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Chapter 7

Scientific Revolutions

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A. THE ESSENTIAL TENSION: TRADITION AND INNOVATION IN SCIENTIFIC RESEARCH

I am grateful for the invitation to participate in this important conference, and I interpret it as evidence that students of creativity themselves possess the sensitivity to divergent approaches that they seek to identify in others. But I am not altogether sanguine about the outcome of your experiment with me. As most of you already know, I am no psychologist, but rather an ex-physicist now working in the history of science. Probably my concern is no less with creativity than your own, but my goals, my techniques, and my sources of evidence are so very different from yours that I am far from sure how much we do, or even *should,* have to say to each other. These reservations imply no apology: rather they hint at my central thesis. In the sciences, as I shall suggest below, it is often better to do one's best with the tools at hand than to pause for contemplation of divergent approaches.

If a person of my background and interests has anything relevant to suggest to this conference, it will not be about your central concerns, the creative personality and its early identification. But implicit in the numerous working papers distributed to participants in this conference is an image of the scientific process and of the scientist; that image almost certainly conditions many of the experiments you try as well as the conclusions you draw; and about it the physicist-historian may well have something to say. I shall restrict my attention to one aspect of this image—an aspect epitomized as follows in one of the working papers: The basic scientist "must lack prejudice to a degree where he can look at the most 'self-evident' facts or concepts without necessarily accepting them, and, conversely, allow his imagination to play with the most unlikely possibilities" (Selye, 1959). In the more technical language supplied by other working papers (Getzels and Jackson), this aspect of the image recurs as an emphasis upon "divergent thinking, the freedom to go off in different directions, ... rejecting the old solution and striking out in some new direction."

I do not at all doubt that this description of "divergent thinking" and the concomitant search for those able to do it are entirely proper. Some divergence characterizes all scientific work, and gigantic divergences lie at the core of the most significant episodes in scientific development. But both my own experience in scientific research and my reading of the history of sciences lead me to wonder whether flexibility and open-mindedness have not been too exclusively emphasized as the characteristics requisite for basic research. I shall therefore suggest below that something like "con-

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vergent thinking" is just as essential to scientific advance as is divergent. Since these two modes of thought are inevitably in conflict, it will follow that the ability to support a tension that can occasionally become almost unbearable is one of the prime requisites for the very best sort of scientific research.

I am elsewhere studying these points more historically, with emphasis on the importance to scientific development of "revolutions."[1] These are episodes—exemplified in their most extreme and readily recognized form by the advent of Copernicanism, Darwinism, or Einsteinianism—in which a scientific community abandons one time-honored way of regarding the world and of pursuing science in favor of some other, usually incompatible, approach to its discipline. I have argued in the draft that the historian constantly encounters many far smaller but structurally similar revolutionary episodes and that they are central to scientific advance. Contrary to a prevalent impression, most new discoveries and theories in the sciences are not merely additions to the existing stockpile of scientific knowledge. To assimilate them the scientist must usually rearrange the intellectual and manipulative equipment he has previously relied upon, discarding some elements of his prior belief and practice while finding new significances in and new relationships between many others. Because the old must be revalued and reordered when assimilating the new, discovery and invention in the sciences are usually intrinsically revolutionary. Therefore, they do demand just that flexibility and open-mindedness that characterize, or indeed define, the divergent thinker. Let us henceforth take for granted the need for these characteristics. Unless many scientists possessed them to a marked degree, there would be no scientific revolutions and very little scientific advance.

Yet flexibility is not enough, and what remains is not obviously compatible with it. Drawing from various fragments of a project still in progress, I must now emphasize that revolutions are but one of two complementary aspects of scientific advance. Almost none of the research undertaken by even the greatest scientists is designed to be revolutionary, and very little of it has any such effect. On the contrary, normal research, even the best of it, is a highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession. Typically, to be sure, this convergent or consensus-bound research ultimately results in revolution. Then, traditional techniques and beliefs are abandoned and replaced by new ones. But revolutionary shifts of a scientific tradition are relatively rare, and extended periods of convergent research are the necessary preliminary to them. As I shall indicate below, only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one. That is why I speak of an 'essential tension' implicit in scientific research. To do his job the scientist must undertake a complex set of intellectual and manipulative commitments. Yet his claim to fame, if he has the talent and good luck to gain one, may finally rest upon his ability to abandon this net of commitments in favor of another of his own invention. Very often the successful scientist must simultaneously display the characteristics of the traditionalist and of the iconoclast.[2]

The multiple historical examples upon which any full documentation of these points must depend are prohibited by the time limitations of the conference. But another approach will introduce you to at least part of what I have in mind—an examination of the nature of education in the natural sciences. One of the working papers for this conference (Getzels and Jackson) quotes Guilford's very apt description of scientific education as follows: "[It] has emphasized abilities in the areas of convergent thinking and evaluation, often at the expense of development in the area of divergent thinking. We have attempted to teach students how to arrive at 'correct' answers that our civilization has taught us are correct.... Outside the arts [and I should include most of the social sciences] we have generally discouraged the development of divergent-thinking abilities, unintentionally." That characterization seems to me eminently just, but I wonder whether it is equally just to deplore the product that results. Without defending plain bad teaching, and granting that in this country the trend to convergent thinking in all education may have proceeded entirely too far, we may nevertheless recognize that a rigorous training in convergent thought has been intrinsic to the sciences almost from their origin. I suggest that they could not have achieved their present state or status without it.

Let me try briefly to epitomize the nature of education in the natural sciences, ignoring the many significant yet minor differences between the various sciences and between the approaches of different educational institutions. The single most striking feature of this education is that, to an extent totally unknown in other creative fields, it is conducted entirely through textbooks. Typically, undergraduate *and* graduate students of chemistry, physics, astronomy, geology, or biology acquire the substance of their fields from books written especially for students. Until they are ready, or very nearly ready, to commence work on their own dissertations, they are neither asked to attempt trial research projects nor exposed to the immediate products of research done by others, that is, to the professional communications that scientists write for each other. There are no collections of "readings" in the natural sciences. Nor are science students encouraged to read the historical classics of their fields—works in which they might discover other ways of regarding the problems discussed in their textbooks, but in which they would also meet problems, concepts, and standards of solution that their future professions have long since discarded and replaced.

In contrast, the various textbooks that the student does encounter display different subject matters, rather than, as in many of the social sciences, exemplifying different approaches to a single problem field. Even books that compete for adoption in a single course differ mainly in level and in pedagogic detail, not in substance or conceptual structure. Last, but most important of all, is the characteristic technique of textbook presentation. Except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution. Rather, these books exhibit concrete problem solutions that the profession has come to accept as paradigms, and they then ask the student, either with a pencil and paper or in the laboratory, to solve for himself problems very closely related in both method and substance to those through which the textbook or the accompanying lecture has led him. Nothing could be better calculated to produce "mental sets" or *Einstellungen.* Only in their most elementary courses do other academic fields offer as much as a partial parallel.

Even the most faintly liberal educational theory must view this pedagogic technique as anathema. Students, we would all agree, must begin by learning a good deal of what is already known, but we also insist that education give them vastly more. They must, we say, learn to recognize and evaluate problems to which no unequivocal solution has yet been given; they must be supplied with an arsenal of techniques for approaching these future problems; and they must learn to judge the relevance of these techniques and to evaluate the possibly partial solutions which they can provide. In many respects these attitudes toward education seem to me entirely right, and yet we must recognize two things about them. First, education in the natural sciences seems to have been totally unaffected by their existence. It remains a dogmatic initiation in a pre-established tradition that the student is not equipped to evaluate. Second, at least in the period when it was followed by a term in an apprenticeship relation, this technique of exclusive exposure to a rigid tradition has been immensely productive of the most consequential sorts of innovations.

I shall shortly inquire about the pattern of scientific practice that grows out of this educational initiation and will then attempt to say why that pattern proves quite so successful. But first, an historical excursion will reinforce what has just been said and prepare the way for what is to follow. I should like to suggest that the various fields of natural science have not always been characterized by rigid education in exclusive paradigms, but that each of them acquired something like that technique at precisely the point when the field began to make rapid and systematic progress. If one asks about the origin of our contemporary knowledge of chemical composition, of earthquakes, of biological reproduction, of motion through space, or of any other subject matter known to the natural sciences, one immediately encounters a characteristic pattern that I shall here illustrate with a single example.

Today, physics textbooks tell us that light exhibits some properties of a wave and some of a particle: both textbook problems and research problems are designed accordingly. But both this view and these textbooks are products of an early twentieth-century revolution. (One characteristic of scientific revolutions is that they call for the rewriting of science textbooks.) For more than half a century before 1900, the books employed in scientific education had been equally unequivocal in stating that light was wave motion. Under those circumstances scientists worked on somewhat different problems and often embraced rather different sorts of solutions to them. The nineteenth-century textbook tradition does not, however, mark the beginning of our subject matter. Throughout the eighteenth century and into the early nineteenth, Newton's *Opticks* and the other books from which men learned science taught almost all students that light was particles, and research guided by this tradition was again different from that which succeeded it. Ignoring a variety of subsidiary changes within these three successive traditions, we may therefore say that our views derive historically from Newton's views by way of two revolutions in optical thought, each of which replaced one tradition of convergent research with another. If we make appropriate allowances for changes in the locus and materials of scientific education, we may say that each of these three traditions was embodied in the sort of education by exposure to unequivocal paradigms that I briefly epitomized above. Since Newton, education and research in physical optics have normally been highly convergent.

The history of theories of light does not, however, begin with Newton. If we ask about knowledge in the field before his time, we encounter a significantly different pattern—a pattern still familiar in the arts and in some social sciences, but one which has largely disappeared in the natural sciences. From remote antiquity until the end of the seventeenth century there was no single set of paradigms for the study of physical optics. Instead, many men advanced a large number of different views about the nature of light. Some of these views found few adherents, but a number of them gave rise to continuing schools of optical thought. Although the historian can note the emergence of new points of view as well as changes in the relative popularity of older ones, there was never anything resembling consensus. As a result, a new man entering the field was inevitably exposed to a variety of conflicting viewpoints; he was forced to examine the evidence for each, and there always was good evidence. The fact that he made a choice and conducted himself accordingly could not entirely prevent his awareness of other possibilities. This earlier mode of education was obviously more suited to produce a scientist without prejudice, alert to novel phenomena, and flexible in his approach to his field. On the other hand, one can scarcely escape the impression that, during the period characterized by this more liberal educational practice, physical optics made very little progress.[3]

The preconsensus (we might here call it the divergent) phase in the development of physical optics is, I believe, duplicated in the history of all other scientific specialties, excepting only those that were born by the subdivision and recombination of preexisting disciplines. In some fields, like mathematics and astronomy, the first firm consensus is prehistoric. In others, like dynamics, geometric optics, and parts of physiology, the paradigms that produced a first consensus date from classical antiquity. Most other natural sciences, though their problems were often discussed in antiquity, did not achieve a first consensus until after the Renaissance. In physical optics, as we have seen, the first firm consensus dates only from the end of the seventeenth century; in electricity, chemistry, and the study of heat, it dates from the eighteenth; while in geology and the nontaxonomic parts of biology no very real consensus developed until after the first third of the nineteenth century. This century appears to be characterized by the emergence of a first consensus in parts of a few of the social sciences.

In all the fields named above, important work was done before the achievement of the maturity produced by consensus. Neither the nature nor the timing of the first consensus in these fields can be understood without a careful examination of both the intellectual and the manipulative techniques developed before the existence of unique paradigms. But the transition to maturity is not less significant because individuals practiced science before it occurred. On the contrary, history strongly suggests that, though one can practice science—as one does philosophy or art or political science—without a firm consensus, this more flexible practice will not produce the pattern of rapid consequential scientific advance to which recent centuries have accustomed us. In that pattern, development occurs from one consensus to another, and alternate approaches are not ordinarily in competition. Except under quite special conditions, the practitioner of a mature science does not pause to examine divergent modes of explanation or experimentation.

I shall shortly ask how this can be so—how a firm orientation toward an apparently unique tradition can be compatible with the practice of the disciplines most noted for the persistent production of novel ideas and techniques. But it will help first to ask what the education that so successfully transmits such a tradition leaves to be done. What can a scientist working within a deeply rooted tradition and little trained in the perception of significant alternatives hope to do in his professional career? Once again limits of time force me to drastic simplification, but the following remarks will at least suggest a position that I am sure can be documented in detail.

In pure or basic science—that somewhat ephemeral category of research undertaken by men whose most immediate goal is to increase understanding rather than control of nature—the characteristic problems are almost always repetitions, with minor modifications, of problems that have been undertaken and partially resolved before. For example, much of the research undertaken within a scientific tradition is an attempt to adjust existing theory or existing observation in order to bring the two into closer and closer agreement. The constant examination of atomic and molecular spectra during the years since the birth of wave mechanics, together with the design of theoretical approximations for the prediction of complex spectra, provides one important instance of this typical sort of work. Another was provided by the remarks about the eighteenth-century development of Newtonian dynamics in the paper on measurement supplied to you in advance of the conference.[4] The attempt to make existing theory and observation conform more closely is not, of course, the only standard sort of research problem in the basic sciences. The development of chemical thermodynamics or the continuing attempts to unravel organic structure illustrate another type—the extension of existing theory to areas that it is expected to cover but in which it has never before been tried. In addition, to mention a third common sort of research problem, many scientists constantly collect the concrete data (e.g., atomic weights, nuclear moments) required for the application and extension of existing theory.

These are normal research projects in the basic sciences, and they illustrate the sorts of work on which all scientists, even the greatest, spend most of their professional lives and on which many spend all. Clearly their pursuit is neither intended nor likely to produce fundamental discoveries or revolutionary changes in scientific theory. Only if the validity of the contemporary scientific tradition is assumed do these problems make much theoretical or any practical sense. The man who suspected the existence of a totally new type of phenomenon or who had basic doubts about the validity of existing theory would not think problems so closely modeled on textbook paradigms worth undertaking. It follows that the man who does undertake a problem of this sort—and that means all scientists at most times—aims to elucidate the scientific tradition in which he was raised rather than to change it. Furthermore, the fascination of his work lies in the difficulties of elucidation rather than in any surprises that the work is likely to produce. Under normal conditions the research scientist is not an innovator but a solver of puzzles, and the puzzles upon which he concentrates are just those which he believes can be both stated and solved within the existing scientific tradition.

Yet—and this is the point—the ultimate effect of this tradition-bound work has invariably been to change the tradition. Again and again the continuing attempt to elucidate a currently received tradition has at last produced one of those shifts in fundamental theory, in problem field, and in scientific standards to which I previously referred as scientific revolutions. At least for the scientific community as a whole, work within a well-defined and deeply ingrained tradition seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved. How can this be so? I think it is because no other sort of work is nearly so well suited to isolate for continuing and concentrated attention those loci of trouble or causes of crisis upon whose recognition the most fundamental advances in basic science depend.

As I have indicated in the first of my working papers, new theories and, to an increasing extent, novel discoveries in the mature sciences are not born *de novo*. On the contrary, they emerge from old theories and within a matrix of old beliefs about the phenomena that the world does *and does not* contain. Ordinarily such novelties are far too esoteric and recondite to be noted by the man without a great deal of scientific training. And even the man with considerable training can seldom afford simply to go out and look for them, let us say by exploring those areas in which existing data and theory have failed to produce understanding. Even in a mature science there are always far too many such areas, areas in which no existing paradigms seem obviously to apply and for whose exploration few tools and standards are available. More likely than not the scientist who ventured into them, relying merely upon his receptivity to new phenomena and his flexibility to new patterns of organization, would get nowhere at all. He would rather return his science to its preconsensus or natural history phase.

Instead, the practitioner of a mature science, from the beginning of his doctoral research, continues to work in the regions for which the paradigms derived from his education and from the research of his contemporaries seem adequate. He tries, that is, to elucidate topographical detail on a map whose main outlines are available in advance, and he hopes—if he is wise enough to recognize the nature of his field—that he will some day undertake a problem in which the anticipated does *not* occur, a problem that goes wrong in ways suggestive of a fundamental weakness in the paradigm itself. In the mature sciences the prelude to much discovery and to all novel theory is not ignorance, but the recognition that something has gone wrong with existing knowledge and beliefs.

What I have said so far may indicate that it is sufficient for the productive scientist to adopt existing theory as a lightly held tentative hypothesis, employ it *faute de mieux* in order to get a start in his research, and then abandon it as soon as it leads him to a trouble spot, a point at which something has gone wrong. But though the ability to recognize trouble when confronted by it is surely a requisite for scientific advance, trouble must not be too easily recognized. The scientist requires a thoroughgoing commitment to the tradition with which, if he is fully successful, he will break. In part this commitment is demanded by the nature of the problems the scientist normally undertakes. These, as we have seen, are usually esoteric puzzles whose challenge lies less in the information disclosed by their solutions (all but its details are often known in advance) than in the difficulties of technique to be surmounted in providing any solution at all. Problems of this sort are undertaken only by men assured that there is a solution which ingenuity can disclose, and only current theory could possibly provide assurance of that sort. That theory alone gives meaning to most of the problems of normal research. To doubt it is often to doubt that the complex technical puzzles which constitute normal research have any solutions at all. Who, for example, would have developed the elaborate mathematical techniques required for the study of the effects of interplanetary attractions upon basic Keplerian orbits if he had not assumed that Newtonian dynamics, applied to the planets then known, would explain the last details of astronomical observation? But without that assurance, how would Neptune have been discovered and the list of planets changed?

In addition, there are pressing practical reasons for commitment. Every research problem confronts the scientist with anomalies whose sources he cannot quite identify. His theories and observations never quite agree; successive observations never yield quite the same results; his experiments have both theoretical and phenomenological by-products which it would take another research project to unravel. Each of these anomalies or incompletely understood phenomena could conceivably be the clue to a fundamental innovation in scientific theory or technique, but the man who pauses to examine them one by one never completes his first project. Reports of effective research repeatedly imply that all but the most striking and central discrepancies could be taken care of by current theory if only there were time to take them on. The men who make these reports find most discrepancies trivial or uninteresting, an evaluation that they can ordinarily base only upon their faith in current theory. Without that faith their work would be wasteful of time and talent.

Besides, lack of commitment too often results in the scientist's undertaking problems that he has little chance of solving. Pursuit of an anomaly is fruitful only if the anomaly is more than nontrivial. Having discovered it, the scientist's first efforts and those of his profession are to do what nuclear physicists are now doing. They strive to generalize the anomaly, to discover other and more revealing manifestations of the same effect, to give it structure by examining its complex interrelationships with phenomena they still feel they understand. Very few anomalies are susceptible to this sort of treatment. To be so they must be in explicit and unequivocal conflict with some structurally central tenet of current scientific belief. Therefore, their recognition and evaluation once again depend upon a firm commitment to the contemporary scientific tradition.

This central role of an elaborate and often esoteric tradition is what I have principally had in mind when speaking of the essential tension in scientific research. I do not doubt that the scientist must be, at least potentially, an innovator, that he must possess mental flexibility, and that he must be prepared to recognize troubles where they exist. That much of the popular stereotype is surely correct, and it is important accordingly to search for indices of the corresponding personality characteristics. But what is no part of our stereotype and what appears to need careful integration with it is the other face of this same coin. We are, I think, more likely fully to exploit our potential scientific talent if we recognize the extent to which the basic scientist must also be a firm traditionalist, or, if I am using your vocabulary at all correctly, a convergent thinker. Most important of all, we must seek to understand how these two superficially discordant modes of problem solving can be reconciled both within the individual and within the group.

Everything said above needs both elaboration and documentation. Very likely some of it will change in the process. This paper is a report on work in progress. But, though I insist that much of it is tentative and all of it incomplete, I still hope that the paper has indicated why an educational system best described as an initiation into an unequivocal tradition should be thoroughly compatible with successful scientific work. And I hope, in addition, to have made plausible the historical thesis that no part of science has progressed very far or very rapidly before this convergent education and correspondingly convergent normal practice became possible. Finally, though it is beyond my competence to derive personality correlates from this view of scientific development, I hope to have made meaningful the view that the productive scientist must be a traditionalist who enjoys playing intricate games by pre-established rules in order to be a successful innovator who discovers new rules and new pieces with which to play them.

As first planned, my paper was to have ended at this point. But work on it, against the background supplied by the working papers distributed to conference participants, has suggested the need for a postscript. Let me therefore briefly try to eliminate a likely ground of misunderstanding and simultaneously suggest a problem that urgently needs a great deal of investigation.

Everything said above was intended to apply strictly only to basic science, an enterprise whose practitioners have ordinarily been relatively free to choose their own problems. Characteristically, as I have indicated, these problems have been selected in areas where paradigms were clearly applicable but where exciting puzzles remained about how to apply them and how to make nature conform to the results of the application. Clearly the inventor and applied scientist are not generally free to choose puzzles of this sort. The problems among which they may choose are likely to be largely determined by social, economic, or military circumstances external to the sciences. Often the decision to seek a cure for a virulent disease, a new source of household illumination, or an alloy able to withstand the intense heat of rocket engines must be made with little reference to the state of the relevant science. It is, I think, by no means clear that the personality characteristics requisite for pre-eminence in this more immediately practical sort of work are altogether the same as those required for a great achievement in basic science. History indicates that only a few individuals, most of whom worked in readily demarcated areas, have achieved eminence in both.

I am by no means clear where this suggestion leads us. The troublesome distinctions between basic research, applied research, and invention need far more investigation.

Nevertheless, it seems likely, for example, that the applied scientist, to whose problems no scientific paradigm need be fully relevant, may profit by a far broader and less rigid education than that to which the pure scientist has characteristically been exposed. Certainly there are many episodes in the history of technology in which lack of more than the most rudimentary scientific education has proved to be an immense help. This group scarcely needs to be reminded that Edison's electric light was produced in the face of unanimous scientific opinion that the arc light could not be "subdivided," and there are many other episodes of this sort.

This must not suggest, however, that mere differences in education will transform the applied scientist into a basic scientist or vice versa One could at least argue that Edison's personality, ideal for the inventor and perhaps also for the "oddball" in applied science, barred him from fundamental achievements in the basic sciences. He himself expressed great scorn for scientists and thought of them as wooly-headed people to be hired when needed. But this did not prevent his occasionally arriving at the most sweeping and irresponsible scientific theories of his own. (The pattern recurs in the early history of electrical technology: both Tesla and Gramme advanced absurd cosmic schemes that they thought deserved to replace the current scientific knowledge of their day.) Episodes like this reinforce an impression that the personality requisites of the pure scientist and of the inventor may be quite different, perhaps with those of the applied scientist lying somewhere between.[5]

Is there a further conclusion to be drawn from all this? One speculative thought forces itself upon me. If I read the working papers correctly, they suggest that most of you are really in search of the *inventive* personality, a sort of person who does emphasize divergent thinking but whom the United States has aleady produced in abundance. In the process you may be ignoring certain of the essential requisites of the basic scientist, a rather different sort of person, to whose ranks America's contributions have as yet been notoriously sparse. Since most of you are, in fact, Americans, this correlation may not be entirely coincidental.

Notes

1. *The Structure of Scientific Revolutions* (Chicago, 1962).
2. Strictly speaking, it is the professional group rather than the individual scientist that must display both these characteristics simultaneously. In a fuller account of the ground covered in this paper that distinction between individual and group characteristics would be basic. Here I can only note that, though recognition of the distinction weakens the conflict or tension referred to above, it does not eliminate it. Within the group some individuals may be more traditionalistic, others more iconoclastic, and their contributions may differ accordingly. Yet education, institutional norms, and the nature of the job to be done will inevitably combine to insure that all group members will, to a greater or lesser extent, be pulled in both direction.
3. The history of physical optics before Newton has recently been well described by Vasco Ronchi in *Histoire de la lumière*, trans. J. Taton (Paris, 1956). His account does justice to the element I elaborate too little above. Many fundamental contributions to physical optics were made in the two millennia before Newton's work. Consensus is not prerequisite to a sort of progress in the natural sciences, any more than it is to progress in the social sciences or the arts. It is, however, prerequisite to the sort of progress that we now generally refer to when distinguishing the natural sciences from the arts and from most social sciences.
4. A revised version appeared in *Isis* 52 (1961): 161–93.
5. For the attitude of scientists toward the technical possibility of the incandescent light see Francis A. Jones, *Thomas Alva Edison* (New York, 1908), pp. 99–100, and Harold C. Passer, *The Electrical Manufacturers, 1875–1900* (Cambridge, Mass., 1953). pp. 82–83. For Edison's attitude toward scientists see Passer, ibid., pp. 180–81. For a sample of Edison's theorizing in realms otherwise subject to scientific treatments see Dagobert D. Runes, ed., *The Diary and Sundry Observatons of Thomas Alva Edison* (New York, 1948), pp. 205–44, passim.