The Structure of Scientific Revolutions

Second Edition, Enlarged

Thomas S. Kuhn

UNIVERSITY OF CHICAGO PRESS

International Encyclopedia of Unified Science

Editor-in-Chief Otto Neurath
Associate Editors Rudolf Carnap Charles Morris

Foundations of the Unity of Science

(Volumes I–II of the Encyclopedia)

The Structure of Scientific Revolutions was originally published in 1962 as part of *Foundations of the Unity of Science,* which constituted volumes 1 and 2 of the *International Encyclopoedia of Unified Science.* The organizational chart above is reproduced from the copyright page of the enlarged, second edition of *The Structure of Scientific Revolutions* published separately by the University of Chicago Press in 1970.

The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London

Second edition, enlarged, 1970
Printed in the United States of America

01 00 99 98 97 96 95 94 21 22 23 24 25

ISBN 0-226-45803-2 (cloth);
0-226-45804-0 (paper)

Contents

Preface

The essay that follows is the first full published report on a project originally conceived almost fifteen years ago. At that time I was a graduate student in theoretical physics already within sight of the end of my dissertation. A fortunate involvement with an experimental college course treating physical science for the non-scientist provided my first exposure to the history of science. To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my basic conceptions about the nature of science and the reasons for its special success.

Those conceptions were ones I had previously drawn partly from scientific training itself and partly from a long-standing avocational interest in the philosophy of science. Somehow, whatever their pedagogic utility and their abstract plausibility, those notions did not at all fit the enterprise that historical study displayed. Yet they were and are fundamental to many discussions of science, and their failures of verisimilitude therefore seemed thoroughly worth pursuing. The result was a drastic shift in my career plans, a shift from physics to history of science and then, gradually, from relatively straightforward historical problems back to the more philosophical concerns that had initially led me to history. Except for a few articles, this essay is the first of my published works in which these early concerns are dominant. In some part it is an attempt to explain to myself and to friends how I happened to be drawn from science to its history in the first place.

My first opportunity to pursue in depth some of the ideas set forth below was provided by three years as a Junior Fellow of the Society of Fellows of Harvard University. Without that period of freedom the transition to a new field of study would have been far more difficult and might not have been achieved. Part of my time in those years was devoted to history of science proper. In particular I continued to study the writings of Alexandre Koyré and first encountered those of Emile Meyerson, Hélène Metzger, and Anneliese Maier.[1] More clearly than most other recent scholars, this group has shown what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today. Though I increasingly question a few of their particular historical interpretations, their works, together with A. O. Lovejoy's *Great Chain of Being*, have been second only to primary source materials in shaping my conception of what the history of scientific ideas can be.

Much of my time in those years, however, was spent exploring fields without apparent relation to history of science but in which research now discloses problems like the ones history was bringing to my attention. A footnote encountered by chance led me to the experiments by which Jean Piaget has illuminated both the various worlds of the growing child and the process of transition from one to the next.[2] One of my colleagues set me to reading papers in the psychology of perception, particularly the Gestalt psychologists; another introduced me to B. L. Whorf's speculations about the effect of language on world view; and W. V. O. Quine opened for me the philosophical puzzles of the analytic-synthetic distinction.[3] That is the sort of random exploration that the Society of Fellows permits, and only through it could I have encountered Ludwik Fleck's almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel, 1935), an essay that anticipates many of my own ideas. Together with a remark from another Junior Fellow, Francis X. Sutton, Fleck's work made me realize that those ideas might require to be set in the sociology of the scientific community. Though readers will find few references to either these works or conversations below, I am indebted to them in more ways than I can now reconstruct or evaluate.

During my last year as a Junior Fellow, an invitation to lecture for the Lowell Institute in Boston provided a first chance to try out my still developing notion of science. The result was a series of eight public lectures, delivered during March, 1951, on "The Quest for Physical Theory." In the next year I began to teach history of science proper, and for almost a decade the problems of instructing in a field I had never systematically studied left little time for explicit articulation of the ideas that had first brought me to it. Fortunately, however, those ideas proved a source of implicit orientation and of some problem-structure for much of my more advanced teaching. I therefore have my students to thank for invaluable lessons both about the viability of my views and about the techniques appropriate to their effective communication. The same problems and orientation give unity to most of the dominantly historical, and apparently diverse, studies I have published since the end of my fellowship. Several of them deal with the integral part played by one or another metaphysic in creative scientific research. Others examine the way in which the experimental bases of a new theory are accumulated and assimilated by men committed to an incompatible older theory. In the process they describe the type of development that I have below called the "emergence" of a new theory or discovery. There are other such ties besides.

The final stage in the development of this essay began with an invitation to spend the year 1958–59 at the Center for Advanced Studies in the Behavioral Sciences. Once again I was able to give undivided attention to the problems discussed below. Even more important, spending the year in a community

1 Particularly influential were Alexandre Koyré, *Etudes Galiléennes* (3 vols.; Paris, 1939); Emile Meyerson, *Identity and Reality*, trans. Kate Loewenberg (New York, 1930); Hélène Metzger, *Les doctrines chimiques en France du début du XVII^e à la fin du XVIII^e siècle* (Paris, 1923), and *Newton, Stahl, Boerhaave et la doctrine chimique* (Paris, 1930); and Anneliese Maier, *Die Vorläufer Galileis im 14. Jahrhundert* ("Studien zur Naturphilosophie der Spätscholastik"; Rome, 1949).

2 Because they displayed concepts and processes that also emerge directly from the history of science, two sets of Piaget's investigations proved particularly important: *The Child's Conception of Causality*, trans. Marjorie Gabain (London, 1930), and *Les notions de mouvement et de vitesse chez l'enfant* (Paris, 1946).

3 Whorf's papers have since been collected by John B. Carroll, *Language, Thought, and Reality—Selected Writings of Benjamin Lee Whorf* (New York, 1956). Quine has presented his views in "Two Dogmas of Empiricism," reprinted in his *From a Logical Point of View* (Cambridge, Mass., 1953), pp. 20–46.

composed predominantly of social scientists confronted me with unanticipated problems about the differences between such communities and those of the natural scientists among whom I had been trained. Particularly, I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called "paradigms." These I take to be universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners. Once that piece of my puzzle fell into place, a draft of this essay emerged rapidly.

The subsequent history of that draft need not be recounted here, but a few words must be said about the form that it has preserved through revisions. Until a first version had been completed and largely revised, I anticipated that the manuscript would appear exclusively as a volume in the *Encyclopedia of Unified Science*. The editors of that pioneering work had first solicited it, then held me firmly to a commitment, and finally waited with extraordinary tact and patience for a result. I am much indebted to them, particularly to Charles Morris, for wielding the essential goad and for advising me about the manuscript that resulted. Space limits of the *Encyclopedia* made it necessary, however, to present my views in an extremely condensed and schematic form. Though subsequent events have somewhat relaxed those restrictions and have made possible simultaneous independent publication, this work remains an essay rather than the full-scale book my subject will ultimately demand.

Since my most fundamental objective is to urge a change in

the perception and evaluation of familiar data, the schematic character of this first presentation need be no drawback. On the contrary, readers whose own research has prepared them for the sort of reorientation here advocated may find the essay form both more suggestive and easier to assimilate. But it has disadvantages as well, and these may justify my illustrating at the very start the sorts of extension in both scope and depth that I hope ultimately to include in a longer version. Far more historical evidence is available than I have had space to exploit below. Furthermore, that evidence comes from the history of biological as well as of physical science. My decision to deal here exclusively with the latter was made partly to increase this essay's coherence and partly on grounds of present competence. In addition, the view of science to be developed here suggests the potential fruitfulness of a number of new sorts of research, both historical and sociological. For example, the manner in which anomalies, or violations of expectation, attract the increasing attention of a scientific community needs detailed study, as does the emergence of the crises that may be induced by repeated failure to make an anomaly conform. Or again, if I am right that each scientific revolution alters the historical perspective of the community that experiences it, then that change of perspective should affect the structure of postrevolutionary textbooks and research publications. One such effect—a shift in the distribution of the technical literature cited in the footnotes to research reports—ought to be studied as a possible index to the occurrence of revolutions.

The need for drastic condensation has also forced me to forego discussion of a number of major problems. My distinction between the pre- and the post-paradigm periods in the development of a science is, for example, much too schematic. Each of the schools whose competition characterizes the earlier period is guided by something much like a paradigm; there are circumstances, though I think them rare, under which two paradigms can coexist peacefully in the later period. Mere possession of a paradigm is not quite a sufficient criterion for the developmental transition discussed in Section II. More important, except in occasional brief asides, I have said nothing about the role of technological advance or of external social, economic, and intellectual conditions in the development of the sciences. One need, however, look no further than Copernicus and the calendar to discover that external conditions may help to transform a mere anomaly into a source of acute crisis. The same example would illustrate the way in which conditions outside the sciences may influence the range of alternatives available to the man who seeks to end a crisis by proposing one or another revolutionary reform.[4] Explicit consideration of effects like these would not, I think, modify the main theses developed in this essay, but it would surely add an analytic dimension of first-rate importance for the understanding of scientific advance.

Finally, and perhaps most important of all, limitations of space have drastically affected my treatment of the philosophical implications of this essay's historically oriented view of science. Clearly, there are such implications, and I have tried both to point out and to document the main ones. But in doing so I have usually refrained from detailed discussion of the various positions taken by contemporary philosophers on the corresponding issues. Where I have indicated skepticism, it has more often been directed to a philosophical attitude than to any one of its fully articulated expressions. As a result, some of those who know and work within one of those articulated positions may feel that I have missed their point. I think they will be wrong, but this essay is not calculated to convince them. To attempt that would have required a far longer and very different sort of book.

The autobiographical fragments with which this preface

[4] These factors are discussed in T. S. Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge, Mass., 1957), pp. 122–32, 270–71. Other effects of external intellectual and economic conditions upon substantive scientific development are illustrated in my papers, "Conservation of Energy as an Example of Simultaneous Discovery," *Critical Problems in the History of Science*, ed. Marshall Clagett (Madison, Wis., 1959), pp. 321–56; "Engineering Precedent for the Work of Sadi Carnot," *Archives internationales d'histoire des sciences*, XIII (1960), 247–51; and "Sadi Carnot and the Cagnard Engine," *Isis*, LII (1961), 567–74. It is, therefore, only with respect to the problems discussed in this essay that I take the role of external factors to be minor.

opens will serve to acknowledge what I can recognize of my main debt both to the works of scholarship and to the institutions that have helped give form to my thought. The remainder of that debt I shall try to discharge by citation in the pages that follow. Nothing said above or below, however, will more than hint at the number and nature of my personal obligations to the many individuals whose suggestions and criticisms have at one time or another sustained and directed my intellectual development. Too much time has elapsed since the ideas in this essay began to take shape; a list of all those who may properly find some signs of their influence in its pages would be almost coextensive with a list of my friends and acquaintances. Under the circumstances, I must restrict myself to the few most significant influences that even a faulty memory will never entirely suppress.

It was James B. Conant, then president of Harvard University, who first introduced me to the history of science and thus initiated the transformation in my conception of the nature of scientific advance. Ever since that process began, he has been generous of his ideas, criticisms, and time—including the time required to read and suggest important changes in the draft of my manuscript. Leonard K. Nash, with whom for five years I taught the historically oriented course that Dr. Conant had started, was an even more active collaborator during the years when my ideas first began to take shape, and he has been much missed during the later stages of their development. Fortunately, however, after my departure from Cambridge, his place as creative sounding board and more was assumed by my Berkeley colleague, Stanley Cavell. That Cavell, a philosopher mainly concerned with ethics and aesthetics, should have reached conclusions quite so congruent to my own has been a constant source of stimulation and encouragement to me. He is, furthermore, the only person with whom I have ever been able to explore my ideas in incomplete sentences. That mode of communication attests an understanding that has enabled him to point me the way through or around several major barriers encountered while preparing my first manuscript.

Since that version was drafted, many other friends have helped with its reformulation. They will, I think, forgive me if I name only the four whose contributions proved most far-reaching and decisive: Paul K. Feyerabend of Berkeley, Ernest Nagel of Columbia, H. Pierre Noyes of the Lawrence Radiation Laboratory, and my student, John L. Heilbron, who has often worked closely with me in preparing a final version for the press. I have found all their reservations and suggestions extremely helpful, but I have no reason to believe (and some reason to doubt) that either they or the others mentioned above approve in its entirety the manuscript that results.

My final acknowledgments, to my parents, wife, and children, must be of a rather different sort. In ways which I shall probably be the last to recognize, each of them, too, has contributed intellectual ingredients to my work. But they have also, in varying degrees, done something more important. They have, that is, let it go on and even encouraged my devotion to it. Anyone who has wrestled with a project like mine will recognize what it has occasionally cost them. I do not know how to give them thanks.

T. S. K.

BERKELEY, CALIFORNIA
February 1962

I. Introduction: A Role for History

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. This essay attempts to show that we have been misled by them in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself.

Even from history, however, that new concept will not be forthcoming if historical data continue to be sought and scrutinized mainly to answer questions posed by the unhistorical stereotype drawn from science texts. Those texts have, for example, often seemed to imply that the content of science is uniquely exemplified by the observations, laws, and theories described in their pages. Almost as regularly, the same books have been read as saying that scientific methods are simply the ones illustrated by the manipulative techniques used in gathering textbook data, together with the logical operations employed when relating those data to the textbook's theoretical generalizations. The result has been a concept of science with profound implications about its nature and development.

If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who, successfully or not, have striven to contribute one or another element to that particular constellation. Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. Concerned with scientific development, the historian then appears to have two main tasks. On the one hand, he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other, he must describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text. Much research has been directed to these ends, and some still is.

In recent years, however, a few historians of science have been finding it more and more difficult to fulfil the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask. Perhaps science does not develop by the accumulation of individual discoveries and inventions. Simultaneously, these same historians confront growing difficulties in distinguishing the "scientific" component of past observation and belief from what their predecessors had readily labeled "error" and "superstition." The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-

date theories are not in principle unscientific because they have been discarded. That choice, however, makes it difficult to see scientific development as a process of accretion. The same historical research that displays the difficulties in isolating individual inventions and discoveries gives ground for profound doubts about the cumulative process through which these individual contributions to science were thought to have been compounded.

The result of all these doubts and difficulties is a historiographic revolution in the study of science, though one that is still in its early stages. Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint—usually very different from that of modern science—that gives those opinions the maximum internal coherence and the closest possible fit to nature. Seen through the works that result, works perhaps best exemplified in the writings of Alexandre Koyré, science does not seem altogether the same enterprise as the one discussed by writers in the older historiographic tradition. By implication, at least, these historical studies suggest the possibility of a new image of science. This essay aims to delineate that image by making explicit some of the new historiography's implications.

What aspects of science will emerge to prominence in the course of this effort? First, at least in order of presentation, is the insufficiency of methodological directives, by themselves, to dictate a unique substantive conclusion to many sorts of scientific questions. Instructed to examine electrical or chemical phenomena, the man who is ignorant of these fields but who knows what it is to be scientific may legitimately reach any one of a number of incompatible conclusions. Among those legitimate possibilities, the particular conclusions he does arrive at are probably determined by his prior experience in other fields, by the accidents of his investigation, and by his own individual makeup. What beliefs about the stars, for example, does he bring to the study of chemistry or electricity? Which of the many conceivable experiments relevant to the new field does he elect to perform first? And what aspects of the complex phenomenon that then results strike him as particularly relevant to an elucidation of the nature of chemical change or of electrical affinity? For the individual, at least, and sometimes for the scientific community as well, answers to questions like these are often essential determinants of scientific development. We shall note, for example, in Section II that the early developmental stages of most sciences have been characterized by continual competition between a number of distinct views of nature, each partially derived from, and all roughly compatible with, the dictates of scientific observation and method. What differentiated these various schools was not one or another failure of method—they were all "scientific"—but what we shall come to call their incommensurable ways of seeing the world and of practicing science in it. Observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of such belief. An apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time.

That element of arbitrariness does not, however, indicate that any scientific group could practice its trade without some set of received beliefs. Nor does it make less consequential the particular constellation to which the group, at a given time, is in fact committed. Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities

of which the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities and what techniques employed in seeking solutions? At least in the mature sciences, answers (or full substitutes for answers) to questions like these are firmly embedded in the educational initiation that prepares and licenses the student for professional practice. Because that education is both rigorous and rigid, these answers come to exert a deep hold on the scientific mind. That they can do so does much to account both for the peculiar efficiency of the normal research activity and for the direction in which it proceeds at any given time. When examining normal science in Sections III, IV, and V, we shall want finally to describe that research as a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education. Simultaneously, we shall wonder whether research could proceed without such boxes, whatever the element of arbitrariness in their historic origins and, occasionally, in their subsequent development.

Yet that element of arbitrariness is present, and it too has an important effect on scientific development, one which will be examined in detail in Sections VI, VII, and VIII. Normal science, the activity in which most scientists inevitably spend almost all their time, is predicated on the assumption that the scientific community knows what the world is like. Much of the success of the enterprise derives from the community's willingness to defend that assumption, if necessary at considerable cost. Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments. Nevertheless, so long as those commitments retain an element of the arbitrary, the very nature of normal research ensures that novelty shall not be suppressed for very long. Sometimes a normal problem, one that ought to be solvable by known rules and procedures, resists the reiterated onslaught of the ablest members of the group within whose competence it falls. On other occasions a piece of equipment designed and constructed for the purpose of normal research fails to perform in the anticipated manner, revealing an anomaly that cannot, despite repeated effort, be aligned with professional expectation. In these and other ways besides, normal science repeatedly goes astray. And when it does—when, that is, the profession can no longer evade anomalies that subvert the existing tradition of scientific practice—then begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science. The extraordinary episodes in which that shift of professional commitments occurs are the ones known in this essay as scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science.

The most obvious examples of scientific revolutions are those famous episodes in scientific development that have often been labeled revolutions before. Therefore, in Sections IX and X, where the nature of scientific revolutions is first directly scrutinized, we shall deal repeatedly with the major turning points in scientific development associated with the names of Copernicus, Newton, Lavoisier, and Einstein. More clearly than most other episodes in the history of at least the physical sciences, these display what all scientific revolutions are about. Each of them necessitated the community's rejection of one time-honored scientific theory in favor of another incompatible with it. Each produced a consequent shift in the problems available for scientific scrutiny and in the standards by which the profession determined what should count as an admissible problem or as a legitimate problem-solution. And each transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done. Such changes, together with the controversies that almost always accompany them, are the defining characteristics of scientific revolutions.

These characteristics emerge with particular clarity from a study of, say, the Newtonian or the chemical revolution. It is, however, a fundamental thesis of this essay that they can also be retrieved from the study of many other episodes that were not so obviously revolutionary. For the far smaller professional

group affected by them, Maxwell's equations were as revolutionary as Einstein's, and they were resisted accordingly. The invention of other new theories regularly, and appropriately, evokes the same response from some of the specialists on whose area of special competence they impinge. For these men the new theory implies a change in the rules governing the prior practice of normal science. Inevitably, therefore, it reflects upon much scientific work they have already successfully completed. That is why a new theory, however special its range of application, is seldom or never just an increment to what is already known. Its assimilation requires the reconstruction of prior theory and the re-evaluation of prior fact, an intrinsically revolutionary process that is seldom completed by a single man and never overnight. No wonder historians have had difficulty in dating precisely this extended process that their vocabulary impels them to view as an isolated event.

Nor are new inventions of theory the only scientific events that have revolutionary impact upon the specialists in whose domain they occur. The commitments that govern normal science specify not only what sorts of entities the universe does contain, but also, by implication, those that it does not. It follows, though the point will require extended discussion, that a discovery like that of oxygen or X-rays does not simply add one more item to the population of the scientist's world. Ultimately it has that effect, but not until the professional community has re-evaluated traditional experimental procedures, altered its conception of entities with which it has long been familiar, and, in the process, shifted the network of theory through which it deals with the world. Scientific fact and theory are not categorically separable, except perhaps within a single tradition of normal-scientific practice. That is why the unexpected discovery is not simply factual in its import and why the scientist's world is qualitatively transformed as well as quantitatively enriched by fundamental novelties of either fact or theory.

This extended conception of the nature of scientific revolutions is the one delineated in the pages that follow. Admittedly the extension strains customary usage. Nevertheless, I shall continue to speak even of discoveries as revolutionary, because it is just the possibility of relating their structure to that of, say, the Copernican revolution that makes the extended conception seem to me so important. The preceding discussion indicates how the complementary notions of normal science and of scientific revolutions will be developed in the nine sections immediately to follow. The rest of the essay attempts to dispose of three remaining central questions. Section XI, by discussing the textbook tradition, considers why scientific revolutions have previously been so difficult to see. Section XII describes the revolutionary competition between the proponents of the old normal-scientific tradition and the adherents of the new one. It thus considers the process that should somehow, in a theory of scientific inquiry, replace the confirmation or falsification procedures made familiar by our usual image of science. Competition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another. Finally, Section XIII will ask how development through revolutions can be compatible with the apparently unique character of scientific progress. For that question, however, this essay will provide no more than the main outlines of an answer, one which depends upon characteristics of the scientific community that require much additional exploration and study.

Undoubtedly, some readers will already have wondered whether historical study can possibly effect the sort of conceptual transformation aimed at here. An entire arsenal of dichotomies is available to suggest that it cannot properly do so. History, we too often say, is a purely descriptive discipline. The theses suggested above are, however, often interpretive and sometimes normative. Again, many of my generalizations are about the sociology or social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology. In the preceding paragraph I may even seem to have violated the very influential contemporary distinction between "the context of discovery" and "the context of justification." Can anything more than profound confusion be indicated by this admixture of diverse fields and concerns?

Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge, and I still suppose that, appropriately recast, they have something important to tell us. Yet my attempts to apply them, even *grosso modo,* to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields. If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate. How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?

II. The Route to Normal Science

In this essay, 'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. Today such achievements are recounted, though seldom in their original form, by science textbooks, elementary and advanced. These textbooks expound the body of accepted theory, illustrate many or all of its successful applications, and compare these applications with exemplary observations and experiments. Before such books became popular early in the nineteenth century (and until even more recently in the newly matured sciences), many of the famous classics of science fulfilled a similar function. Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry*, and Lyell's *Geology*—these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.

Achievements that share these two characteristics I shall henceforth refer to as 'paradigms,' a term that relates closely to 'normal science.' By choosing it, I mean to suggest that some accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation together—provide models from which spring particular coherent traditions of scientific research. These are the traditions which the historian describes under such rubrics as 'Ptolemaic astronomy' (or 'Copernican'), 'Aristotelian dynamics' (or 'Newtonian'), 'corpuscular optics' (or 'wave optics'), and so on. The study of

paradigms, including many that are far more specialized than those named illustratively above, is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition.

Because in this essay the concept of a paradigm will often substitute for a variety of familiar notions, more will need to be said about the reasons for its introduction. Why is the concrete scientific achievement, as a locus of professional commitment, prior to the various concepts, laws, theories, and points of view that may be abstracted from it? In what sense is the shared paradigm a fundamental unit for the student of scientific development, a unit that cannot be fully reduced to logically atomic components which might function in its stead? When we encounter them in Section V, answers to these questions and to others like them will prove basic to an understanding both of normal science and of the associated concept of paradigms. That more abstract discussion will depend, however, upon a previous exposure to examples of normal science or of paradigms in operation. In particular, both these related concepts will be clarified by noting that there can be a sort of scientific research without paradigms, or at least without any so unequivocal and so binding as the ones named above. Acquisition of a paradigm and of the more esoteric type of research it permits is a sign of maturity in the development of any given scientific field.

If the historian traces the scientific knowledge of any selected group of related phenomena backward in time, he is likely to encounter some minor variant of a pattern here illustrated from the history of physical optics. Today's physics textbooks tell the student that light is photons, i.e., quantum-mechanical entities that exhibit some characteristics of waves and some of particles. Research proceeds accordingly, or rather according to the more elaborate and mathematical characterization from which this usual verbalization is derived. That characterization of light is, however, scarcely half a century old. Before it was developed by Planck, Einstein, and others early in this century, physics texts taught that light was transverse wave motion, a conception rooted in a paradigm that derived ultimately from the optical writings of Young and Fresnel in the early nineteenth century. Nor was the wave theory the first to be embraced by almost all practitioners of optical science. During the eighteenth century the paradigm for this field was provided by Newton's *Opticks*, which taught that light was material corpuscles. At that time physicists sought evidence, as the early wave theorists had not, of the pressure exerted by light particles impinging on solid bodies.[1]

These transformations of the paradigms of physical optics are scientific revolutions, and the successive transition from one paradigm to another via revolution is the usual developmental pattern of mature science. It is not, however, the pattern characteristic of the period before Newton's work, and that is the contrast that concerns us here. No period between remote antiquity and the end of the seventeenth century exhibited a single generally accepted view about the nature of light. Instead there were a number of competing schools and subschools, most of them espousing one variant or another of Epicurean, Aristotelian, or Platonic theory. One group took light to be particles emanating from material bodies; for another it was a modification of the medium that intervened between the body and the eye; still another explained light in terms of an interaction of the medium with an emanation from the eye; and there were other combinations and modifications besides. Each of the corresponding schools derived strength from its relation to some particular metaphysic, and each emphasized, as para-

[1] Joseph Priestley, *The History and Present State of Discoveries Relating to Vision, Light, and Colours* (London, 1772), pp. 385–90.

digmatic observations, the particular cluster of optical phenomena that its own theory could do most to explain. Other observations were dealt with by *ad hoc* elaborations, or they remained as outstanding problems for further research.[2]

At various times all these schools made significant contributions to the body of concepts, phenomena, and techniques from which Newton drew the first nearly uniformly accepted paradigm for physical optics. Any definition of the scientist that excludes at least the more creative members of these various schools will exclude their modern successors as well. Those men were scientists. Yet anyone examining a survey of physical optics before Newton may well conclude that, though the field's practitioners were scientists, the net result of their activity was something less than science. Being able to take no common body of belief for granted, each writer on physical optics felt forced to build his field anew from its foundations. In doing so, his choice of supporting observation and experiment was relatively free, for there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain. Under these circumstances, the dialogue of the resulting books was often directed as much to the members of other schools as it was to nature. That pattern is not unfamiliar in a number of creative fields today, nor is it incompatible with significant discovery and invention. It is not, however, the pattern of development that physical optics acquired after Newton and that other natural sciences make familiar today.

The history of electrical research in the first half of the eighteenth century provides a more concrete and better known example of the way a science develops before it acquires its first universally received paradigm. During that period there were almost as many views about the nature of electricity as there were important electrical experimenters, men like Hauksbee, Gray, Desaguliers, Du Fay, Nollett, Watson, Franklin, and others. All their numerous concepts of electricity had something in common—they were partially derived from one or an-

[2] Vasco Ronchi, *Histoire de la lumière*, trans. Jean Taton (Paris, 1956), chaps. i–iv.

other version of the mechanico-corpuscular philosophy that guided all scientific research of the day. In addition, all were components of real scientific theories, of theories that had been drawn in part from experiment and observation and that partially determined the choice and interpretation of additional problems undertaken in research. Yet though all the experiments were electrical and though most of the experimenters read each other's works, their theories had no more than a family resemblance.[3]

One early group of theories, following seventeenth-century practice, regarded attraction and frictional generation as the fundamental electrical phenomena. This group tended to treat repulsion as a secondary effect due to some sort of mechanical rebounding and also to postpone for as long as possible both discussion and systematic research on Gray's newly discovered effect, electrical conduction. Other "electricians" (the term is their own) took attraction and repulsion to be equally elementary manifestations of electricity and modified their theories and research accordingly. (Actually, this group is remarkably small—even Franklin's theory never quite accounted for the mutual repulsion of two negatively charged bodies.) But they had as much difficulty as the first group in accounting simultaneously for any but the simplest conduction effects. Those effects, however, provided the starting point for still a third group, one which tended to speak of electricity as a "fluid" that could run through conductors rather than as an "effluvium" that emanated from non-conductors. This group, in its turn, had difficulty reconciling its theory with a number of attractive and

[3] Duane Roller and Duane H. D. Roller, *The Development of the Concept of Electric Charge: Electricity from the Greeks to Coulomb* ("Harvard Case Histories in Experimental Science," Case 8; Cambridge, Mass., 1954); and I. B. Cohen, *Franklin and Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof* (Philadelphia, 1956), chaps. vii–xii. For some of the analytic detail in the paragraph that follows in the text, I am indebted to a still unpublished paper by my student John L. Heilbron. Pending its publication, a somewhat more extended and more precise account of the emergence of Franklin's paradigm is included in T. S. Kuhn, "The Function of Dogma in Scientific Research," in A. C. Crombie (ed.), "Symposium on the History of Science, University of Oxford, July 9–15, 1961," to be published by Heinemann Educational Books, Ltd.

repulsive effects. Only through the work of Franklin and his immediate successors did a theory arise that could account with something like equal facility for very nearly all these effects and that therefore could and did provide a subsequent generation of "electricians" with a common paradigm for its research.

Excluding those fields, like mathematics and astronomy, in which the first firm paradigms date from prehistory and also those, like biochemistry, that arose by division and recombination of specialties already matured, the situations outlined above are historically typical. Though it involves my continuing to employ the unfortunate simplification that tags an extended historical episode with a single and somewhat arbitrarily chosen name (e.g., Newton or Franklin), I suggest that similar fundamental disagreements characterized, for example, the study of motion before Aristotle and of statics before Archimedes, the study of heat before Black, of chemistry before Boyle and Boerhaave, and of historical geology before Hutton. In parts of biology—the study of heredity, for example—the first universally received paradigms are still more recent; and it remains an open question what parts of social science have yet acquired such paradigms at all. History suggests that the road to a firm research consensus is extraordinarily arduous.

History also suggests, however, some reasons for the difficulties encountered on that road. In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. Furthermore, in the absence of a reason for seeking some particular form of more recondite information, early fact-gathering is usually restricted to the wealth of data that lie ready to hand. The resulting pool of facts contains those accessible to casual observation and experiment together with some of the more esoteric data retrievable from established crafts like medicine, calendar making, and metallurgy. Because the crafts are one readily accessible source of facts that could not have been casually discovered, technology has often played a vital role in the emergence of new sciences.

But though this sort of fact-collecting has been essential to the origin of many significant sciences, anyone who examines, for example, Pliny's encyclopedic writings or the Baconian natural histories of the seventeenth century will discover that it produces a morass. One somehow hesitates to call the literature that results scientific. The Baconian "histories" of heat, color, wind, mining, and so on, are filled with information, some of it recondite. But they juxtapose facts that will later prove revealing (e.g., heating by mixture) with others (e.g., the warmth of dung heaps) that will for some time remain too complex to be integrated with theory at all.[4] In addition, since any description must be partial, the typical natural history often omits from its immensely circumstantial accounts just those details that later scientists will find sources of important illumination. Almost none of the early "histories" of electricity, for example, mention that chaff, attracted to a rubbed glass rod, bounces off again. That effect seemed mechanical, not electrical.[5] Moreover, since the casual fact-gatherer seldom possesses the time or the tools to be critical, the natural histories often juxtapose descriptions like the above with others, say, heating by antiperistasis (or by cooling), that we are now quite unable to confirm.[6] Only very occasionally, as in the cases of ancient statics, dynamics, and geometrical optics, do facts collected with so little guidance from pre-established theory speak with sufficient clarity to permit the emergence of a first paradigm.

This is the situation that creates the schools characteristic of the early stages of a science's development. No natural history can be interpreted in the absence of at least some implicit body

[4] Compare the sketch for a natural history of heat in Bacon's *Novum Organum*, Vol. VIII of *The Works of Francis Bacon*, ed. J. Spedding, R. L. Ellis, and D. D. Heath (New York, 1869), pp. 179-203.

[5] Roller and Roller, *op. cit.*, pp. 14, 22, 28, 43. Only after the work recorded in the last of these citations do repulsive effects gain general recognition as unequivocally electrical.

[6] Bacon, *op. cit.*, pp. 235, 337, says, "Water slightly warm is more easily frozen than quite cold." For a partial account of the earlier history of this strange observation, see Marshall Clagett, *Giovanni Marliani and Late Medieval Physics* (New York, 1941), chap. iv.

of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collection of facts—in which case more than "mere facts" are at hand—it must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident. No wonder, then, that in the early stages of the development of any science different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways. What is surprising, and perhaps also unique in its degree to the fields we call science, is that such initial divergences should ever largely disappear.

For they do disappear to a very considerable extent and then apparently once and for all. Furthermore, their disappearance is usually caused by the triumph of one of the pre-paradigm schools, which, because of its own characteristic beliefs and preconceptions, emphasized only some special part of the too sizable and inchoate pool of information. Those electricians who thought electricity a fluid and therefore gave particular emphasis to conduction provide an excellent case in point. Led by this belief, which could scarcely cope with the known multiplicity of attractive and repulsive effects, several of them conceived the idea of bottling the electrical fluid. The immediate fruit of their efforts was the Leyden jar, a device which might never have been discovered by a man exploring nature casually or at random, but which was in fact independently developed by at least two investigators in the early 1740's.[7] Almost from the start of his electrical researches, Franklin was particularly concerned to explain that strange and, in the event, particularly revealing piece of special apparatus. His success in doing so provided the most effective of the arguments that made his theory a paradigm, though one that was still unable to account for quite all the known cases of electrical repulsion.[8] To be accepted as a paradigm, a theory must seem better than its competitors, but

[7] Roller and Roller, *op. cit.*, pp. 51–54.

[8] The troublesome case was the mutual repulsion of negatively charged bodies, for which see Cohen, *op. cit.*, pp. 491–94, 531–43.

it need not, and in fact never does, explain all the facts with which it can be confronted.

What the fluid theory of electricity did for the subgroup that held it, the Franklinian paradigm later did for the entire group of electricians. It suggested which experiments would be worth performing and which, because directed to secondary or to overly complex manifestations of electricity, would not. Only the paradigm did the job far more effectively, partly because the end of interschool debate ended the constant reiteration of fundamentals and partly because the confidence that they were on the right track encouraged scientists to undertake more precise, esoteric, and consuming sorts of work.[9] Freed from the concern with any and all electrical phenomena, the united group of electricians could pursue selected phenomena in far more detail, designing much special equipment for the task and employing it more stubbornly and systematically than electricians had ever done before. Both fact collection and theory articulation became highly directed activities. The effectiveness and efficiency of electrical research increased accordingly, providing evidence for a societal version of Francis Bacon's acute methodological dictum: "Truth emerges more readily from error than from confusion."[10]

We shall be examining the nature of this highly directed or paradigm-based research in the next section, but must first note briefly how the emergence of a paradigm affects the structure of the group that practices the field. When, in the development of a natural science, an individual or group first produces a synthesis able to attract most of the next generation's practitioners, the older schools gradually disappear. In part their disappearance is caused by their members' conversion to the new paradigm. But there are always some men who cling to one or another of the older views, and they are simply read out of the profession, which thereafter ignores their work. The new paradigm implies a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach themselves to some other group.[11] Historically, they have often simply stayed in the departments of philosophy from which so many of the special sciences have been spawned. As these indications hint, it is sometimes just its reception of a paradigm that transforms a group previously interested merely in the study of nature into a profession or, at least, a discipline. In the sciences (though not in fields like medicine, technology, and law, of which the principal *raison d'être* is an external social need), the formation of specialized journals, the foundation of specialists' societies, and the claim for a special place in the curriculum have usually been associated with a group's first reception of a single paradigm. At least this was the case between the time, a century and a half ago, when the institutional pattern of scientific specialization first developed and the very recent time when the paraphernalia of specialization acquired a prestige of their own.

The more rigid definition of the scientific group has other consequences. When the individual scientist can take a paradigm for granted, he need no longer, in his major works, attempt to build his field anew, starting from first principles and justify-

[9] It should be noted that the acceptance of Franklin's theory did not end quite all debate. In 1759 Robert Symmer proposed a two-fluid version of that theory, and for many years thereafter electricians were divided about whether electricity was a single fluid or two. But the debates on this subject only confirm what has been said above about the manner in which a universally recognized achievement unites the profession. Electricians, though they continued divided on this point, rapidly concluded that no experimental tests could distinguish the two versions of the theory and that they were therefore equivalent. After that, both schools could and did exploit all the benefits that the Franklinian theory provided (*ibid.*, pp. 543–46, 548–54).

[10] Bacon, *op. cit.*, p. 210.

[11] The history of electricity provides an excellent example which could be duplicated from the careers of Priestley, Kelvin, and others. Franklin reports that Nollet, who at mid-century was the most influential of the Continental electricians, "lived to see himself the last of his Sect, except Mr. B.—his Eleve and immediate Disciple" (Max Farrand [ed.], *Benjamin Franklin's Memoirs* [Berkeley, Calif., 1949], pp. 384–86). More interesting, however, is the endurance of whole schools in increasing isolation from professional science. Consider, for example, the case of astrology, which was once an integral part of astronomy. Or consider the continuation in the late eighteenth and early nineteenth centuries of a previously respected tradition of "romantic" chemistry. This is the tradition discussed by Charles C. Gillispie in "The *Encyclopédie* and the Jacobin Philosophy of Science: A Study in Ideas and Consequences," *Critical Problems in the History of Science*, ed. Marshall Clagett (Madison, Wis., 1959), pp. 255–89; and "The Formation of Lamarck's Evolutionary Theory," *Archives internationales d'histoire des sciences*, XXXVII (1956), 323–38.

ing the use of each concept introduced. That can be left to the writer of textbooks. Given a textbook, however, the creative scientist can begin his research where it leaves off and thus concentrate exclusively upon the subtlest and most esoteric aspects of the natural phenomena that concern his group. And as he does this, his research communiqués will begin to change in ways whose evolution has been too little studied but whose modern end products are obvious to all and oppressive to many. No longer will his researches usually be embodied in books addressed, like Franklin's *Experiments . . . on Electricity* or Darwin's *Origin of Species*, to anyone who might be interested in the subject matter of the field. Instead they will usually appear as brief articles addressed only to professional colleagues, the men whose knowledge of a shared paradigm can be assumed and who prove to be the only ones able to read the papers addressed to them.

Today in the sciences, books are usually either texts or retrospective reflections upon one aspect or another of the scientific life. The scientist who writes one is more likely to find his professional reputation impaired than enhanced. Only in the earlier, pre-paradigm, stages of the development of the various sciences did the book ordinarily possess the same relation to professional achievement that it still retains in other creative fields. And only in those fields that still retain the book, with or without the article, as a vehicle for research communication are the lines of professionalization still so loosely drawn that the layman may hope to follow progress by reading the practitioners' original reports. Both in mathematics and astronomy, research reports had ceased already in antiquity to be intelligible to a generally educated audience. In dynamics, research became similarly esoteric in the later Middle Ages, and it recaptured general intelligibility only briefly during the early seventeenth century when a new paradigm replaced the one that had guided medieval research. Electrical research began to require translation for the layman before the end of the eighteenth century, and most other fields of physical science ceased to be generally accessible in the nineteenth. During the same two centuries similar transitions can be isolated in the various parts of the biological sciences. In parts of the social sciences they may well be occurring today. Although it has become customary, and is surely proper, to deplore the widening gulf that separates the professional scientist from his colleagues in other fields, too little attention is paid to the essential relationship between that gulf and the mechanisms intrinsic to scientific advance.

Ever since prehistoric antiquity one field of study after another has crossed the divide between what the historian might call its prehistory as a science and its history proper. These transitions to maturity have seldom been so sudden or so unequivocal as my necessarily schematic discussion may have implied. But neither have they been historically gradual, coextensive, that is to say, with the entire development of the fields within which they occurred. Writers on electricity during the first four decades of the eighteenth century possessed far more information about electrical phenomena than had their sixteenth-century predecessors. During the half-century after 1740, few new sorts of electrical phenomena were added to their lists. Nevertheless, in important respects, the electrical writings of Cavendish, Coulomb, and Volta in the last third of the eighteenth century seem further removed from those of Gray, Du Fay, and even Franklin than are the writings of these early eighteenth-century electrical discoverers from those of the sixteenth century.[12] Sometime between 1740 and 1780, electricians were for the first time enabled to take the foundations of their field for granted. From that point they pushed on to more concrete and recondite problems, and increasingly they then reported their results in articles addressed to other electricians rather than in books addressed to the learned world at large. As a group they achieved what had been gained by astronomers in antiquity

[12] The post-Franklinian developments include an immense increase in the sensitivity of charge detectors, the first reliable and generally diffused techniques for measuring charge, the evolution of the concept of capacity and its relation to a newly refined notion of electric tension, and the quantification of electrostatic force. On all of these see Roller and Roller, *op. cit.*, pp. 66–81; W. C. Walker, "The Detection and Estimation of Electric Charges in the Eighteenth Century," *Annals of Science*, I (1936), 66–100; and Edmund Hoppe, *Geschichte der Elektrizität* (Leipzig, 1884), Part I, chaps. iii–iv.

and by students of motion in the Middle Ages, of physical optics in the late seventeenth century, and of historical geology in the early nineteenth. They had, that is, achieved a paradigm that proved able to guide the whole group's research. Except with the advantage of hindsight, it is hard to find another criterion that so clearly proclaims a field a science.

Postscript—1969

It has now been almost seven years since this book was first published.[1] In the interim both the response of critics and my own further work have increased my understanding of a number of the issues it raises. On fundamentals my viewpoint is very nearly unchanged, but I now recognize aspects of its initial formulation that create gratuitous difficulties and misunderstandings. Since some of those misunderstandings have been my own, their elimination enables me to gain ground that should ultimately provide the basis for a new version of the book.[2] Meanwhile, I welcome the chance to sketch needed revisions, to comment on some reiterated criticisms, and to suggest directions in which my own thought is presently developing.[3]

Several of the key difficulties of my original text cluster about the concept of a paradigm, and my discussion begins with them.[4] In the subsection that follows at once, I suggest the desirability of disentangling that concept from the notion of a scientific community, indicate how this may be done, and discuss some significant consequences of the resulting analytic separation. Next I consider what occurs when paradigms are sought by examining the behavior of the members of a *previously determined* scientific community. That procedure quickly discloses that in much of the book the term 'paradigm' is used in two different senses. On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science. The first sense of the term, call it the sociological, is the subject of Subsection 2, below; Subsection 3 is devoted to paradigms as exemplary past achievements.

Philosophically, at least, this second sense of 'paradigm' is the deeper of the two, and the claims I have made in its name are the main sources for the controversies and misunderstandings that the book has evoked, particularly for the charge that I make of science a subjective and irrational enterprise. These issues are considered in Subsections 4 and 5. The first argues that terms like 'subjective' and 'intuitive' cannot appropriately be applied to the components of knowledge that I have described as tacitly embedded in shared examples. Though such knowledge is not, without essential change, subject to paraphrase in terms of rules and criteria, it is nevertheless systematic, time tested, and in some sense corrigible. Subsection 5 applies that argument to the problem of choice between two incompatible theories, urging in brief conclusion that men who hold incommensurable viewpoints be thought of as members of different language communities and that their communication problems be analyzed as problems of translation. Three residual issues are discussed in the concluding Subsections, 6 and 7. The first considers the charge that the view of science developed in this book is through-and-through relativistic. The second begins by inquiring whether my argument really suffers, as has been said, from a confusion between the descriptive and the normative modes; it concludes with brief remarks on a topic deserving a separate

[1] This postscript was first prepared at the suggestion of my onetime student and longtime friend, Dr. Shigeru Nakayama of the University of Tokyo, for inclusion in his Japanese translation of this book. I am grateful to him for the idea, for his patience in awaiting its fruition, and for permission to include the result in the English language edition.

[2] For this edition I have attempted no systematic rewriting, restricting alterations to a few typographical errors plus two passages which contained isolable errors. One of these is the description of the role of Newton's *Principia* in the development of eighteenth-century mechanics on pp. 30–33, above. The other concerns the response to crises on p. 84.

[3] Other indications will be found in two recent essays of mine: "Reflection on My Critics," in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge* (Cambridge, 1970); and "Second Thoughts on Paradigms," in Frederick Suppe (ed.), *The Structure of Scientific Theories* (Urbana, Ill., 1970 or 1971), both currently in press. I shall cite the first of these essays below as "Reflections" and the volume in which it appears as *Growth of Knowledge*; the second essay will be referred to as "Second Thoughts."

[4] For particularly cogent criticism of my initial presentation of paradigms see: Margaret Masterman, "The Nature of a Paradigm," in *Growth of Knowledge*; and Dudley Shapere, "The Structure of Scientific Revolutions," *Philosophical Review*, LXXIII (1964), 383–94.

essay: the extent to which the book's main theses may legitimately be applied to fields other than science.

1. *Paradigms and Community Structure*

The term 'paradigm' enters the preceding pages early, and its manner of entry is intrinsically circular. A paradigm is what the members of a scientific community share, *and*, conversely, a scientific community consists of men who share a paradigm. Not all circularities are vicious (I shall defend an argument of similar structure late in this postscript), but this one is a source of real difficulties. Scientific communities can and should be isolated without prior recourse to paradigms; the latter can then be discovered by scrutinizing the behavior of a given community's members. If this book were being rewritten, it would therefore open with a discussion of the community structure of science, a topic that has recently become a significant subject of sociological research and that historians of science are also beginning to take seriously. Preliminary results, many of them still unpublished, suggest that the empirical techniques required for its exploration are non-trivial, but some are in hand and others are sure to be developed.[5] Most practicing scientists respond at once to questions about their community affiliations, taking for granted that responsibility for the various current specialties is distributed among groups of at least roughly determinate membership. I shall therefore here assume that more systematic means for their identification will be found. Instead of presenting preliminary research results, let me briefly articulate the intuitive notion of community that underlies much in the earlier chapters of this book. It is a notion now widely shared by scientists, sociologists, and a number of historians of science.

[5] W. O. Hagstrom, *The Scientific Community* (New York, 1965), chaps. iv and v; D. J. Price and D. de B. Beaver, "Collaboration in an Invisible College," *American Psychologist*, XXI (1966), 1011–18; Diana Crane, "Social Structure in a Group of Scientists: A Test of the 'Invisible College' Hypothesis," *American Sociological Review*, XXXIV (1969), 335–52; N. C. Mullins, *Social Networks among Biological Scientists*, (Ph.D. diss., Harvard University, 1966), and "The Micro-Structure of an Invisible College: The Phage Group" (paper delivered at an annual meeting of the American Sociological Association, Boston, 1968).

A scientific community consists, on this view, of the practitioners of a scientific specialty. To an extent unparalleled in most other fields, they have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it. Usually the boundaries of that standard literature mark the limits of a scientific subject matter, and each community ordinarily has a subject matter of its own. There are schools in the sciences, communities, that is, which approach the same subject from incompatible viewpoints. But they are far rarer there than in other fields; they are always in competition; and their competition is usually quickly ended. As a result, the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively full and professional judgment relatively unanimous. Because the attention of different scientific communities is, on the other hand, focused on different matters, professional communication across group lines is sometimes arduous, often results in misunderstanding, and may, if pursued, evoke significant and previously unsuspected disagreement.

Communities in this sense exist, of course, at numerous levels. The most global is the community of all natural scientists. At an only slightly lower level the main scientific professional groups are communities: physicists, chemists, astronomers, zoologists, and the like. For these major groupings, community membership is readily established except at the fringes. Subject of highest degree, membership in professional societies, and journals read are ordinarily more than sufficient. Similar techniques will also isolate major subgroups: organic chemists, and perhaps protein chemists among them, solid-state and high-energy physicists, radio astronomers, and so on. It is only at the next lower level that empirical problems emerge. How, to take a contemporary example, would one have isolated the phage group prior to its public acclaim? For this purpose one must have recourse to attendance at special conferences, to the distribution of draft manuscripts or galley proofs prior to publication, and above all to formal and informal communication networks including those discovered in correspondence and in the linkages among citations.[6] I take it that the job can and will be done, at least for the contemporary scene and the more recent parts of the historical. Typically it may yield communities of perhaps one hundred members, occasionally significantly fewer. Usually individual scientists, particularly the ablest, will belong to several such groups either simultaneously or in succession.

Communities of this sort are the units that this book has presented as the producers and validators of scientific knowledge. Paradigms are something shared by the members of such groups. Without reference to the nature of these shared elements, many aspects of science described in the preceding pages can scarcely be understood. But other aspects can, though they are not independently presented in my original text. It is therefore worth noting, before turning to paradigms directly, a series of issues that require reference to community structure alone.

Probably the most striking of these is what I have previously called the transition from the pre- to the post-paradigm period in the development of a scientific field. That transition is the one sketched above in Section II. Before it occurs, a number of schools compete for the domination of a given field. Afterward, in the wake of some notable scientific achievement, the number of schools is greatly reduced, ordinarily to one, and a more efficient mode of scientific practice begins. The latter is generally esoteric and oriented to puzzle-solving, as the work of a group can be only when its members take the foundations of their field for granted.

The nature of that transition to maturity deserves fuller discussion than it has received in this book, particularly from those concerned with the development of the contemporary social

[6] Eugene Garfield, *The Use of Citation Data in Writing the History of Science* (Philadelphia: Institute of Scientific Information, 1964); M. M. Kessler, "Comparison of the Results of Bibliographic Coupling and Analytic Subject Indexing," *American Documentation*, XVI (1965), 223–33; D. J. Price, "Networks of Scientific Papers," *Science*, CIL (1965), 510–15.

sciences. To that end it may help to point out that the transition need not (I now think should not) be associated with the first acquisition of a paradigm. The members of all scientific communities, including the schools of the "pre-paradigm" period, share the sorts of elements which I have collectively labelled 'a paradigm.' What changes with the transition to maturity is not the presence of a paradigm but rather its nature. Only after the change is normal puzzle-solving research possible. Many of the attributes of a developed science which I have above associated with the acquisition of a paradigm I would therefore now discuss as consequences of the acquisition of the sort of paradigm that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed. Only those who have taken courage from observing that their own field (or school) has paradigms are likely to feel that something important is sacrificed by the change.

A second issue, more important at least to historians, concerns this book's implicit one-to-one identification of scientific communities with scientific subject matters. I have, that is, repeatedly acted as though, say, 'physical optics,' 'electricity,' and 'heat' must name scientific communities because they do name subject matters for research. The only alternative my text has seemed to allow is that all these subjects have belonged to the physics community. Identifications of that sort will not, however, usually withstand examination, as my colleagues in history have repeatedly pointed out. There was, for example, no physics community before the mid-nineteenth century, and it was then formed by the merger of parts of two previously separate communities, mathematics and natural philosophy (*physique expérimentale*). What is today the subject matter for a single broad community has been variously distributed among diverse communities in the past. Other narrower subjects, for example heat and the theory of matter, have existed for long periods without becoming the special province of any single scientific community. Both normal science and revolutions are, however, community-based activities. To discover and analyze them, one must first unravel the changing community structure of the sciences over time. A paradigm governs, in the first instance, not a subject matter but rather a group of practitioners. Any study of paradigm-directed or of paradigm-shattering research must begin by locating the responsible group or groups.

When the analysis of scientific development is approached in that way, several difficulties which have been foci for critical attention are likely to vanish. A number of commentators have, for example, used the theory of matter to suggest that I drastically overstate the unanimity of scientists in their allegiance to a paradigm. Until comparatively recently, they point out, those theories have been topics for continuing disagreement and debate. I agree with the description but think it no counter-example. Theories of matter were not, at least until about 1920, the special province or the subject matter for any scientific community. Instead, they were tools for a large number of specialists' groups. Members of different communities sometimes chose different tools and criticized the choice made by others. Even more important, a theory of matter is not the sort of topic on which the members of even a single community must necessarily agree. The need for agreement depends on what it is the community does. Chemistry in the first half of the nineteenth century provides a case in point. Though several of the community's fundamental tools—constant proportion, multiple proportion, and combining weights—had become common property as a result of Dalton's atomic theory, it was quite possible for chemists, after the event, to base their work on these tools and to disagree, sometimes vehemently, about the existence of atoms.

Some other difficulties and misunderstandings will, I believe, be dissolved in the same way. Partly because of the examples I have chosen and partly because of my vagueness about the nature and size of the relevant communities, a few readers of this book have concluded that my concern is primarily or exclusively with major revolutions such as those associated with Copernicus, Newton, Darwin, or Einstein. A clearer delineation of community structure should, however, help to enforce the rather different impression I have tried to create. A revolution

is for me a special sort of change involving a certain sort of reconstruction of group commitments. But it need not be a large change, nor need it seem revolutionary to those outside a single community, consisting perhaps of fewer than twenty-five people. It is just because this type of change, little recognized or discussed in the literature of the philosophy of science, occurs so regularly on this smaller scale that revolutionary, as against cumulative, change so badly needs to be understood.

One last alteration, closely related to the preceding, may help to facilitate that understanding. A number of critics have doubted whether crisis, the common awareness that something has gone wrong, precedes revolutions so invariably as I have implied in my original text. Nothing important to my argument depends, however, on crises' being an absolute prerequisite to revolutions; they need only be the usual prelude, supplying, that is, a self-correcting mechanism which ensures that the rigidity of normal science will not forever go unchallenged. Revolutions may also be induced in other ways, though I think they seldom are. In addition, I would now point out what the absence of an adequate discussion of community structure has obscured above: crises need not be generated by the work of the community that experiences them and that sometimes undergoes revolution as a result. New instruments like the electron microscope or new laws like Maxwell's may develop in one specialty and their assimilation create crisis in another.

2. *Paradigms as the Constellation of Group Commitments*

Turn now to paradigms and ask what they can possibly be. My original text leaves no more obscure or important question. One sympathetic reader, who shares my conviction that 'paradigm' names the central philosophical elements of the book, prepared a partial analytic index and concluded that the term is used in at least twenty-two different ways.[7] Most of those differences are, I now think, due to stylistic inconsistencies (e.g., Newton's Laws are sometimes a paradigm, sometimes parts of a paradigm, and

[7] Masterman, *op. cit.*

sometimes paradigmatic), and they can be eliminated with relative ease. But, with that editorial work done, two very different usages of the term would remain, and they require separation. The more global use is the subject of this subsection; the other will be considered in the next.

Having isolated a particular community of specialists by techniques like those just discussed, one may usefully ask: What do its members share that accounts for the relative fulness of their professional communication and the relative unanimity of their professional judgments? To that question my original text licenses the answer, a paradigm or set of paradigms. But for this use, unlike the one to be discussed below, the term is inappropriate. Scientists themselves would say they share a theory or set of theories, and I shall be glad if the term can ultimately be recaptured for this use. As currently used in philosophy of science, however, 'theory' connotes a structure far more limited in nature and scope than the one required here. Until the term can be freed from its current implications, it will avoid confusion to adopt another. For present purposes I suggest 'disciplinary matrix': 'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification. All or most of the objects of group commitment that my original text makes paradigms, parts of paradigms, or paradigmatic are constituents of the disciplinary matrix, and as such they form a whole and function together. They are, however, no longer to be discussed as though they were all of a piece. I shall not here attempt an exhaustive list, but noting the main sorts of components of a disciplinary matrix will both clarify the nature of my present approach and simultaneously prepare for my next main point.

One important sort of component I shall label 'symbolic generalizations,' having in mind those expressions, deployed without question or dissent by group members, which can readily be cast in a logical form like $(x)(y)(z)\phi(x, y, z)$. They are the formal or the readily formalizable components of the disciplinary matrix. Sometimes they are found already in sym-

bolic form: $f = ma$ or $I = V/R$. Others are ordinarily expressed in words: "elements combine in constant proportion by weight," or "action equals reaction." If it were not for the general acceptance of expressions like these, there would be no points at which group members could attach the powerful techniques of logical and mathematical manipulation in their puzzle-solving enterprise. Though the example of taxonomy suggests that normal science can proceed with few such expressions, the power of a science seems quite generally to increase with the number of symbolic generalizations its practioners have at their disposal.

These generalizations look like laws of nature, but their function for group members is not often that alone. Sometimes it is: for example the Joule-Lenz Law, $H = RI^2$. When that law was discovered, community members already knew what H, R, and I stood for, and these generalizations simply told them something about the behavior of heat, current, and resistance that they had not known before. But more often, as discussion earlier in the book indicates, symbolic generalizations simultaneously serve a second function, one that is ordinarily sharply separated in analyses by philosophers of science. Like $f = ma$ or $I = V/R$, they function in part as laws but also in part as definitions of some of the symbols they deploy. Furthermore, the balance between their inseparable legislative and definitional force shifts over time. In another context these points would repay detailed analysis, for the nature of the commitment to a law is very different from that of commitment to a definition. Laws are often corrigible piecemeal, but definitions, being tautologies, are not. For example, part of what the acceptance of Ohm's Law demanded was a redefinition of both 'current' and 'resistance'; if those terms had continued to mean what they had meant before, Ohm's Law could not have been right; that is why it was so strenuously opposed as, say, the Joule-Lenz Law was not.[8] Probably that situation is typical. I currently suspect that

[8] For significant parts of this episode see: T. M. Brown, "The Electric Current in Early Nineteenth-Century French Physics," *Historical Studies in the Physical Sciences*, I (1969), 61–103, and Morton Schagrin, "Resistance to Ohm's Law," *American Journal of Physics*, XXI (1963), 536–47.

all revolutions involve, among other things, the abandonment of generalizations the force of which had previously been in some part that of tautologies. Did Einstein show that simultaneity was relative or did he alter the notion of simultaneity itself? Were those who heard paradox in the phrase 'relativity of simultaneity' simply wrong?

Consider next a second type of component of the disciplinary matrix, one about which a good deal has been said in my original text under such rubrics as 'metaphysical paradigms' or 'the metaphysical parts of paradigms.' I have in mind shared commitments to such beliefs as: heat is the kinetic energy of the constituent parts of bodies; all perceptible phenomena are due to the interaction of qualitatively neutral atoms in the void, or, alternatively, to matter and force, or to fields. Rewriting the book now I would describe such commitments as beliefs in particular models, and I would expand the category models to include also the relatively heuristic variety: the electric circuit may be regarded as a steady-state hydrodynamic system; the molecules of a gas behave like tiny elastic billiard balls in random motion. Though the strength of group commitment varies, with nontrivial consequences, along the spectrum from heuristic to ontological models, all models have similar functions. Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each. Note, however, that the members of scientific communities may not have to share even heuristic models, though they usually do so. I have already pointed out that membership in the community of chemists during the first half of the nineteenth century did not demand a belief in atoms.

A third sort of element in the disciplinary matrix I shall here describe as values. Usually they are more widely shared among different communities than either symbolic generalizations or models, and they do much to provide a sense of community to natural scientists as a whole. Though they function at all times, their particular importance emerges when the members of a

particular community must identify crisis or, later, choose between incompatible ways of practicing their discipline. Probably the most deeply held values concern predictions: they should be accurate; quantitative predictions are preferable to qualitative ones; whatever the margin of permissible error, it should be consistently satisfied in a given field; and so on. There are also, however, values to be used in judging whole theories: they must, first and foremost, permit puzzle-formulation and solution; where possible they should be simple, self-consistent, and plausible, compatible, that is, with other theories currently deployed. (I now think it a weakness of my original text that so little attention is given to such values as internal and external consistency in considering sources of crisis and factors in theory choice.) Other sorts of values exist as well—for example, science should (or need not) be socially useful—but the preceding should indicate what I have in mind.

One aspect of shared values does, however, require particular mention. To a greater extent than other sorts of components of the disciplinary matrix, values may be shared by men who differ in their application. Judgments of accuracy are relatively, though not entirely, stable from one time to another and from one member to another in a particular group. But judgments of simplicity, consistency, plausibility, and so on often vary greatly from individual to individual. What was for Einstein an insupportable inconsistency in the old quantum theory, one that rendered the pursuit of normal science impossible, was for Bohr and others a difficulty that could be expected to work itself out by normal means. Even more important, in those situations where values must be applied, different values, taken alone, would often dictate different choices. One theory may be more accurate but less consistent or plausible than another; again the old quantum theory provides an example. In short, though values are widely shared by scientists and though commitment to them is both deep and constitutive of science, the application of values is sometimes considerably affected by the features of individual personality and biography that differentiate the members of the group.

To many readers of the preceding chapters, this characteristic of the operation of shared values has seemed a major weakness of my position. Because I insist that what scientists share is not sufficient to command uniform assent about such matters as the choice between competing theories or the distinction between an ordinary anomaly and a crisis-provoking one, I am occasionally accused of glorifying subjectivity and even irrationality.[9] But that reaction ignores two characteristics displayed by value judgments in any field. First, shared values can be important determinants of group behavior even though the members of the group do not all apply them in the same way. (If that were not the case, there would be no *special* philosophic problems about value theory or aesthetics.) Men did not all paint alike during the periods when representation was a primary value, but the developmental pattern of the plastic arts changed drastically when that value was abandoned.[10] Imagine what would happen in the sciences if consistency ceased to be a primary value. Second, individual variability in the application of shared values may serve functions essential to science. The points at which values must be applied are invariably also those at which risks must be taken. Most anomalies are resolved by normal means; most proposals for new theories do prove to be wrong. If all members of a community responded to each anomaly as a source of crisis or embraced each new theory advanced by a colleague, science would cease. If, on the other hand, no one reacted to anomalies or to brand-new theories in high-risk ways, there would be few or no revolutions. In matters like these the resort to shared values rather than to shared rules governing individual choice may be the community's way of distributing risk and assuring the long-term success of its enterprise.

Turn now to a fourth sort of element in the disciplinary matrix, not the only other kind but the last I shall discuss here. For it the term 'paradigm' would be entirely appropriate, both philologically and autobiographically; this is the component of a group's shared commitments which first led me to the choice of that word. Because the term has assumed a life of its own, however, I shall here substitute 'exemplars.' By it I mean, initially, the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts. To these shared examples should, however, be added at least some of the technical problem-solutions found in the periodical literature that scientists encounter during their post-educational research careers and that also show them by example how their job is to be done. More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science. All physicists, for example, begin by learning the same exemplars: problems such as the inclined plane, the conical pendulum, and Keplerian orbits; instruments such as the vernier, the calorimeter, and the Wheatstone bridge. As their training develops, however, the symbolic generalizations they share are increasingly illustrated by different exemplars. Though both solid-state and field-theoretic physicists share the Schrödinger equation, only its more elementary applications are common to both groups.

[9] See particularly: Dudley Shapere, "Meaning and Scientific Change," in *Mind and Cosmos: Essays in Contemporary Science and Philosophy*, The University of Pittsburgh Series in the Philosophy of Science, III (Pittsburgh, 1966), 41–85; Israel Scheffler, *Science and Subjectivity* (New York, 1967); and the essays of Sir Karl Popper and Imre Lakatos in *Growth of Knowledge*.

[10] See the discussion at the beginning of Section XIII, above.

3. *Paradigms as Shared Examples*

The paradigm as shared example is the central element of what I now take to be the most novel and least understood aspect of this book. Exemplars will therefore require more attention than the other sorts of components of the disciplinary matrix. Philosophers of science have not ordinarily discussed the problems encountered by a student in laboratories or in science texts, for these are thought to supply only practice in the application of what the student already knows. He cannot, it is said, solve problems at all unless he has first learned the theory and some rules for applying it. Scientific knowledge is embedded in theory and rules; problems are supplied to gain facility in their application. I have tried to argue, however, that this localization of the cognitive content of science is wrong. After the student has done many problems, he may gain only added facility by solving more. But at the start and for some time after, doing problems is learning consequential things about nature. In the absence of such exemplars, the laws and theories he has previously learned would have little empirical content.

To indicate what I have in mind I revert briefly to symbolic generalizations. One widely shared example is Newton's Second Law of Motion, generally written as $f = ma$. The sociologist, say, or the linguist who discovers that the corresponding expression is unproblematically uttered and received by the members of a given community will not, without much additional investigation, have learned a great deal about what either the expression or the terms in it mean, about how the scientists of the community attach the expression to nature. Indeed, the fact that they accept it without question and use it as a point at which to introduce logical and mathematical manipulation does not of itself imply that they agree at all about such matters as meaning and application. Of course they do agree to a considerable extent, or the fact would rapidly emerge from their subsequent conversation. But one may well ask at what point and by what means they have come to do so. How have they learned, faced with a given experimental situation, to pick out the relevant forces, masses, and accelerations?

In practice, though this aspect of the situation is seldom or never noted, what students have to learn is even more complex than that. It is not quite the case that logical and mathematical manipulation are applied directly to $f = ma$. That expression proves on examination to be a law-sketch or a law-schema. As the student or the practicing scientist moves from one problem situation to the next, the symbolic generalization to which such manipulations apply changes. For the case of free fall, $f = ma$ becomes $mg = m\frac{d^2s}{dt^2}$; for the simple pendulum it is transformed to $mg \sin\theta = -ml\frac{d^2\theta}{dt^2}$; for a pair of interacting harmonic oscillators it becomes two equations, the first of which may be written

$m_1\frac{d^2s_1}{dt^2} + k_1s_1 = k_2(s_2 - s_1 + d)$; and for more complex situations, such as the gyroscope, it takes still other forms, the family resemblance of which to $f = ma$ is still harder to discover. Yet, while learning to identify forces, masses, and accelerations in a variety of physical situations not previously encountered, the student has also learned to design the appropriate version of $f = ma$ through which to interrelate them, often a version for which he has encountered no literal equivalent before. How has he learned to do this?

A phenomenon familiar to both students of science and historians of science provides a clue. The former regularly report that they have read through a chapter of their text, understood it perfectly, but nonetheless had difficulty solving a number of the problems at the chapter's end. Ordinarily, also, those difficulties dissolve in the same way. The student discovers, with or without the assistance of his instructor, a way to see his problem as *like* a problem he has already encountered. Having seen the resemblance, grasped the analogy between two or more distinct problems, he can interrelate symbols and attach them to nature in the ways that have proved effective before. The law-sketch, say $f = ma$, has functioned as a tool, informing the student what similarities to look for, signaling the gestalt in which the situation is to be seen. The resultant ability to see a variety of situations as like each other, as subjects for $f = ma$ or some other symbolic generalization, is, I think, the main thing a student acquires by doing exemplary problems, whether with a pencil and paper or in a well-designed laboratory. After he has completed a certain number, which may vary widely from one individual to the next, he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group. For him they are no longer the same situations he had encountered when his training began. He has meanwhile assimilated a time-tested and group-licensed way of seeing.

The role of acquired similarity relations also shows clearly in the history of science. Scientists solve puzzles by modeling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations. Galileo found that a ball rolling down an incline acquires just enough velocity to return it to the same vertical height on a second incline of any slope, and he learned to see that experimental situation as like the pendulum with a point-mass for a bob. Huyghens then solved the problem of the center of oscillation of a physical pendulum by imagining that the extended body of the latter was composed of Galilean point-pendula, the bonds between which could be instantaneously released at any point in the swing. After the bonds were released, the individual point-pendula would swing freely, but their collective center of gravity when each attained its highest point would, like that of Galileo's pendulum, rise only to the height from which the center of gravity of the extended pendulum had begun to fall. Finally, Daniel Bernoulli discovered how to make the flow of water from an orifice resemble Huyghens' pendulum. Determine the descent of the center of gravity of the water in tank and jet during an infinitesimal interval of time. Next imagine that each particle of water afterward moves separately upward to the maximum height attainable with the velocity acquired during that interval. The ascent of the center of gravity of the individual particles must then equal the descent of the center of gravity of the water in tank and jet. From that view of the problem the long-sought speed of efflux followed at once.[11]

That example should begin to make clear what I mean by learning from problems to see situations as like each other, as subjects for the application of the same scientific law or law-sketch. Simultaneously it should show why I refer to the consequential knowledge of nature acquired while learning the similarity relationship and thereafter embodied in a way of viewing

[11] For the example, see: René Dugas, *A History of Mechanics*, trans. J. R. Maddox (Neuchatel, 1955), pp. 135–36, 186–93, and Daniel Bernoulli, *Hydrodynamica, sive de viribus et motibus fluidorum, commentarii opus academicum* (Strasbourg, 1738), Sec. iii. For the extent to which mechanics progressed during the first half of the eighteenth century by modelling one problem-solution on another, see Clifford Truesdell, "Reactions of Late Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton's *Principia*," *Texas Quarterly*, X (1967), 238–58.

physical situations rather than in rules or laws. The three problems in the example, all of them exemplars for eighteenth-century mechanicians, deploy only one law of nature. Known as the Principle of *vis viva*, it was usually stated as: "Actual descent equals potential ascent." Bernoulli's application of the law should suggest how consequential it was. Yet the verbal statement of the law, taken by itself, is virtually impotent. Present it to a contemporary student of physics, who knows the words and can do all these problems but now employs different means. Then imagine what the words, though all well known, can have said to a man who did not know even the problems. For him the generalization could begin to function only when he learned to recognize "actual descents" and "potential ascents" as ingredients of nature, and that is to learn something, prior to the law, about the situations that nature does and does not present. That sort of learning is not acquired by exclusively verbal means. Rather it comes as one is given words together with concrete examples of how they function in use; nature and words are learned together. To borrow once more Michael Polanyi's useful phrase, what results from this process is "tacit knowledge" which is learned by doing science rather than by acquiring rules for doing it.

4. *Tacit Knowledge and Intuition*

That reference to tacit knowledge and the concurrent rejection of rules isolates another problem that has bothered many of my critics and seemed to provide a basis for charges of subjectivity and irrationality. Some readers have felt that I was trying to make science rest on unanalyzable individual intuitions rather than on logic and law. But that interpretation goes astray in two essential respects. First, if I am talking at all about intuitions, they are not individual. Rather they are the tested and shared possessions of the members of a successful group, and the novice acquires them through training as a part of his preparation for group-membership. Second, they are not in principle unanalyzable. On the contrary, I am currently experimenting with a computer program designed to investigate their properties at an elementary level.

About that program I shall have nothing to say here,[12] but even mention of it should make my most essential point. When I speak of knowledge embedded in shared exemplars, I am not referring to a mode of knowing that is less systematic or less analyzable than knowledge embedded in rules, laws, or criteria of identification. Instead I have in mind a manner of knowing which is miscontrued if reconstructed in terms of rules that are first abstracted from exemplars and thereafter function in their stead. Or, to put the same point differently, when I speak of acquiring from exemplars the ability to recognize a given situation as like some and unlike others that one has seen before, I am not suggesting a process that is not potentially fully explicable in terms of neuro-cerebral mechanism. Instead I am claiming that the explication will not, by its nature, answer the question, "Similar with respect to what?" That question is a request for a rule, in this case for the criteria by which particular situations are grouped into similarity sets, and I am arguing that the temptation to seek criteria (or at least a full set) should be resisted in this case. It is not, however, system but a particular sort of system that I am opposing.

To give that point substance, I must briefly digress. What follows seems obvious to me now, but the constant recourse in my original text to phrases like "the world changes" suggests that it has not always been so. If two people stand at the same place and gaze in the same direction, we must, under pain of solipsism, conclude that they receive closely similar stimuli. (If both could put their eyes at the same place, the stimuli would be identical.) But people do not see stimuli; our knowledge of them is highly theoretical and abstract. Instead they have sensations, and we are under no compulsion to suppose that the sensations of our two viewers are the same. (Sceptics might remember that color blindness was nowhere noticed until John Dalton's description of it in 1794.) On the contrary, much

[12] Some information on this subject can be found in "Second Thoughts."

neural processing takes place between the receipt of a stimulus and the awareness of a sensation. Among the few things that we know about it with assurance are: that very different stimuli can produce the same sensations; that the same stimulus can produce very different sensations; and, finally, that the route from stimulus to sensation is in part conditioned by education. Individuals raised in different societies behave on some occasions as though they saw different things. If we were not tempted to identify stimuli one-to-one with sensations, we might recognize that they actually do so.

Notice now that two groups, the members of which have systematically different sensations on receipt of the same stimuli, do *in some sense* live in different worlds. We posit the existence of stimuli to explain our perceptions of the world, and we posit their immutability to avoid both individual and social solipsism. About neither posit have I the slightest reservation. But our world is populated in the first instance not by stimuli but by the objects of our sensations, and these need not be the same, individual to individual or group to group. To the extent, of course, that individuals belong to the same group and thus share education, language, experience, and culture, we have good reason to suppose that their sensations are the same. How else are we to understand the fulness of their communication and the communality of their behavioral responses to their environment? They must see things, process stimuli, in much the same ways. But where the differentiation and specialization of groups begins, we have no similar evidence for the immutability of sensation. Mere parochialism, I suspect, makes us suppose that the route from stimuli to sensation is the same for the members of all groups.

Returning now to exemplars and rules, what I have been trying to suggest, in however preliminary a fashion, is this. One of the fundamental techniques by which the members of a group, whether an entire culture or a specialists' sub-community within it, learn to see the same things when confronted with the same stimuli is by being shown examples of situations that their predecessors in the group have already learned to see as like each other and as different from other sorts of situations. These similar situations may be successive sensory presentations of the same individual—say of mother, who is ultimately recognized on sight as what she is and as different from father or sister. They may be presentations of the members of natural families, say of swans on the one hand and of geese on the other. Or they may, for the members of more specialized groups, be examples of the Newtonian situation, of situations, that is, that are alike in being subject to a version of the symbolic form $f = ma$ and that are different from those situations to which, for example, the law-sketches of optics apply.

Grant for the moment that something of this sort does occur. Ought we say that what has been acquired from exemplars is rules and the ability to apply them? That description is tempting because our seeing a situation as like ones we have encountered before must be the result of neural processing, fully governed by physical and chemical laws. In this sense, once we have learned to do it, recognition of similarity must be as fully systematic as the beating of our hearts. But that very parallel suggests that recognition may also be involuntary, a process over which we have no control. If it is, then we may not properly conceive it as something we manage by applying rules and criteria. To speak of it in those terms implies that we have access to alternatives, that we might, for example, have disobeyed a rule, or misapplied a criterion, or experimented with some other way of seeing.[18] Those, I take it, are just the sorts of things we cannot do.

Or, more precisely, those are things we cannot do until after we have had a sensation, perceived something. Then we do often seek criteria and put them to use. Then we may engage in interpretation, a deliberative process by which we choose among alternatives as we do not in perception itself. Perhaps, for example, something is odd about what we have seen (remember the anomalous playing cards). Turning a corner we see mother

18 This point might never have needed making if all laws were like Newton's and all rules like the Ten Commandments. In that case the phrase 'breaking a law' would be nonsense, and a rejection of rules would not seem to imply a process not governed by law. Unfortunately, traffic laws and similar products of legislation can be broken, which makes the confusion easy.

entering a downtown store at a time we had thought she was home. Contemplating what we have seen we suddenly exclaim, "That wasn't mother, for she has red hair!" Entering the store we see the woman again and cannot understand how she could have been taken for mother. Or, perhaps we see the tail feathers of a waterfowl feeding from the bottom of a shallow pool. Is it a swan or a goose? We contemplate what we have seen, mentally comparing the tail feathers with those of swans and geese we have seen before. Or, perhaps, being proto-scientists, we simply want to know some general characteristic (the whiteness of swans, for example) of the members of a natural family we can already recognize with ease. Again, we contemplate what we have previously perceived, searching for what the members of the given family have in common.

These are all deliberative processes, and in them we do seek and deploy criteria and rules. We try, that is, to interpret sensations already at hand, to analyze what is for us the given. However we do that, the processes involved must ultimately be neural, and they are therefore governed by the same *physico-chemical* laws that govern perception on the one hand and the beating of our hearts on the other. But the fact that the system obeys the same laws in all three cases provides no reason to suppose that our neural apparatus is programmed to operate the same way in interpretation as in perception or in either as in the beating of our hearts. What I have been opposing in this book is therefore the attempt, traditional since Descartes but not before, to analyze perception as an interpretive process, as an unconscious version of what we do after we have perceived.

What makes the integrity of perception worth emphasizing is, of course, that so much past experience is embodied in the neural apparatus that transforms stimuli to sensations. An appropriately programmed perceptual mechanism has survival value. To say that the members of different groups may have different perceptions when confronted with the same stimuli is not to imply that they may have just any perceptions at all. In many environments a group that could not tell wolves from dogs could not endure. Nor would a group of nuclear physicists today survive as scientists if unable to recognize the tracks of alpha particles and electrons. It is just because so very few ways of seeing will do that the ones that have withstood the tests of group use are worth transmitting from generation to generation. Equally, it is because they have been selected for their success over historic time that we must speak of the experience and knowledge of nature embedded in the stimulus-to-sensation route.

Perhaps 'knowledge' is the wrong word, but there are reasons for employing it. What is built into the neural process that transforms stimuli to sensations has the following characteristics: it has been transmitted through education; it has, by trial, been found more effective than its historical competitors in a group's current environment; and, finally, it is subject to change both through further education and through the discovery of misfits with the environment. Those are characteristics of knowledge, and they explain why I use the term. But it is strange usage, for one other characteristic is missing. We have no direct access to what it is we know, no rules or generalizations with which to express this knowledge. Rules which could supply that access would refer to stimuli not sensations, and stimuli we can know only through elaborate theory. In its absence, the knowledge embedded in the stimulus-to-sensation route remains tacit.

Though it is obviously preliminary and need not be correct in all details, what has just been said about sensation is meant literally. At the very least it is a hypothesis about vision which should be subject to experimental investigation though probably not to direct check. But talk like this of seeing and sensation here also serves metaphorical functions as it does in the body of the book. We do not *see* electrons, but rather their tracks or else bubbles of vapor in a cloud chamber. We do not *see* electric currents at all, but rather the needle of an ammeter or galvanometer. Yet in the preceding pages, particularly in Section X, I have repeatedly acted as though we did perceive theoretical entities like currents, electrons, and fields, as though we learned to do so from examination of exemplars, and as though in these cases too it would be wrong to replace talk of seeing with talk of criteria and interpretation. The metaphor that transfers 'seeing' to contexts like these is scarcely a sufficient basis for such claims. In the long run it will need to be eliminated in favor of a more literal mode of discourse.

The computer program referred to above begins to suggest ways in which that may be done, but neither available space nor the extent of my present understanding permits my eliminating the metaphor here.[14] Instead I shall try briefly to bulwark it. Seeing water droplets or a needle against a numerical scale is a primitive perceptual experience for the man unacquainted with cloud chambers and ammeters. It thus requires contemplation, analysis, and interpretation (or else the intervention of external authority) before conclusions can be reached about electrons or currents. But the position of the man who has learned about these instruments and had much exemplary experience with them is very different, and there are corresponding differences in the way he processes the stimuli that reach him from them. Regarding the vapor in his breath on a cold winter afternoon, his sensation may be the same as that of a layman, but viewing a cloud chamber he sees (here literally) not droplets but the tracks of electrons, alpha particles, and so on. Those tracks are, if you will, criteria that he interprets as indices of the presence of the corresponding particles, but that route is both shorter and different from the one taken by the man who interprets droplets.

Or consider the scientist inspecting an ammeter to determine the number against which the needle has settled. His sensation probably is the same as the layman's, particularly if the latter has

[14] For readers of "Second Thoughts" the following cryptic remarks may be leading. The possibility of immediate recognition of the members of natural families depends upon the existence, after neural processing, of empty perceptual space between the families to be discriminated. If, for example, there were a perceived continuum of waterfowl ranging from geese to swans, we should be compelled to introduce a specific criterion for distinguishing them. A similar point can be made for unobservable entities. If a physical theory admits the existence of nothing else like an electric current, then a small number of criteria, which may vary considerably from case to case, will suffice to identify currents even though there is no set of rules that specifies the necessary and sufficient conditions for the identification. That point suggests a plausible corollary which may be more important. Given a set of necessary and sufficient conditions for identifying a theoretical entity, that entity can be eliminated from the ontology of a theory by substitution. In the absence of such rules, however, these entities are not eliminable; the theory then demands their existence.

read other sorts of meters before. But he has seen the meter (again often literally) in the context of the entire circuit, and he knows something about its internal structure. For him the needle's position is a criterion, but only of *the value* of the current. To interpret it he need determine only on which scale the meter is to be read. For the layman, on the other hand, the needle's position is not a criterion of anything except itself. To interpret it, he must examine the whole layout of wires, internal and external, experiment with batteries and magnets, and so on. In the metaphorical no less than in the literal use of 'seeing,' interpretation begins where perception ends. The two processes are not the same, and what perception leaves for interpretation to complete depends drastically on the nature and amount of prior experience and training.

5. *Exemplars, Incommensurability, and Revolutions*

What has just been said provides a basis for clarifying one more aspect of the book: my remarks on incommensurability and its consequences for scientists debating the choice between successive theories.[15] In Sections X and XII I have argued that the parties to such debates inevitably see differently certain of the experimental or observational situations to which both have recourse. Since the vocabularies in which they discuss such situations consist, however, predominantly of the same terms, they must be attaching some of those terms to nature differently, and their communication is inevitably only partial. As a result, the superiority of one theory to another is something that cannot be proved in the debate. Instead, I have insisted, each party must try, by persuasion, to convert the other. Only philosophers have seriously misconstrued the intent of these parts of my argument. A number of them, however, have reported that I believe the following:[16] the proponents of incommensurable theories

[15] The points that follow are dealt with in more detail in Secs. v and vi of "Reflections."

[16] See the works cited in note 9, above, and also the essay by Stephen Toulmin in *Growth of Knowledge*.

cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to *good* reasons; instead theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached. More than any other parts of the book, the passages on which these misconstructions rest have been responsible for charges of irrationality.

Consider first my remarks on proof. The point I have been trying to make is a simple one, long familiar in philosophy of science. Debates over theory-choice cannot be cast in a form that fully resembles logical or mathematical proof. In the latter, premises and rules of inference are stipulated from the start. If there is disagreement about conclusions, the parties to the ensuing debate can retrace their steps one by one, checking each against prior stipulation. At the end of that process one or the other must concede that he has made a mistake, violated a previously accepted rule. After that concession he has no recourse, and his opponent's proof is then compelling. Only if the two discover instead that they differ about the meaning or application of stipulated rules, that their prior agreement provides no sufficient basis for proof, does the debate continue in the form it inevitably takes during scientific revolutions. That debate is about premises, and its recourse is to persuasion as a prelude to the possibility of proof.

Nothing about that relatively familiar thesis implies either that there are no good reasons for being persuaded or that those reasons are not ultimately decisive for the group. Nor does it even imply that the reasons for choice are different from those usually listed by philosophers of science: accuracy, simplicity, fruitfulness, and the like. What it should suggest, however, is that such reasons function as values and that they can thus be differently applied, individually and collectively, by men who concur in honoring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say, scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision. In this sense it is the community of specialists rather than its individual members that makes the effective decision. To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has vast fascination. What one must understand, however, is the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive.

That process is persuasion, but it presents a deeper problem. Two men who perceive the same situation differently but nevertheless employ the same vocabulary in its discussion must be using words differently. They speak, that is, from what I have called incommensurable viewpoints. How can they even hope to talk together much less to be persuasive. Even a preliminary answer to that question demands further specification of the nature of the difficulty. I suppose that, at least in part, it takes the following form.

The practice of normal science depends on the ability, acquired from exemplars, to group objects and situations into similarity sets which are primitive in the sense that the grouping is done without an answer to the question, "Similar with respect to what?" One central aspect of any revolution is, then, that some of the similarity relations change. Objects that were grouped in the same set before are grouped in different ones afterward and vice versa. Think of the sun, moon, Mars, and earth before and after Copernicus; of free fall, pendular, and planetary motion before and after Galileo; or of salts, alloys, and a sulpuhur–iron filing mix before and after Dalton. Since most objects within even the altered sets continue to be grouped together, the names of the sets are usually preserved. Nevertheless, the transfer of a subset is ordinarily part of a critical change in the network of interrelations among them. Transferring the

metals from the set of compounds to the set of elements played an essential role in the emergence of a new theory of combustion, of acidity, and of physical and chemical combination. In short order those changes had spread through all of chemistry. Not surprisingly, therefore, when such redistributions occur, two men whose discourse had previously proceeded with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions and generalizations. Those difficulties will not be felt in all areas of even their scientific discourse, but they will arise and will then cluster most densely about the phenomena upon which the choice of theory most centrally depends.

Such problems, though they first become evident in communication, are not merely linguistic, and they cannot be resolved simply by stipulating the definitions of troublesome terms. Because the words about which difficulties cluster have been learned in part from direct application to exemplars, the participants in a communication breakdown cannot say, "I use the word 'element' (or 'mixture,' or 'planet,' or 'unconstrained motion') in ways determined by the following criteria." They cannot, that is, resort to a neutral language which both use in the same way and which is adequate to the statement of both their theories or even of both those theories' empirical consequences. Part of the difference is prior to the application of the languages in which it is nevertheless reflected.

The men who experience such communication breakdowns must, however, have some recourse. The stimuli that impinge upon them are the same. So is their general neural apparatus, however differently programmed. Furthermore, except in a small, if all-important, area of experience even their neural programming must be very nearly the same, for they share a history, except the immediate past. As a result, both their everyday and most of their scientific world and language are shared. Given that much in common, they should be able to find out a great deal about how they differ. The techniques required are not, however, either straightforward, or comfortable, or parts of the scientist's normal arsenal. Scientists rarely recognize them for quite what they are, and they seldom use them for longer than is required to induce conversion or convince themselves that it will not be obtained.

Briefly put, what the participants in a communication breakdown can do is recognize each other as members of different language communities and then become translators.[17] Taking the differences between their own intra- and inter-group discourse as itself a subject for study, they can first attempt to discover the terms and locutions that, used unproblematically within each community, are nevertheless foci of trouble for inter-group discussions. (Locutions that present no such difficulties may be homophonically translated.) Having isolated such areas of difficulty in scientific communication, they can next resort to their shared everyday vocabularies in an effort further to elucidate their troubles. Each may, that is, try to discover what the other would see and say when presented with a stimulus to which his own verbal response would be different. If they can sufficiently refrain from explaining anomalous behavior as the consequence of mere error or madness, they may in time become very good predictors of each other's behavior. Each will have learned to translate the other's theory and its consequences into his own language and simultaneously to describe in his language the world to which that theory applies. That is what the historian of science regularly does (or should) when dealing with out-of-date scientific theories.

Since translation, if pursued, allows the participants in a communication breakdown to experience vicariously something of the merits and defects of each other's points of view, it is a potent tool both for persuasion and for conversion. But even persuasion need not succeed, and, if it does, it need not be

[17] The already classic source for most of the relevant aspects of translation is W. V. O. Quine, *Word and Object* (Cambridge, Mass., and New York, 1960), chaps. i and ii. But Quine seems to assume that two men receiving the same stimulus must have the same sensation and therefore has little to say about the extent to which a translator must be able to *describe* the world to which the language being translated applies. For the latter point see, E. A. Nida, "Linguistics and Ethnology in Translation Problems," in Del Hymes (ed.), *Language and Culture in Society* (New York, 1964), pp. 90–97.

accompanied or followed by conversion. The two experiences are not the same, an important distinction that I have only recently fully recognized.

To persuade someone is, I take it, to convince him that one's own view is superior and ought therefore supplant his own. That much is occasionally achieved without recourse to anything like translation. In its absence many of the explanations and problem-statements endorsed by the members of one scientific group will be opaque to the other. But each language community can usually produce from the start a few concrete research results that, though describable in sentences understood in the same way by both groups, cannot yet be accounted for by the other community in its own terms. If the new viewpoint endures for a time and continues to be fruitful, the research results verbalizable in this way are likely to grow in number. For some men such results alone will be decisive. They can say: I don't know how the proponents of the new view succeed, but I must learn; whatever they are doing, it is clearly right. That reaction comes particularly easily to men just entering the profession, for they have not yet acquired the special vocabularies and commitments of either group.

Arguments statable in the vocabulary that both groups use in the same way are not, however, usually decisive, at least not until a very late stage in the evolution of the opposing views. Among those already admitted to the profession, few will be persuaded without some recourse to the more extended comparisons permitted by translation. Though the price is often sentences of great length and complexity (think of the Proust-Berthollet controversy conducted without recourse to the term 'element'), many additional research results can be *translated* from one community's language into the other's. As translation proceeds, furthermore, some members of each community may also begin vicariously to understand how a statement previously opaque could seem an explanation to members of the opposing group. The availability of techniques like these does not, of course, guarantee persuasion. For most people translation is a threatening process, and it is entirely foreign to normal science.

Counter-arguments are, in any case, always available, and no rules prescribe how the balance must be struck. Nevertheless, as argument piles on argument and as challenge after challenge is successfully met, only blind stubbornness can at the end account for continued resistance.

That being the case, a second aspect of translation, long familiar to both historians and linguists, becomes crucially important. To translate a theory or worldview into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign. That transition is not, however, one that an individual may make or refrain from making by deliberation and choice, however good his reasons for wishing to do so. Instead, at some point in the process of learning to translate, he finds that the transition has occurred, that he has slipped into the new language without a decision having been made. Or else, like many of those who first encountered, say, relativity or quantum mechanics in their middle years, he finds himself fully persuaded of the new view but nevertheless unable to internalize it and be at home in the world it helps to shape. Intellectually such a man has made his choice, but the conversion required if it is to be effective eludes him. He may use the new theory nonetheless, but he will do so as a foreigner in a foreign environment, an alternative available to him only because there are natives already there. His work is parasitic on theirs, for he lacks the constellation of mental sets which future members of the community will acquire through education.

The conversion experience that I have likened to a gestalt switch remains, therefore, at the heart of the revolutionary process. Good reasons for choice provide motives for conversion and a climate in which it is more likely to occur. Translation may, in addition, provide points of entry for the neural reprogramming that, however inscrutable at this time, must underlie conversion. But neither good reasons nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientific change.

6. *Revolutions and Relativism*

One consequence of the position just outlined has particularly bothered a number of my critics.[18] They find my viewpoint relativistic, particularly as it is developed in the last section of this book. My remarks about translation highlight the reasons for the charge. The proponents of different theories are like the members of different language-culture communities. Recognizing the parallelism suggests that in some sense both groups may be right. Applied to culture and its development that position is relativistic.

But applied to science it may not be, and it is in any case far from *mere* relativism in a respect that its critics have failed to see. Taken as a group or in groups, practitioners of the developed sciences are, I have argued, fundamentally puzzle-solvers. Though the values that they deploy at times of theory-choice derive from other aspects of their work as well, the demonstrated ability to set up and to solve puzzles presented by nature is, in case of value conflict, the dominant criterion for most members of a scientific group. Like any other value, puzzle-solving ability proves equivocal in application. Two men who share it may nevertheless differ in the judgments they draw from its use. But the behavior of a community which makes it preeminent will be very different from that of one which does not. In the sciences, I believe, the high value accorded to puzzle-solving ability has the following consequences.

Imagine an evolutionary tree representing the development of the modern scientific specialties from their common origins in, say, primitive natural philosophy and the crafts. A line drawn up that tree, never doubling back, from the trunk to the tip of some branch would trace a succession of theories related by descent. Considering any two such theories, chosen from points not too near their origin, it should be easy to design a list of criteria that would enable an uncommitted observer to distinguish the earlier from the more recent theory time after time. Among

[18] Shapere, "Structure of Scientific Revolutions," and Popper in *Growth of Knowledge*.

the most useful would be: accuracy of prediction, particularly of quantitative prediction; the balance between esoteric and everyday subject matter; and the number of different problems solved. Less useful for this purpose, though also important determinants of scientific life, would be such values as simplicity, scope, and compatibility with other specialties. Those lists are not yet the ones required, but I have no doubt that they can be completed. If they can, then scientific development is, like biological, a unidirectional and irreversible process. Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress.

Compared with the notion of progress most prevalent among both philosophers of science and laymen, however, this position lacks an essential element. A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is "really there."

Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausability of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some

important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. Though the temptation to describe that position as relativistic is understandable, the description seems to me wrong. Conversely, if the position be relativism, I cannot see that the relativist loses anything needed to account for the nature and development of the sciences.

7. *The Nature of Science*

I conclude with a brief discussion of two recurrent reactions to my original text, the first critical, the second favorable, and neither, I think, quite right. Though the two relate neither to what has been said so far nor to each other, both have been sufficiently prevalent to demand at least some response.

A few readers of my original text have noticed that I repeatedly pass back and forth between the descriptive and the normative modes, a transition particularly marked in occasional passages that open with, "But that is not what scientists do," and close by claiming that scientists ought not do so. Some critics claim that I am confusing description with prescription, violating the time-honored philosophical theorem: 'Is' cannot imply 'ought.'[19]

That theorem has, in practice, become a tag, and it is no longer everywhere honored. A number of contemporary philosophers have discovered important contexts in which the normative and the descriptive are inextricably mixed.[20] 'Is' and 'ought' are by no means always so separate as they have seemed. But no recourse to the subtleties of contemporary linguistic philosophy is needed to unravel what has seemed confused about this aspect of my position. The preceding pages present a viewpoint or theory about the nature of science, and, like other philosophies of science, the theory has consequences for the way in which scientists should behave if their enterprise is to succeed. Though

[19] For one of many examples, see P. K. Feyerabend's essay in *Growth of Knowledge*.

[20] Stanley Cavell, *Must We Mean What We Say?* (New York, 1969), chap. i.

it need not be right, any more than any other theory, it provides a legitimate basis for reiterated 'oughts' and 'shoulds.' Conversely, one set of reasons for taking the theory seriously is that scientists, whose methods have been developed and selected for their success, do in fact behave as the theory says they should. My descriptive generalizations are evidence for the theory precisely because they can also be derived from it, whereas on other views of the nature of science they constitute anomalous behavior.

The circularity of that argument is not, I think, vicious. The consequences of the viewpoint being discussed are not exhausted by the observations upon which it rested at the start. Even before this book was first published, I had found parts of the theory it presents a useful tool for the exploration of scientific behavior and development. Comparison of this postscript with the pages of the original may suggest that it has continued to play that role. No merely circular point of view can provide such guidance.

To one last reaction to this book, my answer must be of a different sort. A number of those who have taken pleasure from it have done so less because it illuminates science than because they read its main theses as applicable to many other fields as well. I see what they mean and would not like to discourage their attempts to extend the position, but their reaction has nevertheless puzzled me. To the extent that the book portrays scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks, its theses are undoubtedly of wide applicability. But they should be, for they are borrowed from other fields. Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way. Periodization in terms of revolutionary breaks in style, taste, and institutional structure have been among their standard tools. If I have been original with respect to concepts like these, it has mainly been by applying them to the sciences, fields which had been widely thought to develop in a different way. Conceivably the notion of a paradigm as a concrete achievement, an exemplar, is a second contribution. I suspect, for example, that some of the notorious difficulties surrounding the notion of style in the

arts may vanish if paintings can be seen to be modeled on one another rather than produced in conformity to some abstracted canons of style.[21]

This book, however, was intended also to make another sort of point, one that has been less clearly visible to many of its readers. Though scientific development may resemble that in other fields more closely than has often been supposed, it is also strikingly different. To say, for example, that the sciences, at least after a certain point in their development, progress in a way that other fields do not, cannot have been all wrong, whatever progress itself may be. One of the objects of the book was to examine such differences and begin accounting for them.

Consider, for example, the reiterated emphasis, above, on the absence or, as I should now say, on the relative scarcity of competing schools in the developed sciences. Or remember my remarks about the extent to which the members of a given scientific community provide the only audience and the only judges of that community's work. Or think again about the special nature of scientific education, about puzzle-solving as a goal, and about the value system which the scientific group deploys in periods of crisis and decision. The book isolates other features of the same sort, none necessarily unique to science but in conjunction setting the activity apart.

About all these features of science there is a great deal more to be learned. Having opened this postscript by emphasizing the need to study the community structure of science, I shall close by underscoring the need for similar and, above all, for comparative study of the corresponding communities in other fields. How does one elect and how is one elected to membership in a particular community, scientific or not? What is the process and what are the stages of socialization to the group? What does the group collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration? A fuller understanding of science will de-

[21] For this point as well as a more extended discussion of what is special about the sciences, see T. S. Kuhn, "Comment [on the Relations of Science and Art]," *Comparative Studies in Philosophy and History*, XI (1969), 403–12.

pend on answers to other sorts of questions as well, but there is no area in which more work is so badly needed. Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it.