

AT THE FRINGES OF SCIENCE

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With a New Epilogue

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4 Science and Its Practice

While I have been describing some of the bizarre penumbral science that has often attracted a great deal of attention, I have avoided any detailed examination of the question of how the scientific contents of those episodes are to be judged. What are the standards against which they are measured? Who is qualified to pass judgment on the validity of the claims? Indeed, and most fundamentally, what is science? What is the *scientific method* so often invoked as though it were some magical procedure?

In the generally held picture of the scientific method, there is an endless cycle of experiment and theory. "Experiments" actually consist of carefully designed and controlled experiments and observations of naturally occurring phenomena. From these experiments and observations, broad generalizations are extracted (by induction) and synthesized into theories to "explain" what has been seen. We can put the theory to the test by making more measurements. To the extent that we get agreement with the formula's predictions, we will continue to use it with confidence. But if we find disagreement between prediction and experiment, we may have to modify the theory or even seek something totally new that rests on different assumptions.

This brief sketch of the scientific method is correct as far as it goes—but it omits any number of important factors. What sort of assumptions are acceptable in constructing a theory? What constitutes a satisfactory scientific theory? How do scientists come to accept some theories but reject others? Who makes these decisions?

Over the years these questions have been answered many times, often at great length but without finality. Commentators (like blind people feeling an elephant) tend to describe different aspects; the whole is far more than the sum of the parts. Most scientists have probably not given much thought to these questions. Ask a scientist to define science and the scientific method, and you will probably get a description that is correct as far as it goes but most likely very incomplete. Ask a philosopher of science, and you will probably receive a much broader answer that will often seem (to many of us scientists) to make only occasional contact with



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what we recognize as our activities. In addition to these familiar views of the scientific enterprise, another perspective has been emerging from the the sociologists of science. The social dimension covers the interactions within the scientific community as well as the complex relations connecting science to the wider society.

What are these different views of science? Most working scientists, if asked, would probably give a "realist" opinion: that there exist in nature a number of regularities that relate various entities and processes. The task of the scientist is to discover these regularities and express them as "laws," often using the language of mathematics. Later scientific research may point to the errors or incompleteness of an existing theory and lead to its modification or even its rejection in favor of a more comprehensive theory. The test of a theory lies both in its ability to draw together a broader range of observations than did its predecessor as well as its ability to predict the outcome of future experiments and observations. In the realist view the content of science is cumulative, apart from obvious error. The basic experiments and observations are "facts" that remain true, though their incorporation into theories may change with time, generally gradually but sometimes radically. To that extent the core knowledge of science is reliable.

In contrast, there are strenuous debates among philosophers and sociologists of science regarding the structure of science. One school of thought plays down the role of experiment in the construction and validation of theories to such an extent that, in rebuttal, Allan Franklin felt it necessary to argue that "it is reasonable for scientists to gather data."¹ Even the reality of the success of science has come under scrutiny. A view of science currently held by some sociologists and philosophers is termed "relativist"; it is based on the assumption that scientific knowledge is relative, dependent on the views and social ambience of each commentator and without any underlying physical reality. Consequently, much as social and political views change with time and vary between different groups, so will "scientific" interpretations change. Further, in the words of Stephen Cole, "It is also clear that much of what was commonly accepted by the scientific community as true in the past is currently believed to be wrong. What we currently believe to be true *will* in the future most likely be thought of as wrong" (emphasis added).² The implication of this view is that no scientific knowledge can be trusted, or, as John Ziman has described it, "doctrinaire sociologist relativists seem to suggest that one bit of claimed knowledge is as good as another. ... Scientists have every right to express their opinion that some knowledge claims, such as those made for extrasensory perception, are so contrary to established understanding, and are supported by so little evidence, that they should be dismissed as parascience."³

There is no way in which I can synthesize all of these ways of viewing and describing science into a single comprehensive picture that will satisfy everyone. Nor can I completely evade a discussion of these topics. I therefore give emphasis to the image of science that most scientists see and set out my own descriptions,

leaning heavily on the thoughtful writings of Ziman and Thomas Kuhn, which I have found most congenial to my own perceptions. In doing this, I am well aware that I will attract, at best, the scorn of the relativists, but I am not alone in my views. Larry Laudan, a philosopher of science, has attacked "the displacement of the idea that facts and evidence matter by the idea that everything boils down to subjective interests and perspectives," which he calls "the most prominent and pernicious manifestation of anti-intellectualism in our time"; he believes "the relativist position to be profoundly wrong-headed."⁴ Another trenchant criticism of the "new sociology of science" has come in two lengthy papers from the scientist-philosopher Mario Bunge, who writes that the result is an "utterly grotesque picture of science."⁵

In the main, scientists are likely to agree with my descriptions, and it is scientists whose opinions are going to determine what is welcomed and then incorporated as new components of science or be rejected as erroneous or pseudoscientific. In order to understand scientists' reception of unorthodox ideas, we need to understand science as scientists know and use it, not as others might see it or prescribe procedures for it.

In defense of the realist position, let me cite just two of the clear successes of scientific prediction. After the accidental discovery of the planet Uranus in 1781, careful observations showed that its orbit deviated slightly from what was predicted. Those calculations took into account the gravitational attraction of the sun and other planets. Two mathematicians, John Couch Adams in England and Urbain Leverrier in France, took those orbital deviations to indicate the presence of a hitherto unsuspected additional planet. In 1846, astronomers at the Berlin Observatory found this planet very close to its predicted position, and we now know this body as Neptune. More recently, in the 1980s, a complex theory of what are termed "elementary particles" required the existence of a particle that had never been observed. Two groups of experimenters, using totally different methods, found this particle (now labeled the J/ψ particle).

I find it difficult to accept the idea that Neptune and the J/ψ particle are simply "social constructs." Against this sort of success, we have, as far as I know, no comparable and testable prediction from the relativists. Certainly nothing has emerged that is of any help to us in determining the scientific merits of claims for new discoveries, nothing to evaluate the merits of cold fusion or continental drift or the role of DNA in biological structures and processes.

Kuhn's book *The Structure of Scientific Revolutions* appeared in 1962 and has become the most widely read and influential book of its type over the past fifty years.⁶ Although Kuhn's views have since changed somewhat, his book has in many ways shaped the debates over the descriptions of the methods of science and, more than any other work, has caught the imagination of scientists who have generally been at best skeptical of the philosophy of science and even less complimentary about the sociology of science. (But in honesty I would guess that most scientists have probably not read Kuhn.) I have also found Ziman's *Public Knowl-*

edge² of great value in setting out a description of the scientific enterprise, covering many aspects that Kuhn did not touch upon but that are equally important and extend into the sociology of science—or what is now becoming known as “science studies,” the social structure and dimensions of science. I have no doubt that the reason I find their writings so accurate as descriptions of science is that both authors were trained as scientists who tend to approach this subject as I would. Kuhn was involved in physics research before switching to the history and philosophy of science, and Ziman had a distinguished career as a theoretical physicist whose writings on the structure of science led him into science studies. Another scientist whose writings I have found perceptive and useful is Peter Medawar, 1960 Nobel Prize winner in physiology.⁸

Science represents the human effort to describe and understand the natural world through passive observations, active experiments, and theoretical analysis and synthesis. If we judge a science and its theories by the simple test of their usefulness for making accurate and generally quantitative predictions, we find, in the natural sciences and especially in the physical sciences, that mathematics has become an indispensable language for describing the relationships between various quantities and for permitting the manipulation of those relationships to yield insights and predictions that would be totally impossible if words alone were used. When evaluated in this way, the physical sciences have been the most successful sciences largely because it has been possible to reduce their complexity to the scale of controllable and inanimate systems, with the focus of attention on a very few quantities to the exclusion of all others. In the biological and medical sciences, it is not as easy to identify much less exclude unwanted variables in the study of living organisms, but the application of molecular and cell methods has produced a revolution in some areas. Moving along the spectrum of success, we find some problems in the social and behavioral sciences that have yielded to the application of quantitative methods, but many more that elude even agreement on methods, let alone the meaning of their results. Perhaps very different scientific methods will have to be devised and our definitions of science thus expanded.

A foundation for success in a mature science is the widespread acceptance of a number of basic assumptions and definitions, as well as the methods by which activities and their results will be judged. In the absence of this professional consensus, opposing camps find it difficult to conduct fruitful debates and therefore to accept each other's findings. This doctrinal division can have practical consequences. For example, in psychiatry, one school of thought relies heavily on the physics and chemistry of the body for its diagnoses and treatment, while another school seeks subconscious causes for behavioral dysfunction. In contrast, consensus is overwhelming in the physical sciences: There is virtual unanimity on the foundations, still leaving plenty of room for disagreements along the frontiers. Physicists do explore possible small modifications to Newton's laws, and evolutionary biologists are involved in a vigorous debate on the speed and pathways of evolution. At the same time, we do not find physicists haggling over the major

framework that includes such sweeping principles as the conservation of energy or biologists questioning the central idea of evolution. Both of these have long since passed into the well-established body of accepted science, and it is their fine-tuning and expansions that attract attention.

Accordingly, as scientists, most of us go about our daily work comfortably within the accepted norms of our particular speciality, generally untroubled by skirmishes being fought out over confessional differences. What, then, guides us in our day-to-day science? We do not start each day by using the flip of a coin to decide what to do, which experiments or calculations to pursue. In our “normal science” (as Kuhn has termed it) we work on *problems* whose articulation is drawn directly from the prevailing theory. To this structure Kuhn has applied the label *paradigm*, intended to encompass both the experimental methods and theoretical framework, a consolidation of past success that defines a program for further research. A paradigm can be as broad as the theories of evolution and plate tectonics or as localized as the theory of radioactivity. A *mature* science (to use another of Kuhn's terms) has progressed far beyond the early exploratory stages and in its self-confident stability has paradigms that are widely shared and guide the choice of further research projects. We choose our next problem for attack because it makes sense under the umbrella of the prevailing paradigm. We promptly reject ideas that fail to fit in unless there is a good a priori case for their plausibility. Thus, accepting Velikovsky's theory would have required us to believe in the occurrence of events that ran counter to so many successful uses of the mechanical quantities of force, momentum, and energy that this did not seem (to most scientists) to be worth pursuing. That negative view could, in principle, have been changed if Velikovsky had produced plausible evidence that our mechanical paradigms were at fault or incomplete, but this he failed to do. Velikovsky's supporters, almost exclusively untrained and certainly ill informed in physics, did not accept our paradigms or seemed to be willing to accept capricious relaxations in our laws of motion and provident pathologies in the behavior of well-known forces. There is a difference between having a mind that is open to new ideas and one that is simply vacant. In contrast, when Einstein challenged those same mechanical ideas, he *did* force a change in the ways in which force, energy, and momentum are defined and used. He did this by showing how James Clark Maxwell's equations of electromagnetic theory (a cornerstone of one of the nineteenth century's most successful theories) had certain shortcomings that could be remedied only through some radical assumptions that then had testable implications. A few years later he did the same with Newton's law of gravity. Einstein's theories have subsequently been confirmed repeatedly and with great accuracy.

Most scientists do not think that Newton was wrong but rather that his laws relate to a narrower range of situations than do Einstein's. Although Einstein's formulation rests on a view of time and space that is radically different from Newton's, mathematically it is easily shown that Newton's formulations are contained

within Einstein's. For practical purposes, it is often much easier to use Newton's version, and this is routinely done for most space probes and satellites.

Within a prevailing paradigm, there can be many different reasons to choose a research problem. We might just be curious—what will be the outcome of this calculation or that experiment? This curiosity is generally not entirely innocent. We might wish to measure a quantity because of its potential usefulness in other calculations or experiments. For example, we might want to measure the behavior of some plastic under the influence of solar ultraviolet rays because we are trying to make biodegradable materials. We don't plan a fundamental challenge to a paradigm—applied research is the stuff of industrial success. Or we might undertake a calculation of some quantity that will determine whether an experiment needs to be redesigned to increase its sensitivity. Or we might be planning to measure the region of validity of a theory—does it work at those speeds or at this temperature or under zero gravity or whatever?

In selecting research problems, we are strongly influenced by our impression of the solvability of the problem. As Medawar has pointed out, we are very unlikely to embark on a research project if we do not even know whether an answer, any answer, exists. This may require us to make quick preliminary measurements or calculations to give enough hope that we will learn something from a larger investment of time and resources. Modern science, in Medawar's opinion, has made its great progress by selecting just those problems that *can* be solved and ignoring problems that seem interesting but for which there seems to be no solution with current methods, if at all. In contrast, in the social sciences many problems are obvious because of their great social importance, but they do not necessarily have unambiguous solutions, solutions that do not depend on subjective and unvalidated assumptions. A social problem may still need a solution, but this will not be in the sense of solving the problem but rather of dealing with it by finding a political or social compromise (at least temporarily).

We work within the framework of a prevailing paradigm. How did it emerge? It was assembled from the accumulation of earlier generations of experiment and theory. In some cases *induction* was used: From a number of specific examples a broad generalization was drawn that describes not only those measurements that have been made but also those that have not yet been made. For example, Galileo determined that the distance an object falls increases with the square of the time taken. He did not measure every possible distance and the time for each corresponding fall; his results, though, have been generalized into the formula

$$(\text{distance}) \text{ is proportional to } (\text{time})^2.$$

This result can be used to calculate the time taken for *any* specified distance or, in the inverse calculation, how far an object will fall in a given time. This formula works even for distances not originally measured.

We must refrain, though, from thinking that induction is the only way to scientific progress. Generalizations do not always follow from a hunting-and-gathering phase. One does not assemble a winning baseball team by scanning a selection of baseball cards. Many of the most revolutionary advances have come from flashes of inspiration and intuition, by processes not at all well understood. The same can be said of the assembly of a pennant-winning baseball team.

Why doesn't the formulation of a paradigm close out research in that subject? What need is there for further research? Why test a paradigm? There are several reasons. Take again the example of Galileo and the falling object. We might be interested in testing how far this formula can be extended. We have no conclusive evidence that Galileo actually dropped anything from the tower in Pisa; we do know that he devised some ingenious ways of converting the problem of the falling object into one that allowed him to make useful measurements with the relatively crude timing means at his disposal. But even if he had gone to the top of the tower, he could have conducted his tests on objects falling over distances of no more than the height of the tower, about 200 ft. Does the same formula apply to objects falling 500 ft, 1,000 ft, or more? Here we are trying to *extrapolate*, to go beyond the known region of the paradigm's validity. Experiments show that our simple formula indeed does *not* hold indefinitely, and we have come to understand this in terms of air resistance that builds up as the speed of an object increases. At low speeds this effect is too small to be noticed, but if we plan to make calculations for very large distances of fall, then the theory and resulting formula need to be altered. Even without air resistance, the simple laws do not hold indefinitely. At very high speeds it is necessary to use Einstein's relativity theory. The familiar low-speed laws are then seen as contained within the formulas of relativity. For mathematical convenience, though, we continue to use the old familiar formulas for many everyday computations—even in such cases as the trajectories of space probes—because the accuracy is quite sufficient.

There are additional reasons a paradigm does not represent the last word. In the course of normal science, we might find results that are unexpected, that do not seem to find an explanation in terms of the paradigm. Kuhn terms these findings *anomalies*. When we encounter an anomaly, our immediate response should be to check measurements and calculations. It is very easy to misread a dial or make an error in a calculation, and we have all done so. Sometimes our apparatus produces an artifact—something that we never satisfactorily explain but that never recurs. Reports of some of these anomalies do find their way into the scientific literature, where they may play a useful role, stimulating other scientists to be alert to possible new phenomena and so trying to confirm their existence. But many of these anomalies remain unconfirmed and unexplained; this aspect of the published literature is known to the "in" groups but stands as a potential trap for the unwary. Not everything in print is reliable, as we should know so well from newspapers. Science is not immune to this problem.