THOMAS S. KUHN

THE STRUCTURE OF SCIENTIFIC REVOLUTIONS

MICHEL FOUCAULT

THE HISTORY OF SEXUALITY

VOLUME I : AN INTRODUCTION

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To

JAMES B. CONANT

Who Started It

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 To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my basic conceptions about the nature of science and the reasons for its special success.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 wis-*

[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.

senschaftlichen 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 monograph 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 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, ex-

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cept 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

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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-

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