The Nature of Science

Normal science is succeeded by a creative phase of revolution out of which new concepts emerge.

Charles C. Gillispie

This is a very bold venture, this essay, **The Structure of Scientific Revolutions** by Thomas S. Kuhn [University of Chicago Press, Chicago, 1962. 187 pp. $4]. Kuhn has been turning it over in his mind and developing it in his work ever since the beginning of his career as a physicist-turned-historian. That was some 15 years ago during the course of James B. Conant's program for imparting science through historical examples in the service of general education. Now Kuhn would thrust more deeply into science than pedagogy will reach. His opening sentence is unequivocal: "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" (p. 1). For he is not writing history of science proper. His essay is an argument about the nature of science, drawn in large part from its history but also, in certain essential elements, from considerations of psychology, sociology, philosophy, and physics. The reader is not to expect philosophy of science in the usual Anglo-American sense of a study of logical problems found in scientific proceedings or systems. Rather is this a sketch for a genetic philosophy of science, presented in earnest of a fully developed study promised for the future.

The author starts in the conviction that what he calls the accepted concept of science is misleading. This he describes as a view that science consists in an aggregate of facts, observations, laws, theories, and techniques for getting more such results—in short, that science is a body of information which has accumulated in a linear series of discrete discoveries about how the world is made. Kuhn's critique of the very notion of scientific discovery may, indeed, be the strongest part of his argument, and is certainly at the heart of it. It was not, of course, difficult for him (though it is still very useful) to demolish the notion that inventions of theory —Maxwell's laws of the field, say, or Newton's laws of motion, or general relativity—were found like hidden treasure or a misplaced hat, there in the logic or structure of things, wanting mainly to be revealed. Neither, however, will he allow novelties of fact—the indentification of oxygen, for example, or of radioactivity, or even of Uranus—to be counted as discoveries, if we mean thereby elementary increments of information by which science grows in volume and, in growing, has its history and progress. He tends to annul the distinction between finding fact and inventing theory. A fact is itself only by virtue of theory and vice versa.

In what, for instance, did the discovery of oxygen consist? The gas appears to have been first isolated by Scheele, who did not publish and had no influence in the matter. Priestley prepared an impure sample from the red oxide of mercury in 1774, mistook it for laughing gas, and when he came to recognize it rather as a new species, dubbed it dephlogisticated air. Lavoisier, for his part, instantly recognized Priestley's gas (though not Priestley's priority) as what combines in combustion and calcination, initially took it for pure air, and when he did see it as a fraction of the atmosphere, always considered its essence to be the principle of acidity requiring to be combined with caloric to assume the gaseous form. Valence, diatomicity, specific heats—knowledge or definition of those and other properties lay in the future, and who may say at what moment oxygen took on the identity that it has since attained? This depends on what one means by oxygen. One could mean simply what we breathe, and what was fundamental for science was less the specification of the gas than the chemical revolution wherein study of its combinations played a forcing role.

That revolution consisted not just in a rush of new reagents, new reactions, new techniques, new methods of analysis, though these there were in plenty, but also in a new way of seeing these materials of a science. Very important to Kuhn's argument are findings of the modern psychology of perception which make the literal notion of seeing as ambiguous as that of scientific discovery. He refers to the well-known shifts of gestalt in which an observer switches back and forth between seeing a rabbit or a duck in some appropriate design, and he also calls attention to more elaborate experiments which make seeing a function of habitual expectation as well as of optics and physiology. A man may, for example, be conditioned by his expectations of the world to overcome the initial malaise inspired by inverting lenses and see things right side up, and only on repeated exposure will one recognize for what it is the anomaly of a black five of hearts planted in the deck of cards. Kuhn would extend the consequences to the sense of seeing in which the verb means conceiving.

A science, then, is how its practitioners as a highly articulated group see the ensemble of its phenomena, and this proposition takes Kuhn into considerations of the psychology and sociology of scientific communities and of what factors lead them to respond to, or to resist, innovation. They are the keepers of what the author calls *paradigms*. For, although science does not develop as a deposit of empirical discoveries economized now and again by theory, it nevertheless does have its evolutionary pattern. To a phase of "normal science." when scientists are agreed upon their paradigm and seek mainly to perfect it, succeeds, as a consequence of anomalies, a creative phase of revolution out of which emerge new paradigms to replace the old and run their course. What is more, the shift from one to another occurs rather as the conversion of a community than as the persuasion of persons by bits of new evidence or shorthands in theory.

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Paradigms of Normal Science

This is an interesting schema. Let us develop and exemplify its elements a little more fully. Kuhn gives to the word *paradigms* a special significance and an importance all his own. One sees what he means, although he gives no precise definition. *Paradigms* are what give coherence to modes, or better, perhaps, to schools of science: to Ptolemaic or, contrariwise, to Copernican astronomy, to Aristotelian dynamics or to statistical mechanics, to phlogiston chemistry or to uniformitarian geology. Usually they are born in achievements like Newton's *Principia* or Lavoisier's *Traité élémentaire*, works exemplary enough to impart a tradition to a train of scientific research. They may be sufficiently comprehensive to contain a whole science like classical physics. Or—paradigms within paradigms—they may govern special and often quite restricted domains, such as the corpuscular picture of light which was displaced in the revolutionary way by the undulatory.

For Kuhn the concept has an importance going beyond a physical model, though it contains that. He particularly-wishes to emphasize that paradigms are what lay down the law to neophytes: "The study of paradigms is what mainly prepares the student for membership in the particular scientific community in 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, that is for the genesis and continuation of a particular research tradition" (p. 11).

Nevertheless, a paradigm in Kuhn's sense is no closed set of propositions and practices, not quite an object for replication as it is in grammatical usage where one verb is the pattern for an entire conjugation. "Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions" (p. 23). Of such is the business of "normal science," each era of which is lived in service to some paradigm. Gathering fact, choosing that which is relevant, building and using appropriate instruments, developing applications, determining constants, formulating theory in more economical or more general expression—amid such "puzzles" do all scientists lead most of their lives and most scientists all of their lives. Normal science, moreover, is cumulative in the fashion which has been mistakenly attributed to the whole course of scientific progress. It solves its puzzles, extends its range, and refines its measurements. Not here, however, not in this work-a-day activity, does anything original happen. For normal science never innovates. Kuhn is consistent with his argument. He represents most scientists as rather a hidebound lot, not at all eager for fundamental innovation, as men who like to know where they are and where their work fits, who even tend to resist novelties which unsettle the paradigm and with it the intellectual security of the community.

Anomalies, Innovations, and Scientific Revolutions

Novelty will out, however, and inevitably. Anomalies do occur—the problem of what moves the mobile in Aristotelian dynamics, gathering complexity in Ptolemaic astronomy, augmented weight after combustion in phlogiston chemistry, contradictions required of the ether first after Fresnel and more generally after Maxwell. Logically anomalies are not to be distinguished from puzzles which are the business of normal science. Both may be described as counter-instances to some theory not quite adequate to the matter in hand. Anomalies, however, have the property of persistently suggesting, not simply that we do not quite know how to work the paradigm, but more seriously that at certain points the fit with nature fails. When these points begin to seem crucial or numerous enough, the failure becomes a scandal; the affair reaches the state of physics in Einstein's time or of astronomy in that of Copernicus. Then it is that a revolution occurs, a shift of gestalt into a new mode of seeing things which breaks with the established order, that of the old paradigm, and by first converting and then commanding the allegiance of a (usually new) set of practitioners becomes in its turn the paradigm of a new phase of normal science. Kuhn is very severe with positivists, however. These events are never brought about by the simple sort of methodological precept which has it that a theory is abandoned or modified should some instance of it fail: "The act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other" (p. 77).

Thus, Kuhn sets great store by the necessity for scientific revolutions. He will have none of the sort of reconciliation which makes classical physics a special case of relativistic physics. Newton's laws are derivable from Einstein's work only by presupposing the latter, which created a quite incompatible system of physical referents. "What had previously been meant by space was necessarily flat, homogeneous, isotropic, and unaffected by the presence of matter. If it had not been, Newtonian physics would not have worked. To make the transition to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole. Only men who had undergone or failed to undergo that transformation would be able to discover precisely what they agreed or disagreed about. Communication across the revolutionary divide is inevitably partial" (p. 148).

I do not think that it is overstating Kuhn's argument to say that he regards the revolutions of which he writes as the only creative episodes in science. They shape the research by which the emergent paradigm is perfected throughout the course of its usefulness. Even more fundamentally, the work of imagination and discipline which the men who make the revolutions bring to bear on phenomena are constitutive of nature itself. Afterwards science transpires in a different world, heliocentric as opposed to geocentric, sequential and curvilinear rather than enduring and rectilinear.

Summary

It is not for a historian of science to pass judgment on the central critique of Kuhn's essay, since that is directed to science itself. A few reservations may be ventured, however, of a sort which the author of so searching a discussion will certainly expect, the more so since, in his own terms, he proposes nothing less than a revolution in our concept of science, if not of nature. But it is not clear to me that anyone really holds the view of science which he would demolish. I for one find a great deal more in this book to agree with than might be expected in an exponent of a counterrevolutionary school. The argument depends very heavily on the viability of the terms—*paradigm, normal science, revolution, anomaly, crisis*, and the like. So it has been with many a philosophy of history from Comte to Toynbee. So it has been with many a chapter in the history of science—phlogiston, calorie, ether—and the student of either of these genres (which Kuhn, like Comte, combines) will have learned to be wary of mistaking the terms he gives his subject for its elements, the definitions for the happenings. The argument sometimes comes perilously close to circularity: that is, normal science does not aim at novelty, ergo what is novel is not normal science but an anomaly. On strictly historical grounds, moreover, strong cases might be made for considering books like Newton's *Principia* and Lavoisier's *Traité élémentaire* as summaries of a heritage rather than as models shaping the future. The reader may be referred, for example, to E. J. Dikjsterhuis's treatment of Newton in his recently translated *Mechanization of the World Picture*, where it appears that Newton himself did not adumbrate the laws of motion in the sense in which they were fundamental to classical physics. For example, the proportionality of force to the product of mass into acceleration was imported into the second law in the development of analytical mechanics, not forced upon a school by a revolutionary law-giver. Newton was thinking of impact.

Still, there are not many books which find one making eager jottings in the margin, nor fortunately need one act on these; one may instead, and indeed in candor must, await the full development that Kuhn intends to provide. Meanwhile there can be only admiration for the erudition, the scholarship, the fidelity, and the seriousness that the enterprise reflects on every page. One is safe in predicting that whatever the final success, there will be no petty faults to find. Every historian, moreover, will surely applaud one recurrent and fundamental emphasis, which is that the development of science must be set into the context of a Darwinian historiography and treated as a circumstantial evolution from primitive beginnings rather than the ever closer approach to the telos of a right and perfect science. It is odd, and Kuhn is absolutely right about this, that by instinct scientists tend to see it the latter way. At least their students do, and who else could be responsible for that?

Subsidized Irrigation

The Value of Water in Alternative Uses. With special application to water use in the San Juan and Rio Grande basins of New Mexico. Nathaniel Wollman, Ralph L. Edgel, Marshall E. Farris, H. Ralph Stucky, and Alvin J. Thompson. University of New Mexico Press, Albuquerque, 1962. xxii + 426 pp. Illus. $10.

Is traditional irrigated agriculture really the most efficient use of water in arid regions? If industrial use were expanded, would an adequate labor force and sufficient investment capital be available? With development of the San Juan River and a diversion to supplement flow into the Rio Grande in prospect by 1975, these questions are pertinent for the State of New Mexico, and indeed, for other arid regions. Resources for the Future financed a study, carried out at the University of New Mexico, directed by Wollman and a committee (his coauthors) with the support of several subcommittees composed of faculty members and representatives of the state government. A summary of the study constitutes the first 125 pages of this book; thirteen appendixes, written by the subcommittees or individuals responsible for specific phases of the study, present supporting data and detail the methods employed.

Eight possible patterns of water use were tested. Two rates of diversion from the San Juan River to the Rio Grande and four different schemes of allocation between municipal-industrial and agriculture-recreation were analyzed. Estimated benefits for each pattern of use were obtained by estimating the primary value added for agriculture and industry plus the value added by purchases for all three uses. That industrial water yields by far the greatest return per unit of water use will surprise no one who has considered this problem. That recreational uses rank second and agriculture a poor third may be more surprising. In fact, the conclusion is that irrigation is not possible without subsidy. Utilization of all new water in New Mexico for industry is not feasible, however, because neither adequate labor nor capital can be anticipated. The so-called "high industry" models utilized only about half of the available water for industrial purposes.

The reader may be troubled to find that a situation exists that suggests that the water must be put to use, even if a subsidy is necessary, in order to avoid its diversion to other areas. The reader may also be troubled by evidence that a truly adequate basis for evaluating recreational benefits is lacking, and he may question the inclusion of secondary benefits in the calculations. Methodology, the impact of the various models on the labor force, and the differences between the state and national viewpoints are discussed in an interesting manner.

Since the project would be built with federal funds, the reader may wonder how it would rank among all possible projects which the nation might contemplate. No matter how much one may wish to debate the details of the analysis, it serves to make its intended point without ambiguity—no rational water plan can be developed without a careful study of the value of water in competing uses.

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Earth Science

Petrology. Walter T. Huang. McGraw-Hill, New York, 1962. vii + 480 pp. Illus. $9.75.

This book, which is intended for beginning students and others who need a comprehensive introduction to the broad field of petrology, is not a distinguished effort, although it does have some advantages when compared with many older texts. Its advantages include some use of recent experimental studies on oxide and silicate systems, abundant illustrative material from recent geological literature, and a more up-to-date treatment of the rock-forming minerals from the viewpoint of crystal chemistry. The numerous illustrations are rather well selected,