facts as the ultimate authority, this proof must be based on correlations with observed or measured facts, unless the item is itself something that can be observed or measured and thus proved directly. Two types of indirect proof are available, one of which rests upon the antecedents of the concept in question, the other on its consequences.

A scientific proposition may be proved by showing that it is a necessary and unavoidable consequence of certain positively established facts, or of some other proposition or propositions that have been proved previously. Alternatively, it may be proved by showing that its consequences are consistent with all of the pertinent facts. Since one can rarely, if ever, be sure that all of the pertinent facts are known, this latter type of proof must rely upon probability considerations, and in order to reduce the probability of a hidden conflict somewhere in the system to the point where it is negligible, it must be shown that the consequences of the proposition in question are consistent with the known facts in a *large number* of random cases throughout the area involved, *without exception*, and without the use of contrived methods of evading contradictions or inconsistencies.

Practical considerations necessitate a certain amount of relaxation of these rigorous standards of proof, since our factual knowledge is still far from complete and few scientific principles could qualify as true if we were to demand strict mathematical and logical compliance with the requirements set forth in the preceding paragraph. In order to establish any body of scientific knowledge at all, we must compromise to some extent and accept those propositions which are established beyond what we consider a reasonable doubt, even though we know that there is a possibility that these propositions may be overturned by future additions to scientific knowledge. Unfortunately, when it becomes necessary to open the door at all, there is always a temptation to open it still wider and, in particular, to accept certain hypotheses which do not even come close to meeting the requirements, simply because they appear to be the best explanations of the observed facts currently available. The distinction between fact and assumption thus becomes blurred, and there is a very definite tendency in present-day scientific practice to regard general acceptance as equivalent to proof. In undertaking a critical reexamination of currently accepted ideas it is, of course, essential to distinguish clearly between those items which have actual factual support and those which owe their present standing merely to general acceptance.

Another of the loose practices with which we will be particularly concerned in this discussion is that of interpreting evidence which *is consistent* with a particular hypothesis as *proof*of the validity of that hypothesis. Where only one explanation of a set of facts can be found on the basis of existing knowledge, we are justified, from a practical standpoint, in accepting this explanation as true, at least tentatively, even though we recognize that there may be some other explanation at present unknown. But where more than one possible explanation can be derived from existing knowledge, there is no justification for considering the observed facts as proof of any one of them. Furthermore, this situation is not materially altered if an explanation is consistent with *many* such facts, as long as alternative explanations are available for each of them, unless *all* of the requirements for a proof by the probability method can be met.

Both of these practices, that of accepting inconclusive evidence in lieu of proof and that of accepting today's best guess as the equivalent of an established fact, are so foreign to the spirit of scientific inquiry that unless one has had reason to make a critical examination of the situation it is hard to believe that they would be allowed to enter into scientific work to any significant extent. Actually, however, they are not only widespread, but they are symptomatic of a general change of attitude that has taken place in the scientific community in the current century. What this change amounts to is the subordination of all other considerations to the maintenance of the *status quo* in the field of basic theory.

We are taught that a scientific theory is valid only so long as it agrees with the facts derived from observation and measurement, and that when and if the time comes that a substantial body of new facts is discovered which cannot be reconciled with the theory, it must step aside in favor of something more adequate. Thus the Ptolemaic theory of the universe, the caloric theory of heat, and other once highly valued concepts of earlier days have faded from the picture. Thus, too, it can logically be expected that many of the theories of the present day will ultimately be superseded.

But now a new element has entered into the situation, and dislodgement of a firmly entrenched theory has become an almost impossible task, even when the theory is completely erroneous. The supporters of the older theories had to capitulate when the contradictory facts became too numerous, but the ingenious and resourceful modern theorist is no longer at the mercy of the facts. He has invented a whole new armament of novel weapons that can be used against any challenger. If only a single inconvenient fact has to be faced, the answer is an *ad hoc* assumption, tailor-made to remove the obstruction; if some established physical law stands in the way, the theorist simply postulates that the law does not apply to this particular situation; if the theory fails to solve a problem, all that is necessary is to proclaim a principle of impotence, according to which a solution to this problem is impossible, or alternatively, to assert that the problem has been solved "in principle" and that only the extraordinary mathematical complexity of the solution prevents getting answers that are applicable to specific cases.

The skeptic may be reluctant to accept results obtained by such means, but he has little on which to hang an objection, since these devices are of such a nature that they are inherently immune to attack. Besides which, there are not very many sincere skeptics to be found. The great majority of scientists go along willingly with the currently accepted basic theories and raise no inconvenient questions. It would probably be difficult to find even a handful who would question the validity of the nuclear theory itself. The quantum development of the basic theory brings out a few more dissenters. Some are inclined to echo Cornelius Lanczos' complaint that "strange and obscure principles are forced on us"[19]; others would second Erwin Schrodinger's fervent hope that we may find "something better than the mess of formulas which today surrounds our subject,"[20] but few are ready to discard any major portion of existing theory. Whenever the accepted theory arrives at a crisis, the alternatives are either to abandon the entire theoretical structure or to avoid the necessity for so doing by using one of these somewhat questionable recent inventions. In view of the extreme reluctance to abandon ideas of long standing, which is characteristic not only of the scientist, but also of the entire human race to which he belongs, there is little doubt as to which alternative will prevail.

It is obvious, however, that this situation is made to order for perpetuation of any error that may have been made in the formulation of a basic theory. Such an error will inevitably lead to a series of contradictions and inconsistencies as the development of the theory progresses. In earlier years the accumulation of a number of these contradictions would necessitate abandoning the theory, but today, when a wide variety of devices for evading the contradictions is at the command of the theorist, the erroneous basic theory can remain intact almost indefinitely. Under these circumstances, it would be nothing short of a miracle if all of the basic theories of the present day were sound and free from error. The analysis of the nuclear theory of the atom that will be made in the pages that follow will demonstrate that the Age of Miracles has not yet arrived. It will be shown that a serious mistake in interpretation of the observed facts was made in the initial formulation of this theory. As could be expected, the theory built upon this error was in conflict with established physical laws almost immediately. This conflict was brushed aside by postulating that the established laws did not apply, and the theory proceeded on its way, until it encountered another of the inevitable recalcitrant facts. This, in turn, was removed by the use of another of the ingenious modern techniques, permitting the theory to move on to the next crisis, and so on and on.

In the kaleidoscope of changing patterns during the course of this development, the basic nuclear theory has come to occupy a position as the one permanent element in the picture. Established principles may be repudiated, interpretations of observed facts may be altered beyond recognition, hypothetical forces and behavior characteristics may come and go, even physical reality itself may be questioned, but through it all the concept of the nuclear atom remains intact, simply because this is the one thing to which all else is subordinated. As matters stand, it is no wonder that the standards of proof have been relaxed; that our "delicacy of feeling with reference to such questions has been blunted," as Schroedinger puts it. Why worry about whether our arguments are sound and logical–the general attitude seems to be–since they are only perfunctory anyway? We know the answer before we start. This attitude reaches a fitting climax in the strange upside-down thinking of the author who solemnly assures us that "Quantum physics presents a strong case against traditional logic."[21]

In undertaking a critical reexamination of the validity of the nuclear theory it will be necessary to take an altogether different approach. Since we will be looking for an answer, rather than working toward an answer already defined in advance, the prevailing carefree policy of accepting every favorable argument at face value without anything more than a casual scrutiny can no longer be tolerated; it will be necessary to demand strict conformity with logical principles and reasonably rigid standards of proof. We can no longer assume that because an idea is generally accepted that it is necessarily true, nor that an explanation of an observed fact is necessarily the *correct* explanation. On the contrary, the fact that the accepted theories are sheltered behind a series of assumptions and postulates which by their very nature lend themselves readily to abuse, but cannot be attacked directly, makes it all the more imperative that we hold these theories to a strict accounting wherever they are exposed and *can* be subjected to the usual tests that distinguish truth from falsity.

Chapter II
The Nucleus

The primary basis for the present acceptance of the theory of the nuclear atom is the practically universal belief that the existence of an atomic nucleus was definitely proved by the experiments of Rutherford in 1911 and subsequent years. Prior to that time it had been believed that a solid material was just what the name implies in common parlance: a continuous and essentially impenetrable substance. But when Rutherford directed alpha particles against a thin metallic plate, he found, contrary to all expectation, that most of these particles passed directly through the plate just as if there were no obstacle in the way at all, and that the majority of those which were deflected changed their direction by only a relatively small angle. Only a very small proportion experienced major direction changes. By mathematical analysis of his results Rutherford was able to determine the approximate size of the region in which major resistance was encountered, and as a result of his work he arrived at the conclusion that practically all of the mass of the atom is concentrated in an extremely small volume, and that the remainder of the region which the atom occupies in the solid state is mostly open space.

Rutherford's experiments have been repeated with additional precision by other investigators, and it appears safe to say that the experimental facts have been firmly established. It must therefore be conceded that Rutherford's first conclusion, as expressed in the foregoing paragraph, is entirely consistent with the observed facts. But here we encounter an example of a surprisingly prevalent feature of present-day physical science: a curious failure to explore possible alternatives. Time and again in the course of the investigation from which this present discussion originated, critical examination of a commonly accepted idea or conclusion has disclosed that it is only one of the possible explanations of the observed facts, and that there are other, sometimes many other, explanations which have an equally good, if not better, claim to acceptance, but which, so far as the records reveal, have never been explored.

In this case the observed facts are entirely consistent with the hypothesis that most of the mass of the atom is concentrated in a very small region, to be sure, but they are equally consistent with the hypothesis that *all* of the mass is concentrated in this region; in other words, that this is the atom, not the nucleus of the atom. This alternative conclusion gives us a complete and consistent explanation of the results of Rutherford's experiments in terms of existing knowledge. On this basis there is no need to postulate the existence of an atomic nucleus, and Occam's Principle, one of the sound commonsense rules of science, tells us that we should not make unnecessary hypotheses. All of the facts disclosed by the experiments are entirely in harmony with the conclusion that they merely establish the true size of the atom, indicating that it is very much smaller than was previously believed.

Why, then, we may ask, was the hypothesis of a nucleus accepted so readily and so uncritically? The answer is that this is just one of the many cases in science where an assumption seems so plausible on casual consideration that no one takes the trouble to examine it carefully. In the earliest attempts at explaining the structure of matter, a solid was visualized as a continuous body of material substance. Later, when the atomic theory was proposed, the atomists made the very natural assumption that the apparent continuity of the solid is due to the fact that the atoms of a solid are in contact, whereas the much different properties of a gas result from the presence of empty space between the atoms, which leaves them free to move. There is no physical evidence to support this assumption, but when no one questions it, such an assumption acquires the standing of an axiom. ". . . In a liquid or solid," says Slater, "we may merely assume that the atoms fill up most of the space,"[22] and it is indicative of the general relaxation of critical standards in current practice that he characterizes this assumption as "direct evidence" of the atomic dimensions.

In such an atmosphere, where everyone assumes that the dimensions of the atom are known, the discovery that all or nearly all of the mass is concentrated in a very small volume in the center of the region occupied by the atom logically leads to just the kind of a conclusion that Rutherford reached: the conclusion that an atomic nucleus has been located. On the other hand, if the true situation is recognized, and it is realized that the previous ideas as to the size of the atom are pure assumptions and that in reality the atomic dimensions were completely unknown prior to Rutherford's experiments, the only legitimate conclusion that can be drawn from these experiments is that they have determined the size of the atom.

It is frequently stated that the dimensions of the atom can be determined by observations on gases, interpreted with the aid of the kinetic theory, utilizing such phenomena as viscosity which are related to the mean free path of the molecules. But when this "evidence" is examined carefully, it is apparent that all we actually find out from this source is that the minimum inter-atomic distance in the gaseous state is comparable with that which exists in the condensed states. This gets us right back to the question as to the significance of the inter-atomic distance in the solid, a quantity which can be readily measured under a wide range of conditions by the use of modern techniques. In Rutherford's era there seemed to be good reason to believe that this distance was a relatively constant quantity, and even today we find statements in our textbooks such as the following: "Each kind of atom maintains, nevertheless, a rather well-defined volume, which shrinks hardly at all even under the influence of strong pressures."[23]

If statements of this kind are indicative of the general thinking of the profession, it is no wonder that the physicists have been unable to break away from the pattern of 1911 atomic theory. An enormous amount of experimental work in recent years has established just the opposite; it has been demonstrated beyond question that a specific kind of atom may have a wide range of "atomic volumes," if we use this term to designate the volume determined by the inter-atomic distance, as in the foregoing quotation. Furthermore, this recent work shows that instead of "shrinking hardly at all" under pressure, all solid substances undergo very substantial decreases in volume under high pressures. Cesium, for example, loses nearly two thirds of its original volume under 100,000 atm., potassium more than half. Most substances are much less compressible than these alkali metals, but if sufficient pressure is applied they behave similarly. Metals such as iron, copper, zinc, silver, cadmium, and tin have been reduced to the neighborhood of half their original volumes by pressures around 3 to 4 million atmospheres, and there is no indication that we are approaching any kind of a limit even at the extreme upper end of the experimental pressure range.

This observed compressibility pattern is very difficult to reconcile with current atomic concepts. It is completely at odds with Bohr's original ideas. The sizes of the orbits in the Bohr atom are fixed by quantum considerations and no intermediate orbits are permitted. But if the atoms are in contact, as assumed, then the contraction under pressure means that the orbits (at least the outer orbits, if there are several) are assuming a continuous succession of values. This is a direct contradiction of the basic postulates of the theory. A few decades ago it probably would have been assumed, in order to save the theory, that this continuous decrease in orbital size is a statistical effect, and that the individual orbits maintain one or another of the permitted values, but the latitude for *ad hoc* assumptions narrows as experimental knowledge increases, and such an assumption is untenable as matters now stand.

A direct comparison of this kind with the current atomic theories which have been developed through extension and modification of Bohr's original ideas is more difficult, since these theories have been dematerialized to the point where they are essentially nothing but mathematical abstractions and it is hard to come to grips with anything specific. As it happens, however, the one major feature of the original theory that has been maintained intact is the quantization–whatever other changes may have been made, it has always remained a *quantum* theory-and it is precisely this point at which the conflict with the experimental compressibility occurs. These compressibilities are therefore incompatible with current atomic concepts as well as with the original Bohr atom.

On the other hand, if we accept the straightforward interpretation of Rutherford's experiments and conclude that they establish the true size of the atom as the size now assigned to the nucleus, it is evident that the inter-atomic distance merely represents a point of equilibrium at which the forces of attraction and forces of repulsion between the atoms are equal. On this basis it follows that application of an external pressure will move this equilibrium point inward, and also that the amount of the displacement will be a function of the applied pressure. Thus we arrive easily and naturally at a theoretical explanation of the exact situation which we observe experimentally.

We also find from compressibility measurements on individual crystals that the compression in any one dimension is largely independent of that in the other two. This information fits in naturally and logically with the concept of an equilibrium distance between atoms, but if this static equilibrium is replaced by a dynamic equilibrium of the type required by the nuclear theories, an explanation of the observed characteristics of the response to linear compression becomes very difficult.

Next we note that the sizes of the various atoms as determined from the inter-atomic distances are entirely inconsistent with the atomic magnitudes. Whatever the structure of the atom may be, there seems to be ample evidence to show that the atomic weight is a measure of the number of units of the primary atomic component (whatever it is), which this particular atom contains. There likewise appears to be adequate evidence to support the conclusion that the units of this primary component are all alike, or at least very nearly alike. Current theory visualizes two kinds of primary units, protons and neutrons, but these entities are very much alike and interchangeable, and each proton is supposed to be accompanied by a single electron. Irrespective of the theoretical viewpoint from which the subject is approached, whether we ascribe the volume to the primary component itself or to some secondary component such as the hypothetical electrons, the true volume of the atom should be at least roughly proportional to the atomic weight, since we can hardly take the stand that units of the same kind come in assorted sizes. But the "atomic volumes" as calculated from the inter-atomic distances or from the observed densities are grotesquely out of harmony with the corresponding atomic weights. For example, the sodium atom, which has less than one-eighth of the atomic weight of the gold atom, occupies more than double the volume of the latter. If the supposed nucleus is actually the atom, everything falls into line, since the experimental information indicates that the volume of the "nucleus" is directly proportional to the atomic weight, as the volume of the atom should be.

Furthermore, if we accept the view that the atoms are in contact in the solid state, as present theories contend, we are forced to the rather bizarre conclusion that the sizes and even the shapes of the atoms are extremely variable. On this basis the carbon atom, for example, is spherical in the diamond crystal, with an inter-atomic distance of 1.54 A, but in a graphite crystal the same atom stretches out to 3.40 A in one dimension, while the inter-atomic distances in the other two dimensions remain approximately the same as in the diamond. A variability of this kind in the atom is not inherently impossible, but it is, to say the least, implausible. The alternative interpretation of Rutherford's findings, which puts the atom where the so-called nucleus is supposed to be, does not require any variability in the atom; in this case the variability is in the nature of the relationship between adjoining atoms.

The important point here is that there is independent evidence of the existence of variability in the inter-atomic relationships. Differences in valence such as those which we encounter in the various oxides of nitrogen, for example, show that there are differences in the manner in which the oxygen and nitrogen forces interact. This alternative interpretation therefore requires no *ad hoc* assumption; the variability in the inter-atomic distances is readily explained, at least qualitatively, by facts already established. On the other hand, there is no independent evidence of any variability in the atom itself of the kind which is required in order to reconcile the observed facts with the nuclear theory. No matter how many different forms of a solid may exist, when we get the substance into the gaseous state and raise the temperature high enough to dissociate it into atoms, we find the atoms of any particular element all alike (except for isotopic differences, which do not enter into this picture). The nuclear theory therefore has to *assume* an atomic variability of which we have no observational evidence.

This is the same situation that we find all along the line. If we make no unnecessary assumption to start with, and interpret Rutherford's findings simply as a determination of the size of the atom, then we can explain all of our subsequent observations in terms of facts already known from independent sources. But if we make the wholly unnecessary assumption of the existence of a nuclear structure to explain Rutherford's discoveries, then wherever we go we have to set up additional *ad hoc* assumptions to reconcile the nuclear theory with the observed facts. This is the old familiar pattern that develops whenever we stray from the truth, whether it be deliberately or unconsciously, and it brands as false the whole theoretical fabric to which it applies. The available factual evidence lends no support to the hypothesis that the inter-atomic distance in the solid state is a reflection of the size of the atom, as the nuclear theory contends; on the contrary, this evidence is wholly in accord with the conclusion that the so-called nucleus is in reality the atom itself: the same conclusion which is the most logical interpretation of the results of Rutherford's experiments.

The importance of this conclusion, so far as the status of the nuclear theory is concerned, can hardly be overemphasized. As will be brought out in the subsequent discussion, the basic elements of the theory, without which it is hopelessly lost, rest entirely on the belief that the existence of a nucleus was *definitely proved* by Rutherford's experiments. The nature of the assumptions involved in setting up these basic elements of the theory is such that even a reasonable doubt as to the validity of Rutherford's conclusion is sufficient to eliminate all justification for making these assumptions, and by so doing, to destroy the theory completely. But the facts brought out in the foregoing paragraphs show that there is much more than a reasonable doubt. There is actually definite evidence that Rutherford's hypothesis is wrong. His work, with the subsequent corroboration of his experimental findings, provided ample proof of the existence of something massive in the center of the region occupied by the atom, to be sure, but neither his work nor any other has produced any evidence to corroborate the existence of the hypothetical outer parts of the atom. On the contrary, a whole series of facts points to the conclusion that there are no such outer parts, and that the massive "something" which Rutherford called the nucleus is actually the atom.

It seems almost incredible that a basic concept such as that of the atomic nucleus could have slipped into the structure of scientific thought without any critical examination of its claim to validity, but the literature of the Rutherford era shows that this is just what happened. The accuracy of Rutherford's experimental work was checked in the usual manner by repeating his experiments under carefully-controlled conditions, and it also took a little time for the scientific community to become accustomed to the idea that a solid substance is composed mostly of empty space, but so far as the records show, this is as far as the scrutiny ever went. No one, either then or since, seems to have given any consideration to the next point that should have been examined after Rutherford's experimental results were verified: the question as to whether the conclusions which he drew from these experimental results were justified.

The scientific profession is quite willing to concede, in principle, the need for the kind of a periodic reexamination of its basic concepts that was stressed in the preceding chapter. Louis de Broglie, for example, emphasizes the point that the history of science shows that "it is proper to submit periodically to a very searching examination, principles that we have come to assume without any more discussion."[24] But there is no indication that he ever applied such a "searching examination" to the concept of a nucleus. Similarly we find von Weizsaecker speaking of "making a critical examination of the foundations"[25] of atomic physics, but he starts this "critical examination" with the assumption that the existence of a nucleus was proved by Rutherford's work. These searching and critical examinations have simply failed to get down to bedrock.

One of the strangest aspects of the whole situation is that practically every elementary chemistry textbook published in the last halfcentury contains a diagram of the sodium chloride crystal in which the sodium and chlorine atoms are pictured as occupying relatively small regions at alternate corners of the unit cube, with nothing but empty space in the remainder of the structure. This is just exactly Ihe picture which emerges when we make a careful and critical examination of all of the available evidence, along the lines discussed in the preceding paragraphs. Here is one of those ironies so often encountered in life. The answer has been right in front of us all the time, but no one has been able to rise far enough out of the traditional channels of thought to be able to see that it is the answer.

There are, of course, many other items of more recent origin which are now regarded as evidence in favor of the nuclear hypothesis, and it will be necessary to consider these items individually and in some detail in the subsequent pages. In analyzing them, however, it should be kept in mind that the accepted ideas in these areas have been formulated in an atmosphere dictated by the prevailing impression that the existence of the nuclear atom was already proved by Rutherford's work, and the general attitude toward other developments has largely been determined by this common understanding as to the firmly established status of the nuclear concept. The physicists have not considered it necessaly for any of these new items to furnish a proof of the validity of the nuclear theory, since this would merely duplicate something which presumably had already been done; all that has been required is that the new information should be consistent with the nuclear hypothesis, and even this rather modest requirement has been waived in important instances, notably in connection with the theories of the structure of the nucleus, which will be the next subject of discussion. Now that it is evident that the existence of an atomic nucleus was not proved by Rutherford's work, and that the massive "something" which he located is actually the atom itself, it is clearly appropriate to examine the subsequent developments in this new setting, to see just what difference this will make in the general picture.

According to currently accepted ideas, the atomic nucleus consists of a number of protons equal to the atomic number of the particular element, and enough neutrons to account for the remainder of the atomic weight. Even without the complication of having to consider details this hypothetical structure immediately encounters two formidable obstacles. First, the protons are, by definition, positively charged hydrogen atoms, and at such short distances they will exert very powerful repulsive forces on each other. Existing knowledge therefore tells us that such a structure is impossible; if it were ever formed it would disintegrate with explosive violence. Second, experimental evidence indicates that the neutron is unstable in the terrestrial environment, with a half-life of only about 13 minutes. On the basis of existing knowledge, therefore, the neutron cannot be a constituent of a stable atom.

Nevertheless, if we have positive knowledge that atomic nuclei *do* exist and that they are composed of protons and neutrons, it then necessarily follows that existing knowledge of the behavior of these particles is incomplete and that they have some different behavior characteristics under nuclear conditions. In the belief, therefore, that the existence of the nucleus was *proved* by Rutherford's findings, two *ad hoc* assumptions have been made to reconcile the contradictory items: (1) that some kind of a "nuclear force" exists in opposition to the force of repulsion that would otherwise destroy the hypothetical structure, and (2) that the normally unstable neutron is stable in the nuclear environment.

Such unsupported assumptions should not be lightly made. Their use is a legitimate scientific device and occasionally one of them serves a very worthwhile purpose. The discovery of the neutrino is a case in point. But the employment of such assumptions is uncomfortably close to the ancient custom of attributing all unexplained events to the actions of spirits and demons, and all too often it simply diverts attention from the real problem and impedes the march of scientific progress, just as any other appeal to the supernatural is likely to do. Certainly the piling of one of these unsupported assumptions on top of another cannot be justified under any circumstances, and this is just exactly the situation that the proton-neutron theory is in, now that it has been shown that Rutherford's experiments did not prove the existence of a nucleus. Without definite and positive proof that a nucleus exists, there is no basis on which we can even talk about nucleons, in the face of the known facts which specifically contradict the nuclear structure.

It will no doubt be contended that an important and generally accepted concept of long standing deserves something more than this summary dismissal, and that some further consideration of the matter is in order. But there is nothing further to be said. Even if the positive evidence that the so-called nucleus is actually the atom did not exist, the mere fact that the assumption of the existence of a nucleus is *unnecessary* is in itself sufficient to eliminate all justification for the drastic steps that have to be taken to put protons and neutrons into a nucleus. We can justify *one* unsupported assumption, such as the original postulate of the existence of the neutrino, if the result of this one assumption is to bring everything else into line with observed facts and established physical principles, but even in these days when the latitude for speculation and hypothesis is extremely wide, we cannot justify making assumptions that are completely at odds with established facts and principles merely to enable retaining another unsupported assumption.

The hypothetical nucleus was already in an extremely precarious condition, and physicists have realized that unless an answer could be found soon to the oft repeated question, "What holds the nucleus together?", the proton-neutron concept was likely to collapse of its own weight. The only thing that has kept it alive in the face of the complete lack of progress toward an answer to this crucial question is the hitherto firm conviction that the existence of a nucleus of some kind is a positively established fact. Now that it has been shown that the supposed proof of this point is non-existent, the last vestige of justification for the proton-neutron concept is swept away, and further comment is superfluous.

Let us then turn to a consideration of some of the other aspects of the nuclear theory. In this connection it should be pointed out that the assumptions already discussed are by no means the only ones involved in the foundations of the theory. Actually this theory rests upon a long chain of assumptions: an extraordinary product of scientific imagination which is remarkable not only because of the unprecedented number of assumptions that have been called into service in the construction of this one theory, but also because of the drastic nature of some of the assumptions, which postulate behavior characteristics totally unlike anything ever encountered elsewhere in the physical world. The following list of the major assumptions of the theory, which has been prepared to show the relevance of the various subjects that will be discussed herein, illustrates this point, particularly when it is realized that dozens of additional assumptions have been made in working out the details of the theory. In order to arrive at the currently popular atomic picture it is necessary to assume:

(1) that the atom is constructed of "parts."
(2) that the parts are known sub-atomic particles.
(3) that these parts are arranged in a nuclear structure.
(4) that the orbital components are electrons.
(5) that the orbital electrons do not follow the usual physical laws.
(6) that the nucleus is composed of protons and neutrons.
(7) that there is an unknown "nuclear force" holding the nucleus together.
(8) that there is an unknown factor which makes the neutron stable inside the nucleus.

Thus far, this discussion has shown that the "proof" of assumption (3) hitherto relied upon is invalid, and that without a definite proof of (3), assumptions (7) and (8) are completely unjustified, which leaves assumption (6) without a leg to stand on. Let us next turn our attention to assumptions (4) and (5), which deal with electrons.

Chapter III
The Electrons

It is quite unlikely that the acceptance of Rutherford's nuclear hypothesis would have been so immediate and so uncritical had it not been for the fact that the ground was already prepared for such a hypothesis by the discovery of the electron and of radioactivity, which indicated (1) that particles smaller than atoms exist, and (2) that such particles are ejected by atoms in the radioactive disintegration process. The inference that the atom is a composite structure built up of these sub-atomic entities follows naturally and logically; hence the question which Rutherford and his contemporaries were trying to answer was not the general question of atomic composition, the answer to which they considered self-evident, but the question as to *how* the electrons and other sub-atomic particles are arranged in the atom.

However, the natural and logical inference on first consideration does not always stand up under more deliberate and thorough analysis, and so it has been in this case. The original argument based on the known characteristics of radioactivity may be summarized as follows:

(a) Under certain conditions atoms disintegrate.
(b) Electrons are found among the disintegration products.
(c) Therefore, electrons are constituents of atoms.

At first glance this argument may seem sound, and in the formative years of the nuclear hypothesis it was accepted without question. Even today it is still orthodox doctrine. But the true status of the argument can be brought out clearly by stating the analogous argument concerning the photon.

(a) Under certain conditions atoms disintegrate.
(b) Photons are found among the disintegration products.
(c) Nevertheless, photons are not constituents of atoms.

Here we find that on the basis of exactly the same evidence, the physicist arrives at diametrically opposite conclusions. Because preconceived ideas concerning the electron suggest that it *could* be an atomic constituent, the evidence from the disintegrations is accepted as proof that it is, whereas similar preconceived ideas concerning the photon suggest that it could not be an atomic constituent, and exactly the same evidence is therefore taken to mean that the photon was created in the process. Actually, of course, the physical evidence does not distinguish between these alternatives, nor does it preclude the possibility that some other explanation may be correct. What the evidence shows is that the electron either

(a) was a constituent of the atom, or

(b) was preexisting within, but not a part of, the atom, or
(c) was derived from the surrounding space, or
(d) was created in the disintegration process, or
(e) originated from some combination of the foregoing, or
(f) had some other origin consistent with the evidence.

At the time the nuclear atom was originally conceived, the existing physical knowledge was not extensive enough to permit visualizing these alternatives that have been listed. The idea that electrons might be created in some physical process, for instance, was probably altogether inconceivable to Thomson or to Rutherford. But today this is commonplace. Such creation is currently being observed in a great variety of processes, ranging all the way from the production of a single electron-positron pair by an energetic photon to the production of a shower of millions of particles by a cosmic ray primary. This new information has made it apparent that the emission of electrons from radioactive material does not necessarily have the significance which was originally attached to it. Current thinking favors the creation hypothesis as the best explanation of this phenomenon, and the textbooks are slowly and reluctantly trying to incorporate this new viewpoint. Kaplan tells us, for example, "... it must be concluded that in beta radioactivity, the electron is created in the act of emission."[26]

But the same textbook which gives us, on page 154, this conclusion based on up-to-date evidence, still repeats on page 39 the completely contradictory nineteenth-century judgment that the emission of electrons by matter is "convincing evidence that electrons exist as such inside atoms," and it goes on to present the atomic theory based largely on that outmoded idea as if it were fully in accord with present-day factual knowledge. This is not a peculiarity of this particular text. Any other modern text which we might select gives us essentially the same contradictory picture. For instance, another text book tells us, "The disintegration experiments (which indicated emission of protons by atoms) provided definite proof that protons are components of nuclei of all elements."[27] Then on the very next page the text goes on to say, "It might be argued that if an electron can be emitted from a nucleus, it must have been there before," but in spite of the fact that this is exactly the same argument which is characterized as "definite proof" on the preceding page, it is here dismissed with the statement, "This solution... could not, however, be upheld." Here is a graphic example of what was meant in the introductory chapter when the present-day atomic theory was described as a curious and contradictory mixture of half-century old ideas with up-to-date conclusions. Any theory which is so confused that the textbook authors can "prove" a basic point of far-reaching importance on one page and flatly contradict this proof on the following page, without anyone seeing that there is a conflict, is badly in need of an overhauling.

The information now available makes it quite clear that the electron is not the permanent "building block" type of entity that was envisioned in 1911, but an evanescent particle that can be created or destroyed with relative ease. Recognition of this fact should carry with it the realization that it is not only radioactivity that has ceased to be evidence of the presence of electrons in matter; the appearance of electrons in *any* physical process can no longer be taken as an indication that these electrons existed prior to the initiation of that process. In fact, the weight of evidence is now strongly in favor of the conclusion that in most cases they are created in the process, and that where electrons do actually have a prior existence, they exist in, and not as a part of, the atoms of matter.

This conclusion applies not only to electrons, but to electric charges in general, irrespective of whether or not they can be definitely identified with the presence or absence of electrons. A century ago it was considered that the appearance of positively and negatively charged ions when a material goes into solution constituted definite proof that the charges also exist in the undissolved substance, and even today we find the chemistry textbooks making such statements as this, "We now know that ionic compounds exist as ions even in the crystalline state."[28] But again the advance of knowledge has invalidated the prevailing conclusion. It has been found that many substances which form ions in solution are definitely not of the "ionic" nature in the solid, and the same textbook from which the foregoing statement was taken tells us a few pages later, "If ions are not present in an electrolyte before it is dissolved, they must be formed from the molecules of the compound as it dissolves."

This is precisely the same kind of a situation which we encountered in connection with the question of the origin of the electrons which make their appearance in radioactive disintegration. Many substances break up into ions, at least partially, on going into solution, and if the substance is of a type which, according to currently accepted theory, *could* be composed of ions in the solid state, the formation of ions in solution is commonly interpreted as proof that the substance is thus composed. But where there are reasons why the existence of ions in the solid state is incompatible with present-day theory, exactly the same evidence is taken to mean that the charges are created in the ionization process. Here again, when we examine the situation carefully, it is clear that the physical evidence does not distinguish between these alternatives, but as long as it is necessary to assume that *some* ions are created in the process, it is obviously quite possible, and even probable, that *all* ions are thus created; that is, this is *the* way in which ions are formed. Thus the hypothesis that the ions exist in the solid prior to solution is not only without the proof that is claimed; it is not even the most probable of the readily available explanations of the observed facts. The creation explanation has the distinct advantage that it applies the same ionization mechanism to all substances, whereas the alternative and generally accepted explanation requires two different mechanisms.

Summarizing the foregoing, it is now apparent that electrons, and electric charges in general, are easily created in physical processes of various kinds, and hence the emission of electrons from matter during such processes can no longer be considered as proof, or even as good evidence, that the electrons, as such, existed in the matter before the process took place.

At this juncture someone will probably point out that even though the emission of electrons from matter can no longer be considered as *proof* that the electron is a constituent of matter, the emission is still *consistent* with such a hypothesis, and definite proof from this source is no longer necessary in view of the large amount of supporting evidence now available elsewhere. The bald truth is that this other evidence is chimerical; the whole history of the development of the concept of the atomic electron is a story of piling one unsupported assumption on top of another, and without the definite and positive proof which the emission of electrons from matter was presumed to furnish, the whole structure collapses. Bohr's original postulates, for example, are simply ridiculous if he first has to *assume* that the electron is a constituent of matter, and then goes on to postulate behavior characteristics for these hypothetical atomic constituents totally unlike anything ever observed. If his action in abandoning the solid ground of established physical facts and striking out on an uncharted course of pure hypothesis can be justified at all, which is questionable, it can only be justified on the ground that he thought that definite and positive proof that the electron is a constituent of matter already existed, and hence if the behavior of these atomic electrons could not be explained in a normal manner, it was reasonable to presume that they must follow some different laws.

As long as there is any question at all as to whether or not the electron is actually a constituent of matter, the fact that the atomic electron cannot be reconciled with known physical laws is a strong argument against the existence of any such entity, not a justification for formulating new physical laws. Had Bohr been in possession in 1913 of the experimental knowledge of the present day, including the now well-established fact that electrons are transient entities that can be readily produced or destroyed, it undoubtedly would have been obvious to a scientist of his competence that the reason for his inability to fit the atomic electron into the existing framework of physical laws was not that this constituent of the atom is governed by a different set of laws, but that no such atomic electron exists.

One of the characteristics of a sound physical theory is that it leads in an easy and natural way "with the appearance of a certain inevitableness," as Bridgman puts it, to explanations of physical phenomena other than that for which it was originally developed. Planck's original Quantum Theory, for example, was developed to explain the behavior of radiation from an energy distribution standpoint, but one of its first important consequences was a simple and logical explanation of the photo-electric effect: a related but totally different phenomenon. Similarly, we could expect that if the concept of the electron as a constituent of matter were valid, we would find it leading easily and naturally to solutions of other related problems. But the whole history of this concept has been just the opposite. Nothing has developed easily and naturally; every step that has been taken has been forced and artificial, and each advance into new territory has been made only by sacrificing some part of existing physical knowledge, so far as its application to the atom is concerned.

As one observer expresses it, "Bohr solved the problem of the stability of a system of moving electric charges simply by postulating that the cause of the instability... did not exist."[29] To the layman his might seem to involve a rather drastic redefinition of the word "solve," but be that as it may, the ensuing history of the Bohr atom and its lineal descendants is one long series of problems for which there seems to be no solution other than to postulate that they do not exist. The orbits which Bohr postulated for the electrons could not be located specifically, hence it was postulated that no definite orbits exist; the theoretical momentum and position of an individual electron could not be reconciled, and a "Principle of Uncertainty" was therefore formulated, asserting that the electron could not have a definite momentum and a definite position at the same time; even with the benefit of this extraordinary principle, identification of positions was found to be impossible, so it was postulated that the impossibility was inherent and that the best that could be done was to calculate a probability that the electron might be found at a certain location; some of the theoretical consequences were inconsistent with the usual cause and effect relationships, and it was therefore postulated that causal relations are not operative at the subatomic level. Now in relatively recent years, the long list of assumptions and postulates has been climaxed by the assumption, sponsored by the Copenhagen school of theorists (who represent the "official" viewpoint of present-day theoretical physics), and expressed by Heisenberg in the previously quoted passage, that this atomic electron does not even "exist objectively."

All of these "solutions" of the problems that have been encountered in the development of the concept of the electron as an atomic constituent have, of course, modified the characteristics of the atomic electron very drastically. As the nuclear atom was originally conceived, the negatively-charged constituent was presumed to be the same electron that is observed experimentally. This experimental electron is a definite and well-defined thing, notwithstanding its impermanence. We can produce it at will by specific processes. We can measure its mass, its charge, and its velocity. We can control its movement and we have methods by which we can record the path that it takes in response to these controls. Indeed, we have such precise control over the electron movement that we can utilize it as a powerful means of producing magnified images of objects which are too small for optical magnification. In short, the experimental electron is a well-behaved and perfectly normal physical entity. But such an electron cannot even begin to meet the requirements which have been established step by step for the atomic electron, as the concept of this particle has been gradually modified to "solve" one problem after another. The atomic electron, as it is now portrayed, is not a definite and tangible entity such as the experimental electron. It does not conform to the usual physical laws in the manner of its experimental counterpart, but has some unique and unprecedented behavior characteristics of its own, including a strange and totally unexplained ability to jump from one orbit to another (or to do something entirely incomprehensible which has the same effect) with no apparent reason and, so it seems, complete immunity from all physical limitations. We can deal with it only on a statistical basis, and even then, as Herbert Dingle points out, we can make our statistical methods for dealing with such particles effective "only by ascribing to the particles properties not possessed by any imaginable objects at all."[30] Furthermore, as already mentioned, the leading theorists of the present day tell us that the atomic electron cannot be accommodated within the three-dimensional framework of physical space; it must be regarded merely as a symbol rather than as an objectively real particle.

In view of this fact that the atomic electron no longer has even a remote resemblance to the experimental electron, it is manifestly absurd to continue basing physical theory on the fiction that the two are identical. The previous conclusion that there is no proof that the electron is a constituent of the atom must therefore be extended to assert specifically that the electron as known experimentally is definitely *not* a constituent of the atom. The hypothetical negatively-charged atomic constituent currently sharing the name "electron" with the experimental particle is something of a totally different character, a purely theoretical creation, unrelated to anything that has ever been observed and itself not capable of being observed: an "abstract thing, no longer intuitable in terms of the familiar aspects of everyday experience,"[31] as Margenau describes it.

It should be emphasized that the conclusion just stated-the conclusion that the negatively charged constituent of the atom (if such a constituent exists) is a purely hypothetical entity unrelated to the experimental electron-is not something that has been developed in this work, or that depends in any way on any of the arguments presented herein. It is simply a necessary consequence of an obvious fact that modern physicists have chosen to ignore: the fact that two physical entities are not identical if they have little or no resemblance to each other. If the theorists wish to contend that the hypothetical negative constituents of the atom are identical with the experimentally observed particles that we call electrons, then they must accept, with no more than reasonable modifications justified by the environment, the properties of the experimental electrons; if they find it necessary to invest their hypothetical atomic constituents with a totally different set of properties, then they cannot identify them with the experimental electrons. Even the physicists, who in these days are permitted to "get away with murder," as James R. Newman expresses it, must be required to conform to some of the elementary rules of logic.

Long years of effort have convinced the theoretical physicists that the first alternative, the construction of an atom in which the experimental electron is the negatively charged constituent, is impossible. As brought out earlier in this discussion, the concept of a nucleus composed wholly or in part of a group of positively charged particles is likewise untenable in the light of present-day knowledge. Assumption (2) in the list previously given is therefore invalid; that is, if the atom is constructed of "parts," these parts are not known subatomic particles; they are purely hypothetical concepts of which no independent experimental evidence exists. This is a difficult pill to swallow: a conclusion that the scientific world will find very hard to accept, not only because it invalidates many of the cherished ideas and concepts of modern physical science, but also because it is in direct conflict with the seemingly natural and logical inference which is immediately drawn from the existence of radioactivity.

The original concept of the atom was that it is the indivisible ultimate particle of matter; the word atom actually *means* indivisible. But the discovery of radioactivity showed that the atom is not indivisible, as this is a process of disintegration, in which particles are ejected and the original atom is transformed into one of a different kind. The natural conclusion to be drawn from this new knowledge-the conclusion that was drawn when the knowledge was new, and which is still one of the principal supports for the present-day theory of the atom-is that the atom is a complex structure composed of sub-atomic particles. The validity of this conclusion in its general aspects will be discussed later. For the moment we are dealing only with the question of the nature of these particles.

Just as it is natural to conclude that the existence of radioactive disintegration proves that the atom is composed of individual parts, so it is natural to conclude that the particles ejected from the atom in the process of disintegration are the parts of which the atom is composed. In fact, this conclusion seems to be implicit in the first. But this second of the natural and seemingly obvious conclusions turns out to be entirely erroneous. Three types of particles emanate from the disintegrating atom, and existing knowledge indicates that not one of these three existed as such in the atom prior to the disintegration. The alpha particles are positively charged helium atoms, and it was quickly realized that they could not be primary atomic "building blocks"; present-day opinion, as previously noted, is that the beta particles, which are electrons, are created in the disintegration process; and the gamma particles (if we stretch the definition of "particle" far enough to include them) are photons, units of radiation, and have always been considered to be products of the disintegration, not as pre-existing entities.

This throws an entirely different light on the picture. If we were able to show that the particles ejected by the atom were of such a character that it could be logically concluded that they were the "building blocks" of which the atom is constructed, then we could take the stand that radioactivity furnishes a satisfactory explanation of the general nature of atomic structure. But the physicists cannot and do not contend that this is true; when we boil their statements down to the essence, we find that they are, in effect, advancing the curious contention that the emission of certain particles from the atom during radioactive disintegration is a proof that the atom is constructed of certain other particles. This is a far cry from the conclusions which seemed so natural and logical on first consideration of the phenomenon of radioactivity. The physicists' present stand is neither natural nor logical, and it destroys the whole force of the original argument. Not only does it leave the question of the identity of the atomic constituents entirely up in the air, but the fact that it has been necessary to conclude that *all* of the particles ejected by the radioactive atom are created in the disintegration process also raises some serious questions as to the validity of the basic assumption that the atom is composed of "parts."

In any event it is now clear that the electron or any other particle that is proposed as an atomic constituent will have to stand on its own feet without any support from radioactivity. The conclusions of the early 1900s to the contrary will simply have to be rewritten, in the light of modern knowledge, no matter how reluctant the theorists are to take this step. From the viewpoint which the advances in experimental knowledge have given us, the textbook statement that "the emission of electrons by atoms is convincing evidence that electrons exist as such inside atoms" must be rewritten to read that "the creation of electrons in physical processes such as radioactivity is convincing evidence that electrons exist as such inside atoms," which, of course, reduces it to an absurdity. If the electron is to be advocated as an atomic constituent, then some consistent picture of an atom constructed wholly or in part of electrons will have to be devised and, as has been brought out earlier in the discussion, it is now admitted that this cannot be done if the atomic electron has the properties of the electron which is observed experimentally. Hence we come back to the fact that if there is a negatively charged constituent of the atom, it is not the experimentally observed electron; it is a purely hypothetical particle of a much different nature.

At this point, then, we can say that the nuclear atom, as currently conceived, is impossible. It has been shown that the two items which are relied upon to furnish proof of the validity of the basic assumptions on which the nuclear theory rests not only do not supply any such proof, when they are carefully analyzed, but actually furnish strong evidence to the contrary. It has also been shown that without the pro(JNS1)of which these two items, radioactivity and Rutherford's scattering experiments, are supposed to furnish, the entire structure of the nuclear theory collapses. Every one of the eight major assumptions of this theory, as previously listed, topples in this general collapse, except assumption (1), which we have not yet considered.

A most heretical conclusion? Perhaps so. But consider the following statement by Erwin Schroedinger, one of the principal architects of modern physical theory, who can hardly be classified as a scientific heretic, and ask yourself whether he is not saying exactly the same thing in more cautious words:

> Once we have become aware of this state of affairs, the epistemological question: "Do the electrons really exist in these orbits within the atom?" is to be answered with a decisive *No*, unless we

prefer to say that the putting of the question itself has absolutely no meaning. Indeed there does not seem to be much sense in inquiring about the real existence of something, if one is convinced that the effect through which the thing would manifest itself, in case it existed, is certainly *not* observed. Despite the *immeasurable* progress which we owe to Bohr's theory, I consider it very regrettable that the long and successful handling of its models has blunted our theoretical delicacy of feeling with reference to such questions. We must not hesitate to sharpen it again, lest we may be in too great haste to content ourselves with the new theories which are now supplanting Bohr's theory, and believe that we have reached the goal which indeed is still far away.[32]

Chapter IV
Particle Problems

The conclusion that the nuclear theory of the atom is erroneous and that in reality there is no such thing as an atomic nucleus will be difficult for the present generation of scientists to accept. The individual who has from childhood visualized the atom in the manner pictured by Bohr, who has participated in the great debates over the use of "nuclear" energy, who reads *Nucleonics* and the *Annual Review of Nuclear Science*, and who has perhaps taught classes in "nuclear physics" cannot be expected to look with enthusiasm on the prospect of life without a nucleus. We can, of course, remind him that the Bohr atom has long since vanished from the scene and that the "official" atom of modern physics cannot even be imagined, so the experts say, much less pictured. We can also point out that "atomic energy" and "atomic physics," the terms that will have to be substituted when the nucleus is discarded, are already in common use. Most of the "nuclear physicists" in the United States work, directly or indirectly, for the Atomic Energy Commission. But this will probably be cold comfort. One is not easily reconciled to the loss of an old friend in the world of ideas.

The tenacity with which the human mind clings to familiar ideas is truly remarkable. "Our first reaction" to a new mode of thought, says Lande, quoting G. Sykes, "is one of pain and distaste."[33] It is quite understandable that this should be true in the metaphysical fields, religion particularly, since in these areas what one believes is paramount. Science, on the other hand, sets up an external objective standard and, at least in principle, scientists agree that any opinion which turns out to be in conflict with observed facts must be abandoned. Bridgman states the credo in these words, "In the face of a fact there is only one possible course of action for the scientist, namely acceptance, no matter how much the fact may be at variance with his anticipations, and no matter what havoc it may wreak on his carefully thought-out theories."[34] It would simplify matters greatly if we could be assured that the scientific world in general would follow the principle thus laid down by Bridgman. But if we judge by past performance, we get an altogether different picture. Any new thought which is essentially nothing more than an extension or minor revision of existing theory finds the "Welcome" mat outside the door if it has any merit at all, or even if it is merely an interesting speculation, but an altogether different reception awaits any findings that challenges one of the basic tenets of current science. "Usually such new ideas are looked upon with indifference or suspicion," says Raman, "and many years of persistent advocacy and powerful observational support are required before the investigator can hope to see his ideas generally accepted."[35] Max Planck was even more pessimistic. He complained bitterly about the way his "sound arguments fell on deaf ears," and concluded that new ideas *never* succeed in convincing their opponents, but must wait for a new generation of scientists to grow up.

In any event it is obvious that the case against such a popular theory as that of the nuclear atom must be extremely strong to be convincing, but the case that has been presented herein *is* strong; it is a prima facie case. The three preceding chapters have gone directly to the heart of the matter. It has been shown that the idea that the atoms are in contact in the solid state is nothing but an assumption, and that there are many items of evidence which indicate that this assumption is erroneous. It has been shown that there is nothing in Rutherford's findings which requires or justifies postulating the existence of a nucleus, and that in the light of what is now known about conditions in the solid, it is clear that the small but massive "something" that Rutherford located is the atom itself, not a nucleus. It has been shown that if there are any negatively-charged constituents in the atom, they cannot be electrons of the type that we observe experimentally; if such constituents exist at all, they will have to be purely hypothetical particles of a totally different and unprecedented nature. Without a nucleus and without electrons, there can be no nuclear atom of the kind postulated by present-day theory.

This is a solid and airtight case. When the nuclear theory is analyzed it is clear that it rests entirely on two assumptions: (I) that Rutherford's scattering experiments proved the existence of a nucleus, and (2) that radioactivity proves that electrons constitute one of the components of matter. Neither of these assumptions can be maintained in the light of existing physical knowledge, and the other primary assumptions of the theory-Bohr's postulates as to the behavior of the atomic electrons, and present-day ideas as to the composition of the nucleus--are preposterous without the positive proof of the existence of the nuclear structure which Rutherford and radioactivity were supposed to furnish. The theory therefore collapses. But the scientific world has been so sure of its nuclear atom for so long that there will undoubtedly be a tendency to feel that there must be a catch in it somewhere. One of the immediate reactions will no doubt be, "How can this theory be wrong when it has given us so many right answers all these years?"

This question is easily answered. The same thing is true of *all* theories that finally have to be given up after a long period of acceptance. It was true of the Ptolemaic theory and all of the others mentioned earlier in the discussion. All of these theories gave the right answers to many questions over a long period of time, but they were ultimately pronounced wrong nevertheless. It should be recognized, however, that characterizing such a theory as "wrong" does not really do it complete justice. There is no sharp line of demarcation between true and false in ordinary physical theory, because most theories are compound structures in which both truth and error are present simultaneously. Most erroneous theories contain at least some truth, otherwise they never would have been advanced in the first place; whereas most presumably correct theories contain at least a small degree of error. This is the reason why erroneous theories have often led to important scientific advances. If these theories were 100 percent wrong, they would be more likely to impede discovery of the truth than to facilitate it, but where they are partly true, this element of truth may be all that is needed for the purpose at hand. As Reichenbach puts it, "Knowledge of half the truth can be a sufficient directive for the creative mind on its path to the full truth."[36]

The whole foundation of the science of astronomy, for instance, was laid at a time when it was believed that the sun revolved around the earth. We now say that this theory is wrong, and if we look upon right and wrong as mutually exclusive, and visualize our concepts of the universe as the answers to a series of true-false questions, the striking results obtained from the use of the geocentric hypothesis are totally inexplicable. If we recognize, however, that in the field of ideas we have not only pure black and pure white, but also an infinite gradation of shades of gray, the explanation is simple. The geocentric hypothesis can be split into two parts:

(1) In the system sun-earth, one component revolves around the other.
(2) The earth is the stationary component.

In the light of present knowledge, we assert that statement (2) is wrong, but we accept statement (1) as correct. A great many of the propositions with which astronomy is concerned are dependent only on assumption (1) and are not necessarily affected by the nature of assumption (2). So far as these propositions are concerned, therefore, the ancient astronomers were equally as well prepared, from the standpoint of theory, as the astronomers of today, and not until the invention of the telescope multiplied the scope and accuracy of the observational information manyfold was the need for any revision of assumption (2) apparent.

Even after a theory has been superseded by something more general, this element of truth which it contains may be sufficient to justify retaining it for use in a special field. It has been found, for example, that Newton's Laws of Motion are not a correct expression of the general situation, and for general use they must be replaced by Einstein's expressions or some equivalent. But aside from the workers in a few of the more exotic fields, all of the thousands of engineers that man our vast technological system still tune their slide rules to Newton's Laws and go about their business as if they had never heard of Einstein. We must nevertheless admit that Newton's Laws are "wrong” in the sense that they are not equal to all of the demands now made upon them, and it has been necessary to devise a theory of a wider scope.

The criterion by which we judge whether a theory is wrong in this sense, and must therefore be superseded by something more adequate, is not what it has done, but what it is now failing to do. One of the principal reasons why it is so difficult to dislodge a theory once it becomes embedded in the structure of scientific thought is the lack of general recognition of this point. The tendency is to judge currently accepted theories by their accomplishments, and not until these theories have been overwhelmed by the advance of knowledge does it bcome apparent that it is not their accomplishments but their failures that are significant in determining whether they stand or fall.

Some interesting illustrations of this point can be seen by comparing the statements made about a theory just before it was superseded with the statements made afterward. Here we find that while the same facts are being described in both cases, there is a very radical change in emphasis, so that a totally different impression is conveyed to the reader. Before the fall of the theory, the emphasis is upon its successes, and while the deficiencies or failures are mentioned, they are played down and their significance is minimized. After the fall, the emphasis is completely reversed. Now the successes of the theory are given perfunctory and patronizing comment, while the failures are portrayed as fatal weaknesses. For instance, Max Born's book *The Constitution of Matter*,[37] published in 1923, just before wave mechanics elbowed the original Bohr theory aside, portrays this theory as a resounding success. "The correct solution was found by Bohr ...", Born assures us. Extension of the mathematical formula developed for the hydrogen spectrum to the elements beyond hydrogen was giving trouble, and Born so reports, but his tone is definitely optimistic; he speaks of "signal success" in the qualitative explanation of the spectra of these heavier elements. Furthermore, he sees bright vistas opening up ahead. In the theory of chemical combination, he tells us, "... it is to Bohr's theory of the atom that we must look for the complete solution of the problem."

Today the same picture is seen in an altogether different light. Aside from the educators, who still present pure Bohr theory to all but their most advanced classes, just as if the clock had stopped about 1920, anyone who comments on the application of the Bohr theory to spectra other than those of hydrogen and singly- ionized helium uses the term "failure" rather than Born's "signal success," and it is well recognized that it is this failure that has determined the fate of the original Bohr theory. Whatever successes the theory may have enjoyed are from this standpoint completely irrelevant; a physical theory cannot live on the strength of its past accomplishments; it must keep abreast of the advancing tide of knowledge or give way to something else that can meet today's requirements.

It will therefore be appropriate to take a look at the nuclear theory from this standpoint, putting aside for the moment any considerations connected with the successes, real or alleged, which the theory has enjoyed, and concentrating our attention on the failures, to determine whether or not these are serious enough to suggest that the days of this theory are numbered. From a cold-blooded scientific viewpoint all this is irrelevant, since it has already been demonstrated in the preceding chapters that the nuclear theory is not a correct representation of the physical facts, and it is therefore obvious that if it has not already failed in some important aspect, it will inevitably do so sooner or later. However, the full objectives of this present work are not necessarily reached when the cold, hard facts that demonstrate the falsity of the nuclear hypothesis are assembled and laid before the scientific world. As indicated in the preceding paragraphs, there still remains a major psychological obstacle to be overcome before the full significance of these facts will be generally recognized, and in this respect it may be helpful to show that the weaknesses and failures of present-day atomic theory have by this time reached such proportions that collapse of the theory is imminent, irrespective of the conclusions reached through factual studies of the kind described in the previous chapters.

It is no secret that a large and growing number of physicists, as well as scientists in allied fields, are profoundly dissatisfied with the general state of physical theory as it now stands, and are convinced that some drastic overhauling will be necessary. David Bohm describes the situation in this manner: "Moreover, physics is now faced with a crisis in which it is generally admitted that further changes will have to take place, which will probably be as revolutionary compared to relativity and the quantum theory as these theories are compared to classical physics."[38] J. R. Oppenheimer agrees, "It is clear that we are in for one of the very difficult, probably very heroic, and at least thoroughly unpredictable revolutions in physical understanding and physical theory."[39]

Some are openly rebellious. Cornelius Lanczos tells us defiantly, "The majority of us will not be willing to accept the particle-wave dualism.... In spite of all discouragements ... we will continue to look for a unified structure which we feel must exist behind the appearances."[19] Alfred Lande joins in the attack, "We want to have a unitary theory; and (if I may use the word) even after thirty years of persuasion we still want a unitary theory."[40] Philip M. Morse sums up the situation, "It is an unhappy time for theory."[41]

On first consideration, this chorus of disapproval might seem to contradict the statement in the introductory chapter to the effect that present-day scientists are strongly opposed to any major changes in the framework of accepted theory. But those scientists who recognize the existing situation still shy away from the clear implications of that recognition. It is obvious that changes of the magnitude which Bohm predicts, "as revolutionary compared to the quantum theory as (that theory is) compared to classical physics," cannot leave much of the quantum theory intact. But Bohm is unable or unwilling to face the inevitable consequences of his prediction; he makes it clear that he seeks only to modify quantum theory, not to replace it. The same is true of most of the others who have taken a definite stand.

Lande, for instance, expresses much the same thoughts as Bohm. Perhaps there are some dissenters who are ready for stronger measures. D. I. Blokhintsev implies something of the kind in a recent statement, "Like many physicists all over the world I think that the well-known difficulties of the quantum field theory will be overcome only by a radical change of the very essence of the modern theory."[42] In any event, irrespective of the extent to which the need for replacement rather than revision is recognized, it cannot be denied that there is a widespread feeling that existing theory is not at all satisfactory.

Since the general structure of modern physical theory is to a large extent based on the theory of the atom, the nuclear atom theory must accept a big share of the responsibility for the unsatisfactory state of physical theory in general. It is also apparent that there are major sectors of the field which an adequate atomic theory should cover that are as yet almost completely untouched. For example, a complete theory of the atom must necessarily explain the physical states of matter, yet after nearly fifty years of the nuclear theory Prof. G. Careri found it necessary to open a recent international conference on liquids with the flat statement, "We are still far from having a 'theory' of the liquid state...."[43]

But the real testing ground for atomic theory today is what is popularly known as "elementary particle physics.” "... the future of physics,” says George Gamow, "lies in further studies and understanding of elementary particles."[44] Here is a field in which atomic theory should be directly applicable; here is a rapidly expanding field in which the experimental facts are puzzling and confusing, and the help of an adequate theory is urgently needed; and here is a place where the currently accepted nuclear theory, faced with a major test of its capabilities, falls flat on its face.

The term "elementary particle” is in itself a claim to the possession of some knowledge of the structure of the atom, as it is based on the assumption, an integral part of current theory, that the atom is constructed of "parts” and that these parts cannot be further subdivided; thus they are elementary. If the nuclear atomic theory correctly portrays the structure of the atom, then it should be capable of producing the answers to the questions we find it necessary to ask with respect to the elementary particles. This point is commonly recognized, and "elementary particle physics” is classed as a subdivision of "nuclear science."

How well, then, has modern atomic theory measured up to this, the most significant task now facing it? Let us ask Gamow, whose statement as to the importance of the task has just been quoted. "... for the last few decades,” Gamow replies, "not a single successful step has been made in obtaining these answers."[44] This very recent evaluation of the situation was already foreshadowed years ago by keen observers who realized that the discovery of so many new "elementary” particles neither anticipated nor explained by the accepted theories raised grave doubts as to the validity of these theories. "Questions like these,” said James B. Conant, "raise doubts as to whether the conceptual scheme of nuclear physics is a 'real' account of the structure of the universe,"[45] and Jones, Rotblat and Whitrow asked the very pertinent question, "... is this multiplicity of particles an expression of our total ignorance of the true nature of the ultimate structure of matter?"[46]

In the light of all of the additional information that has been accumulated since these words were written, there remains little doubt but that this question must be answered in the affirmative, and that present-day atomic theory must be judged wholly inadequate for the tasks that confront it. "The physics of this century has set itself the task of interpreting all observed phenomena in terms of the behavior of atoms and molecules, and the electrical particles-electrons, protons, etc.-of which they are composed,"[47] says E. U. Condon. The result of this single-minded dedication of the full resources of the profession to the development of the nuclear atom theory is revealed by another statement from this same author. Speaking of the period from 1926 to 1955 he reports, "... little progress has been made in interpreting the fundamental problems of atomic theory."[48] From von Weizsaecker we get this judgment, "Quantum theory does not explain the characteristic properties of the different sorts of elementary particles which we know to-day. It is, therefore, certain that it must be supplemented by a new, as yet unknown theory."[49]

The nuclear theory has thus been weighed in the balance and found wanting. Even without the devastating disclosures of the preceding chapters, it is evident that this theory falls far short of meeting present-day demands upon it, and a drastic overhauling is inevitable. In the light of the information developed herein, however, it is clear that existing theory cannot merely be *supplemented* by something new as von Weizsacker suggests; it must be *replaced*. The nuclear theory is not simply incomplete, it is basically wrong; the atom is not so constructed.

Chapter V
Postulates Unlimited

The four preceding chapters present the primary thesis of this work. They show that when the arguments in favor of the nuclear atom concept, developed fifty years ago on the basis of a very limited amount of experimental information, are reexamined in the light of the immense store of factual knowledge now available, they collapse completely and leave the theory entirely without support. However, during the half century that the nuclear theory has been the accepted doctrine in its field, it has become entwined and intermeshed with many other phases of physical theory, and it cannot be summarily discarded without a certain amount of effect in these other fields. Indeed, it is generally believed, quite erroneously, that these collateral items furnish some corroboration for the nuclear theory itself. Some discussion of these related items is therefore appropriate.

One of the most interesting and revealing features of present-day atomic physics is the fact that the original Bohr theory, already buried deep under successive layers of modifications and revisions, is still the picture of atomic structure that is presented to all those who undertake a study of physics. This (Bohr) atom... is wrong. It is, alas! still taught in school and university,"[50] laments a British observer. There is no much physical and chemical evidence for the correctness of the modern atomic picture that there can be no reasonable doubt of its validity,"[51] says one elementary physics textbook, which then goes on to present pure Bohr theory, apparently oblivious to the fact that such a statement is the height of absurdity when the modern atomic picture" to which it refers is flatly repudiated by the leading theorists in the physical field.

Here is an extraordinary situation that deserves some thoughtful consideration. Two points stand out particularly. First, it is quite significant that those who are responsible for the college curricula deliberately choose not to include any of the post-Bohr developments in their basic physics courses. The textbook from which the foregoing quotation was taken has 1271 pages, including one complete section on Modern Physics"—surely space enough to permit at least some mention of the official" doctrines of the present day. But nowhere in all of these pages is the student given the slightest inkling that, according to the acknowledged leaders in the field, his efforts to understand the modern atomic picture" are in vain; that the atom of modern physics can only be symbolized by a partial differential equation in an abstract multidimensional space...", and that an understanding of the 'first order' is... almost by definition, impossible for the world of atoms."[18]

One can only speculate as to the reasons why this policy is being followed, but whatever they are, they certainly demonstrate a decided lack of confidence in the work of the modern theorists. This is a very curious phenomenon, yet, on reflection, it is quite understandable that those who undertake to teach the subject of physics should be somewhat less than enthusiastic about presenting to an audience of youthful skeptics a theory that has severed all ties with everyday experience and has gone on an uncontrolled excursion into a weird land of fantasy completely divorced from physical reality: a theory which has made the atom into something inaccessible to our senses or to our imagination."[52] The university of today has enough of a task on its hands in attempting to give its students an understanding of the first order" of so many subjects for which such an understanding *is* possible. It can hardly be blamed for reluctance to undertake the assignment of teaching a theory which admittedly cannot be clearly understood. The Principle of Incomprehensibility which Heisenberg, in effect, enunciated in making the statement quoted in the preceding paragraph, may actually have had a more profound effect on the physics curricula than his famous Principle of Uncertainty.

Even the most casual consideration of the history of the development of the nuclear concept, if undertaken by someone who is not so close to the subject that the trees interfere with his view of the forest, will inevitably suggest a direct connection between this incomprehensibility and the unprecedented degree to which established physical principles have been thrown overboard in the course of the development. Many centuries of experience in the pursuit of scientific knowledge should have made it abundantly clear by this time that one of the most hazardous assumptions that can be made in scientific research is that things are different here." An imaginative approach and a "certain daring in considering and testing new ideas," to use the words of John A. Wheeler, are essential to progress, but the lesson to be learned from history is that the new ideas which are actually productive are those which lead to methods of fitting the points at issue into the existing structure of knowledge, not those which dodge the problem by postulating that existing knowledge does not apply to the matters in question. This point can be demonstrated by consideration of a few examples.

The discovery of the neutrino has already been mentioned as an illustration of a hypothesis which was completely unsupported when originally advanced, yet has achieved both general acceptance and experimental confirmation. The important aspect of this case, so far as the present discussion is concerned, is that the neutrino hypothesis did not involve postulating any deviation from established physical laws; on the contrary, the primary objective of postulating the new particle was to *avoid* the necessity of assuming the violation of an established physical principle: the conservation of energy.

Planck's theory of the quantum of radiant energy is widely hailed as an imaginative and revolutionary hypothesis, and indeed it does represent a revolution in scientific thinking, probably one of the most significant changes in outlook in all scientific history, but it did not alter any existing physical laws; it merely identified the correct set of existing laws applicable to this particular situation. Science already had laws applicable to continuous phenomena and other laws applicable to phenomena existing only in discrete units (matter, for example). What Planck did was to show that the previously existing concept of radiation as a continuous entity is wrong and that it actually exists in discrete units. The known physical laws applicable to discrete units therefore apply to radiation. This rather prosaic manner of describing the innovation hardly does full justice to the feat of the imagination which Planck exhibited in breaking out of the narrow groove of established thought and formulating his new theory, but it is a correct statement of the situation, nevertheless.

Another celebrated exercise of scientific imagination is Kekule's hypothesis of the benzene ring, which is said to have been suggested to him by a dream in which he saw a snake with its tail in its mouth. Here is another bold hypothesis, now solidly established, but entirely without experimental support when originally proposed. Again, this successful product of the imagination is an explanation which takes care of the special features of benzene and aromatic compounds in general *within* the existing framework of physical and chemical theory, and not by postulating that any of the principles applicable to other organic compounds are inapplicable to the aromatics.

Now let us compare Bohr's original postulates with these three highly successful imaginative hypotheses. In all cases the hypotheses under consideration were totally lacking in direct support to begin with; it would be hard to visualize anything more truly hypothetical than the neutrino when it was first postulated. The difference is that whereas the three successful hypotheses *accept* existing laws as entirely valid wherever they apply to the phenomena in question, Bohr's postulates *deny* the validity of established physical laws so far as application to the atom is concerned. His postulates do not merely elaborate the existing structure of theory in the manner of the hypothesis of the neutrino, or that of the benzene ring; they set up a totally new structure entirely outside the existing system.

This is a very questionable procedure, and it is extremely doubtful whether results obtained in such a manner should ever be recognized on anything more than a speculative basis. It is certainly true that Bohr's hypotheses were accepted with almost indecent haste. Rutherford's proposal of the nuclear atom was originally advanced in 1911. Two years later, in 1913, Bohr came forth with his postulates. Now let us ask, can it be contended in all seriousness that two years of study of a totally new hypothesis of a revolutionary nature affecting the very foundations of physical theory is sufficient to establish beyond a reasonable doubt (1) that the new hypothesis is itself sound, and (2) that it cannot be accommodated within the existing framework of physical theory? Would it not be much more in order to insist, where as basic a concept as that of Rutherford's is involved, that the problem be studied for decades before such a drastic conclusion as number (2) is accepted on anything more than a very tentative basis?

It is now apparent that no serious consideration was ever given to the possibility that Rutherford's nuclear hypothesis might be unsound, let alone giving it the careful and critical examination that an important theoretical proposal of this kind should have had. Surely if this question had been actively pursued, someone would have seen that it is completely unnecessary to postulate the existence of a nucleus in order to account for the results of the scattering experiments, and that no other basis for such a postulate has ever been suggested. This is merely another of those cases where a plausible and superficially attractive theory appears, and without making any critical examination, the scientific profession simply cries "Eureka!" in the manner of Archimedes, and swallows it whole.

Instead of demanding a thorough and exhaustive study of the entire situation before jettisoning any established physical laws, a study which in all probability would have revealed the true status of the nuclear hypothesis sooner or later, and thus would have stopped the Bohr development in its tracks, the physicists promptly embraced Bohr's theory, and this acceptance, by pushing Rutherford's nuclear hypothesis into the background, had the effect of exempting the nuclear hypothesis from any further questioning. Later, when the inevitable difficulties arose, these were taken to indicate the necessity of revising some of Bohr's ideas, instead of being recognized in their true significance as results of an erroneous basic hypothesis. And so the modification and revision has continued, retreating farther from reality with every change, apparently without a suspicion that the whole complex structure rests on a totally fictitious foundation.

As the origins of the currently accepted versions of the theory have receded farther and farther into the background, any thought of questioning the validity of the basic concepts has become more and more inconceivable, even though, at the same time, it has become increasingly clear that there were some very definite and serious irregularities in the early work on the theoretical structure. The effect of this has been to turn the current thinking of the scientific profession into some strange and devious channels. C. N. Yang, for instance, comments on the work of the founders of present-day atomic theory in this manner, We could only wonder what it was like when to reach correct conclusions through reasonings that were manifestly inconsistent constituted the art of the profession."[53] An unprejudiced observer can hardly avoid doing a little wondering on his own account: wondering why it should be necessary to accept this altogether implausible paradox with such complete docility and without giving any consideration at all to the obvious possibility that the conclusions reached by these faulty reasoning processes are, in fact, erroneous, as the products of faulty reasoning almost always are. It is apparent that one of the characteristics of the present-day art of the profession" is an excessive timidity in taking issue with the conclusions of earlier eras.

Before passing on to another phase of the subject under discussion it may be well to point out that a repudiation of established physical principles of the scope and magnitude of that involved in the hypotheses of Bohr and his successors, is unprecedented in physical science. There is a general tendency to couple these modern atomic theories with Einstein's Theory of Relativity as conceptual revolutions of a similar nature, but it should be emphasized that Einstein's hypotheses are of a totally different character, and irrespective of whether or not they are actually valid, they are not open to the same objections that apply to the hypotheses of Bohr *et al.*

Einstein does not say that the properties of objects moving with high velocities, the phenomena with which he is primarily concerned, follow laws that are different from those applicable to objects moving at relatively low velocities. According to his postulates, all objects follow exactly the same laws, but the expressions of these laws previously deduced for objects moving at low velocities are simplified forms of the generally applicable expressions, and they are valid only where the other terms of the general expression are negligible.

Except for the fact that Einstein's postulates apply to some of the fundamental entities of physical science, and therefore have unusually far-reaching consequences, they are no different from many another new development in science. This is, in fact, the way in which scientific knowledge normally develops. Studies in a restricted area first show that the particular phenomenon under consideration follows a certain pattern of behavior, and a mathematical expression is developed to represent this behavior. Later these studies are extended to a wider field and it is discovered that the original pattern is a special case of a more general pattern, and that by modifying the original mathematical expression, usually by means of additional terms which are negligible in the special case, a new expression can be obtained which is applicable to the entire field. Each of these generalized expressions, including Einstein's, must justify itself in the usual manner, but this procedure by which they are developed is entirely sound and logical.

Such hypotheses as Einstein's still leave us with but one universe. We may have differences of opinion as to whether or not these hypotheses correctly express the behavior of the one universe to which we all subscribe, but this is a question which we can reasonably expect will be resolved in due course. On the other hand, the hypotheses advanced by Bohr and those who have modified and enlarged upon his work have had the effect (to the extent that these theories are accepted) of splitting the universe into separate parts, each with its own set of laws. From some standpoints it can no doubt be argued that there is no valid reason why there should not be two separate universes, or for that matter, an infinity of universes. At the moment it is fashionable to take the attitude that those who expect the same physical laws to be followed everywhere are simply naive. Indeed, we should not expect," says F. Waismann, in a field which lies so outside the reach of our senses, to find the same sort of relations and laws as those which hold in our large-scale world."[54]. But the arguments which are advanced in support of this view are singularly unconvincing; they have a very definite odor of sour grapes. As Lande points out in connection with the attempts to justify another of these new viewpoints, It is significant, however, that this detached neutrality was accepted by the theorists only *after* their efforts to establish a unitary theory of matter had failed–temporarily at least."[55]

The inference that the reach of our senses" has a bearing on the range of applicability of physical laws is merely another of those anthropomorphic concepts from which science has been trying to free itself for centuries. There is no sound basis for believing that we occupy any preferred or unique position, or that the region accessible to our direct observation is in any way set off from the rest of the universe. So far as we know, there is only one universe, a single all-embracing universe, and it is reasonable to believe that the laws applicable to any one part of this universe are applicable to all. Certainly we are not justified in *assuming* anything to the contrary merely to save some pet theory from collapse, or to accommodate theorists who have failed in their attempt to solve the problems with which they are confronted, which is essentially what the proponents of present-day atomic theory are asking us to do.

Another modern technique of equally dubious character which has been employed freely in the development of the currently accepted theory of the atom, along with the repudiation of established physical laws, is the use of principles of impotence, which rationalize failure to solve difficult problems by setting up postulates that solutions to these problems are impossible. Again quoting Alfred Lande, in short, if you cannot clarify a problematic situation, declare it to be 'fundamental,' then proclaim a corresponding 'principle.'[56] Such propositions (principles of impotence) play a very large part at the present time in the fundamental theories of physics,"[57] says R. B. Braithwaite.

It should hardly be necessary to point out that from their very nature these principles of impotence are incapable of proof and even at best introduce a very large element of uncertainty into any situation wherein they are used. It is actually quite doubtful whether there is adequate justification for recognizing them as legitimate scientific devices, to say nothing of giving them any authoritative standing. Some of them should certainly be barred. The underlying concept upon which all scientific research is based, and without which the application of time and effort to this task would be wholly unjustified, is the conviction that the physical universe is essentially reasonable and operates according to fixed principles. Thus far we have never encountered any actual evidence to the contrary; as scientific knowledge has expanded, one after another of the phenomena that were inexplicable to our ancestors has been found to follow fixed and unchanging laws. But now there is a growing use of a practice which is not only questionable by nature, in that it is an easy way of evading the difficult task of solving complex problems, but also leads to conclusions which are in direct opposition to the philosophical premise which is our only justification for undertaking scientific research in the first place.

Conclusions such as this from Heisenberg, "... the idea of an objective real world whose smallest parts exist objectively in the same sense as stones or trees exist, independently of whether or not we observe them... is impossible,"[16] or this from Bridgman, The world is not intrinsically reasonable or understandable; it acquires these properties in ever-increasing degree as we ascend from the realm of the very little to the realm of everyday things,"[58] or this from Herbert Dingle, The 'real' world is not only unknown and unknowable but inconceivable-that is to say, contradictory or absurd,"[30] are completely at odds with the underlying philosophy of scientific research. If we had some definite and positive evidence that they were true, we would have to accept them, however unpalatable they may be, but accepting them purely on the strength of principles of impotence, or long chains of unsupported and highly questionable assumptions, which actually means that they have no factual support whatever, is totally illogical.

The plain and unvarnished truth is that these principles of impotence are simply strategems designed to avoid the necessity of admitting failure. The theorists have failed in their attempts to discover the exact physical and mathematical properties of the component parts of the atom, and rather than admit that their approach has been wrong, or as C. N. Yang suggested in the interview previously mentioned, that their abilities are unequal to the task, they prefer to postulate that these properties do not exist and that even the atom itself, as Heisenberg says, has no immediate and direct properties at all." The ironic part of it is that this present reexamination of the situation in the light of modern experimental knowledge shows that the task at which the theorists have failed is a completely meaningless task; the nuclear structure for which they have sought in vain to develop a consistent and workable theory simply does not exist at all.

Here is the real reason why physical science has gotten itself into a mess" as Schroedinger describes it. Reichenbach lets the cat out of the bag when he says, Only the interior structure of the atom, in which lighter particles like electrons play a leading part, requires the quantum mechanical duality of interpretations."[59] This is the answer. It is only when we try to ascertain the details of something that does not exist and conjure up all manner of explanations of wholly imaginary happenings, that we enmesh ourselves in the kind of difficulties characteristic of modern physics. The whole story is one of trying to force the universe into a pre-determined mold. The concept of the nuclear atom has been taken as a fixed, unalterable truth, and anything that conflicts with this concept has been given up, no matter how great the sacrifice. But no amount of manipulation, no exercise of ingenuity, no sacrifice of principle, can make a success of a theory that is based on a false premise. As this work brings out, the nuclear theory is wrong; there never was any sound basis for accepting it in the first place, and the advance of knowledge in the intervening half century has demonstrated that it is completely without foundation. However brilliant and ingenious the work of Bohr and his colleagues has been, their attempt to force all physical knowledge into conformity with this erroneous concept was doomed from the start.

This does not imply that their work has been completely wasted. Many results of genuine value have been accomplished as by-products of the development of this erroneous theory of the atom, since here again the theory is not 100 percent wrong; it contains some elements of truth that have been put to good use. These items of real value can be salvaged for use with whatever new theory replaces the present concepts, and this situation will be given some further consideration in the next chapter, but the theory itself-the concept of the atom that has been derived through modification and revision of Bohr's original ideas-will have to be discarded. In addition to being completely without factual foundation, now that the true status of the nucleus" has been revealed, this structure is a tangled mass of unsupported assumptions, principles of impotence, and repudiation of established physical knowledge that is entirely out of harmony with the underlying philosophy of scientific research.

Chapter VI
Other Names For Roses

The second point that emerges as very significant when we analyze the attitude of the colleges toward modern atomic theory is that they find the Bohr atom more suitable for the applications with which they are particularly concerned than its highly sophisticated successors: a fact which helps to explain their coolness toward the more recent developments. The atomic picture for which the textbook finds so much "physical and chemical evidence" is the original Bohr picture, which all of the leaders in the field, including Bohr himself, have long since characterized as wrong.

Just offhand, this seems ridiculous. How can there be "much physical and chemical evidence" of the correctness of a theory that is admittedly wrong? The answer to this question goes back to a point which was brought out in the introductory chapter: the fact that prevailing practice tends to interpret observations which are *consistent* with a particular hypothesis as *proof* of that hypothesis. If we require the textbook authors to restrain their enthusiasm for currently accepted ideas and to limit their assertions to statements which they are prepared to back up with facts, they will have to amend their comments and say, in effect, "There is much physical and chemical evidence which is consistent with the modern atomic picture." But this, of course, will not support the previous conclusion that "there can be no reasonable doubt of its validity."

It not infrequently happens that a particular set of facts is consistent with many theories, and on the basis of present practice, the protagonists of all of these theories can (and sometimes do) offer exactly the same facts as "proof" of their respective contentions. The original Bohr theory is wrong, as the front-line theorists now admit, because it is inconsistent with *some* physical facts, but these fatal contradictions are not apparent in the less complex applications with which the elementary physics textbooks deal, and hence these text books can still offer evidence which is consistent with the original Bohr theory as "proof" of that theory.

It is the aim of this present chapter to show that most of the "physical and chemical evidence" to which the textbook writers refer is equally as consistent with many other hypotheses as with the theory of the nuclear atom, and that it therefore is not a proof of any hypothesis. Since this is the case, collapse of the nuclear theory does not cause the widespread dislocation that would ordinarily be expected; in general the previous conclusions expressed in terms of the nuclear atom remain just as valid as ever; all that is necessary is to change the language in which they are expressed. This does not weaken the previous conclusions in any essential respect. To borrow an expression from another field of human activity, "A rose by any other name would smell as sweet." All that we need to do in this instance to accommodate our experimental findings to the absence of the nuclear theory is to provide some new names for our old roses.

One major class of items of this kind is illustrated by Moseley's Law, which is widely hailed as one of the bulwarks of modern atomic theory. Holton tells us, for example, "In the year 1913... there came yet another profound contribution (to the theory of the nuclear atom)."[60] Let us see just how "profound" this contribution actually is.

Moseley's work, in essence, established a mathematical relationship between the atomic number and the frequencies of the characteristic x-rays of the various elements. His unimpeachable conclusion, as quoted by Holton, is that "We have here a proof that there is in the atom a fundamental quantity, which increases by regular steps as we pass from one element to the next." But then he, too, falls into the trap unintentionally set by Rutherford, and goes on to say, "This quantity can only be the charge on the central positive nucleus, of the existence of which we already have definite proof."

Let us bear in mind that this conclusion was reached in 1913, only two years after the formulation of the nuclear postulate, and the "definite proof" to which Moseley refers was furnished by Rutherford's interpretation of the results of his scattering experiments, which we now see is wholly unjustified. Here, and in the original work of Bohr, carried on contemporaneously, we can see the beginning of the great build-up, in which a conclusion of Rutherford's, never properly substantiated and now completely refuted, has been pyramided step by step into the great mass of detail that now constitutes the nuclear theory of the atom. Like Bohr's theory, which is preposterous unless the existence of a nucleus is previously established beyond all reasonable doubt, this second conclusion of Moseley's falls flat unless it is preceded by the "definite proof" of which he speaks. But since Rutherford's hypothesis as to the existence of a nucleus was accepted without question, these two subsidiary theories, Bohr's and Moseley's, which could not have been entertained at all if the nuclear hypothesis had been subjected to any reasonably careful analysis, were likewise accepted. In the next step these Bohr and Moseley concepts were utilized to "prove" still other conclusions, and the same process was repeated over and over again until the present imposing structure was built up.

Holton's characterization of Moseley's findings as a "profound contribution" to the nuclear theory is an example of a widespread misconception as to the true status of the so-called "evidence" in favor of the theory. This statement reveals a mental image, apparently shared by most physicists, in which the various items of "evidence" are regarded as independent and cumulative. Rutherford first arrives at a conclusion. Moseley makes some further discoveries and arrives at another conclusion, based on these discoveries, which is consistent with that of Rutherford. Bohr does the same, and so on. According to this popular point of view, each additional finding reinforces what has gone before. The findings of Rutherford plus those of Moseley are more secure than those of Rutherford alone. Bohr's results add still more solidity to the structure, etc.

If these various findings were truly independent, this point of view would be entirely justified. The work of Einstein on the photoelectric effect, previously mentioned, is in this category. Einstein utilized the theory developed by Planck, but his findings were not in any way contingent on any previous proof of that theory; on the contrary, his demonstration of the validity of Planck's theory in application to the photoelectric effect would have remained valid even if Planck's own conclusions with respect to the distribution of frequencies in black-body radiation had turned out to be in error. Under these conditions Einstein's work actually was a "profound contribution" in support of Planck's theory. But the findings of Moseley, so far as they have any bearing on the nuclear atom theory, are *not* independent of those of Rutherford. Unless the "definite proof" of the validity of Rutherford's hypothesis, to which Moseley refers, is forthcoming, Moseley's experimental results cannot be connected with the nuclear atom at all. Similarly, Bohr's conclusions, as already pointed out, are completely dependent on the validity of Rutherford's hypothesis. Hence Rutherford plus Moseley plus Bohr are no stronger than Rutherford alone. Since he falls, then all fall. This is a general principle, applicable in all cases where an entire structure of theory is pyramided on one basic hypothesis and is completely dependent on the validity of that hypothesis.

Pursuing this subject farther, let us examine the nature of Moseley's results. Since he was unable to look behind the facade and see that Rutherford's hypothesis might be wrong, Moseley concluded that the "fundamental quantity," the existence of which was indicated by his experiments, was the nuclear charge. But the charge, as such, does not enter into Moseley's mathematical expressions of his results. In these expressions the "fundamental quantity" enters only as a dimensionless number. Clearly this represents a number of units of some kind, but the nature of these units is not indicated by Moseley's findings, and the kind of unit involved is completely immaterial so far as the relationship expressed by Moseley's Law is concerned. The element potassium, for instance, must contain 19 units of some kind in order that the relationships which Moseley established may be satisfied, but they can be 19 units of any kind, without restriction.

It is quite obvious that *any* atomic theory that might be seriously proposed in the light of present factual knowledge must provide for some quantity corresponding to the atomic number: some quantity which, as Moseley says, "increases by regular steps as we pass from one element to the next." But this means that this theory, whatever it may be, is automatically in conformity with Moseley's Law. The contention that Moseley's findings constitute a "profound contribution" to the nuclear theory is thus completely erroneous. These findings are consistent with *any* plausible atomic theory and they cannot be uniquely connected with the nuclear theory unless that theory is first proved correct by some other means.

Essentially the same thing can be said about every item of this kind that is now being advanced as "proof" of the modern atomic theory or some specific phase of that theory. All of these items purport to show that the observed relationships confirm the existence of certain numbers of electrons, protons, neutrons, electric charges, etc., in the atom, but when we examine the alleged evidence we find that in no case do such particles or charges actually enter into the relations that are established experimentally. The entities which appear in the mathematical expressions are dimensionless numbers in all cases, just as in Moseley's Law, and the observed facts give us no indication as to what kind of units might be involved. The labels that are currently attached to these numbers come from the theory, not from experiment. The mathematical expressions that have been derived from the experimental work are consistent with the currently accepted theories, to be sure, but they are equally consistent with any other theory which arrives at the same numerical values, regardless of the names which such other theory may attach to the units, and since these numerical values are all related to some quantities such as the atomic number or atomic weight, for which *any* theory must furnish an explanation, agreement with the observed mathematical relations is no problem for any theory. If it does not come automatically, as in the case of Moseley's Law, it can certainly be achieved with no more manipulation than is required in current practice.

The situation is altogether different in the case of relationships in other areas of physical science where the pertinent terms in the mathematical expressions that define these relations have specific dimensions. If our answer has the dimensions of force, then we must somewhere have a term with the dimensions of mass, and where ten units of this kind are involved they must be ten units of mass; they cannot be ten units of any other kind. But the relations which are supposed to involve numbers of electrons, etc., are not of this character. These electrons, or other hypothetical particles, do not have any unique significance in relation to the experimental results; the numbers are dimensionless, so far as the experimentally observed relations are concerned, and if anyone chooses to say that they refer to some units other than electrons, this is equally consistent with the observed facts.

We may conclude, therefore, that in general the substitution of some other theory for the present theory of the nuclear atom will not affect relations of the kind just discussed, except that different language will have to be used. Wherever these items are consistent with the nuclear theory, they will be equally consistent with any other plausible theory that may be proposed.

The points which have been brought out with reference to the lack of any definite connection between Moseley's Law and the nuclear theory apply with equal force to the items which are advanced as "proof" of the validity of Bohr's model of the atom, but in view of the major role which these items have played in building the nuclear atom theory up to its present quite undeserved eminence, it will be in order to discuss this situation more specifically. The exact status of these so-called proofs is, of course, rather vague, now that the original Bohr theory has been officially repudiated. It is not uncommon in scientific practice to have some alleged proof of a theory refuted, without seriously affecting the standing of the theory itself, but here we have the extraordinary situation of a theory being abandoned, leaving its proofs still standing, and still being taught on a wholesale scale in our universities. This has somewhat the same flavor as Victor Borge's account of the cure for which there is as yet no known disease.

The original and most impressive success scored by Bohr was his interpretation of the line spectrum of hydrogen. J. J. Balmer discovered empirically in 1885 that the principal series of lines in the hydrogen spectrum can be represented by a mathematical formula, which in its modern form is expressed as R $(1/4-1/b^2)$, the factor btaking successive integral values beginning with 3. Subsequently it was found that this Balmer formula is a special case of a general expression R $(1/a^2-1/b^2)$ in which both a and b take successive integral values. When $a= 2$, the Balmer series results. Other values of a produce the Lyman series, the Paschen series, the Brackett series, and so on. Later, in 1908, Ritz extended these findings to line spectra in general by means of his Combination Principle, which asserts that *any* spectral series can be represented by a similar combination in which the first term remains constant while the denominator of the second term assumes successively higher values.

At this stage Bohr entered the picture and addressed himself to the task of discovering the reason why each spectral series assumes this particular mathematical form. He was convinced from the start that the then very recent discovery by Planck of the existence of discrete units or quanta of radiant energy would provide the key to the problem, and the integral values of the factors a and b in the modified Balmer formula obviously fitted in very well with this idea. The question then arose: What is the nature of these two terms that enter into the Ritz combinations? Bohr's answer to this question constitutes his major contribution to the theory of spectra. He reasoned that since the quantum of radiant energy is a function of the frequency of the radiation, the frequency represented by the difference between two Balmer or Ritz terms is a quantity of energy. From this it can reasonably be deduced that those terms also represent energy, and Bohr thus arrived at a picture in which the atom is able to assume certain specific energy levels and emits or absorbs radiation when it changes from one of these permissable energy levels to another.

This has been a very fruitful concept, and it has provided a solid foundation upon which it has been possible to build a logical and systematic classification of atomic spectra. As matters now stand, the identification of the terms in the spectral formulas as atomic energy levels appears to be firmly established. Here is a significant scientific accomplishment that can be credited to Bohr and those who have followed in the paths which he originally defined. But the scientific community has not stopped with a well-deserved approbation of this important step forward; it has gone on to accept this accomplishment as evidence confirming the validity of present-day atomic theory: something for which there is absolutely no justification.

It may be mentioned in passing that Planck's quantum is commonly termed a quantum of "action," because the constant h has the dimensions energy x time, but there is no indication that this so-called "action" has any significance so far as the atomic spectra are concerned. It is, in fact, quite doubtful if "action" has any physical significance at all. The quantity which enters into the spectral relations is the quantum of energy, which is the product of Planck's constant h and the frequency of the radiation.

Now let us go back for a moment to Bohr and his original work. Having arrived at the concept of discrete energy levels from an adaptation of Planck's quantum hypothesis, the next objective was to connect this with Rutherford's atom-model, the validity of which Bohr accepted without question. In this model the electrons circling around the central nucleus must possess kinetic energy and it seemed logical to assume that the energy level changes corresponding to the observed radiation reflected changes in the electronic motion. Here the established laws of physical science were in direct conflict with the new ideas, but Bohr was already prepared to throw these laws overboard. "... Rutherford's discovery of the atomic nucleus revealed at once the inadequacy of classical mechanical and electromagnetic concepts ,"61 he tells us, and on the basis of this philosophy, he postulated that the electrons are able to occupy only certain specific orbits defined by quantum considerations, that they do not radiate while moving in these orbits, and that they possess the ability to jump from one orbit to another and, in so doing, to emit or absorb radiation with a frequency corresponding to the difference between the energy levels of the two orbits.

If we appraise this "solution" of the problem from a cold-blooded scientific viewpoint, it is clear that while it is an answer to the problem, there is nothing at all to indicate that it is the *correct* answer. Furthermore, it is not even a very plausible answer. In order to accomplish his objective of connecting Rutherford's atom-model with the discrete atomic energy states, Bohr had to use three completely unprecedented postulates in direct contradiction to established physical principles; that is, it took three wild cards to win the trick. As Lanczos expresses it, "The principles from which he [Bohr] developed his model were incomprehensible and, in fact, hardly credible."19 Now after more mature consideration, the front-line theorists, including Bohr himself, tell us that it was all a mistake, that there are no specific orbits, that the electron itself is only a "symbol," and so on.

The most astounding feature of this whole situation is that after having thrown the only connection between the atomic energy levels and the nuclear atom to the wolves, and putting nothing in its place-even going to the extent of contending that nothing *can* be put in its place-the physicists still insist on using the genuine successes of the theory of atomic energy levels as evidence of the validity of the nuclear theory to which it is no longer even distantly related. Furthermore, the whole jargon of spectroscopy is based on the features of the original theory that have now been repudiated. While Schroedinger tells us that there really are no electrons in orbit, Heisenberg says that there actually is no physical electron at all, only a "symbol", and the whole Copenhagen school insists that we cannot conceive of the atom or any of its parts in anything but purely mathematical terms, the spectroscopists tell us just how many electrons there are in the atom and exactly how they are arranged in "shells," etc., and proceed on this basis with the calculation of spectroscopic terms to an accuracy of eight or nine significant figures. "The terms result from definite configurations and motions of the outer electrons of the atom and are explained by a well-established theory of spectral structure,"[62] says the National Bureau of Standards.

This utterly ridiculous situation in which one group of physicists is defining specifically and in great detail the properties of entities which, according to an even more eminent group of physicists, have "no immediate and direct properties at all " and do not even "exist objectively" is another example of the same confusion that was pointed out in connection witll Moseley's Law. Here again, as in the Moseley case, a name derived from currently popular theory has been arbitrarily attached to a particular physical phenomenon, and the scientific profession has fallen into the habit of accepting the connotations of that arbitrary nomenclature on the same basis as the observed properties of the phenomenon itself. The Bureau of Standards tells us, "... the atoms of a gas or vapor, when excited by radiation, absorb certain wavelengths corresponding to transitions of their outer electrons from lower energy levels to higher ones." Here we have an assertion which contains three statements of totally different origin, all lumped together as if they were equally authoritative. The statement that the atoms absorb certain wavelengths when excited by radiation is a description of an observed fact. The statement that these particular wavelengths correspond to transitions from lower to higher energy levels is a theoretical conclusion which is strongly backed by evidence from experimental sources. The further statement that these energy levels are energies of electrons is pure hypothesis without the least vestige of experimental support. These levels are "Atomic Energy Levels"-the title of the Bureau of Standards publication from which the foregoing statement was taken-and that is all we know. Evidently some kind of units enter into the situation, but applying the name "electrons" to these units is pure guesswork.

Similarly the same N.B.S. circular says, "Each chemical element can emit as many atomic spectra as it has electrons," but what is actually known is that the number of different spectra is equal to the atomic number; the further conclusion that this represents a number of electrons is wholly gratuitous, and the use of the name "electron" serves no purpose that would not be fulfilled equally well by any other name. As pointed out earlier, any theory of atomic structure that could be given any serious consideration at all must necessarily make some provision for a quantity corresponding to the atomic number, and the name of this "something," whatever it may be, can be substituted for "electron" in the language of spectroscopy without affecting spectroscopic theory in the least. The current contention that the successful application of the theory of energy levels to the study of spectra constitutes an argument in favor of the nuclear theory of the atom is therefore pure fantasy.

The most spectacular research activity currently under way in the physical sciences, aside from the space exploration program, in which the increase of scientific knowledge is a secondary objective, is the concentrated attack that is now being made on the baffling problems of the atomic "nucleus." A great army of research workers equipped with a vast array of amazingly accurate instruments and ingenious devices of all kinds, including huge and enormously expensive machines unprecedented in scientific research, is making a powerful and determined effort to find the answers to the many unresolved questions in this area. To those who are actively engaged in this interesting and formidable task, the conclusion that there is no such thing as a nucleus is likely to seem the height of absurdity. But they should be reminded that "nucleus" is merely a label, and that natural phenomena do not come equipped with labels; the labels are put on afterward. The experiments and observations only reveal the existence and properties of a "something"; the further assertion that this "something" is a nucleus is tacked on later by the theorists.

The conclusions that have been reached in the preceding pages as a result of a critical and comprehensive analysis of the present state of atomic theory in the light of the vast amount of experimental evidence now available do not deny the reality of the "something" that these workers are investigating or the validity of the experimental information that has been accumulated regarding the properties of that "something." They merely indicate that when the "something" of the experiments is translated into the language of physical theory, the proper word in that language is "atom," not "nucleus."

One group of investigators, for example, is busily engaged in measuring what they call "nuclear cross-sections." They accelerate particles of various kinds to high velocities and project them against matter, observing in detail the effects produced. But these investigators do not actually know that they are dealing with a "nucleus." They are measuring cross-sections of a "something," and the idea that this "something" is a nucleus is a purely theoretical interpretation that is completely independent of the experimental work. Hence rejection of the nuclear theory introduces no complications here. We simply alter the terminology and speak of atomic cross-sections rather than nuclear cross-sections, and everything else goes on just as before. Much the same comments can be made about the greater part of the other experimental work now being done in this area. The properties that are currently attributed to the nucleus are in most instances equally appropriate to the atom. When we measure size, shape, mass, magnetic moment, etc., we can simply change the terminology from "nuclear" to "atomic" and transfer the essential meaning intact from one concept to the other.

In some other instances the theoretical interpretations and labels are introduced at a lower level, and current ideas as to what actually takes place in the experiments are influenced to a considerable degree by the theoretical viewpoint of the investigator. This is not peculiar to studies of atomic structure; it is a general situation encountered in all investigations of a complex nature, and James B. Conant give us this warning, "I think that a certain degree of caution is appropriate in reading some of the popular expositions of the implications of the new physics. For in simplifying a complex experiment, the writer is almost forced to intrude an interpretation before he draws the conclusion from the evidence."[63]

A conclusion such as this, "The relative stability of the deuteron... shows that the force between a proton and a neutron... is of appreciable magnitude,">[64] is not a statement of an experimental finding; it is an interpretation of the experimental results on the basis of the author's theoretical views. All that the experiment shows is that the deuteron is relatively stable; the rest is theory. In order to be entirely accurate *all* such statements as the one just quoted should be preceded by the same kind of an introduction which appears in the same textbook a few pages later: "An interpretation of these results has been based on the supposition that...." Statements about properties of the "nucleus" are all interpretations based on suppositions. The collapse of the nuclear theory will make it necessary to discard all of these current interpretations and go back to the actual observed facts for a fresh start. In the case cited, we will simply have to accept the fact that all we *know* is that the deuteron is relatively stable, in that it takes about 2.18 m.e.v. to break it up, and we will have to build a new theory from there. We cannot even justify calling the 2.18 m.e.v. a "binding energy," since we are not sure that there are any separate parts to be "bound."

It is to be expected that the theorists working in this field will take a very dim view of this conclusion that the products of their labors must be relegated to the wastebasket, but this issue must be faced, nevertheless. Even the highest degree of competence cannot derive the right answers from the wrong premises, and Gamow's statement, previously quoted, that "not a single successful step has been made" in this area "in the last few decades" shows how completely unproductive the present line of approach has been. Now that it has been demonstrated that, whatever the structure of the atom may be, it is certainly not a combination of any of the observed sub-atomic particles, it is obvious that the severe limitation which has been placed on the thinking of the theoretical physicists by the presumed necessity of accommodating the structure of the atom to the properties of these sub-atomic particles has been the major obstacle in the way of forward progress. A new start without the burden of this fatal handicap is essential.

Chapter VII
Electric Questions

The two preceding chapters have shown that in most cases the collateral relationships which are now interpreted in the light of the nuclear theory will not be altered, except as to the nature of the language in which they are described, by the collapse of that theory and the consequent necessity of substituting something else, and that in the most notable instance where one of the subsidiary structures does fall along with the nuclear concept itself, the structure that is destroyed is one whose loss need not be mourned. There is, however, one other casualty that warrants some special comment.

The objective of the presentation in this volume is to show that the nuclear theory of the atom, the concept of the atom as a minute positively charged nucleus surrounded by electrons bearing oppositely directed (negative) charges equal in magnitude, is not valid in the Iight of existing factual knowledge. No attempt has been made in the preceding pages to inquire into the validity of the electrical theory of matter in general, since this is, to some degree at least, a separate question, inasmuch as a demonstration that the concept of an atom constructed of certain specific kinds of charged particles arranged in a specific manner is no longer tenable does not necessarily exclude the possibility that electric charges may still be important factors in the atomic structure, nor does it necessarily invalidate the assumption that the inter-atomic forces have an electrical origin. It is probably apparent to most readers, however, that the powerful arguments now available against the nuclear hypothesis, particularly those which stem from the experimental discovery that the electron is not a permanent "building block" type of entity, but a particle that can be created or destroyed with relative ease, will also deliver the *coup de grace* to the electrical theory of matter as a whole, when these arguments are appropriately developed. In order to avoid leaving the impression that this conflict with a popular and generally accepted theory in some way weakens the force of those arguments, it will be desirable to take a brief look at the inter-atomic aspects of the electrical theory.

To account for the existence of solid substances, at least two forces of an inter-atomic character are required, irrespective of the theoretical viewpoint from which the subject is approached. As indicated in Chapter 2, the weight of evidence now indicates that the atoms are widely separated and that the inter-atomic distance simply represents the point of equilibrium between the attractive and repulsive inter-atomic forces. The currently popular electrical theory postulates that the atoms are held in contact by the electrical forces of attraction, but this does not eliminate the need for a repulsive force; it merely puts this force inside the atom. There still has to be a force resisting deformation. This requirement of two opposing forces imposes a very severe restriction on the formation of theories of the constitution of matter, since the number of known types of force is extremely limited. Aside from the forces known to result directly from motion, such as centrifugal force, there are only three kinds of force of which we have any definite knowledge: gravitational, electric and magnetic. Of these three, the only one that appears to be strong enough to account for the cohesion of solids is the electric force. This, of course, creates a strong predisposition on the part of the scientific community to look favorably on any theory which attributes the observed phenomena to electrical causes, and to be very tolerant of the shortcomings of such a theory.

In view of this background, the discovery early in the nineteenth century that certain substances, on being dissolved, separate into positively and negatively charged ions, was naturally accepted as definite proof that matter is composed of charged particles and that the force of attraction between unlike charges furnishes the explanation of the cohesion of solids. The more recent finding that electric charges of all kinds are easily created and easily destroyed has, however, cut the ground out from under these conclusions. It is now generally admitted that at least some of the ionic charges must be created in the solution process, and since there is no good reason to believe that two different mechanisms are operative, this gives rise to a rather strong presumption that *all*ionic charges are created in the solution process and that no charges exist prior to solution.

The new information throws an altogether different light on the situation within the solid. As long as it seemed certain that the positively charged sodium ions and negatively charged chlorine ions which we find in salt solutions were obtained from the solid sodium chloride, then since we know that there is a force of attraction between oppositely directed charges, it was only natural to conclude, without going into the matter very deeply, that this force of attraction accounts for the cohesion of the solid sodium chloride structure. But if we approach the same question with the benefit of present-day knowledge, which tells us that the ions which we find in solutions are probably created in the solution process, so that we now have to face the basic issue as to whether or not any such ions are actually present in the NaCl crystal, the picture looks very much different. We are now confronted with the awkward fact that the existence of positive and negative charges in contact or in very close proximity is totally foreign to the known behavior of electric charges. Everything that we actually know about such charges indicates that they destroy each other on contact, and that mixtures of oppositely charged particles can exist only under conditions such as those in solution, where the charges are created as fast as they are destroyed.

Furthermore, there is no other evidence of the existence of solid ions that can stand up under critical examination. The complete lack of merit of one of the arguments commonly advanced for this purpose was pointed out in the introductory chapter. The same text from which this argument was taken goes on to characterize the high melting points of compounds of the NaCl type as evidence that they are composed of ions. But when this new argument is fully developed in the text we find that all it amounts to is that the higher melting points indicate that the force of cohesion is greater in these substances than in compounds of the organic type, and hence it is deduced that the nature of the cohesive force must be different. The conclusion is then drawn that since it is different, it must be ionic: an excellent example of the kind of reasoning that has to be used to bolster this electrical theory. The truth is that the solid state contributes no evidence of the existence of ions, and without the benefit of the positive knowledge that the occurrence of ions in solution was formerly supposed to provide, but no longer does, a consideration of all of the available facts must lead us to the conclusion that the atoms are not charged in the solid state, and that their cohesion is attributable to something other than electrical forces.

When we thus take off the rose-colored spectacles before looking at the ionic theory, it is also evident that the superficially attractive features of this theory apply only to one class of compounds, and that outside of this one class it is practically lost. The theory explains the cohesion of the so-called "ionic" compounds as the result of the attractive forces between ions of opposite charge, but there are "non-ionic" compounds, even more numerous than the ionic variety, which possess cohesive forces that, so far as we can determine, are qualitatively identical with those of the ionic compounds. It thus becomes necessary to advance two totally different explanations for what is, to all appearances, the same problem. This is typical of what the electrical theory encounters on every hand. If we attempt to get down to details, the situation becomes even worse. Here the theorists cannot even agree as to the manner in which the theory should be applied. Physicists give us one answer, says V. F. Weisskopf, chemists another, but "neither of these answers is adequate to explain what a chemical bond is."[65]

The physicists who are attempting to apply the latest quantum concepts to the problem have been singularly unsuccessful, if we appraise their results by any realistic standards. Some very broad claims on their behalf are often made by overenthusiastic supporters, the following from G. G. Hall being a typical example: "Quantum mechanics... gives the solution, in principle, to almost every chemical problem," but the true significance of such statements becomes apparent when Hall goes on to say, "Very unfortunately, however, there is an enormous gap between this solution in principle and the practical calculation of the properties of any specific molecule."[66]

The chemical theory is more completely developed but, as Weisskopf says, it does not answer the essential question either. This theory is based on the hypothesis that ions are created by the transfer of electrons from one component of the solid structure to the other. Since the inert gases have no tendency toward chemical combination, the originators of this theory deduced that the numbers of electrons presumably contained by these elements, 2, 10, 18, etc., must be unusually stable, and as the elements immediately adjoining the inert gases are very reactive and have unit positive and negative valences, it was further deduced that these elements have the ability to lose or gain electrons, thereby attaining the stable electron content of the inert gas and at the same time acquiring positive and negative charges respectively. Thus potassium, with 19 electrons, is assumed to lose one, reducing it to the same 18 status as argon and producing a singly-charged positive ion. Similarly chlorine, with 17 electrons, is assumed to accept the electron lost by potassium, producing a singly-charged negative ion, and increasing the chlorine electrons to the stable value 18. The cohesion of the compound KCl is then explained by the attraction between the positive and negative charges. So far as simple compounds of the KCl type are concerned, this theory is quite plausible, or perhaps it would be more accurate to say that the theory would be plausible if someone could come up with a reasonable explanation as to how the ions are originally produced. The test of such a theory, however, is not how well it agrees with the particular set of facts that it was specifically designed to fit, but how well it agrees with the other facts in the area it purports to cover, and this one runs into serious difficulties as soon as it gets beyond the KCI class of compounds. For instance, vanadium forms three binary compounds VO, VN, and VC, which crystallize in the same simple cubic structure as KCI, and so far as we can tell, are held together by the same kind of forces. But the theory that seemed so plausible in the case of KCI has a hard time explaining the composition of any one of these compounds, to say nothing of producing any explanation for the fact that this element has a different valence in each. In none of these cases does the number of electrons presumably transferred to the electronegative component leave vanadium with the 18 residue which is supposed to be so significant in the theory of the KCl structure.

By the time we reach the "non-ionic" compounds practically all resemblance to the original theory has been lost. Here the concept of achieving stability by transfer of electrons from one component to the other is clearly inapplicable. In the compound ClF, or the chlorine molecule Cl_2, neither component is in a position to give up an electron, since each is one electron short of the inert gas figure to begin with. A new idea is therefore introduced into the theory: that of "shared electrons." It is postulated that each chlorine atom in Cl_2 contributes one electron to a sort of common pool, under joint management, so to speak. Each atom then has 16 electrons of its own and is entitled to claim both of those in the joint account, making a total of 18 each, thus complying with the postulated requirements for stability.

For present purposes, an extended analysis of this "shared electron" concept is not necessary. It should be sufficient to point out first, that this is purely an invention designed to fit the existing situation, and not something derived from the underlying atomic theory; second, that it has all of the earmarks of a crude contrivance, indeed it is about as weird an idea as was ever advanced in the name of science; and third, that it makes no attempt to accomplish the primary objective of a theory of chemical combination, an explanation of the nature of the cohesion, and devotes itself entirely to considerations of a secondary nature.

The inability of the electrical theory to furnish a consistent and comprehensive explanation of the cohesion of solids, together with the absence of any valid evidence of the existence of ions in the solid state, indicates that it will be necessary to abandon this theory as a whole, along with the concept of the nuclear atom. As the situation now stands, there is nothing tangible to support any aspect of the electrical theory of matter. The theory maintains its standing only by reason of the inertia of familiar habits of thought and the general reluctance to abandon an explanation, however unsatisfactory it may be, as long as there is nothing available to take its place. This is one of those instances in which the scientist allows himself to be governed by his emotional reactions as a human being rather than by the objective principles of science. A strong reluctance to admit ignorance is a pronounced human characteristic, and in everyday life one of the most devastating retorts that can be made to a critic is to ask, "Have you anything better to offer?" From a strictly scientific standpoint, on the other hand, such a question is completely irrelevant; if the accepted explanation proves to be wrong and we have no acceptable substitute for it, good scientific practice requires us to admit that we simply do not know the answer.

Furthermore, even though this means that the scientific profession is faced with the difficult task of finding some other theory to replace the electrical theory of cohesion, a task which scientists can hardly be expected to welcome, it should be remembered that this necessity of finding some other explanation of the cohesion of solids has been present all the time; it has merely been pushed to one side and ignored. Even the most enthusiastic supporter of the electrical theory must admit that the theory does not pretend to explain the cohesion of *all* solids, and as long as the coverage is incomplete the search is not ended. None of the interpretations of the electrical theory thus far devised gives us any reasonable explanation of the cohesion of metals, for instance. As Weisskopf puts it, in an admission which follows the statement previously quoted, "I must warn you I do not understand why metals hold together." This situation cannot be ignored indefinitely. Sooner or later the issue must be faced, and since it will then be necessary to look for a completely new explanation in any event, the demise of the electrical theory does not change the over-all problem materially. In all probability, when we finally *do* discover the correct explanation of the cohesion of metals, we will have the correct explanation of solid cohesion in general.

Although the electronic theory of chemical combination is a distinct failure in the primary task of such a theory, explaining why the combinations hold together, it does a better job on some of the other aspects of the subject, particularly in connection with the properties portrayed by the periodic table. It should therefore be noted that whatever value these accomplishments of the theory may possess will not be lost when the electrical theory of matter has to be discarded, as these items can be transferred bodily to any new theory that is set up. The reason is that the structure of the electronic theory is almost entirely independent of the terminology employed, and replacement of the electron concept by something else is a matter of changing labels rather than of rebuilding the structure of the theory.

Most of the real knowledge in this area is numerical. As one chemistry textbook says, "The most important test which the theory of electronic configurations must meet, in order to satisfy the chemist, is that of providing an explanation for the periodic law,"[67] and the periodic table which expresses this law is a representation of a purely numerical relationship. Here we find that if we arrange the elements in the order of atomic number so that they fall into successive periods and into eight periodic groups, the chemical properties of these elements are then related to their positions in this table. Thus the elements in group one of the short periods and of the first half of the long periods constitute the well-known series of alkali metals: lithium, sodium, potassium, etc. The chemical properties of any one of these elements are very similar to those of the other members of this series, but quite different in many respects from those of other elements. Each of the alkali elements, for instance, has a positive valence of one, which is a condensed way of saying that an atom of one of these elements will form a stable compound of the NaCl class, or some equivalent, with an element of negative valence one, that two such atoms will form a stable compound similar to Na_2O with an atom of negative valence two, that such an atom will acquire a one-unit positive charge if the compound containing it dissociates on being dissolved, and so on.

The important fact that should be realized, so far as the point now at issue is concerned, is that all of these relationships are purely numerical, and that the *numbers* express the full extent of our actual knowledge in this field. The *name* "electron" is a purely arbitrary label derived from theory and attached by the theorists to the numbers that represent the actual meaning. This label does not enter into the relations in any way-all of these relations are based on dimensionless numbers-and replacement of the label "electron" by some other label will not alter these relations in the least.

It should also be noted that the numerical relations were not derived from the atomic theory. These relations are primarily a result of the periodicity; that is, of the existence of distinct groups of elements. But there is nothing in the nuclear theory of the atom to indicate the existence of such groups. The nuclear hypothesis does not require, or even infer, such an arrangement. The only numerical property of the electrons that is inherent in this hypothesis is the total number of electrons in the atom. The further conclusion that the electrons are arranged in groups or shells is simply an *ad hoc* assumption formulated to fit the observed facts. "It became clear at once," says Slater, "that several different assumptions were required to give a consistent statement of the principles underlying the structure of the periodic system."[68] In other words, the theory does not explain the facts; the facts explain the theory. It is the existence of the experimental facts with respect to the periodic properties, chemical and spectroscopic, that has dictated the form of the assumptions and has thus shaped the electronic theory. The important feature of that theory is not what came from the atomic theory-the label "electron" that does not enter into the mathematical expressions at all-but what was put in by means of the assumptions–the specific numbers of elements in the successive groups.

Present-day textbooks tend to give the impression that the knowledge in this field is a product of the electron theory, but this is not true; the knowledge came first and the electron theory was hooked on afterward. The periodic table was devised by Mendeleeff in 1869, a full quarter of a century before the electron was discovered. The actual procedure which was followed was not to arrive at an answer by developing the consequences of the basic theory, as it is now commonly portrayed, but to devise a scheme for filling in the gap between the atomic theory and what necessarily has to be the end result: the existing chemical knowledge. It is quite obvious that any other theory of atomic structure that might be proposed can be connected with the periodic table and other chemical properties by exactly the same means; the only difference, aside from minor details, will be that the numbers expressing such factors as the positions of the elements within the periods will no longer be identified by the name "electron" but by some other designation. Whatever degree of validity the electronic thory may possess will then be equally applicable to this new theory.

When the *correct* theory finally appears, the situation will, of course, be quite different. The correct theory of atomic structure must necessarily be of such a character that it not only carries with it a "built-in" explanation of the inter-atomic forces, but also an explanation of the periodic groupings, which will permit the theory of the periodic system to be derived directly from the atomic theory without the necessity of any *ad hoc* assumptions. In the meantime, since the chemists have to content themselves with something less than this correct theory, there is no reason to believe that there will be any particular difficulty in fitting the chemical picture to whatever new theory of the atom the physicists may devise. It should actually be a relief to be rid of the necessity of dealing with such patently forced concepts as "shared electrons" and "resonance."

All the way through these three chapters which have been devoted to a discussion of the various collateral subjects that are tied in to the atomic theory in current scientific thought, essentially the same situation has been encountered. Instead of being products of the atomic theory, as is now generally contended, the developments in these collateral fields-atomic energy levels, periodic properties of the elements, mathematical correlation of x-ray spectra, etc.-have taken place independently, and the connection with atomic theory exists only through the agency of the *language* that is used to describe the observed facts and the conclusions derived therefrom.

If we appraise the general situation in these fields critically, it is apparent that most of the knowledge that does exist is purely mathematical. The matter of interpretation of the mathematical relationships thus becomes extremely important and, as H. Margenau points out in a recent work, since the non-mathematical information of a factual nature is so meager, it is seldom possible to subject an interpretive hypothesis in these areas to any conclusive tests. The physicist thus "has an embarrassing amount of freedom in making his interpretations,"[69] as Margenau expresses it. Under these circumstances common prudence certainly calls for the exercise of particular care in distinguishing these untested interpretations from established facts. Had such a policy been followed in the past, this present discussion would have been wholly unnecessary, as it is quite obvious that the identification of the units that enter into the theory of spectra, the theory of chemical combinations, etc., as "electrons" is simply one of these unverified interpretations.

It has been apparent ever since Moseley published his findings in 1913 that the atomic number is an important basic feature of the atom; that we have, as Moseley said, "a fundamental quantity, which increases by regular steps as we pass from one element to the next." But Moseley and his contemporaries were under the impression that they had "positive knowledge" of the existence of an atomic nucleus, and they therefore identified these Moseley units, as we may call them, with the positive charge on the hypothetical atomic nucleus, and by extension, with the number of electrons necessary to neutralize this positive charge. From then on, the genuine significance of the Moseley units, which has become more and more apparent as the scope of physical knowledge has widened, has been mistakenly attributed to protons and electrons, and has constituted the bulk of the "evidence" offered in support of the present-day atomic theories.

If we analyze this situation rather than just taking it for granted that current thinking is correct, we find that the significant statements that are now made about atomic electrons are actually statements about the Moseley units, and they are valid only to the extent that the word "electron" can be used as a synonym for "Moseley unit." Whenever an attempt is made to go beyond this point and introduce some meaning appropriate to the experimentally observed electron but not to the Moseley unit, this leads to trouble. The endless difficulties that have been experienced in the attempt to treat the atomic electron as a particle, which have culminated in the complete surrender of the front line theorists, who now say that it is not a particle, but only a "symbol," have already been discussed. The explanation is that the real electron, where we actually know it, is a particle, but the Moseley unit is not. Similarly, the impasse that is faced whenever any attempt is made to apply electronic theory to the metals is a result of trying to force one of the characteristic properties of electrons, their negative charge, into a situation where it does not belong. The units of the so-called "electronic theory," the units which arrange themselves into periods and groups, are actually Moseley units, not electrons, and there is no reason to believe that the Moseley units are charged; on the contrary, the fact that current theory finds it necessary in some cases to identiry them with the negatively charged electron, and in other cases to identify them with the positively charged proton, strongly suggests that they are not charged. The complete indifference of the cohesive forces in the solid state to the direction of the hypothetical ionic charge (an atom of an electropositive element will join another atom of the same element, or an atom of another electropositive element, just as readily as it will join an atom of an electronegative element) points to the same conclusion. Use of the term "electron" instead of "Moseley unit" therefore introduces from the *language* that is employed, a foreign element, the electric charge, that is not present in the phenomenon itself. This, of course, has the effect of confusing the whole situation.

The collapse of the nuclear theory of the atom does not destroy the electronic theory of chemical combination or other similar theories; it merely necessitates abandoning the use of the terms "electron" and "nuclear charge" in connection with these theories, and recognizing that the units to which these terms are currently applied are in reality Moseley units: entities about which little is known with certainty, other than that they are units of atomic number, but are not individual particles and are not associated with electric charges. One of the requirements of a fully satisfactory theory to replace the nuclear theory of the atom is that it should provide an adequate explanation of the origin and nature of these Moseley units. It is quite possible that this development may result in some rather drastic revisions of existing thought, but in the meantime, until such a theory appears, these various side branches of the atomic theory can continue along substantially the same lines as heretofore simply by substituting "Moseley unit," or some equivalent term, in those place where the expressions "electron" or "nuclear charge" are now employed.

Chapter VIII
Pictures vs. Models

At this point it may be well to say that condemning the nuclear theory of the atom on the grounds that it does not present a true picture of the atomic structure is, in a way, somewhat unfair to the originators of the theory, in that we are condemning their product for not doing something that it was never designed to do in the first place. Rutherford did not discover the structure of the atom; what he did was to construct a *model* atom which would be in accord with the results of his experiments.

In the earlier days of the nuclear theory this was well understood. A particularly good discussion of this point, which could profitably be consulted by present-day students of the subject, appears in Karl Darrow's "Introduction to Contemporary Physics,"[70] published in 1926, only fifteen years after the original formulation of the theory. Darrow points out that every atom-model is designed to fit only "a very small fraction of the available knowledge about properties of matter," and that "... if we were to demand that an atom-model be compatible with all known phenomena not one of those now in the field (including Rutherford's) would survive." The use of models is a very convenient and helpful device in the development of physical theory, but it should be remembered that a model is not a picture; as Schroedinger tells us, it is "only a mental help, a tool of thought."[71] In adopting such a model as a conceptual tool we are not in any way committing ourselves to the belief that it is a true representation of the physical facts. Schrodinger himself accepted (with some reservations) the currently popular nuclear atom as a model, a useful conceptual device, but it is evident from the statement which was quoted at the end of Chapter 3, that he did not regard it as a picture. He was willing to accept the orbiting electron as a "mental help," but he makes it clear that if he could look inside an atom he would not expect to find any electrons in orbit.

Of course, it is the hope of everyone who invents a model to represent some physical entity that sufficient factual evidence may ultimately be discovered to advance this model, either in its original form or after suitable modification, to the standing of a picture: a true representation of the facts. Occasionally this happens. The atom itself was originally nothing but a model-a model of the structure of matter-but we now feel certain that atoms actually exist, since we have accumulated a vast amount of information pointing to the existence of such units and no significant evidence to the contrary. Bridgman says, "It is one of the most fascinating things in physics to trace the accumulation of independent new physical information all pointing to the atom, until now we are as convinced of its physical reality as of our hands and feet."[72] But the number of models proposed is greatly in excess of the number of items for which models are needed, and most models must therefore expect to fall by the wayside sooner or later when the progress of experimental knowledge advances to a new stage.

It does not necessarily follow that these discarded models are entirely without merit. As pointed out in Chapter 4, the mere fact that a hypothesis is advanced at all is an indication that it contains a certain amount of truth (a model is, of course a hypothesis), and even though the remaining aspects may be totally in error, this content of truth may be sufficient to make the hypothesis very definitely useful. Bohr's original theory of the atom, for example, was based on a completely erroneous assumption, as has been pointed out in the preceding pages, and has now been repudiated by the originator and his associates (on other grounds), but it cannot be denied that it has served a very useful purpose. It is true that the extent of the contribution made by this theory is generally overestimated. When we find the achievements of the Bohr atom in connection with the interpretation of the hydrogen spectrum hailed with such praises as "This is success beyond expectation,"[73] it would be well to remember that equally competent observers look at the situation in this manner, "The fact that it gave the correct energy levels for hydrogen is an accident; it failed badly even for helium."[50] It is also evident from the facts brought out in this present work that the advance of physical knowledge has been seriously handicapped in recent years by the retention of Bohr's basic ideas long after their usefulness had come to an end. But even so we can legitimately give this model considerable credit as an interim expedient pending the accumulation of sufficient experimental knowledge on which to build a better model.

Darrow points out that the same is true of the "billiard ball" model of the atom. It served very satisfactorily as a basis for the kinetic theory and associated developments, and these achievements will stand to its credit even though it had to be superseded when a wider range of atomic properties was discovered.

When the time comes that a model is no longer adequate to meet the demands upon it, good scientific practice requires that it should be ruthlessly cast aside, no matter how faithfully it may have served in the past. Unfortunately, this is easier said than done. A model, or an idea of any kind, which has achieved general acceptance and has had a degree of success establishes itself firmly in the structure of contemporary thought and is extremely difficult to dislodge. As Alfred Lande puts it, the revolutionary hypotheses of yesterday are "today hardened into axioms."[74] So it has been with the nuclear atom-model. It was quite clear even in 1926 when Darrow's book was published, that this model was far from satisfactory and the possibility of utilizing some different line of approach to devise a new model was already being considered, but as Darrow says, "The desire to retain the nuclear atom-model, and the simple and beautiful explanation of alpha particle deflections which it provides, discouraged and continues to discourage any such attempt."

This is certainly a striking example of what Louis de Broglie characterized as the "tyrannical influence of certain conceptions that finally came to be considered as dogma."[24] Here is a model, a purely hypothetical atom, that provides a "simple and beautiful" explanation of *one* of the many properties of the atom, and this one success is then allowed to override all other considerations. Ironically, we now find, on more careful examination of the significance of Rutherford's findings, that we can keep this "simple and beautiful" explanation without the nuclear hypothesis-without any new hypothesis at all-but for more than fifty years the scientific world has been so obsessed with the idea that Rutherford "discovered" an atomic nucleus that any conflicting fact has simply been pushed out of the way, no matter how drastic a measure has been necessary to accomplish this pllrpose. Where the theory stands in direct contradiction to the observations or established physical principles, the contradictions are removed by postulating that they do not exist; where there are discrepancies in numerical values, a Principle of Uncertainty makes such discrepancies legal; where causal connections cannot be found, causality is outlawed. Now we have the weird spectacle of the experimental physicists, confident that the atom is constructed of protons, neutrons, electrons, etc., in specific numbers and specific arrangements, spending countless hours trying to determine the exact details of these arrangements, while at the same time the theorists who are responsible for dreaming up the nuclear theory in the first place tell us that this is all a mistake, that the atom has no "immediate and direct properties." In the words of Jordan, "The atom, as we know it today... can only be characterized by a system of mathematical formulae."[75] Heisenberg expresses exactly the same thing even more forcibly in the statement quoted in Chapter 5.

Surely it is not unreasonable to expect the physicists to do better than this. After all, we are well into the last half of the twentieth century. It is over 2000 years since Democritus and his colleagues first advanced the atomic concept, and by this time if we do not have a true picture of the atom, we should at least have an atom-model that we can all use, rather than having one for the educators, another for the experimental physicists, and a third for the theorists. But we have gone astray because we have allowed the distinction between a model and a picture to become obscured. If we had a reliable *picture* of the general structure of the atom, definite knowledge with accurate factual confirmation, then there would be some justification for the enormous amount of time that is being spent investigating the specific cletails of this structure. H. A. Bethe tells us, for example, that the study of the so-called "nuclear force" has consumed "probably more man-hours than have been given to any other scientific question in the history of mankind."[76] But if we realize that the nuclear atom is only a *rnodel*, a purely imaginary concept invented to explain a *part* of the behavior of the atom, the expenditure of this much time and effort on any specific detail is completely out of order. When and if we meet an unusually difficult obstacle of this kind in the development of the consequences of a model, the logical conclusion is that the model itself is inadequate, and we should direct our attention to the necessary reconstruction or replacement of the model, rather than wasting our time and resources on futile efforts to discover by experiment features of a model that actually have no counterparts in the physical world.

A model becomes a picture when it meets the requirements for proof outlined in Chapter1; that is, when it can be shown that the model and the consequences thereof are consistent with the observed facts in a large number of random cases throughout the areas involved, without exception, and without the use of contrived methods of evading contradictions and inconsistencies. The atomic theory itself-a model of the structure of matter-has qualified under these strict requirements and, as Bridgman pointed out in the statement previously quoted, we now regard this theory as a picture of the situation that actually exists. This model has graduated into the more advanced class.

It is obvious that no atom-model yet proposed comes anywhere near meeting these requirements. Even if we were to take the modern theories at their face value, without regard to the skeletons in the closet that have been exposed in the previous chapters of this book, no such model even attempts to cover the whole field, and every one is beset with inconsistencies which cannot be overcome except through the use of unprecedented numbers of *ad hoc* assumptions. All of the present-day atom-models are therefore purely models; none of them has the slightest claim to the status of a true picture of the actual physical atom. As Schroedinger reminds us, the goal of a real understanding of atomic structure is still far away.

A recognition of this point is all that is needed to clear up a large part of the confusion that now exists in atomic circles. An outside observer cannot fail to marvel at a situation in which the leading theorists insist that the atom has "no immediate and direct properties," while at the same time their colleagues in the laboratories are enjoying remarkable success in observing and measuring these properties which the theorists say do not exist. But if we realize that the theorists are dealing with models whereas the experimenters are dealing with actual physical atoms, the anomaly disappears. When Heisenberg tells us that the atom has no properties and can only be symbolized by a mathematical equation, he is not talking about an actual physical atom, the kind of an atom that is being studied in the laboratories. He may *think* that he is, but he is not speaking on the authority of experimental facts-the observed facts flatly contradict him-and he has no *picture* of the atom that he can use as a basis for conclusions that will be applicable to the physical atom.

In effect, he concedes this point when he says, "... the new mathematical formulae no longer describe nature itself but *our knowledge* of nature."[77] If our knowledge of nature is complete and correct, then a description of our knowledge is a description of nature, and by making a distinction between the two, Heisenberg is admitting the obvious fact that the "knowledge" of which he speaks, the incomplete knowledge of the atom represented by the atom-model which he champions, is not a true picture of the atom as it actually exists. What he is actually telling us, then, regardless of the kind of language that he uses, is that the Copenhagen atom-model has no properties and can only be symbolized by a mathematical equation. This is all that he *can* tell us in this connection, because his conclusions are based on an atom-model, and a model cannot tell us anything about the physical atom; it can only tell us about itself. It may *suggest* something about the atom and thereby give us a hint as to where to look for additional knowledge, indeed that is the primary purpose of a model, but this additional knowledge does not exist unless and until we verify it experimentally.

The statement that the Copenhagen atom-model cannot be conceived in anything but mathematical terms has exactly the same kind of significance as the statement that the billiard-ball atom-model has no internal structure. Neither of these statements has any applicability to the actual physical atom; they are statements about the models, not about the atoms which the models are intended to represent. The billiard-ball model was designed to fit the behavior of gases and similar phenomena, and it is applicable only where the presence or absence of internal structure is immaterial. The lack of any internal structure in the model does not signify, or even infer, that there is no internal structure in the physical atom; it simply means that the billiard-ball atom is incomplete, as models usually are, and gives us no information as to whether the physical atom has an internal structure or not. Similarly, the statement that the Copenhagen atom-model is purely mathematical and has no physical properties does not signify that the physical atom has no such properties; it merely means that the Copenhagen model is likewise incomplete. This model was originally designed to fit certain properties which are primarily mathematical in nature, and because it has been successful in the mathematical applications and unsuccessful elsewhere, it has gradually been modified to eliminate the non-mathematical aspects, until it is now almost entirely mathematical in character. The result is that it is now applicable only to the mathematical aspects, particularly to the quantum aspects, of the atomic behavior, and it has no relevance to anything else. This does not signify that nothing else exists; it merely means that the Copenhagen model is unable to deal with those other aspects.

The historical record of the development that has culminated in this Copenhagen model shows very clearly what has happened. Bohr found himself confronted with some serious contradictions in his first efforts to set up a nuclear atom-model in detail and, as the textbook author expresses it, solved the problem by postulating that it did not exist. As might be expected, this kind of a "solution" led to further difficulties of an equally serious nature and in casting about for means of overcoming these obstacles, Bohr hit upon the expedient of eliminating the recalcitrant aspects of the situation by assumptions and "principles" and confining the theoretical development to the more tractable remainder. "... Adequate tools were found," he says, "in highly developed mathematical abstractions." This set the pattern for future revisions and modifications. When new problems were encountered, they must inevitably be handled in the same way. "... We are here confronted with new problems," says Bohr, "whose solution obviously demands further abstractions."[78] By this means Bohr and his fellow theorists have simply excluded everything which resisted treatment in this particular manner, and have restricted their atom-model to a representation of only a small portion of the behavior of the actual physical atom. What they did, says Herbert Dingle, "was to establish the fact that the hypothetical atoms were pure conceptions, that they belonged essentially to a different category from the facts of observation. They were creatures of the imagination, to be formed into the image of our fancies and restricted by whatever laws we cared to prescribe, provided only that when they behaved in accordance with those laws they should produce phenomena." This, Dingle continues, "is the essence of the famous 'quantum theory' though it is not the aspect under which it was first revealed and from which it derives its name."[79]

Under these circumstances it is simply preposterous to contend that the Copenhagen atom-model is in any sense a true picture of the physical atom. This has been recognized by many observers. Ernest Nagel tells us, for instance, "Many physicists have therefore concluded that... the (quantum) theory must be regarded simply as a conceptual schema or a policy for guiding and coordinating experiments."[80] The only surprising thing about this statement is that it should be necessary to say "many" physicists rather than "all" physicists. Whatever one may think about the Copenhagen atom-model, whether he agrees with "the proponents of the Copenhagen interpretation" who, according to David Bohm, "regard the development of any alternative to their point of view as logically impossible"[81]; whether he is cautiously skeptical in the manner of Louis de Broglie, who says, "... quantum physics has found itself for several years tackling problems which it has not been able to solve and seems to have arrived at a dead end,"[82] or Norwood R. Hanson, who goes a little farther and suggests, "The whole (quantum) theory may topple; in places the foundations seem far from secure"[83]; or whether he is definitely disillusioned in the manner of P.A.M. Dirac, who states, "One is thus led to doubt the validity of the whole structure of quantum field theory "[84], the fact still remains that all of these opinions refer to an atom-model; indeed the very existence of such diversity of opinion is further evidence that we are dealing with a model, not a picture, of the atom.

Let us now take Jordan's statement that the atom, as we know it today (that is, the Copenhagen atom-model), "can only be characterized by a system of mathematical formulae," and set this up against the following from Paul F. Schmidt; "Mathematical propositions tell us nothing about the character of nature; they are uninterpreted formalisms."[85] Here we have the whole situation in a nutshell. Bohr's atom-model attempted to explain the observed facts, but the attempt was unsuccessful and this model has been succeeded by the Copenhagen model, which no longer attempts to explain anything. This new model merely provides us with a mathematical system and a set of rules for operating it.

When we attempt to ascertain the *meaning* of the operations prescribed by the Copenhagen theory, we are thus in the same position as when we attempt to ascertain the internal structure of the billiardball atom-model; the billiard-ball model has no internal structure and the Copenhagen model has no meaning. There are reasons to believe that the actual physical atom has some kind of internal structure and that there are logical explanations for the mathematical relationships which this physical atom follows, but the respective models have been so constructed as to exclude these aspects of the physical atom. Even the original Bohr atom had only a very tenuous tie with physical reality. Mendelssohn speaks of "... the strange emptiness of this ingenious and successful model."[86] In the revisions that followed, the few explainable features that did exist were eliminated and as Lanczos puts it, only "one feature of the theory remained unaltered up to our days: its incomprehensible character."[19]

Essentially the same comment has been made by other observers. Herbert Dingle, for instance, asserts, "If there is one word that more aptly than another describes modern intellectual activity in its widest generality, that word is 'unintelligibility."[87] Presumably these remarks by Lanczos and Dingle are intended as criticism, but if we recognize that the present-day descendent of the Bohr atom is an atom-model, not a picture of the physical atom, and that, according to the frontline theorists, this model does not purport to represent anything but the mathematical properties of the atom, these particular criticisms, so far as they apply to the atomic theory, are actually unjustified. After all, we cannot logically criticize a theory for being exactly what it claims to be. If the Copenhagen atom-model is "the solution of a wave equation and nothing more,"[88] as Andrade puts it, we cannot expect it to be understandable in any other terms.

But although we cannot legitimately criticize the Copenhagen atom-model for being incomprehensible, since it is deliberately designed to be understandable only in mathematical terms, this merely emphasizes the fact that it is only a model, a creature of the imagination, not a picture of the actual physical atom, and whatever conclusions may be drawn from it, whether they are legitimate or not, are conclusions about the model, not about the physical atom.

As long as no true picture of the atom is yet available, a reasonably good model serves a useful purpose, and the foregoing discussion is not intended to imply anything to the contrary. The preceding chapters have demonstrated that the currently popular atom-models are incomplete and inconsistent with experimental knowledge, and are based on a completely erroneous concept of the nature of the atomic structure. Their usefulness is severely limited by these considerations, but if they are recognized for what they actually are, and utilized accordingly, they can be of considerable value, just as the earlier "billiard-ball" model played an important role in its day. On the other hand, the present tendency to look upon today's models as "final and ultimate" pictures of the true physical situation, just because they happen to be the best explanations currently available, is a very serious impediment to scientific progress, not only because it diverts a huge amount of research activity into unproductive channels, but also because it prevents recognition of the correct answers if and when they do appear.

Chapter IX
The Philosophical Aspect

The preceding discussion has developed the point that we have no picture of the atom; we have only atom-models. All of these models are limited in scope and, for that reason, the more specific they attempt to be, the less validity they possess. The simple nuclear model, which attempts to set up a detailed account of the atomic structure, has been shown to be completely erroneous; the billiard-ball atom is in agreement with the facts in its own field, but this is a very restricted field; the Copenhagen version of the nuclear atom also agrees with the facts in certain mathematical areas, but it has withdrawn completely from any attempt to fit the non-mathematical aspects of the physical atom. None of these models can give us any actual information about the atom; whatever conclusions are drawn from the model are conclusions about the model.

In the light of these facts much of the current discussion about the impact of modern physics on philosophy is simply meaningless. An enormous crop of literature has grown up, all based on the assumption that a "revolution" has occurred in our understanding of basic physical processes, and that the nature of this revolution is such as to necessitate revision of many previously accepted concepts in philosophy-even in religion. But the newly developed items upon which this assumption is based are not facts from experimental sources; they are conclusions drawn from theory or interpretations of the experimental facts in the light of current theories. In other words, this new information to which such extraordinary importance has been attached is not information about the physical world; it is information about the incomplete and purely speculative models that have been set up to represent the physical realities. It follows that the "revolution" that has taken place has not been a change in the physicists' *understanding of* nature, as we are led to believe, but in their *thinking about* nature. The profound alteration of the philosophical outlook now visualized by the philosophers is thus based on nothing more substantial than a shift in the direction of the physicists' imaginations.

By far the majority of these revolutionary philosophical implications have been drawn from the various applications of the quantum ideas to the behavior of the atom, the Copenhagen atom-model in particular. Contrary to statements that are often made, these implications do not come from the quantum concept itself. The scientific world was already familiar with many physical entities which exist only in discrete units, matter being the most prominent, and all that Planck did in formulating his theory was to add radiant energy to this list. This came as quite a surprise, but it was certainly not the kind of a radical change in outlook which justifies reconstruction of basic philosophical ideas, and it was not so interpreted at the time. The revolutionary implications have not come from this experimentally verified concept of the existence of quanta of radiation, but from purely theoretical aspects of the atom-models invented in an effort to apply quantum ideas to events on the atomic level.

Of all these inventions, the favorite from the standpoint of philosophical speculation is Heisenberg's Principle of Uncertainty, or Principle of Indeterminacy, as some prefer to call it. This principle asserts that because of the quantum considerations which enter into the structure of matter, there is an inherent and unavoidable uncertainty in our knowledge of specific physical situations, the magnitude of which is related to Planck's constant h. If we attempt to be more accurate in our definition of some particular magnitude, the principle says, we can achieve this only at the expense of becoming less accurate in our definition of some conjugate magnitude. Thus an attempt to obtain exact information as to the momentum of a particle necessarily introduces an uncertainty into our knowledge of its location.

An even more important and far-reaching development is the abandonment of the causal concept that underlies the entire structure of science outside of the modern atomic theory: the principle that "from nothing nothing comes." In the happy, carefree land of the quantum theories things just happen; there does not have to be any reason for the happening, as in the dull and prosaic realms of everyday life. Nor do these events need to be of a rational or reasonable character. As Bridgman explains, in the statement previously quoted, "The world is not intrinsically reasonable or understandable" in the "realm of the very little." There appears to be some difficulty in formulating an acceptable "principle" which will express this assumption that the atomic events do not follow any fixed principles, but some authors talk about a Principle of Anomaly, [89] the gist of which seems to be that strange things can be expected to happen in the atomic regions. The philosophical discussions that are concerned with the repudiation of causality are often rather vague as to the point of departure from which the speculations take off. Not infrequently the Principle of Uncertainty gets the credit (or the blame, depending on the point of view) for something that is more legitimately chargeable to the "anomaly" concept.

After formulating these "revolutionary" principles on the basis of purely theoretical considerations, the originators have naturally tried to find some physical evidence to support their conclusions, and it may be worth while to take a look at what they have dredged up. The argument that is advanced to support the Principle of Uncertainty is that events at the atomic level cannot be observed without radically changing them by the act of observation, since the agencies by means of which the observation is made, light for example, are composed of units (photons) which are of the same order of magnitude as the phenomena that are to be observed. Speaking specifically of electrons, Reichenbach asserts, "When you observe them, you have to disturb them; and therefore you do not know what they did before the observation."[90] Thus, say the theorists, there is an actual physical uncertainty corroborating the theoretical uncertainty represented by Heisenberg's principle.

Now let us stack this against the work of the investigators who actually discovered the electron and determined its major properties. These experimenters worked primarily with scintillation screens, in which the impact of the electron causes a flash of light that is visible to the observer. Here we have exactly what the present-day theorists are talking about; the only way by which Thomson and his contemporaries were able to observe the electron involved a violent collision which completely altered its behavior. But did this prevent the investigators from obtaining the information they were seeking? Definitely not. They were able to establish the magnitudes of the major properties of the electron, and the nature of the response of the particle to various forces that might be applied to it, so that we can now proceed with confidence to the construction of devices such as the electron microscope, in which the entire utility of the device depends on our being able to define the behavior of the electrons with extreme precision.

The essential point here is that the scientific investigator does not attempt to determine the behavior of an individual electron, or an individual molecule, or an individual projectile, from observations of that electron, molecule, or projectile. What he does is to make observations of many members of each class, thousands of them if necessary, until he has accumulated sufficient information to reveal the characteristics of the behavior of the individual entities and to enable him to set up general prinaples and mathematical equations describing their behavior. When it then becomes desirable to know what will happen to some particular individual, a ballistic missile, for example, the path which this individual will follow is determined by calculation, not by observation. Whether or not the observations involved in the original study "disturb" the individuals being studied is entirely immaterial. In many instances the only methods that are available not only disturb, but destroy, the subject of observation, but this has no bearing on the validity of the results that are obtained.

These facts are well known to the scientists and philosophers who are advancing the "disturbance" argument in an effort to bolster the Uncertainty Principle, and it is rather odd that such an empty argument would be used so widely by competent people. Here again, however, we encounter the same situation that was the subject of comment earlier in this volume: the fact that those who are thoroughly convinced of the validity of a theory are inclined to accept at face value any arguments which support that theory, without making any real effort to determine whether these arguments are actually sound. Reichenbach, for example, follows his statement of the "disturbance" argument with a discussion of the question as to whether the desired information might be obtained in some way that did not involve a disturbance of the subject. He admits that this would be possible if the entities being investigated follow the usual physical laws, but he dismisses this possibility with the statement that "The analysis of quantum mechanics, however, has given a negative answer...." In essence he is telling us that the assertion of quantum theory with respect to uncertainty is verified by the fact that quantum theory itself says that any alternative is impossible. Here is a plain case of circular reasoning that would not be given the slightest consideration if it were presented as an argument in anything other than a case that had already been pre-judged before the evidence was submitted.

Ernest Nagel discusses this "disturbance" argument at considerable length in his book *The Structure of Science* and concludes that it is "neither entirely clear nor persuasive." He emphasizes particularly that the argument rests primarily on the contention that the disturbances which the electrons or other entities under observation are alleged to suffer in the act of observation are "uncontrollable" and "unpredictable," but that these uncontrollable and unpredictable aspects are not factual items derived from experimental evidence; they are "part of the *consequences* drawn from the [uncertainty] relations":[91] another instance of the circular reasoning that is far too prevalent in the atomic domain.

If there actually were any electrons in the atomic structure, their properties and behavior characteristics could no doubt be determined in the same way that the properties of electrons or other particles are determined wherever they do exist. But since the atomic electron is non-existent, it is no wonder that the modern physicists have been forced to conclude that it "is not a material particle in space and time but, in a way, only a symbol . . .", [17] as Heisenberg says. We could very appropriately rewrite the Principle of Uncertainty in this manner: The properties of non-existent particles cannot be determined with precision.

The stock argument in support of the contention that causal relationships do not apply to individual events on the atomic level is radioactivity. It is pointed out that although we can predict accurately what proportion of the atoms of a radioactive substance will disintegrate during any particular period of time, we have no knowledge whatever of the fate of any particular atom. From this it is argued that there are no rational considerations governing the behavior of the individual atom; that the orderly and predictable behavior of the aggregate is simply a statistical effect.

The validity of this argument depends entirely on an assumption which is necessary to complete the chain of reasoning: the assumption that since we do not know the reason for the disintegration now, we will *never* know it; that is, there is no reason. Here is a manifestation of the egotism that is characteristic of the human race; a trait that science tries to subdue, with considerably less than complete success. "We cannot say of any grain [of plutonium] whether it will fall into the decayed or the surviving half," says Bronowski. "There are no physical laws to tell us-and there cannot be."[92] These last four words tell the story-"and there cannot be." From the vantage point of our minuscule knowledge of the physical world-a knowledge which Newton compared to a few pebbles on the shore of the great ocean of truth—the human race, at least a substantial part of it, feels competent to lay down dicta for all time to come.

Such statements are simply nonsense. As matters now stand we know essentially nothing about the radioactive disintegration process, other than that it happens. We do not know what the structure of the atom was to begin with, what changes take place in the process, or what initiated the radioactive event in the first place. Any claim to omniscience on the subject of what can be or cannot be is ridiculous.

On the strength of these curious principles, the Principle of Uncertainty and the somewhat nebulous Principle of Anomaly, the philosophers have had a field day. After arguments have gone on for centuries in some philosophical areas, it now appears from these "discoveries" in the field of physics that nature has taken a hand in the game and has laid down some rules. To be sure, the exact meaning of these rules is still uncertain and controversial, but irrespective of details, if there is a limit to the precision with which the properties of physical entities can be specified, and if phenomena at the atomic level are determined by statistical rather than causal considerations, these are matters which are clearly of great significance to philosophy.

But all this is based on the assumption that nature is speaking, whereas the truth is that the speakers are Bohr, Heisenberg, and their associates. Uncertainty is not a property of the physical atom or the physical electron; it is a property of the Copenhagen atom-model. Heisenberg is uncertain, but this is no proof, or even a good indication, that nature is uncertain. Likewise we have no evidence that physical events involving atomic or sub-atomic entities are determined solely by statistical laws; this again is a property of the atom-model, not of the atom. The philosophers, in spite of their rigorous training in the art of avoiding logical pitfalls, have fallen into a trap; they have simply been talked into accepting an atom-model, a very incomplete atom-model, as a true picture of the atom.

This is all the more remarkable in that these philosophers have come very near a recognition of the true situation. Ernest Nagel, for example, tells us that "a model may be a potential intellectual trap as well as an invaluable intellectual tool."[93] Here is a statement that is so very much to the point that one might easily get the impression that Nagel had a full realization of the kind of a package that the physicists were handing to him, particularly since, as pointed out previously, he has noted that some physicists are inclined to place quantum theory even lower in the scale than a model. But when we follow the development of his line of thought we find that he is referring to something altogether different. In this present work the terms "theory" and "atom-model" are used interchangeably, following the general practice in the field of physics. Nagel, however, draws a distinction between the theory itself and any kind of a conceptual model that might be used as an aid in visualizing-the theory, and his concern is that such a conceptual model might not be a true representation of the theory, and that improper conclusions might result from confusing the two. Whatever merit this point may have, it is totally irrelevant to the present issue. All that has been said about atom-models applies equally as forcibly to the theory and to any conceptual model that might be devised to represent the theory. No theory or model of the atom yet proposed even comes close to qualifying as a picture of the atom, and consequently no statement about such a theory or model is a statement about the physical atom. Whatever its authors may *claim*, it is a statement about the theory or about the model, and nothing more.

This seems to be the stumbling block that has tripped up all those who have examined the situation from the philosophical standpoint. It has been generally recognized that the Uncertainty Principle and associated concepts are purely creatures of the quantum theory. Ernst Cassirer, for instance, admits that "... the limitation... expressed in the uncertainty relations is only valid relative to the quantum principle and to the general formalism of quantum mechanics."[94] This, of course, carries with it the realization that if the quantum theory topples, all of the philosophical implications collapse with it. Cassirer goes on to say that if the advance of empirical knowledge ultimately requires replacement of the quantum theory by "some other basic assumption, the question of observability would then also appear in a different light."

What he is telling us in this statement is that if, at some future time, it develops that the quantum theory is not a true representation of the physical facts, then the conclusions that have been drawn from uncertainty and other features of the quantum concepts must be discarded. But Cassirer and his colleagues apparently fail to see that this is just exactly the situation which exists now. The atom has many different properties, each of which has both physical and mathematical aspects: qualitative and quantitative aspects, as we may say. This we know on the basis of the kind of empirical evidence that Cassirer is talking about-evidence from observation and experiment. But quantum theory makes no pretense of giving us a true representation of the physical atom that has these characteristics, the atom that is studied in the laboratories; it specifically disclaims any resemblance between its atom and any physical object. The atom of quantum theory is "a solution of a wave equation and nothing more." Regardless of how successful or unsuccessful the theory may have been in the limited area which it attempts to cover, it is not, and never has been, a picture of the physical atom; it is a model which, like the billiard-ball atom, admittedly successful in its own field, has been devised to represent one particular aspect of the physical atom.

Thus we do not need to wait to see whether future experimental discoveries will invalidate the quantum theory. Whether valid or not valid in its limited field, this theory is simply an atom-model and any conclusions that are drawn from it are conclusions about this particular atom-model, not about the atom. Max Planck, who originated the quantum concept, makes this very clear in his "Scientific Autobiography."[95] He emphasizes the same point brought out in the foregoing discussion; that is, we are dealing with two entirely different things. One is the physical atom, which in conjunction with other similar entities Planck calls the "sense world."

The other is the atom-model of the quantum theory, for which Planck uses the term "world picture." The world picture of quantum mechanics (the atom-model), he says, is a "provisional and alterable creation of the human power of imagination," and "even a cursory glance shows how far (it) has shifted from the sense world...." He goes on to point out that the conclusions with respect to the need for abandoning causality in the microscopic realm are "founded on a confusion of the world picture with the sense world: " a confusion, we may say, in which the voice from Copenhagen is mistaken for the voice of nature.

The inferences with respect to free will and other philosophical subjects, which have been drawn from the Uncertainty Principle and other aspects of the Copenhagen atom-model, are not resting on the revelations of nature, as the philosophers are assuming; they are resting upon products of the imagination of Bohr and his associates: exactly the same kind of assumptions and postulates that constitute the starting point for most philosophical speculations. All that the philosophers have actually accomplished in this effort is to substitute the imaginations of the physicists for their own.

As a final comment on the philosophic aspect of atomic theory, it may be well to point out that the whole mass of speculation and controversy over the relation of modern physical theory to the problem of free will is essentially meaningless, no matter how much or how little validity the modern theories may possess. Regardless of whether the course of physical events is governed by a rigid determinism, or is entirely a result of chance processes, or comes about through some combination of the two, it still follows that these physical processes, whatever they are, produce a specific result in each case. This is true irrespective of the extent to which chance may enter into the selection of the particular result. Chance does not produce an indeterminate result; when a chance event does occur, it is just as specific and definite as if it had been produced by a fully determinate process. Free will, if any such thing exists, must be able to overrule this event that would otherwise take place, and substitute some other result which is physically possible. The essence of free will is the option. It must be possible either to allow the normal result to take place or, alternatively, to dictate a course of events which would not result from the normal physical processes, whatever these normal processes may be, whether they are deterministic or indeterministic.

The question of determinism is therefore wholly irrelevant so far as the problem of free will is concerned. It makes no difference how much or how little chance enters into the physical processes that produce specific events in the physical universe; as long as the events are dictated by these processes, there is no free will. Free will exists only if the physical processes can be overruled at the option of that will. The existence of an option to accept or reject the decision of nature is incompatible with Laplacian determinism, to be sure, but it is equally incompatible with chance, and no amount of sophistry or circumlocution can evade this simple and obvious fact. Once again, let us call upon Erwin Schroedinger for a summation:

If these statistics (the statistics which, according to present-day theory, determine the behavior of the atom) are interfered with by any agent, this agent violates the laws of quantum mechanics just as objectionably as if it interfered-in pre-quantum physics-with a strictly causal mechanical law.... The net result is that quantum physics has nothing to do with the freewill problem. If there is such a problem, it is not furthered a whit by the latest developments in physics.[96]

Chapter X
Facts and Fancies

The objective of this present discussion is simply to make a long overdue critical examination of the status of the nuclear theory of the atom in the light of present-day experimental knowledge, and to show that this theory is now completely untenable, however satisfactorily it may have met the less exacting requirements that prevailed in its youth. The act of making such an examination imposes no obligation to provide an acceptable substitute for any theories which may have to be discarded as a result of the findings; clearing away the dead wood is a necessary preliminary to the erection of a new structure, but it is an entirely separate operation. Nevertheless, there are those who feel that destroying an existing theory without putting something in its place is, in some way, immoral, and in deference to this school of thought the final three chapters will be devoted to an examination of the road ahead. Chapters 10 and 11 will discuss some of the conditions that need to be corrected to clear the way for the development of a new and adequate theory of the atom, and Chapter 12 will indicate the general features that such a theory must possess in order to be compatible with present-day experimental knowledge.

The findings of the investigation on which this present work is based point very definitely to the conclusion that the most serious obstacle standing in the way of scientific research in general and the formulation of a satisfactory theory of atomic structure in particular is the lack of a clear distinction between factual and non-factual material in present-day practice. In theory, the rnost distinctive feature of science is its reliance upon the established facts as the ultimate authority. Speculation and hypothesis play an important part in scientific research, to be sure, but the products of such activity are not supposed to be considered in any way authoritative unless and until they are verified by experiment or observation. As it happens, however, scientists are not only scientists, they are also human beings, and in the latter capacity they are subject to the ordinary weaknesses of the human race, including a strong bias in favor of familiar and commonly accepted ideas, a totally unscientific reliance on presumably authoritative pronouncements, and a distinct reluctance to admit ignorance. All of these add up to a marked tendency to regard general acceptance as equivalent to proof, a tendency that has had the effect of diluting the firmly established factual material of science with a large admixture of matter of an unproved and uncertain character.

The strangest feature of the whole situation is that this confusion of fact with fancy is not only completely at odds with the basic philosophy on which all science rests; it is so utterly unnecessary. It is possible to understand why there might be a tendency in some other fields where actual verifiable facts are few and far between, to stretch a point and make broader claims for some of the currently popular ideas than can actually be substantiated, but science needs no padding of this kind. The factual knowledge in the scientific field is already so greatly in excess of that in any other sphere of human activity that exaggerating its extent is pointless.

Nevertheless, the exaggerations are widespread. Even the most casual survey discloses an almost incredible number of non-factual items masquerading as facts. This statement is not made on the strength of any unusually strict definition of the term "fact." It is true that if we split enough hairs we can set up rigid definitions of such concepts as "proof," "truth," and "fact," which will exclude almost everything. But the present discussion is based on the rather liberal interpretation of the nature of practical scientific proof outlined in the introductory chapter: a viewpoint which recognizes that as a practical matter some relaxation of the theoretically correct standards of proof is necessary in order to build up any body of scientific knowledge at all. It is not the intention here to contest the adequacy of the proof where proof is offered; the point which is to be emphasized is that a substantial part of present-day scientific "knowledge" consists of items which admittedly cannot be proved at all, or for which the alleged proofs that are oflfered are clearly inapplicable. In short, the scientist has developed a habit of saying "We know..." when he should say "We think...."

Probably no other single item has been more costly in terms of its effects in diverting scientific research from the straightforward path and turning it into unproductive detours than the failure to recognize and explore alternatives. All too often, as soon as *an* explanation is offered, the job is considered done, and possible alternatives, even if they are in plain sight, are ignored. As brought out in Chapter 2, the basic error made in the construction of present-day atomic theory was the failure to consider the possibility of an alternate explanation of Rutherford's scattering experiments. Not until such possibilities have been thoroughly explored and rejected is it legitimate to say that the original explanation is correct, no matter how liberal tve may make the standards of proof. In the Rutherford case, if any attempt at all had been made to look for possible alternatives, it would have been almost immediately obvious that such an alternative not only existed, but was a much better explanation of the facts than the original theory.

Bohr had an opportunity to rectify this mistake when he found that Rutherford's atom was irreconcilable with established physical laws. For him there were clearly two alternatives; either one or the other of the conflicting structures of thought must be wrong. But, as Rutherford had done before him, Bohr looked at only one of the alternatives and, as is now evident, chose the wrong one. Beyond this point the situation became more complicated. Those who undertook to solve the many problems faced by the Bohr atom in its subsequent development no longer had a clear-cut case of choosing between alternatives. By this time it had become a matter of fighting it out on the lines already laid down or of questioning the validity of the whole structure of existing theory.

This rather curious failure to explore alternatives is by no means confined to atomic theory; it is widespread throughout the entire fabric of physical science. The First Postulate of Relativity is a striking example. The real substance of the Relativity Theory is contained in the postulates of the constant velocity of light and the equivalence of gravitational and inertial mass. The validity of these postulates is firmly established; actually they are not postulates at all, they are experimental facts, the authenticity of which most physicists were willing to concede even before Einstein incorporated them into his theory. The First Postulate, on the other hand, is simply a principle of impotence. As such it cannot contribute anything of a positive nature to the theory; it merely serves the purpose of evading the contradiction which otherwise exists between the constant velocity of light and the Newtonian concept of motion. Furthermore, it does not even do a very good job of meeting this quite modest requirement. In the first place, this postulate, which seems rather plausible in application to linear motion, falls flat in application to rotational motion, where the existence of a velocity can often be detected by external means. For instance, we can tell that the galaxies are rotating, and we can even get a general idea as to the relative magnitudes of their rotational velocities, simply by looking at them. This situation has never been reconciled with the First Postulate. "We see at once," says Eddington, "that a relativity theory of translation is on a different footing from a relativity theory of rotation. The duty of the former is to explain facts; the duty of the latter is to explain away facts."[97] Mach has advanced the hypothesis that the rotatiorr is relative to the rest of the universe, rather than absolute, but since we have only one universe (so far as we know), this is merely an exercise in semantics. The other weakness of the First Postulate is that it introduces unnecessary complications of its own making into the operation of the system-a series of "paradoxes."

On the strength of the evidence available, it can reasonably be concluded that the First Postulate is not an expression of a physical fact; it is, like a model, "only a mental help, a tool of thought." It is rather strange, therefore, that no serious consideration has been given to the possible alternatives. From a strictly logical standpoint, the best policy would probably be just to accept the fact that a contradiction exists, and to look upon attempts at explanation, such as the First Postulate, simply as interesting speculations, until some fully satisfactory explanation finally does appear. Essentially, this is what is being done in the field of radiation, where there is a similar contradiction between the wave and particle aspects of the photon. Here the physicists, as James B. Conant describes the situation, "have learned to live with a paradox that once seemed intolerable."[98]

In view of the historical background and the general feeling that even a poor explanation is better than none, a frank recognition of the true situation may be to much to expect. There is no valid excuse, however, for failing to explore the possible alternative explanations. Obviously such alternatives exist. Within the framework of accepted ideas as to the nature of space and time the constant velocity of light is definitely in conflict with the concept of absolute motion. Something therefore has to be modified in order to conform with the observed facts, but this does not mean that the absolute motion concept necessarily has to be jettisoned. It is equally possible to modify the currently accepted space-time concepts and retain absolute motion. There is no *a priori* reason why one should be preferred over the other, and the experience with Einstein's choice actually favors the alternative approach, as Einstein found that the denial of absolute motion was not sufficient, and he had to tinker with the space-time concepts as well.

A great many other similar instances of failure to explore alternatives could be cited, but in such cases as those described thus far the fault has been primarily a failure to recognize the existence of the alternatives. This is a serious and costly oversight, to be sure, but criticizing it amounts to essentially nothing more than contending that scientists ought to do a better job: a contention which, even if true, is rather pointless. But in addition to those instances where the alternatives have gone unrecognized, there are many situations in which the existence of alternatives is realized, but where some one of the possible alternative explanations is arbitrarily selected and proclaimed as an established fact. Unlike the failure to recognize the alternatives, which can be excused on the ground that the human brain is far from being a perfect instrument, this equally costly practice of investing pure assumptions with the habiliments of positive facts is wholly unnecessary and inexcusable.

Once again Einstein's work furnishes a striking example. Throughout scientific literature his theory that mass is a function of velocity is described as having been "proved" by the results of experiment and by the successful use of the predictions of the theory in the design of the particle accelerators. Yet at the same time that a host of scientific authorities are proclaiming this theory as a firmly established and incontestable experimental fact, practically every elementary physics textbook admits that it is actually nothing more than an arbitrary selection from among several possible alternative explanations of the observed facts. The experiments simply show that if a particle is subjected to an unchanged electric or magnetic force, the resulting acceleration decreases at high velocities and approaches a limit of zero at the velocity of light. The further conclusion that the decrease in acceleration is due to an increase in mass is a pure assumption that has no factual foundation whatever.

It should be emphasized that, so far as the present issue is concerned, it is immaterial whether this assumption of an increase in mass at high velocities is ultimately found valid or not valid. The important point is that as matters now stand, we do not know what the ultimate verdict will be, and if the assumption happens to be wrong, which is not at all unlikely, the prevailing arbitrary refusal to consider any alternative places an almost impassible roadblock in the way of further progress. Nothing dampens the enthusiasm of the research worker more, or constitutes a greater obstacle to recognition of the right answer if the researcher does uItimately find it in spite of everything, than this practice of building pure assumptions up to the status of articles of faith whose validity must not be questioned.

This is not intended to imply that there is anything inherently wrong about making assumptions. Newton thought that there was, and he expressed some rather critical opinions about hypotheses in general, but an examination of his work shows that he actually utilized them rather freely. The truth is that we have no option but to use assumptions, unless we restrict our inquiries to the regions accessible to direct observation. The only method that is available for investigating phenomena in inaccessible regions is to make some assumption, then determine what consequences will result in the observable regions if the assumption is valid, and finally compare these theoretical consequences with the results of observation or measurement. We may legitimately take the stand, therefore, that assumptions are indispensable tools of science. The trouble starts only when the distinction between assumptions and facts is allowed to break down.

There are, of course, many kinds of assumptions in scientific practice, and the arbitrary selection from among equally possible alternatives is neither the most prevalent nor the most dangerous. As pointed out in a previous chapter, the latter distinction belongs to the postulates which deny the validity of established physical principles in application to the special classes of phenomena under consideration. The most common is the *ad hoc* assumption invented for the purpose of overcoming some specific obstacle in the theoretical development. Nothing more than a very elementary knowledge of human nature is required for a full understanding of the immense popularity that the privilege of making such assumptions now enjoys. No scientist likes to face the prospect of spending long years of effort in a fruitless endeavor to solve a difficult problem, and few can achieve such perfect scientific detachment that they are able to contemplate with equanimity the necessity of giving up a cherished concept or theory of long standing. But here is a marvellous device by which the scientist can circumvent both of these distasteful prospects as if by magic. Following the example of Bohr, he "solves" his problems simply by "postulating that they do not exist."

The *ad hoc* assumption is the morphine of the scientific world. When used sparingly and in the appropriate circumstances it is invaluable, but when it is used indiscriminately the results are disastrous. Common prudence certainly calls for the exercise of a careful control over any device which is so open to abuse: a close scrutiny of the alleged justification for the assumption, an insistence on adequate study of all possible alternatives, and above all, a permanent and sharply defined line of demarcation between assumptions and positively established facts. But present practice is just the reverse. The scientist who solves a problem receives no more credit than the one who postulates that a solution is impossible; indeed our highest honors are reserved for those who construct evasive postulates of a particularly novel and ingenious character. Furthermore, the current tendency is to elevate these ingenious assumptions to a level where they are on a par with, or even superior to, the established facts.

The most vicious aspect of the present incredibly liberal policy with respect to the employment of *ad hoc* assumptions is that it perpetuates basic errors when they are once made. The facts that have been brought out in the preceding pages with respect to the nuclear hypothesis provide a graphic illustration of this point. Here is a hypothesis whose antecedents could never have survived any kind of a critical examination, and whose consequences have been one long story of continued and repeated conflict with observed facts and established principles. The mere layman, in his innocence, might think that somewhere along the line someone would suggest that it would be simpler to drop the nuclear idea than to force the rest of physical theory through such a painful series of contortions. But no, the theorist tells us, this is unthinkable. Back on page one of The Book it says that Rutherford discovered the nucleus in 1911 and this, like the laws of the Medes and Persians, cannot be changed. And as long as there are no restrictions on the use of *ad hoc* assumptions, it will *not* be changed (unless through the agency of some irreverent work such as this), since the question always comes up in this form: Shall we make another assumption or shall we abandon our entire theoretical structure?

An important class of non-factual material that is all too often confused with fact consists of extrapolations from the known to the unknown regions. Here again, the process itself is not open to criticism. Extrapolation is a sound and indispensable tool of science, and as long as the products of the extrapolation process are recognized for what they actually are, this device serves a very useful purpose. As brought out earlier, we must use assumptions in order to deal with regions beyond the reach of direct observation, but if we had to make our assumptions completely at random, the amount of work that would have to be done to test one hypothesis after another until we just happened to hit on the right one would make scientific research practically impossible. Extrapolation is the device that we use to determine the kind of assumption which has the greatest probability of being correct.

Let us assume, for example, that we are doing some work which involves the melting point of iron under pressures on the order of those existing at the center of the earth. We have no direct knowledge of the behavior of matter under such high pressures and we therefore have no option but to make an assumption of some kind. Since we are dealing with the unknown, anything is possible, theoretically, and there is practically no limitation on the assumptions that we *could* make, but it is clear that here, as is almost always true, there is one possible assumption that is so far superior to all others, so much more likely to represent the true facts, that we are never justified in considering any other possibility until after we have given this one a thorough examination. This greatly superior hypothesis is, of course, the assumption that the same pattern which we find in the known region also prevails in the unknown region; that is, it is an extrapolation from the known to the unknown.

In the example cited, the procedure that is followed is to determine the mathematical relation between pressure and the melting point of iron in the pressure range where direct measurement is possible, and then to extend this relation to a determination of the value in question. In this case, one of the primary uncertainties inherent in the extrapolation process is immediately apparent, as we find that there is much difference of opinion as to just what the true relationship within the experimental pressure range actually is. Several different mathematical expressions of this relationship have been proposed by competent investigators, all of which agree with the observed values within the experimental error, but which arrive at widely different results when extrapolated to the pressures at the earth's core. Even in those cases where this uncertainty does not exist, and the mathematical formulation of the observed values seems to be beyond question there is always the possibility of an unrecognized term which is negligible within the experimental range, but may be very significant in a long extrapolation. Then, of course, it is also possible that the pattern may change at some point beyond the experimental limit: an unsuspected critical point of some kind. Here again the chance of running into this kind of a situation is much greater if the extrapolation is a long one.

The length of the extrapolation is therefore a very important factor in determining the true status of conclusions which are reached by means of an extrapolation process. If the extrapolation is very short, the results which are obtained can usually qualify as facts under the very liberal definition of scientific proof which has been adopted for purposes of this discussion. But many of the extrapolations now being made are far from short. As Bridgman puts it, some of them are "perfectly hair-raising." A good example is the almost universal belief that we now "know" the nature of the processes which furnish the energy supply for the stars. Even in a day when "hairraising" extrapolations are somewhat commonplace, this one sets some kind of a record. In view of the gigantic extrapolation that is required to pass from the relatively insignificant temperatures and pressures obtainable on earth to the immensely greater magnitudes which we believe (also through extrapolation) exist in the stellar interiors, even the thought that the answers *might* be correct calls for the exercise of no small degree of faith in the validity of our processes; any contention that the extrapolated results constitute actual knowledge is simply preposterous.

Actually the currently accepted ideas in this field are based to a considerable degree on atomic theories which, in the light of the points brought out in the earlier chapters, are no longer tenable. This, in conjunction with a number of contradictory items of a factual character, principally from the astronomical field, furnishes enough evidence to justify the conclusion that the present theories as to the nature of the stellar energy generation process are not merely unconfirmed assumptions, but are almost certainly wrong. The current practice of presenting such products of long extrapolations as positive knowledge is a definite and serious hindrance to scientific progress.

It is true that the atomic physicists, who are not themselves deceived, generally dilute their statements with a few qualifying words, and if we read the fine print we can see that they are giving tentative conclusions rather than facts. For example, Robert E. Marshak, in an article entitled "The Energy of the Stars," makes this assertion "So we can safely assume that the stars produce energy by the combination of light elements through the collisions of their swiftly moving nuclei."[99] Technically, this statement is unimpeachable. It does not purport to be a statement of fact; on the contrary, it is specifically labeled as an assumption. But physical science today is highly compartmental. As Leprince-Ringuet describes the situation, a physicist "rarely comments on anything that lies outside of his own well-defined specialty,"[100] to say nothing of questioning the conclusions of specialists in other areas. A confident expression such as Marshak's, coming from a specialist, therefore acquires the status of gospel truth as soon as it gets into the general realm of physics, and we find the textbooks and semi-popular scientific works repeating it in such uncompromising terms as, "Fusion reactions of light nuclei account for the production of energy in the sun,"[101] or "Nuclear fusion . . . has been generating the power of the sun and other stars for billions of years,"[102] or "Actually we all live by hydrogen fusion, for that is how the sun's heat and light are generated."[103]

By the time this product of a "hair-raising" extrapolation reaches the astronomers it has become an article of faith against which factual evidence is powerless. E. T Opik tells us, "This knowledge is so well founded that it furnishes a reliable basis for the calculation of time rates of stellar evolution."[104] The high estate to which the assumption and belief of the atomic physicist has now risen is all the more remarkable since Opik admits on the very next page that this "reliable basis" is clearly unreliable. "The energy source of the giants remains a puzzle," he says, and hence "a more powerful source of energy must be assumed." A little later he further concedes that "some uneasiness may be felt" about the application of the theory to the white dwarfs. Throughout astronomy this same situation prevails. On every hand facts revealed by astronomical observation are openly or inferentially in conflict with necessary and unavoidable consequences of the energy generation process "safely assumed" by the physicists. "It is no small matter," says Bart J. Bok, "to accept as proven the conclusion that some of our most conspicuous supergiants, like Rigel, were formed so very recently on the cosmic scale of time measurement."[105] Cecelia Payne-Gaposchkin tells us that the results of age calculations based on this hydrogen conversion process are "staggering."[106] Otto Struve even finds it necessary to characterize factual knowledge from his own field as "apparent defiance of the modern theory of stellar evolution."[107]

But rather than question the conclusions of specialists in another field, the astronomers have chosen to ignore the contradictory evidence from their own observations and to distort the entire astronomical picture to fit the "hair-raising" extrapolation of the physicists. This is exactly the same kind of thing that has happened in atomic theory, where all else has been subordinated to the demand that the interpretation of Rutherford's scattering experiments as the discovery of a "nucleus" be maintained at all costs. Both of these situations furnish eloquent testimony to the serious consequences of the lack of adequate discrimination between factual and non-factual material in presentday scientific practice.

Another type of extrapolation, which is all the more dangerous because the extrapolator does not always realize the true nature of the step which he is taking, is what we may call extrapolation of the negative. Here we find that certain things do not happen in the regions directly accessible (the earth, primarily). We then generalize this observation by extrapolating it to the regions that are not accessible, and we say that such things *never* happen. A good example of this type of extrapolation is provided by the question of isotopic stability. Under terrestrial conditions the isotope Fe^{56} is stable. In the absence of any evidence to the contrary we assume that stability is an inherent property and that Fe^{56} is always stable. Similarly, we assume that the neutron is always (with one curious exception) unstable because it is unstable in the region where we can observe it.

These are natural and logical assumptions under the circumstances, and as long as it is recognized that they are *only* assumptions, not facts, they are entirely proper. But here again this important distinction is, for the most part, ignored. Much of the current thinking about the cosmic rays, for instance, follows along lines dictated by the belief that those particles which are short-lived in the terrestrial environment are likewise short-lived in inter-stellar and inter-galactic space. Similarly, we find the neutron relegated to a comparatively minor role in hypothetical atom-building processes because it is short-lived on earth. But the observed instability of neutrons, mesons, and other such particles, under terrestrial conditions does not *prove* that they are unstable under other conditions; it is evidence suggesting such a conclusion, but we are making an enormously long extrapolation of the negative when we apply this conclusion to the universe as a whole, and it is a serious mistake to assume that the little that we now know closes the door to all other possibilities. In this connection it is interesting to note that however firm the modern physicist may be in his conclusion that the instability of the neutron is an inherent property of the particle, he does not hesitate to throw this firm conviction overboard when it conflicts with some other cherished concept. The same physicist who reacts violently to the suggestion that stability may be a function of the environment and that his conclusions as to atom-building and similar processes in extra terrestrial environments may therefore be wide of the mark, does not hesitate to advance exactly the same hypothesis when he finds this necessary in order to fit his theory that the normally unstable neutron is a constituent of stable atoms.

Chapter XI
The Role of Clear Thinking

All through the quotations that have been used in the preceding pages to illustrate the present attitude of the scientific profession toward the subjects under discussion, the incomprehensibility of modern physical science is a constantly recurring theme. Dingle says that the word "unintelligibility" is an apt description of current science in its totality; Lanczos tells us that the only aspect of the Bohr theory of the atom that has remained unchanged through all of the revisions and modifications is its incomprehensibility; the world as a whole is not intrinsically understandable, on the basis of present-day concepts, says Bridgman; Heisenberg makes it plain that an understanding "of the first order" is impossible if the theory which he champions is correct; and so on. Lande reflects a very general impression when he says, "Quantum mechanics has the reputation of being incomprehensible to all but a select group of theoretical physicists. . . ."[74]

This widespread acknowledgement of the incomprehensible character of much of the currently accepted theory, not only by the opponents of the official doctrines, but by some of the originators and principal defenders of the theory as well, should have raised some very serious questions long ago. If our search along these lines for an explanation of the physical world must terminate in an answer that we cannot understand, is there any adequate justification for spending time and effort in the search? Since this answer that we obtain gets us nowhere even if it is correct, would we not be far better off if we directed our efforts toward exploring the possibility that this answer is not correct, and that an understandable answer does exist? As matters now stand, it is evident that, had this latter question been asked, experience would have answered it in the affirmative.

For the benefit of those who may be inclined to point out that some worth-while results have been accomplished in connection with the development of these incomprehensible ideas, it should be emphasized that, as brought out in detail in previous chapters, these tangible results have been accomplished independently of, and to a large extent in spite of, the theoretical explanations which have been attached to them. For instance, the entire structure of spectroscopic theory, which is one of the major accomplishments of this kind, the first one that is usually mentioned, is based on the concept of energy levelt, and it is entirely independent of the current theories which are intended to explain the existence of these energy levels. In fact, the spectroscopist has long since ceased to follow the theorists on the strange path along which they are guiding atomic theory. As Candler puts it in his text on "Atomic Spectra," written at the time when the development of the "modern" theories was in full swing, the new atomic models will not replace the older ones, so far as spectroscopy is concerned, because the physicist working in this field asks ". . . as he often says, for something he can understand,"[108] and of atomic theory he puts himself into a position where ". . . should by setting up his system on a basis independent of the wild gyrations the present theory ever be displaced by a betcer, the experimental facts would still be intelligible."[109]

Another claim that is often made is that ideas which are difficult for the present generation to understand will be readily intelligible to a newer generation who have always been accustomed to thinking in these terms. Freeman J. Dyson predicts, "Eventually . . . quantum mechanics will be accepted by students from the beginning as a simple and natural way of thinking, just because we shall all have grown used to it."[110] After the disclosures of the earlier chapters, a consideration of this issue is purely academic so far as the quantum theory itself is concerned, but as a general proposition it may be pointed out that the accurary of such predictions is decidedly questionable. They are largely predicated on the fact that many other ideas which were difficult to understand and accept when they were originally proposed are now familiar and commonplace habits of thought, but the instances of this kind that are usually cited are primarily cases where the original difficulties were due to unfamiliarity rather than to any inherent qualities of the ideas themselves. Atomicity, for example, has always encountered trouble of this kind, initially, 'in any field in which it has been introduced-in matter, in electricity, in radiant energy, and so on-but there is nothing of an incomprehensible nature in the concept of discrete units per se, and in this case a general understanding is simply a matter of time. Much of present-day physical theory, on the other hand, is inherently difficult to understand, a point which Dyson tacitly admits in the same article, when he says, "There is hope that quantum mechanics will gradually lose its baffling quality." Unfamiliarity is a characteristic that ultimately disappears, but "baffling qualities" are likely to be permanent.

Actually it is somewhat doubtful whether those who do claim to have a full understanding of the latest quantum theories are being entirely candid. Wost of the explanations which they give us are far from being models of lucidity, particularly where they approach such questions as complementarity or the relation of the observer to the physical event. Heisenberg's explanation, ". . . the different pictures are contradictory and therefore we call them mutually complementary,"[111] is rather revealing with respect to the use that is being made of the term "complementary," but it can hardly be described as a contribution toward understanding. Furthermore, we not only find the members of the "select group" of theorists differing among tlremsclves as ro the meaning of those concepts which they are all supposed to understand- "even the founders of quantum theory are not in harmony in their various expositions of the bases of that theory"[112] says Margenau-but we also find numerous inconsistencies in separate statements by the same quantum authority. There is ample room for suspicion that the alleged understanding may have some similarity to the ability to see "The Emperor's New Clothes" in Hans Christian Andersen's story.

It is really quite amusing to observe the predicament in which an author finds himself if he tries to explain some of the present-day theories related to atomic structure under circumstances where he is precluded from taking advantage of the protective screen of excess verbiage that is normally employed to conceal the flimsy foundations on which these theories rest. David Dietz, for example, devotes three pages oI his book "The Story of Science"[113] to an explanation of inter-atomic forces as they are visualized by the electrical theory. In this limitecl space he cannot beat around the bush; he must make his statements definite and succinct. But as matters now stand in the realm of atomic theory, a definite statement cannot be a positive statement, if the author knows his subject and has any regard for accuracy. He cannot say that an atom with only one electron in its outer shell loses it easily, since this statement is full of unconfirmed assumptions; he finds himself forced to say that such an atom seems to have this behavior. Similarly, he cannot tell us explicitly how rwo neutral atoms create the electrical forces that are supposed to cause them to combine, as current theory has no explanation for this phenomenon. Hence Dietz is here forced to use another evasive expression and tell us that in some way a rearrangement takes place and attractive forces come into play.

In the three short pages in which he describes the electrical theory of atomic cohesion it is necessary eight times to tell us that something seems to be true; the expression in some way is used twice, such words as perhaps, probably, and apparently, are used four times, and twice Dietz admits that some phase of the currently accepted theoretical structure is difficult to understand. Here is a very eloquent commentary on the state of the theory.

This theory of the forces between the atoms, with which Dietz has such a struggle in three pages, acquires no greater content of truth when it is expanded to forty or fifty pages in the usual textbook presentation, and is dressed up with all of the vague and "baffing" concepts of the quantum theory. These embellishments merely serve the purpose of obscuring the true situation, and it is quite apparent that this retreat into obscurantism is another measure of the type discussed in Chapter 1: another expedient that can be, and is, employed to avoid the necessity of discarding a basic concept or theory which has come into conflict with the established facts. It is freely admitted that the unintelligibility is deliberate. "The present-day atomic model," according to Holton and Roller, "quite intentionally no longer presents any simple picture to guide our imagination."[114] Like the devices discussed in the introductory chapter-the ad hoc assumption, the repudiation of established physical principles, and ihe principle of impotence-this device is inherently of such a nature that it lends itself readily to illegitimate purposes, such as the perpetuation of false theories, and it should always be looked upon with suspicion. Whenever we are told that an understanding of some aspect of the physical universe is "for ever beyond our reach,"[115] as Jeans characterized the atomic situation, there is a very strong possibility that the unintelligibility is merely the result of unwillingness on the part of the theorist to abandon some favorite concept that should be on its way to the ashcan.

This does not imply that all valid new ideas must necessarily be fully understandable at first sight; on the contrary, it is inevitable that new theories which replace old familiar ideas will involve some major conceptual difficulties on initial consideration. But as long as the proponents of a new theory contend that a clear understanding is possible, the validity of the theory can be tested. If such an understanding eventually turns out to be unattainable, this is, in itself, a test, since it disproves the contentions of ehe advocates of the theory. On the other hand, a theory which claims to be correct, but inherently incapable of a complete understanding, cannot be tested, and acceptance of such theories carries with it a considerable risk of perpetuating basic errors.

Closely connected with the use of obscure and incomprehensible concepts is the utilization of unusual and complicated methods of mathematical treatment. This device itself is perfectly legitimate. There are a multitude of applications in science and technology where the use of complex mathematics is essential to solution of the existing problems. But this is another tool which lends itself very readily to misuse, and there is a definite tendency in present-day practice to call upon complicated mathematical procedures as a means of forcing the observed facts into conformity with preconceived basic concepts, rather than adopting the logical but distasteful alternative of giving up these basic ideas that are erroneous or inadequate.

The great versatility of this mathematical tool is the feature that makes it dangerous. If the first application of complex mathematics is fruitless, the present-day theorist is not in the least dismayed. He mereIy introduces still further complexity, secure in the knowledge that if tangible results continue to elude him, the complexity will riltirnately exceed the capabilities of the available mathematical methoels, at which point he can claim that the problem has been solved "in principle" and that it is only the limitations of existing mathematical methods which stand in the way of accomplishing any actual results. The availability of the modern high speed computers will extend the practical application of complex mathematical processes to a considerable extent, but the opportunities for added complexity are unlimited, and the theorists can easily keep ahead of the computers. Anyone who tlrinks that this picture is overdrawn should take another look at the statement quoted in Chapter 7, which contends that quanturn mechanics gives the solution, in principle, to almost every chemical problem, and at the same time admits tlrat in actual practice it cannot solve arry specific problem, or at Stater's comment regarding Heisenberg's theory of ferromagnetism, in which he says that there is a "universal conviction that Heisenberg's fundamental idea was correct," and then goes on to say, "Here, unfortunately, as in the molecular problem, ic is extremely difficult to get quantitative results out of the theory, on account of its great mathematical complication,"[116] or at any number of equally revealing statements which can be found throughout scientific literature.

The misuse of mathematics has not gone unrecognized. Lande tells us, for example, "The mathematical sign language with its complex symbols and non-commutative matrix algebra has become a veil shielding the simple meaning of the quantum laws from the scrutiny of common sense,"[117] and Bridgman calls our attention to the fact that the statistical methods may be used to "conceal a vast amount of actual ignorance."[118] But the general tendency has been to glorify the complex and the abstruse. A liberal use of non-commutative mathematics, non-Euclidean geometry, and complicated statistical procedures has come to be regarded as the hallmark of erudition, and any publication, in the field of physics at least, which does not bristle with integral signs and complex equations is looked upon as lamentably deficient in scholarly quality, irrespective of the actual need for anything more than simple arithmetic.

The astounding lengths to which the physicists have been able to carry the nuclear theory of the atom, in spite of its complete Iack of factual foundation and the contradictions and inconsistencies which it has encountered at every turn, cannot be attributed exclusively to any one cause. This extraordinary performance is the result of innate reluctance to abandon ideas of long standing, plus the confusion of fact and fancy, plus the use of obscurity as an evasive tactic, plus the utilization of abstruse mathematics, plus the repudiation of established principles of science, plus the lavish use of ad hoc assumptions, plus the free employment of principles of impotence: all of the great arsenal of evasive devices which the ingenuity of the modern scientist has created to aid him in his attempt to force nature into the patterns which he has chosen for it.

But it is painfully apparent by this time that nature will not be coerced, and that in order to find the correct answers, the theorist must resign himself to the humbIe role of seeker after the truth, rather than the role of lawmaker, which he has been aspiring to fill. It is no doubt a highly satisfactory state of af£airs to be permitted to take command of a sector of the universe, to populate it with "creatures of the imagination , . . formed into the image of our fancies and restricted by whatever laws we cared to prescribe," and to receive the acclaim of the scientific world for the construction of this ingenious theoretical structure. But all this contributes nothing toward the increase of knowledge. It is not science; it is merely a sophisticated kind of science fiction, and in the long run it can only end in the same kind of a debacle that now faces the nuclear theory. True forward progress requires some clear thinking to separate fact from fancy and to recognize the path which these facts delineate, and then a conscientious and determined effort to follow that path wherever it leads, irrespective of personal prejudices or preferences.

The need for clear and unprejudiced thinking is by no means confined to those who are engaged in the construction of the new theoretical structure. All those who undertake to appraise or evaluate new ideas in the scientific field, or who pass a negative judgment by refusing to consider them at all, share in this responsibility.

If it were possible to eliminate the existing tendency to treat extrapolations, currently popular hypotheses, and other non-factual items on the same basis as positively established facts, and to sweep away the veil of obscure language and abstruse mathematics that conceals the weaknesses of existing theory, most of the tangible obstacles that now stand in the way of developing a satisfactory replacement for the nuclear atom theory would be removed. Even then, however, the road would still be blocked by an intangible, but extremely formidable, obstacle in the strong pressure for conformity to accepted lines of thought which exists throughout the scientific world. It is rather ironic that it should be necessary to make a statement of this kind, since the university professors, among whom are numbered most of the leading theorists in the field of physical science, are the most vociferous objectors to any pressure for conformity in other fields, particularly conformity to the prevailing political and economic viewpoints of the general public. The same individual who stoutly defends his "academic freedom" and defiantly asserts his right to hold-and even to teach-political doctrines that are anathema to the great majority of the individuals who pay his salary, is the first to condemn any deviation from the dogma of his own specialized fieId.

It is true that this situation is not quite as pronounced in Europe as in the United States. This is a highly conformist nation, so much so that "some critics would have us believe," an investigator tells us in a recent report, "that social conformity is exclusively a U.S. phenomenon."[119] But science cuts across national boundaries, and the pressure for scientific conformity is world-wide. Even Einstein found himself relegated to the sidelines when he persisted in opposing the currently popular doctrines. In the words of Lanczos, "The same Einstein who, a few years earlier, was hailed as the greatest scientific genius of all times, now had difficulties in even publishing his results. Soon he faded out altogether from the scientific arena of his time."[19] There are, of course, many scientific questions which are wide open for debate: questions on which no."official" viewpoint has yet been formulated. In the field of cosmology, for example, one has a free choice from among several competing theories of a general nature, and practically unlimited latitude for introducing any variations which he might happen to favor. But once the "party line" has been established, dissent is, in effect, prohibited. Some of the more daring thinkers may raise questions of detail; that is, men like Bohm or Lande take issue with the Copenhagen *interpretation* quantum theory, but the basic theory must not be touched, and these dissenters evidently feel that it is imperative to make it very clear that they have no such intention. Any member of a university physics department who today repudiated the quantum theory in its entirety would find himself in much the same position as an atheist in the priesthood.

So far as atomic theory is concerned, the disastrous results of this fantastic overstressing of the significance of winning the current popularity poll have been brought out in detail in the previous chapters. It is simply appalling to think of all of the time and effort that have been wasted in trying to find answers to meaningless questions, merely because the erroneous basic concepts of this theory have been elevated to a status which makes them exempt from critical examination. The effect of ignoring this privileged status in this present work and subjecting currently accepted theory to a critical analysis has been to show that it is nothing but a hollow shell. There is hardly enough of the atomic theory itself left intact after this disclosure to constitute any serious obstacle to the formulation of a new theory, but it should be recognized that, in all probability, conflicts with other accepted theories will develop during the process of construction of an alternate theory, and unless some relaxation of the prevailing ban on challenging these theories can be effected, the independent thinking necessary for the success of this undertaking will be curtailed to a serious, perhaps fatal, degree.

Just how to go about accomplishing such a relaxation of the strict taboos that now surround the basic doctrines of science is a difficult question to answer. One of the major elements in the situation is the fact that, as science is now set up, evaluatiori of new ideas is left almost entirely to the individual scientists. In view of the high degree of specialization now existing, this means that the evaluation is carried out by the specialists in the area affected, more particularly, by the recognized authorities in that specialized field. In the final analysis, therefore, the verdict on a new idea is pronounced by the very individuals who are the least likely to give it the unbiased consideration that is necessary in order to arrive at an accurate evaluation: individuals who are busy with their own affairs and not inclined to be bothered with trying to understand new points of view, who are fully immersed in the details of their own specialties and thoroughly indoctrinated with the currently accepted theories, and who have a definite vested interest in the maintenance of the *status quo* in the theory of their subjects. If the new proposal is a minor addition to or revision of existing knowledge, of such a nature that it can be evaluated quickly and easily, appraisal by these specialists serves the purpose quite adequately, but the more a new line of thought diverges from currently accepted concepts (and consequently, the more important the new idea is, if it is valid) the less likely it is to get any hearing at all, much less the kind of a careful and unbiased evaluation that is needed. When we recognize the way in which existing thought is thus insulated against attack, the failure to apply any kind of critical scrutiny to the foundations of the atomic theory during the past half century, which seems so completely inexplicable on first consideration, becomes quite understandable.

One of the possible expedients that naturally suggests itself as a corrective measure is to set up some kind of an agency, under scientific society, university, or government auspices, which would undertake the task of giving a preliminary hearing to new scientific proposals, not as a matter of coming to a decision as to their merits, but merely to determine whether or not they appear to be worth more extended consideration by the scientific community. Similar measures have been utilized very successfully under wartime conditions to cope with the large number of unconventional proposals that come up in connection with the military effort, most of which are worthless, but which cannot be summarily rejected because among the worthless stones there are a few uncut diamonds of great potential value. Whether or not such a system would operate effectively without the stimulus of a national emergency is problematical; there is a general tendency for such agencies to settle down into a comfortable routine and to become strongholds of the established order, rather than instruments of progress. It is well understood that the publication committees of the various journals and societies are ultraconservative, and even though an agency is originally set up for the express purpose of smoothing the path for new ideas of merit, it is quite possible that this agency might develop a similar aversion to anything that could turn out to be controversial. But since there are a few valuable diamonds among the worthless stones of the scientific field too, some experimentation with measures of this kihd would seem to be justified.

If nothing else, an agency of this kind could provide a remedy for one of the most serious weaknesses of the present system, which stems from the fact that the originator of a new idea normally has no opportunity to present a rebuttal to any adverse opinion that is reached, unless he already has an established standing which enables him to secure publication irrespective of adverse opinions. For the ordinary investigator, the decision of the "authority" in the particular field, or of the publication committee or the book publisher's advisor, is essentially final. In view of the apparently inescapable prejudice against new ideas, particularly against those which represent the greatest departure from existing thought, it is inevitable that the arguments in favor of the new theories will be judged more harshly than the arguments against them. In particular, it is likely that the familiar arguments of long standing which are advanced in support of current theory will be accepted at face value in most cases even though, as has been brought out in the previous discussion, many of them are completely lacking in merit. Furthermore, there is a strong tendency to judge new ideas within the frame of reference provided by the existing theoretical structure, which imposes an almost insurmountable handicap on theories that involve any major divergence from established lines of thought. This situation could easily be corrected if an agency of the type suggested herein is created, as the procedures of this agency could be set up in such a way as to provide an opportunity for the rebuttal that is now lacking; perhaps even an oral argument in cases where this seems to be justified.

A favorable decision by such an agency would not be in any sense an endorsement of the idea; it would simply state that, in the opinion of the agency staff, the new idea has enough merit of one kind or another to justify further and more detailed examination by the scientific profession at large. This would automatically have the effect of making discussion or advocacy of the idea scientifically respectable, and would remove the barriers which now inhibit the rank-and-file scientist from any open display of interest in unconventional ideas: barriers created by such things as reluctance to participate in any challenge of recognized authority, unwillingness to appear out of step with colleagues, fear of ridicule for espousing "crackpot" ideas, etc. In effect, such a program would use the weight of authority, represented by the official pronouncement of the agency, to restore the freedom of discussion and exchange of opinion which is essential for maximum scientific progress, but is now blocked by the pressure for conformity to currently accepted thought.

Another expedient which might have considerable merit is the establishment of a new profession, that of *scientific critic*, analogous to music or literary critics: individuals who are not performers themselves, but who make a business of passing judgment on the performances of others. It will no doubt be argued that we already have an ample, perhaps excessive, amount of scientific criticism, since all scientists are to some extent critics. But the scientists who criticize scientific theories and interpretations are not acting as critics in the same manner that a music critic acts; rather they are acting as partisans, whose primary interest is not impartial criticism, but the promotion or defense of their own viewpoints. Such criticism not only fails to give new ideas the unbiased consideration to which they are entitled, but is also seriously deficient in that it does not come into play at all unless some such new idea actually appears to challenge the accepted doctrine. The type of fully effective criticism that is needed, in order to make the maximum contribution to scientific progress, is criticism which keeps the entire fabric of existing theory under close and continuous scrutiny. Every significant new experimental discovery should initiate a full-scale review of all portions of existing theory that are in any way affected, on the order of the examination of the nuclear atom theory carried out in this volume, so that errors such as those made in the interpretation of radioactivity and Rutherford's scattering experiments will be detected before so many years of effort have been wasted in following false trails.

The need for more adequate consideration of the effect of new discoveries on currently accepted concepts has been pointed out by Bridgman, who notes that under existing conditions if an appraisal of this kind is made at all, it is likely to be a mere formality. "Such an examination," he says, "because it is nobody's business, and because the fundamental concepts have already been accepted, is in danger of being made superficially, without the care that would have bcen given it if the effect had been known at the time the concept was formulated."[120] If we had professional scientific critics, this kind of work would no longer be "nobody's business."

In this connection it should be mentioned that a large part of Bridgman's time during his later years was devoted to scientific criticism somewhat along the lines suggested herein, and such works as "The Nature of Physical Theory" and "The Logic of Modern Physics" illustrate the type of critical analysis that is greatly needed. Even though he did not dig deep enough to uncover the weaknesses in the foundations of current atomic theory that are the theme of this volume, many of his comments come remarkably close to the conclusions reached in this present work. Speaking of wave mechanics, for example, he asks this question (which applies with equal force to the more sophisticated successors of that theory), "Is this honestly . . . a very impressive performance? Is it not exactly the sort of compromise that we would have predicted in advance would be the only possible one if it should prove that we were incapable of inventing any vitally new way of thinking about small scale things? "[121] Here we have a recognition of one of the major points developed in this work: the fact that the present standard practice in physical science is to use "compromises" and ingenious constructions of all kinds to avoid the necessity of breaking out of the comfortable groove of familiar thought and inventing the "vitally new way of thinking" that is the first requisite for progress.

These books of Bridgman's also demonstrate why the profession of scientific critic must be independent in order to be fully effective. Much of the force of his criticism is lost because of his personal commitments to certain specific viewpoints, such as "operationalism," of which he was commonly regarded as the foremost advocate. These commitments not only color the critical arguments to a noticeable degree, and thus weaken them considerably, but also give them a controversial flavor which has robbed them of much of the influence which they might otherwise have had. Bridgman recognized this situation, and admitted that his work as a critic was merely auxiliary to his work as a theorist. "But for me as a physicist," he says, "criticism is an enterprise entered into solely for practical reasons,"[122] and he tacitly concedes that the nature of his primary objective has had an effect on his conclusions, when he brings out the same point that has been emphasized in the present discussion: the fact that "... there is a fundamental difference in kind between his (the physicist's) critical and theoretical activities."[123]

Another useful function that a professional scientific critic could perform is that of preparing special textbooks for research workers. As matters now stand, the individual entering upon a career of research gets his basic information about his own and collateral fields primarily from works that were prepared for general instructional purposes. Such texts must necessarily take a somewhat positive attitude toward their subject matter, and even if the authors recognize the contradictions and weaknesses of existing theories, which is not always the case, they are attempting to present a clear picture to the student and a frank admission of the doubts and uncertainties that actually cloud this picture is incompatible with their principal objective. Advanced texts are no better in this respect; they are even more misleading as they assume the validity of the doubtful and uncertain basic concepts and devote their pages to developing these concepts in greater detail. What the research worker needs is not this clear picture of today's best guess that he gets from the ordinary textbook, but a frank and honest presentation of the basic elements of the theoretical structure, so that he can know which of these elements he must necessarily accept and which are open to possible modification if his findings seem to require some change.

This kind of a presentation would be particularly helpful to those who have to utilize material from some source outside of their own field of specialization. Under existing conditions, such theories and concepts from other fields have to be taken largely on trust, and as the present situation in astronomy clearly demonstrates, this trust is not always justified. Unfortunately, the conclusions that have the widest areas of application, and are therefore the most commonly borrowed, are the very ones that are most likely to be erroneous, or at least incomplete. This necessarily follows from the fact that conclusions of this kind are normally reached on the basis of the information available within one specialized field. If the subject matter is such that no other field is affected, the prevailing policy gives us conclusions based on 100 percent of the available evidence, but if the subject matter also applies to other fields, the information on which the conclusions are based (that is, the information within the particular specialized field) may be only a small fraction of the total information bearing on the subject. Obviously the smaller the proportion of the pertinent facts taken into consideration, the greater the possibility of error. It is quite unlikely, for instance, that the present-day concept of the nature of the stellar energy generation process would have achieved any widespread acceptance if the available astronomical evidence had been considered by the atomic physicists along with the data from their own field. And even if this theory did win favor among the physicists in spite of the contradictory evidence from astronomical observations, candid textbooks for research workers, along the lines that have been suggested, probably would have alerted the astronomers to the true nature of the physicists' assumptions, and would have avoided the present chaotic situation in the theory of stellar evolution.

Of course, the reviewing of scientific books and articles also falls within the province of the scientific critic; indeed it is the principal means by which his functions would normally be performed, just as the review is the principal product of the literary or dramatic critic. Under present conditions it would probably not be possible for a scientific critic to devote his entire time to this occupation and use it as a means of livelihood, since the commercial standing of scientific criticism is currently (although not necessarily permanently) quite different from that of such activities as dramatic criticism. It should not be difficult, however, to set up some satisfactory arrangements along the lines that most basic research work is now carried on; that is, as a part-time activity of college and university faculty members.

In addition to accomplishing the primary objective, this measure might well have a very beneficial effect in providing the university staff with an acceptable alternative to research as a "prestige" activity. It is generally recognized that the present strong emphasis on research is creating an awkward situation in the universities, because research has become the key to academic standing, even though it cannot be denied that the primary function of the university is instruction. "The trend, in the major universities of this country," reports F. Reif, is "to minimize the importance attached to the teaching functions of the faculty.... Teaching undergraduates is a local activity which may be appreciated by the students but does not serve to enhance the scientist's international prestige, on the basis of which the university will decide whether he is worthy of promotion."[124] But teaching and research are totally different kinds of activity and call for quite different talents. The existing system is therefore faulty in that it requires many individuals either to embark upon research projects for which they have neither the inclination nor the aptitude, or else to forfeit the advancement to which they should be entitled on the basis of their performance in their primary task. Recognition of the need for and the importance of capable and qualified professional scientific critics will not solve this problem in its entirety, but it will at least provide an alternative for those who are not attracted by research. At the moment, science is more in need of effective criticism, which will point the way toward an understanding of the facts that we already have at our command, than it is of more research that will simply add to the huge store of undigested facts now available. The opportunities for publication and "prestige building" therefore are, or could be, just as good for the capable critic as for the research worker.

All in all, it would appear that professional scientific critics would serve some very worthwhile purposes. This suggestion will no doubt arouse strong opposition among the "authorities" in the various scientific fields who now have everything their own way, but there is no good reason why their personal preferences should be given any more weight than we now give to the personal feelings of the musician or the actor, who dislike professional criticism just as much as the scientist does, but who have to accept it just the same. The arguments in favor of the critic are the same in both cases. A good professional dramatic critic is a better judge of a theatrical performance than a good actor, and a good professional scientific critic would be a better judge of a scientific theory than a good theorist, for exactly the same reasons. The existence of some type of inclependent professional criticism in the scientific field would go a long way toward minimizing the undesirable and detrimental practices discussed in Chapter 10, and would greatly facilitate such projects as the development of a new and better atomic theory. Certainly it would be extremely helpful to the innovator to have his new and unconventional ideas appraised by someone who welcomes the opportunity of making such an appraisal and who has no personal axe to grind, rather than, as at present, being completely at the mercy of individuals who prefer not to be bothered with making the appraisal at all, and whose personal interests are strongly identified with maintaining the existing structure of scientific thought intact.

In view of the extent to which the current thinking of the scientific profession is tied in to the nuclear atom theory, either in the actual substance of current thought or in the language in which current thought is expressed, it is quite apparent that some major conceptual innovations will be required in order to lay the foundations for a new and better atomic theory. The suggestions that have been made thus far in this chapter are directed toward minimizing the obstacles that now stand in the way of consideration and acceptance of new ideas, and thus preparing the way for those innovations that must be forthcoming before an adequate new theory can be constructed.

It might naturally be assumed that all scientists would agree, in principle, as to the desirability of measures of this kind-improvements in scientific practice which will make recognition of meritorious new ideas more prompt and certain-but oddly enough, this is not true. There is a very prevalent *laissez faire* attitude in the scientific community, which disapproves of and belittles any attempt to modify traditional methods and practices, an attitude that probably originates in a subconscious desire to protect the familiar currently accepted ideas from the challenges offered by strange and disquieting new thoughts. Some tell us that such efforts to improve the situation are unnecessary, inasmuch as scientific research has always encountered obstacles, but nevertheless moves steadily forward from one achievement to another. Other apologists for the existing order claim that scientific discovery is inevitable, irrespective of the ability of the investigators or the efficiency of their procedures. According to R. Taton, ". . . most discoveries were made at a time when they had become practically inevitable and, even if the scientists to whom they are attributed had never lived, they would not have been long delayed."[125] There are even those who go so far as to claim that it is beneficial to make things difficult for new ideas. "The fitness of truths is most advantageously shaped and most convincingly demonstrated in vigorous contest,"126 contends Holton.

There is some small element of truth in all of these assertions, of course, but any contention that these elements of truth furnish support for or justification of the "stormy and hostile reception" accorded to new ideas (Holton, [126]), or the "hard battles they have to wage to gain acceptance" (Taton, [125]), or the "indifference or suspicion" they normally encounter (Raman, [35]), is preposterous. New ideas should certainly be given careful and critical examination, and they should not be accepted until after they have been thoroughly checked and tested, but this does not justify hostile, or even indifferent, reception. These new ideas are the most important raw material of scientific progress, and the procedures of science should be so set up that development of such ideas is not opposed or passively accepted, but actively and positively encouraged.

Whether or not Taton is correct in his statement that *most* discoveries are made when the time is ripe, irrespective of the actual individuals involved, scientific history certainly shows that *many* discoveries, including some of the most important, do not fall in this category. The record shows that where major changes in thinking are involved, some one individual usually grasps the situation far in advance of anyone else, and if the work of this original discoverer is not understood or appreciated, a great many years normally elapse before some other investigator succeeds in picking up the threads.In the meantime a tremendous amount of time and effort is wasted in following false trails, or else there is complete stagnation.

The case of Gregor Mendel is a classic example. The discoveries that established the basic principles of heredity were published by Mendel in 1866, but not until his results were rediscovered in 1900 was any attention given to his work. In the meantime this important branch of science simply stood still for thirty-four years. Many and varied explanations are advanced for this astounding neglect of a major scientific discovery, but they all boil down to the one salient fact that the scientific community did not then--and does not now--have any specific mechanism whereby new ideas of this kind are assured of getting adequate consideration. As has been brought out in detail in the preceding pages, conceptual innovations face a great many obstacles because of the firmly entrenched positions of the ideas which they seek to supplant, but the most serious of all these obstacles is the diflicuIty of getting any attention in the first place. Under the haphazard system that now prevails, any one of a large number of factors may block the new thought out of the scientific field completely. Mendel's work simply met an impassible barrier of indifference and disinterest.

An equally striking demonstration of the shortcomings of the existing system is provided by the reception accorded to the work of J. J. Waterston, whose paper containing the first essentially complete development of the kinetic theory was rejected by the Royal Society in 1845, with the opinion that "the paper is nonsense, unfit even for reading before the society." J. S. Haldane comments on the case as follows:

> It is probable that, in the long and honourable history of the Royal Society no mistake more disastrous in its actual consequences for the progress of science and the reputation of British science than the rejection of Waterston's papers was ever made... . There is every reason for believing that, had the papers been published, physical chemistry and thermodynamics would have developed mainly in this country and along much simpler, more correct and more intelligible lines than those of their actual development.[127]

The many instances of this kind in the pages of scientific history take on a still greater significance when we reflect upon what course scientific knowledge might have taken if the same treatment had been accorded to the work of Newton, or Maxwell, or Planck. And then let us go a step farther and ask how many Mendels or Waterstons, or perhaps even Newtons, may have come and gone unrecognized? Some "mute, inglorious" Maxwell may rest in Thomas Gray's country churchyard. And how many decades-or perhaps centuries-will have to pass before someone else repeats the discoveries of these unrecognized pioneers, and gives the world the benefit of these findings which could be at our service now if our procedures for dealing with new ideas had been more efficient?

Returning to the particular subject under discussion in this volume, let us further consider what the result will be if the scientific world now closes its eyes to the completely erroneous nature of the nuclear theory. How much longer will we go on spending millions of man-hours seeking answers to meaningless questions and vainly striving to establish the properties of non-existent particles? Another half century, perhaps?

In his discussion of the Waterston case, Haldane suggests that it is unfortunate that Waterston did not have a "less retiring and more combative disposition." But can we afford to be content with a scientific organization which recognizes important new developments only if they happen to be the work of a "combative" individual? Such developments are rare at best, and in view of their tremendous value to science and to the community at large, it is almost criminal negligence to let them go to waste because of a loose and haphazard way of handling the evaluation of new ideas. The present practice, in effect, treats the acceptance of new ideas and concepts as if it is simply a concession to the originator: a reward that we give him if he is able to present an airtight case and promotes his point of view aggressively over an extended period of time. Actually society as a whole is the principal beneficiary of these new ideas, not the originator, and the scientific profession is derelict in carrying out its responsibilities to the community at large when it thus sits back complacently and throws the entire burden of establishing the merits of new ideas on the originators. The inevitable results of this policy are to discourage the Mendels and the Waterstons and to lose the benefit of their discoveries.

Science is not properly organized unless and until it sets up proceclures which insure prompt recognition of meritorious new ideas even if they are poorly expressed, timidly presented, and without adequate factual support at the time they first appear. It is the scientific community, acting through whatever agencies are required, that should display the aggressiveness—actively seeking out and encouraging new developments rather than accepting only those that force their way in—and it is the scientific community that should be quick to perceive the value of any new thought that is advanced, regardless of whether or not it happens to be presented on a silver platter. These points are practically self-evident if any attempt is made to face the issue squarely. Even some of those who argue in defense of current practices turn around and concede the desirability of a more effective organization. Taton, for example, who talks bravely about scientific discoveries being "inevitable," actually recognizes that the inevitability requires some encouragement and organized prodding, and he gives us this advice: "Very careful consideration should therefore be given to the best ways of organizing creative scientific and technological work so as to provide the most effective stimulus for the harmonious and fruitful development of science."[125]

Chapter XII
Where Do We Go From Here?

Predicting the course of future events is a hazardous undertaking at best, as any weather forecaster can testify, but ordinarily it is possible to derive sufficient information from the existing situation and from current trends to give at least some indication of the nature of the road ahead. So far as atomic theory is concerned, however, the attitude with which physical scientists approach the question is conditioned very strongly by their evaluation of the status of currently accepted theory. In general, those who are confident of the essential soundness of current theory are inclined to believe that a fully satisfactory explanation of basic physical processes is impossible. Jeans expresses this point of view in the following words: "The most we can aspire to is a model or picture which shall explain and account for some of the observed properties of matter; where this fails, we must supplement it with some other model or picture which will in its turn fail with other properties of matter, and so on."[128]

But this conclusion is reached by way of a very dubious line of reasoning. Jeans and the others who adopt this viewpoint have recognized that existing theory gives a very inadequate and, in many respects, contradictory picture of basic physical relationships, but at the same time they have convinced themselves that the existing concepts are essentially correct. They then argue that since correct theories give us answers that are so far from being complete and comprehensive that no conceivable amount of refinement of these ideas can reach such a goal, the attainment of this goal must be impossible.

The alternative, and decidedly more logical, viewpoint is that the inadequacies and contradictions of present-day theory indicate that it is *not* correct: a hypothesis which, of course, leaves room for the existence of a theory that is adequate and without contradictions. Those who subscribe to this belief do not deny the contention that existing theory has met with much success; they merely say that whatever success has been achieved is due to those elements of truth which the theory does contain, and that a different theory which approaches the ultimate truth more closely will have correspondingly greater success. As Reichenbach puts it, "... contradictory theories can be helpful only because there exists, though unknown at that time, a better theory which comprehends all observational data and is free from contradictions."[36]

The findings of this work lend strong support to this alternative conclusion. Critical analysis of the currently accepted theories shows that they are basically wrong, and that this is the reason why they are inconsistent and contradictory. It is true that this merely destroys the negative argument of Jeans *et al*; it does not definitely establish the affirmative position taken by Reichenbach. But as long as there is no valid reason for believing that a complete and understandable theory is impossible, the traditional spirit of scientific inquiry certainly demands that we should make the development of such a theory our goal, and not content ourselves with any lesser objective. The discussion which follows is based on this premise.

When we turn to a consideration of the specific features which this complete and intelligible atomic theory of the future must have in order to be consistent with the knowledge thus far accumulated from observation and experiment, it is evident to begin with, that the concept of atomic "building blocks" will have to be discarded. One of the most unexpected, but by this time firmly established, experimental discoveries of recent years is that all of the basic physical entities--atoms, particles, radiation, energy, electrical and magnetic charges-are interchangeable. Particles are materialized from radiation and are "annihilated" back to radiation again, protons become neutrons and vice versa, atoms undergo "fission" and "fusion," mesons are created from kinetic energy and ultimately decay into electrons and neutrinos, the atomic reactors transform mass into energy, while at the same time the particle accelerators are busily engaged in converting energy back into mass.

Physicists generally recognize that this interchangeability is simply devastating so far as existing theory is concerned. "... the fact that so many (elementary particles) change from one type to another is extremely disconcerting," says Robert E. Marshak, and he goes on to admit, "... the field is wide open for a deep-going theory which would elucidate the nature and role of the elementary particles of physics."[129] Here is an admission that goes straight to the heart of the problem with which this book is concerned. The present-day theory of the atom claims to *know* the "nature and role" of the elementary particles-the designation "elementary particle" is actually d erived from the nature of this supposed knowledge-and the discoveries that have produced this plaintive call for "elucidation" have dealt a body blow to currently accepted theory. We can no longer suppose that the atom is constructed of elementary "building blocks." The demonstrated interchangeability demands some altogether different explanation.

So far, at least, it appears that there are some restrictions on the nature of the transformation; entity A cannot necessarily be transformed *directly* into entity B. But it is now clear that matter, radiation and energy all have some kind of a common denominator, and if the transformation of entity A into entity B cannot be accomplished directly, present indications are that it can always be done in an indirect way. It is now apparent that if we want to use a construction analogy from our everyday experience, we will have to compare the basic substance of the universe to something on the order of modelling clay rather than to an assortment of building blocks. Even Heisenberg admits, "There is only one kind of matter...."[130] The authors who still write confidently about building blocks and go to the extent of identifying some of the so-called elementary particles as the "cement" that holds the building blocks together are simply indulging in a subtle form of science fiction.

But it is not only the "building block" concept that must be thrown overboard; the rapid advance of experimental knowledge is steadily making it more and more evident that the whole idea of an atom constructed of "parts" is doomed. The immediate reaction to this statement will probably be that it is preposterous, since we can readily break the atom into separate parts. But let us examine this situation a little more closely. Suppose we have a certain object moving with a high velocity, and we then detach that velocity by transferring it to something else. Must we then conclude that the original object consisted of two separate parts and that we have broken it into its two constituents? It is doubtful whether anyone would ever support such a conclusion as this, but if we compare this situation with the break-up of the atom, it is evident that the only basis on which we can claim that we have done something different with the atom is by contending that the "parts" which are detached from the atom are inherently of a different character than the motion which was detached from our hypothetical object.

Can such a contention be justified? We have found that both the proton and the electron can be transformed into radiation simply by contact with their respective antiparticles. "All matter seems to be radiation,"[41] says Morse, and so far as we know, radiation is nothing more than a vibratory motion. Can we say that the proton is inherently different from motion when we can transform it into motion? Are we not forced to the conclusion that the atom could very well be an integral entity endowed with specific amounts of various kinds of motion (or something equivalent to motion) and that what we call breaking it up into parts amounts to nothing more than detaching portions of this motion (or the equivalent thereof)?

And is it not true that the trend of discovery in the sub-atomic field is driving us slowly but inexorably in this direction, toward just such a conclusion as the foregoing? It is becoming increasingly evident that there are no "elementary particles" and that both the atoms and the sub-atomic particles belong essentially to the same class: a class that should be called "primary" rather than "elementary," in that these are the entities which are formed directly from the basic substance of the universe, the permissible forms, we might say, into which the basic clay can be shaped.

After all, much of this should have been suspected long before the advance of experimental knowledge actually forced us to such conclusions. In retrospect it is clear that serious consideration should have been given many years ago to the possibility that the atom is not constructed of "parts." When the most strenuous efforts over a long period of years by the best minds in the scientific profession fail to clarify the properties of the hypothetical constituents of the atom, and finally lead in desperation to the conclusion that these entities have no definite properties and do not even "exist objectively," mere common sense certainly calls for a thorough examination of the obvious possibility that they do not exist at all.

Similarly, the absurd situation into which the notion of the "elementary particle" has led us should have raised the warning signal long before this. In the early years of this century, when the only sub-atomic particles known to the physicist were the electron and the supposedly sub-atomic proton, and when the discovery of radioactivity had demonstrated that the atoms are subject to disintegration, it was entirely logical to conclude that atoms are constructed of parts, and that the then known particles which could be extracted from atoms are the "elementary particles" from which the atoms are built. But when contradictory evidence began to pile up at a fantastic rate, when it became clear that all of the products of radioactive disintegration are created in the process rather than preexisting in the atom, when it was demonstrated that the necessary properties of the hypothetical constituents of the atom were completely at odds with those of the known sub-atomic particles, when the number and variety of "elementary particles" multiplied to such an extent that the apparent necessity of calling them all "elementary" had become definitely embarrassing, when it was no longer possible even to imagine elementary roles which some of these particles, the mu meson for example, might fill, and above all, when it became necessary to admit that the concept of an elementary particle had become so hazy that it could no longer even be defined, it should have been strongly suspected that the answer might lie in the fact that there is no such thing as an elementary particle. In essence this, of course, amounts to the same thing as the conclusion that the atom is not constructed of "parts."

The evidence now available indicates that the sub-atomic particles such as the electron, the neutron, etc., are not constituents of atoms but incomplete atoms; that is, they are independent entities of the same general nature as atoms, but they lack, either qualitatively or quantitatively, something of what it takes to meet all of the requirements that qualify a particle for the status of an atom. They can all, atoms and sub-atomic particles alike, be converted to motion (that is, to radiation) and hence they must all be constructed of the same basic substance, or of substances that are essentially equivalent.

This brings us down to the question of the identity of the basic substance or substances. Since all units can be transformed into motion in the form of radiation, we might be inclined on first consideration to express the question in this manner: What is there that can exist in a variety of forms and is equivalent to motion? But this is a difficult, perhaps impossible, question to answer, and it leads us into a cul-de-sac. So we need to go back and take note of the fact that radiation is not only motion; it is a particular form of motion. In the light of this additional information we are entitled to rephrase the question in this way: What is there that can exist in a variety of forms and is equivalent to a particular form of motion? The answer is now obvious: other forms of motion.

On the basis of present-day knowledge, it will therefore be necessary to replace existing theories of the atom and the sub-atomic particles with some new theory wherein all such physical entities are complexes of different forms of motion, and wherein both the atom and the sub-atomic particles have the status of primary particles; that is, particles which are constructed directly from the basic motions.

It was pointed out in Chapter 7 that, in order to meet present-day requirements, the new atomic theory which we will need as a replacement for the nuclear theory must have a theoretical feature corresponding to the "Moseley units," the units of atomic number. The true nature of these units is almost entirely unknown, as the prevailing practice of identifying them with electrons and with "nu clear charge" has effectively blocked any investigation along other lines, but it is quite evident from the kind of difficulties encountered in the application of present-day theory to the existing situation that the Moseley units are not individual particles and they are not electrically charged. The new theory must therefore accommodate itself to these facts.

Another essential feature of a satisfactory theory of atomic structure is that it must make some provision for the additional force of a general nature which is clearly required by many physical phenomena. As explained in Chapter 7, one of the principal reasons why the shortcomings of the electrical theory of matter have been ignored for so long is that the electric force is the only known force of a general nature that seems to be strong enough to account for the observed cohesion of solids. The general attitude has been that inasmuch as no other force of adequate strength is known, the interatomic force of attraction must be electrical and the many items of evidence to the contrary must be susceptible of being explained away in some manner. In this present work this reasoning is reversed. The factual items of evidence showing that the cohesive force is not electrical are conclusive and inescapable, and since this force clearly does exist (that is, the atoms of a solid do hold together) it must have some other, as yet unknown, origin.

There is a very understandable reluctance on the part of the physicists to accept the idea that there is still an unknown force of general applicability in the universe, after centuries of intensive study of all physical phenomena. But it should be emphasized that this is not an unknown force; it is a known force of unknown origin, which is something entirely different. The force *does* exist, and the explanations which have hitherto been applied to it are no longer plausible. Some alteration of existing concepts is therefore unavoidable.

Furthermore, it should be recognized that the existing practice of attributing cohesion to electrical forces does not actually accomplish the objective of eliminating the necessity of dealing with a force of unknown origin, since the application of the electrical theory requires postulating the existence of another unknown force: one which is genuinely unknown, as there is no independent evidence of its existence. The electrical attraction between positive and negative ions would explain the cohesion of such ions, if they actually exist, but the electrical theory has no acceptable explanation as to how these ions can be formed to begin with. The atoms of the components of a chemical compound are electrically neutral before combination, and we know that ionization of such atoms is not a spontaneous process; it requires a substantial amount of energy. In order to accomplish the hypothetical ionization of the components some kind of a force is required, and we have never been given any indication of the nature of that force. We are told that certain numbers of electrons are more stable than others, and that the atoms tend to readjust themselves to the most stable condition. "The work needed to separate the charges is supplied by the spontaneous tendency toward the octet of valence electrons,"[131] explains one textbook. But this is an explanation of the "nature abhors a vacuum" type; it gives us no idea as to the kind of force involved, or the origin thereof, and even if we dress up this explanation in all of the trappings of quantum mechanics we still have an unknown force to contend with.

Then when we move to the non-ionic compounds, we find it necessary to look for still another unknown force. Here we not only have the problem of figuring out what kind of a force could induce the individual atoms to give up some of their electrons (if they had any), but also the problem of how these loose electrons could create a force of cohesion. Current theory is equally as vague on one point as the other-the mere assertion that rapid oscillation of the electrons between the two atoms creates some kind of an "exchange force" means nothing without some plausible explanation of how such a force originates-and when we get past the doubletalk and the "mathematical veil" of the quantum language, we find ourselves contending with *two* unknown forces.

Such attempts to fit the observed phenomena into a preconceived structure of thought get us nowhere; the alleged solution of one problem creates two new problems. In the construction of a new atomic theory it is imperative to recognize that there is a force which is responsible for cohesion. The atom of potassium attaches itself just as readily to another atom of the same element, or to an atom of another electropositive element, as it does to an atom of an electronegative element such as chlorine, and a crystal lattice of one of the regular types is formed in each case. The attractive force which the potassium atom exercises therefore something of a far more general nature than an electrical force between unlike charges. If it is possible to identify the origin of this force, so much the better, but if not, this cohesive force is in no different position than magnetic force or gravitational force. Current theory cannot explain the origin of these forces either. The essential thing is to set up a theory which will recognize the existence of this cohesive force, and will take advantage of the tremendous simplification of the whole chemical picture that can be accomplished by substituting this single force of wide applicability for the crazy quilt of forces and "bonds" that has been developed on the basis of the electrical theory.

The previous pages have expressed some harshly critical views concerning the theory of atomic structure originated by Bohr and developed through a long series of modifications and revisions into the present-day ideas championed particularly by the so-called Copenhagen school of physicists. It has been pointed out that Bohr's original postulates were of a highly questionable nature. There is serious doubt whether postulates of this kind, postulates which in effect set up a separate universe subject to totally different physical laws, should ever be recognized as legitimate scientific practice. Certainly they are of such a dangerous character that if they are allowed at all they should only be permitted as a last resort after the most strenuous efforts to meet the situation by the usual sound and proven methods of science have failed.

Even if no other fault could be found with the procedure that was followed, the two years of study of the problem that intervened between Rutherford's hypothesis of 1911 and Bohr's postulates of 1913 were completely inadequate to justify taking such a leap in the dark as that represented by Bohr's theory. The consequences of this inadequate preliminary study are now painfully apparent, when we find that Rutherford's hypothesis, on which the whole of the subsequent development rests, cannot endure critical scrutiny.

Furthermore, the developers of this theory, although they are among the foremost scientists of modern times, have made free, even lavish, use of the most questionable devices ever admitted into the scientific repertory: principles of impotence, *ad hoc* systems of physical laws to fit special circumstances, denial of physical reality, weird "principles" of an unprecedented character, and so on. The remarks of James R. Newman in this connection, which have already been mentioned briefly, are worth quoting in full. Newman says, "In this century the professional philosophers have let the physicists get away with murder. It is a safe bet that no other group of scientists could have passed off and gained acceptance for such an extraordinary principle as complementarity, nor succeeded in elevating indeterminacy to a universal law."[132]

Yet it must be admitted that in spite of all that can be said against this development, it has produced and, to a large extent, verified one idea that must necessarily be incorporated into the new theory that we will build on the ruins of the old. It is clear that whatever successes this theory may legitimately claim are due to Bohr's conviction that Planck's Quantum Theory, then in its infancy, should be extended and applied to the atomic situation in some manner. As a result of this conviction, he included, as one of the essential features of his system, the quantization of angular momentum. If we analyze the history of Bohr's theory and its extensions and modifications, we can readily see that the theory has met with considerable success wherever it is dealing with *numbers*, while it has almost invariably run into difficulties when it has attempted to apply *names* to those numbers. The inevitable result has been to attach less and less significance to the verbal description of the theory, and to develop an almost exclusively mathematical system which not only gives us no explanation of the meaning of the terms which it uses, but contends that no rational explanation of this kind exists. As expressed by Holton and Roller, "Some fundamental changes in (Bohr's) theory have been needed.... In essence, these changes have centered on the abandonment of the last vestiges of visualization of single events in the electron cloud surrounding the nucleus; the intuitively meaningful orbits, shells, and "jumps" of electrons had to be given up as being essentially meaningless ... and, in fact, misleading...."[114]

Bohr's work has been described as a marriage of Rutherford's theory of the nuclear atom with Planck's theory of the quantum. (Ernest Nagel calls it an "eclectic fusion.") The subsequent developments have had the effect of a divorce. The "abandonment of the last vestiges of visualization of single events" is the abandonment of the last vestiges of Rutherford's theory: the decree that makes the divorce final. All that we have left is what came originally from Planck, and since it is clear that some valid results of a mathematical nature have emanated from Bohr's work and its subsequent extensions, this can be taken as a vindication of Bohr's contention that Planck's concept of the quantum should be applicable to atomic processes.

The new atomic theory that replaces the nuclear atom must therefore embody the quantum concept in some manner. Since this present discussion is intended only to indicate the general nature of such a theory, no attempt will be made to explore details, but it may be mentioned that the existing evidence points very strongly to the necessity of a major enlargement of the quantum concept: an extension of this idea drastic enough to quantize *all* motion. Only in this manner can we introduce *numbers* at a basic enough level to give us the "built-in" mathematical relations that we need in order to eliminate the necessity for *ad hoc* assumptions to supply the numerical values.