been discussed without success at the last six successive councils of ministers. Unless the research budget is sanctioned, money for research at the EEC's laboratories at Ispra, and in Belgium, the Netherlands and West Germany, will run out at the end of June, though there is enough money to go on paying salaries until the end of the year. Herr Guido Brunner, the EEC commissioner responsible for research and energy policy, hoped that a decision on JET might finally be reached at a meeting of Foreign Ministers on 21 June or at a meeting of EEC heads of government at the end of June. Presumably, if a decision is made—and even if it goes against Britain's site for JET, at Culham near Oxford—the British government will be prepared to loosen the purse strings.

Seveso Revisited

There is some encouraging evidence that the pollution at the Italian town of Seveso, where a chemical plant ran out of control last July and sprayed the countryside with the poisonous chemical dioxin, is not as serious as some had feared. Admittedly, most of the reassurance has come from the plant's owners, Hoffmann–La Roche, and therefore has to be treated with caution, but there do seem to be grounds for optimism.

The children injured by the dioxin—which causes an unpleasant skin rash known as chloracne—have made good progress, and experts who have treated them (including a British specialist with no Roche connections) are confident that they will recover completely. Three children, Roche says, may be left with permanent scars. As to fears that dioxin contamination would have teratogenic effects on pregnant women exposed to it, all those at a critical stage of pregnancy at the time of the incident have now had their babies without any higher than average incidence of abnormality.

Decontamination of the less affected areas around the factory, which has been proceeding during the winter and spring, also seems to have achieved good results. Inside the houses, special suction devices fitted with fine filters were used, and the outsides of the houses were washed with a special detergent solution. To prevent contamination of the sewage system, all the fluids used in this process were collected in containers. The gardens of the affected houses were cleared of plants, their topsoil was removed and replaced by fresh soil, a process also being applied to nearby fields. Analysis by wipe test in every house showed that it was possible to remove between 90 and 100 percent of the dioxin by these techniques.

Whether that will prove to be enough will soon be known. The chairman of the regional administration, Sesare Golfari, announced in June that 600 of those evacuated from their homes in the less contaminated areas would be able to return home "within a month." A further 200 individuals from the most contaminated area next to the factory would have new homes found for them elsewhere in Seveso, he said. Roche says that those going home will be in no danger, pointing out that after the accident they had continued to live in their houses for 3 weeks, before they had been decontaminated, without ill effects.

The remaining uncertainty arises over the decontamination of the worst affected areas, both houses and agricultural land, immediately around the plant. The houses could be cleaned by the techniques already used in the less affected areas, but the land is a tougher problem. The Italian authorities originally favored the incineration of the top layer of soil at a temperature of 1200°C in a specially built furnace which could later be used as a regional garbage incinerator. The local population is unenthusiastic about this idea, suspecting that Seveso will then become the region's refuse dump. As an alternative, Roche has suggested the use of agricultural techniques to promote the growth of the surface vegetation and accelerate the breakdown of the dioxin. This would be slower than incineration, the company admits, but more acceptable environmentally.

—NIGEL HAWKES

Thomas S. Kuhn: Revolutionary Theorist of Science

Notions of what scientists do tend to be shaped not by scientists themselves but by historians and philosophers of science. It is they who describe what scientific method is, and the process by which old theories give way to new. What is still probably the most generally held view of science, among both scientists and the public, is one that was shaped during the 1930's and 1940's by the school of positivist philosophers known as the Vienna Circle.

According to this view, science is a strictly logical process. Scientists propose theories on the basis of inductive logic, and confirm or refute them by experimental test of predictions deductively derived from the theory. When old theories fail, new theories are proposed and adopted because of their greater explanatory power, and science thus progresses inexorably closer to the truth.

Logical empiricism, as this view is known, deliberately ignores the historical context of science as well as the psychological factors which many people would consider important in science, such as intuition, imagination, and receptivity to new ideas. Logical empiricism still has its defenders, but many philosophers and historians of science now favor perceptions of the scientific enterprise that take human factors into account as well as the purely logical structure. And some, such as philosopher Paul Feyerabend of Berkeley, expound an extreme relativism which sees science as an ideology, for which only a historical or cultural explanation can be given.

Perhaps the principal force behind this major change of views about science was a book that encapsulated many of the ideas and discontents of the time and presented them in a new synthesis which cut blithely across the demarcation lines between the philosophy, history, and sociology of science. *The Structure of Scientific Revolutions* is a landmark in intellectual history which has attracted attention far beyond its own immediate field. Only 180 pages long in its original

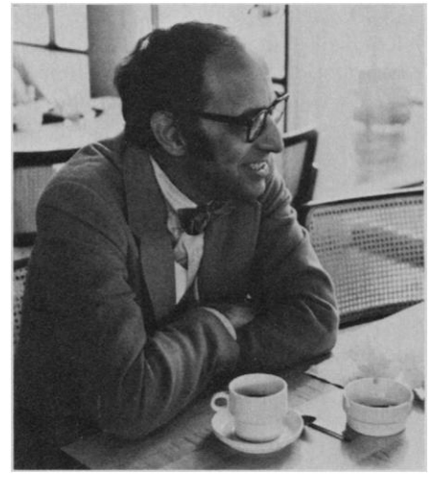
form, it is written with a combination of depth and clarity that make it an almost unbroken series of aphorisms. Its author, Thomas S. Kuhn, wastes little time on demolishing the logical empiricist view of science as an objective progression toward the truth. Instead he erects from ground up a structure in which science is seen to be heavily influenced by nonrational procedures, and in which new theories are viewed as being more complex than those they usurp but not as standing any closer to the truth. "Objectivity and progress, the pride of traditional interpretations of science, have both been abandoned," was one critic's dismayed verdict.

Kuhn, the architect of this startling edifice, is now a member of the history department at Princeton as well as of the Institute for Advanced Study. Trained as a solid state physicist, he switched to the history of science immediately after obtaining his doctorate, being introduced to his new field by Harvard president James B. Conant. Even though first published 15 years ago, Kuhn's book still evokes a set of reactions that defies any general consensus. That is partly because it means different things to different groups: historians of science have one set of attitudes, philosophers another, social scientists, political scientists, and general historians yet a different view. Common among science historians is the paradoxical view that the book is both brilliant and refutable.

"My own attitude toward the book," says a historian of science who declines to be named, "is the same as toward a number of other books, that they are classics in the sense that they have been completely disproved in detail by the professionals in the field and yet they somehow survive, which I suppose is the definition of a classic." Another historian, who also declines attribution, gives this opinion: "It's a very puzzling book to talk about, because it is the most brilliant and influential contribution to the history of science in at least two decades, and at the same time it is a brilliant wrong idea."

Kuhn's thesis, in rough outline, goes as follows. Science is not the steady, cumulative acquisition of knowledge that is portrayed in the textbooks. Rather, it is a series of peaceful interludes punctuated by intellectually violent revolutions. During the interludes, scientists are guided by a set of theories, standards, and methods which Kuhn refers to as a "paradigm."

The paradigm is the basis of the research tradition; it defines which problems are interesting and which are irrelevant. During the paradigm-governed interludes, called periods of "normal science" by Kuhn, scientists essentially solve puzzles generated by the paradigm. Study of mechanics after Newton's *Principia* is one example of a period of normal science; astronomy after Copernicus is another.



Thomas S. Kuhn

Nature is too complex to be explored at random; the paradigm is an exploration plan which both points to puzzles and guarantees that they are soluble. That is the reason for the rapid progress of the paradigm-based natural sciences compared with those in a preparadigm stage such as the social sciences.

But the tranquility of normal science does not last. Sooner or later, scientists trying to extend the paradigm find that there are puzzles they cannot solve. Often such anomalies were there from the outset but could be ignored during the heady process of paradigm explication. In fact, during normal science, scientists try to suppress novelties. Yet against the background of the paradigm the anomalies stand out with increasing prominence. The time comes when they can be ignored no longer. Then the field enters into crisis, such as befell earth-centered astronomy before Copernicus, or the phlogiston theory before the understanding of oxygen.

During crises, scientists turn from puzzle-solving to worried discussion of fundamentals. A new paradigm may be proposed, its underlying discoveries almost always being made, Kuhn states, by men who are "either very young or very new to the field whose paradigm they change." But defenders of the old paradigm patch it up with ad hoc fixes, and battle is joined by each paradigm's supporters for the allegiance of the community.

The means by which this battle is waged is central to the thesis because in Kuhn's view nonrational factors play an essential role in the contest. Logic and experiment, says Kuhn, are not sufficient: "The competition between paradigms is not the sort of battle that can be resolved by proofs." In fact the transfer of allegiance from one paradigm to another "is a conversion experience that cannot be forced." The grounds for conversion may include arguments that "appeal to the individual's sense of the appropriate or the aesthetic," and faith that the new paradigm will be better able to resolve the anomalies that precipitated the crisis.

Why is logic alone not enough to resolve the competition between two paradigms? Because paradigms are logically incommensurable. Two paradigms may seem to use the same words and concepts, but in fact these elements are logically different. Mass, for example, is conserved in Newtonian physics, but is convertible with energy in Einsteinian. Earth, in pre-Copernican theory, denotes a point of fixity. Proponents of rival paradigms are not speaking exactly the same language; they are bound to talk past each other because their terms of reference are not comparable.

The incommensurability of competing paradigms has another important consequence in the Kuhnian thesis. A new paradigm cannot build on the one it succeeds; it can only supplant it. Science is not the cumulative process portrayed in the textbooks; it is a succession of revolutions, in each of which one conceptual world view is replaced by another. But Kuhn sees no ground for believing that the new paradigm gives a better understanding of the world than did the old. The idea of progress in science can only be conceded in the relative sense that new paradigms can be recognized as more evolved than those they replace. We may, says Kuhn, "have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth."

The notion of science as an enterprise that draws constantly closer to some goal set by nature is one that is deeply held. That may be in part, says Kuhn, because of the way that scientific textbooks persistently rewrite the past in terms of the prevailing paradigm. The aim is pedagogic efficiency, so that the student does not have to master all the "wrong" ideas of the past. But the effect is to create a quite spurious tradition of uninterrupted progress, of the cumulative acquisition of knowledge. This revisionist practice explains the invisibility of scientific revolutions; the victors of

each revolution, who write the textbooks, present the past as if scientists had always striven for the objective embodied in today's paradigms.

It is probably still too early to assess the impact of Kuhn's potent thesis because its intellectual history is still in the making. Kuhn has made various modifications to the thesis, which are described in a postscript to the 1970 edition. But he has never written the expanded version that was originally promised. "I came to realize that I didn't have anything more to say in the same general vein," he said in a recent interview.

The response to Kuhn's thesis varies with discipline, being perhaps most reserved among his fellow historians of science. Some quibble with his interpretation of the case studies used to support the thesis. "Historians of science assent to it in a general way but not in specific examples," says a West Coast historian. "There has been surprisingly little work by historians of the natural sciences seeking either to vindicate or disprove his thesis," an East Coast colleague observes.

Historians of science value the book for its insights and as a heuristic tool, but seem not to use it as a guide for writing history. Historians tend to be skeptical of general theories of history, but there seem to have been rather few attempts even to put Kuhn's theory to test by applying it to particular historical episodes. John Greene, of the University of Connecticut, used the thesis in describing the development of Darwin's theory of evolution. "On the whole the paradigm doesn't work very well," Greene says, although it could with effort be made to fit some aspects.

There is in fact no Kuhnian school of history, and a general observation in the field is that Kuhn himself is not using the thesis in his present work, the early history of quantum theory. ("He spawned a monster, and doesn't know what to do with it," a colleague suggests.) Kuhn confirms that when writing history, "I try to forget the categories of that book." The theory outlined "is highly schematic and it is not going to apply in any detailed way to any particular episode," Kuhn says. As for his history of quantum theory, "I think there is going to be some grist for my mill, but I have tried very hard to tell this story for its own sake." Kuhn hopes to complete the book by the end of the year.

Philosophers of science have taken a much more active interest in *The Structure of Scientific Revolutions* than have historians. At least two symposia have been held to discuss Kuhn's thesis, and its influence on the philosophy of science is generally acknowledged. His ideas, although not his terminology, have permeated the middle ground between the logical empiricist position, where human psychology is allowed no place in scientific discovery, and the extreme relativism of the Feyerabendian school.

Kuhn's thesis has been welcomed as an antidote to the falsificationist theory of science propounded by Karl Popper of London University, which holds that theories cannot be confirmed, only refuted, and when refuted in any serious instance are abandoned. Kuhn's version, which has it that theories are only discarded when a better alternative is available, is psychologically more satisfying. "Kuhn believes that doing science is a deeply human activity," remarks philosopher Hilary Putnam of Harvard: "He is attacking the idea that the scientific method is a rule which factors out human psychology." Putnam rejects as jejune the relativism of Kuhn's notion that science cannot be said to progress toward truth, but does not consider the argument central to the thesis. In his view, the book is "certainly one of the major events of the period," at least in the English-speaking world.

Kuhn's emphasis on human factors naturally disturbed the logical empiricists and others. He was accused of portraying science as an irrational and subjective process, a charge he denies. The critics' reaction was understandable, because the emphasis of Kuhn's thesis is that logic alone cannot be decisive in a choice between theories. But, as Kuhn explained later, that does not mean that logic and experiment are not of great importance. Philosopher Dudley Shapere of the University of Maryland, one of Kuhn's severest critics on this point, concedes that Kuhn's aim was probably to show that science, "far from being a routine mechanical cranking out of results according to a prescribed method, without interesting intellectual content, was really creative, like art." But the implication of the argument is profoundly antiscientific, Shapere says, because it implies that what one accepts in science, as in art, is only a matter of taste. Yet even Shapere describes the *Structure of Scientific Revolutions* as "without question the most widely influential book on the interpretation of science in the past quarter century."

The appeal of Kuhn's thesis has not been confined to philosophers of science: it has had profound resonances in the social sciences and in fields as distant as general history and economics. "Not since the publication of R. G. Collingwood's *Idea of History* [in 1946] has a work of 'theory' won from historians the amount of interest recently accorded Thomas S. Kuhn's *The Structure of Scientific Revolutions*," commented David A. Hollinger in a 1973 article in the *American Historical Review*. The book "excites the imagination of working historians chiefly because much of what it says about scientific communities seems to apply so strikingly to other kinds of communities. . . . Historians are moved by Kuhn's sense of what a tradition is, what conditions sustain it, and what the relation is between tradition and innovation." But Hollinger acknowledged that some historians had found incongruous uses for the thesis, such as the comparison with a Kuhnian scientific revolution of the American decision to withdraw from Vietnam under the pressure of antiwar demonstrations.

Social scientists seized on the book for different reasons, and in part because it seemed to deflate the aura of the hard sciences in implying that they were not really so different in structure from the softer sciences. "I think this book doubtless seemed to have relieved them of an albatross," comments Kuhn.

Between 1970 and 1975, there occurred a "veritable explosion of interest in Kuhnian thought among social scientists," historian John D. Heyl observed in a review in *Society*. The thesis, says Heyl, "was bound to strike a responsive chord among scholars who were questioning the philosophical basis and the future direction of their disciplines." But amid the excitement over Kuhn's analysis of the physical sciences, few paused to examine Kuhn's brief remarks on the social sciences, in which he strongly suggests that they are in a preparadigmatic state. The "Kuhnian interlude" may not leave much of permanence in some of these disciplines, Heyl implies, but it has nevertheless occasioned a lively debate among thousands of scholars: "This experience has been enormously invigorating to the individuals involved and to the intellectual environment of which they are a part. Such an achievement, all too rare in our day, should be appreciated for its own sake."

Since Kuhn does not permit truth to be a criterion of scientific theories, he would presumably not claim his own theory to be true. But if causing a revolution is the hallmark of a superior paradigm, the *Structure of Scientific Revolutions* has been a resounding success.

—NICHOLAS WADE

Erratum: The name of the director of the New Orleans Sewage and Water Board was misspelled (24 June, page 1421). The correct spelling is Stuart H. Brehm.