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Scientific Literacy and the Myth of the Scientific Method

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about the world around us (if, indeed, there is a world around us). But further thought and observation reveal that some facts are indubitably more theory-laden than others, to a degree that matters quite profoundly: one can see a rabbit or a duck, but few people would see here a map or an extraterrestrial, let alone a typewriter or a screwdriver. That theory-ladenness is a matter of degree leaves open the possibility that there really is something objectively out there, even though our knowledge of it rests on interpretation. There are actually black lines and white spaces in that diagram, though there may not really be a duck or a rabbit. So the realism of scientists can be seen as legitimate even as we learn not to accept as necessarily real every item that is talked about by scientists as though it were real.

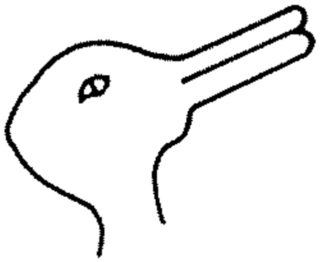

Figure 1. Is it a duck or a rabbit? Can there be a fact in the absence of a prior point of view?

We become steadily more sophisticated the more we learn of the history of science and about current scientific activity, and the more we examine in particular instances what the evidence indicates about a particular phenomenon and its explanation. Thus we can come to understand why scientists have enormous confidence in such things as the Periodic Table of the Elements and why that confidence is well placed even when the apparently equal confidence of some scientists in other things (cold fusion, say) may be obviously misplaced. It is not only sensible to think of scientific literacy as learning STS, it is eminently practical too.

The aim of this book, as already mentioned in the preface, is to show that we can think intelligently about science even if we know little about the substance of science—albeit we can enjoy a much richer life if we are able to appreciate what science has achieved and what extraordinary advances it is making.

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The So-called Scientific Method

It is widely believed that the essence of science is its method. The earlier-mentioned definition used in surveys of scientific literacy expresses commonly held notions of what the scientific method is: systematic, controlled observation or experiment whose results lead to hypotheses, which are found valid or invalid through further work, leading to theories that are reliable because they were arrived at with initial open-mindedness and continual critical skepticism.

One universally acknowledged source of this view is Francis Bacon (1561–1626); knowledge, he said, should come by generalizing from what one actually observes in the world—by contrast with the classic, Aristotelian approach of deducing with logical rigor from axiomatic first principles. But over centuries of argument and refinement, it has become clear that Baconian, inductive work could never establish truly certain knowledge: the *next* observation might force a different view or theory to be adopted even if the previous million observations had not—for example, just because the first million swans observed were white, one could not guarantee that the next one would be white also.

Distinctions were then suggested between observable and nonobservable things, it being supposed that at least one could be certain about observables, even if knowledge about nonobservables was inherently less reliable. Karl Popper introduced the influential insight that theories could never be positively proven to be true, whereas some theories could sometimes be definitively disproved; so, he suggested, to be scientific meant to deal in theories that could—at least in principle—be falsified.

Certain other significant issues in the philosophy of science will be taken up later, for example, the "theory-ladenness of facts"—to what degree do we observe what we believe we shall observe, by contrast with what may (or may not) be really there? For our present purpose, it is sufficient to recognize that these are the salient acknowledged elements of the popular view of being scientifically methodical: empirical, pragmatic, open-minded, skeptical, sensitive to possibilities of falsifying; thereby establishing objective facts leading to hypotheses, to laws, to theories; and incessantly reaching out for new knowledge, new discoveries, new facts, and new theories.

The burden of the following will be how misleading this view—which I shall call "the myth of the scientific method"—is in many specific directions, how incapable it is of explaining what happens in science, how it is worse than useless as a guide to what society ought to do about science and technology.

Are Chemists Not Scientists?

The scientific method is empirical. Scientific theories result when observation confirms tentative hypotheses. When the evidence speaks against them, hypotheses are falsified and therefore discarded.

One of my fellow graduate students in chemistry at the University of Sydney many years ago was trying to calculate certain properties of molecules, and he was the first to try to take account of one relatively subtle factor. Unfortunately, his calculations turned out to differ from the experimental values by more than earlier calculations had.

According to the Method for being Scientific, Dave should have considered his calculations falsified and tried another tack. Instead, he and his faculty advisor *ignored the comparison with experiment!* They were both mighty pleased with the progress Dave had made. He graduated top of our class, not much later he was on the faculty at Oxford, and soon after that he was a Fellow of the Royal Society.

Dave is far from alone among chemists in trusting theory more than experiment. A few years ago, a review article in *Science* listed many instances in which calculations had been right while experiment had been wrong: for the energy required to break molecules of hydrogen into atoms; for the geometry and energy content of CH_2 (the unstable "molecule" in which two hydrogen atoms are linked to a carbon atom); for the energy required to replace the hydrogen atom in HF (hydrogen fluoride) by a different hydrogen atom; and for others as well. The author, H. F. Schaefer, argued that good calculations—in other words, theory—may quite often be more reliable than experiments . . .

That is the view of one who is a theoretician, of course. You do not have to be long in a chemistry department to learn that chemists are no homogeneous tribe but rather a (sometimes uneasy) confederation of several distinct tribes: the analytikers, the inorganikers, the organikers, and the physical chemists are almost universally recognized to be distinctly different; and among or within these, or occasionally as separate tribes in their own right, there are electrochemists, polymer chemists, theoretical chemists, and others as well. And there are further subdivisions still: for instance, within many of these tribes, into experimentalists and theorists.

Naturally, each tribe and subtribe thinks its own way of doing things to be the best way, the *scientific* way. So theorists tend to believe that experimental evidence is important only insofar as it suggests new theory; and if experiment and theory happen not to agree, the theorists will often prefer to believe the theory rather than the (experimental) evidence. Experimentalists, on the other hand, regard that as perverse; they know it is observation and experiment that teach us about how the world works, theories being only devices that make it easier to remember the facts.

Both sides have something of a point (though they rarely manage to get much beyond it). Taking for the moment the side of the theorists, it is unquestionably the case that failure to go beyond what experiment shows can mean that discoveries are missed. A dramatic instance is that of the structure of DNA (the molecules that convey hereditary information), whose elucidation is generally credited to James Watson and Francis Crick. One crucial bit of information was that DNA contains equal amounts of the substances A (adenine) and T (thymine), and also equal amounts of G (guanine) and C (cytosine). Those equalities had been indicated by the lengthy, painstaking experimental work of Erwin Chargaff, who has in more recent years made abundantly clear his belief that Watson, Crick, and the Nobel Prize Committee did not give him due credit. But when you look at Chargaff's publications, what you find are tables like those in figure 2, in which the amounts of A and T, and of G and C, are only *approximately* equal (or, what amounts to the same thing, in which the ratio A:G is only *about* the same as T:C). Chargaff wrote about his observations: "The results serve to disprove the tetranucleotide hypothesis. It is, however, noteworthy—whether this is more than accidental, cannot yet be said—that in all desoxypentose nucleic acids examined thus far the molar ratios of total purines to total pyrimidines, and also of adenine to thymine and of guanine to cytosine, were not far from 1."

By refusing to stick his neck out beyond the actual results and say plainly that they *mean* exact equality and hence some sort of pairing in the molecular structure, Chargaff may have missed out on a share in the Nobel Prize. Watson and Crick, on the other hand, were speculating and theorizing about the molecular nature and biological functions of

	In Ox Thymus			In Ox Spleen		In Human Sperm		In Human Thymus	In Avian Tubercle Bacilli	In Yeast	
	1	2	3	1	2	1	2			1	2
A(adenine)	0.26	0.28	0.30	0.25	0.26	0.29	0.27	0.28	0.12	0.24	0.30
G(guanine)	0.21	0.24	0.22	0.20	0.21	0.18	0.17	0.19	0.28	0.14	0.18
C(cytosine)	0.16	0.18	0.17	0.15	0.17	0.18	0.18	0.16	0.26	0.13	0.15
T(thymine)	0.25	0.24	0.25	0.24	0.24	0.31	0.30	0.28	0.11	0.25	0.29

	In Ox Thymus	In Ox Spleen	In Ox (average)	In Human Thymus	In Human Sperm	In Human Liver		In Human (average)	In Yeast
						Normal	Cancer		
A/G	1.3	1.2	1.29(±0.13)	1.5	1.6	1.5	1.5	1.56(±0.008)	1.72
T/C	1.4	1.5	1.43(±0.03)	1.8	1.7	1.8	1.8	1.75(±0.03)	1.9

Figure 2. Erwin Chargaff's data about the chemical composition of DNA, taken from his "Chemical Specificity of Nucleic Acids and Mechanism of Their Enzymatic Degradation," *Experientia* 6 (1950): 201–40.

DNA; and they postulated a structure in which the equalities are *exactly* 1—deviations found from that in actual practice could be regarded as experimental errors. Watson and Crick turned out to be (largely) right; so, once again, ideas or theory had turned out to be a better guide than raw data, to what it all means. Inevitably so, for raw uninterpreted data do not mean anything: meaning rests on interpretation.

Evidently, then, some of the most successful chemists have not practiced the proper scientific method, which is supposed to put evidence first and theorizing second.

Science is also supposed to seek new discoveries; but, it turns out, chemists often do not welcome new discoveries. For example, if you read about chemical reactions that oscillate periodically, you find that William C. Bray's discovery of such a reaction in 1921 was simply not believed. Some thirty years later, in 1951, a paper by B. P. Belousov on the same subject was rejected, the editor saying that the reported results were simply impossible. Finally in the 1970s these results came to be accepted, *but only after a theoretical treatment had shown how oscillations could come about.* Again, more heed had been paid to theory—which is to say to preconceived belief—than to plain empirical fact.

Is Anyone a Scientist?

I should make quite plain that I do not *really* want to say that chemists are not proper scientists. What I just did with chemists (because I happen to know them best, being one myself) could equally be done with astronomers, or biologists, or geologists, or physicists, or with any of the other tribes within science. The point I *do* wish to make is that purportedly authoritative pronouncements as well as popular ideas about how science works are very seriously mistaken. One can find innumerable examples in all the sciences where theory was believed in the face of apparent evidence to the contrary; one can even find such an approach explicitly defended by eminent scientists—for example, the physicist Sir Arthur Eddington: "it is also a good rule not to put overmuch confidence in the observational results that are put forward until they have been confirmed by theory."

Even worse, theory—which, remember, is preconceived belief—may cause scientists to think they are observing things that in actuality do not exist, like the canals of Mars. And all the sciences offer instances where major new discoveries have not been accepted for quite a while because they ran counter to existing beliefs—consider the discoveries of Hermann Helmholtz and Max Planck, of Joseph Lister in medicine, of Oliver Heaviside in mathematical physics, Thomas Young's wave theory of light, and the cases of Louis Pasteur and Gregor Mendel and Svante Arrhenius and on and on and on.

So the classical and common view of science misconceives the actual relationship between theories and facts; and (consequently, inevitably) it misconceives the nature of the scientific method—the things that scientists actually do. It misconceives the behavior of science and of scientists in the face of surprising discoveries; and it misconceives much else about science, about technology, and about their interaction with one another and with the wider society.

An important misconception is implicit in the very use of the terms "science," "scientists," "scientific." To talk of scientists is to imply that astronomers, biologists, chemists, geologists, and physicists are all somehow much the same in some significant respect. To talk of science is to imply that astronomy, biology, chemistry, geology, and physics are all much the same sort of things. When there is talk about being scientific, it is commonly implied that one can be that, scientifically methodical, irrespective of the particular nature of what is being done; that one can be scientific about anything, like canvassing for new members for a bridge club: "Westchester County . . . has a tremendous program . . . working on memberships scientifically—how to get the people and how to keep them."

As soon as one looks in any depth, however, it becomes less and less clear what is really the same about astronomy and biology, say, or about what astronomers do and what biologists do. Sure enough, both astronomy and biology (and the other sciences as well) have to do with the study of selected aspects of nature. Sure enough, their findings are always subject to the commands of reality: false results are discarded (sooner or later, as their falsity becomes sufficiently obvious). Sure enough, each of the sciences now offers impressively detailed, coherent, and reliable insights, far more than they did fifty years ago, vastly more than a century ago, almost unrecognizably more than two centuries ago.

But there, or about there, the identity among the sciences comes to an end. The diversity among them includes that they vary in the degree to which they use mathematics: physics and astronomy cannot do without high mathematics, whereas much of biology or geology needs little more than arithmetic, and various bits of chemistry fall into one or the other of those categories. Though this diversity is commonly acknowledged, not generally recognized is the degree to which that difference entails other significant differences of practice: whether or not quantification is seen as the ultimate aim, for example, and whether or not mathematics is a required part of a student's initial training, and whether or not one comes to equate "quantitative" with "scientific" or "good."

Again, the distinction between observational and experimental science is a commonplace, but appreciation of the corollaries is not. Yet the way in which observational astronomers work has little in common with the way experimental chemists work: they differ in the sorts of funding they apply for (telescope time or chunks of actual money), in their reliance on graduate students (optional as opposed to essential), in the frequency with which they are expected to publish, in the way their peers interpret the significance of articles published with many coauthors, and in all sorts of other ways as well. Observation is so much more at the mercy of nature than is experiment that meaningful distinctions are obscured when—as is so often done—the two are lumped artlessly together as alternative but somehow equivalent modes of being empirical.

Many other consequential distinctions among the sciences are less frequently remarked. For example, whereas astronomy and biology and geology are fundamentally and inherently concerned with large-scale change that seems always to have gone in the same direction, chemistry and physics are not. Astronomy has to deal with the evolution of the universe, the birth and development and death of stars; biology and geology seek to account for the evolution of living things and of the Earth. But physics and chemistry share no such concern with inherent, directional change: they delight, by contrast, in the discovery of permanent relationships, and they do experiments in which time is just another controllable factor. Again, astronomy and biology and geology are, by and large, observational sciences, studying whatever nature presents them with, whereas chemistry and physics, by and large experimental sciences, can decide what to study, within increasingly wide limits—to the extent of making materials and arranging conditions that nature never before knew.

With these and other differences among sciences come far-reaching differences in attitude and method on the part of those who do the science, differences not often explicitly recognized. For example, chemists and physicists do not mean quite the same thing when they call a thing "stable": physicists mean that it is stable for all time, that it is in its lowest state of energy and will remain there until disturbed by another object or a force, whereas chemists mean that the thing does not by or of itself change into something else *at a noticeable rate* in a normal environment. The differences among adepts of the various sciences go beyond matters of theory, method, and vocabulary to subtler habits of thought and even to customs of behavior, to such an extent that the differences among the sciences, not only between the sciences and the humanities, can aptly be described as *cultural* ones: they involve a great deal more than just knowing about separate and distinct aspects of nature. Thus biologists and experimental physicists use visual imagery more than do theoretical physicists. Much theoretical speculation and argumentation over very few facts is commonplace in paleoanthropology or in astronomy but not in chemistry or in geology. Physicists look to crucial experiments to decide among theories at one fell swoop, whereas astronomers are used to waiting for long periods of time for the accumulation of data to bring an end to the speculation. Nobel Prizes in physics have been awarded about twice as often for experimental novelties as for theoretical ones, but in chemistry, experimentalists have been so honored five or six times as often as have theorists. Eminent physicists were found to feel pressed for time more than were eminent biologists; and the physicists gave up research in favor of administration at an earlier age. In matters of politics, physicists are considerably more liberal, on the average, than other scientists. Rates of divorce were found to be three times as great among biologists as among physicists (some decades ago, one should perhaps stress, when all the rates were lower than they now are). Though there seems not to have been any systematic study made of the matter, illustrations such as these are readily enough found to make the point that sciences differ among one another along many dimensions and not merely in commanding "knowledge" about separate pieces of the natural world.

Once the point is recognized, reasons can readily be suggested for some of these variations. As science developed over the last few centuries, the growth of knowledge demanded specialization. But the specialization unavoidably and soon became much more than a concern with distinct sets of phenomena. Those who studied some things found that they progressed best by taking more note of theory, whereas others found themselves going astray if they ventured too far from observation—and so some specialties came to understand that experimental evidence should not be accepted until it has been confirmed by theory, whereas most sciences and most scientists at least claim to believe the opposite. Each science—and to a degree each specialization within each science—has thus come to be an idiosyncratic blend of theorizing and empiricism; and that brings inevitably with it distinct notions about what knowledge (in general!) is and about the degree to which knowledge can be said to be "certain." In turn, disparate views about the nature of knowledge lead to different judgments about what might be interesting, valuable, fruitful to study. What we call "science" nowadays encompasses a wide range not only of knowledge but also of diverse views about the nature of knowledge.

Consider, as a last illustration, geology and physics. Physics is very oriented toward theory: one learns physics as a set of mathematically formulated laws more than as a set of observed phenomena; theory serves as a substitute for individual facts. Most physicists asked about the attraction between two of the planets will look up their masses and do a calculation; it would not occur to them to seek direct observational data. Geology, by contrast, is taught primarily through description—of minerals, geographic and geophysical features, strata, and fossils; theory in geology is less specific than in physics and serves to explain after the fact and not as a substitute for individual facts. Naturally, then, physicists tend to regard quantitative theory as the epitome of science and of scientificity; and, secretly or not so secretly, they see geology and geologists as somewhat less than highly scientific. So, too, physicists have learned that it is possible to find distinct, single causes for the variety of phenomena with which they deal, the phenomena themselves being identifiably and distinctly discrete. And for these reasons, and also because they can control all the relevant factors, physicists know that they can perform "crucial experiments" that compel nature to deliver definite answers. Geologists, on the other hand, learn that their phenomena overlap one another, that diverse "causes" conjointly produce any given geological circumstance, and that the most scientific approach is not that of seeking crucial tests but that of "multiple working hypotheses," for in geology one must, over long periods of time, be willing to countenance the possibility that any one of several competing explanations may ultimately turn out to be the best.

In view of such differences, it should not be surprising, for example, that it was a physicist who pushed most dogmatically the view that the dinosaurs were killed off in a discrete event by the simple cause of the collision with Earth of an asteroid, the impact of which remains demonstrable through the layer of iridium deposited at that time at the boundary between the Cretaceous and the Tertiary strata. To paleontologists, however, that seems absurdly oversimplified. For them, the layer of iridium-rich material is rather or partly an absence of carbonate sedimentation from the oceans during long eras in which almost no limestone was formed; the extinction of the dinosaurs itself is not seen as an event but as part of the change, over the course of millions of years, in the number of species as well as of individual dinosaurs; and, moreover, that extinction is looked at as only one of a number of occasions within geologic time when the diversity of living species decreased markedly, making it a statistical fluctuation within the perpetual flux of the appearance and disappearance of species, not a discrete, unusual individual occurrence.

Thus geologists and physicists tend to approach even scientific problems in disparate ways. They learn differently what it is to be scientific, what the scientific method is; and so too do chemists and biologists and other scientists come to different and even contradictory views of what science is. Yet these characteristic differences are but little recognized, and the misconception remains widespread that there exists a single method whose utilization marks the whole of science. In point of fact, as just illustrated, there is not any single thing that one can usefully and globally call science; rather, there are many different sorts of science. Once one has said that science is the study of nature, and that scientific knowledge is valid only so long as it is not contradicted by nature, one has said essentially all that is truly common, without qualification, among all the sciences. Beyond that, one finds nothing but variation: in the degree of weight put upon evidence in comparison with theory, in the ease with which data can be gathered, and in innumerable other details.

Diverse Aspects of Science

Some of the ways in which diverse bits of science differ from one another are indicated in figure 3. In what follows, consequences of this diversity will be illustrated in an oversimplified way by comparing or contrasting whole sciences with one another; but these distinctions actually characterize most faithfully only quite small bits of science within any of the major disciplines. Thus chemistry as a whole is relatively mature, data rich, experimental, and data driven, but many bits of chemistry are not mature (the chemistry of metal clusters, for instance, or of catalysts); quantum chemistry is a recognized subdiscipline that is notably theory driven, not data driven; and so on.

Some of the variations among bits of science have to do with their individual degrees of maturity. Physics is as a whole the most mature of the major sciences, and it seems plausible that this is a cause underlying the fact that physics also has the greatest degree of unanimity within its ranks: hardly anyone within the discipline questions relativity or quantum mechanics, or that the salient task for the discipline is to achieve the theoretical Grand Unification of the Four Forces. There is little ferment over what should be taught in physics courses, or how. The professional journals stand in well-recognized differentiation of function and hierarchy of status. The professional societies work quietly and without fuss (except, occasionally, over matters external to physics itself, like what to do about the civil rights of physicists in the Soviet Union). By contrast, the young computer science is in ferment over most of those things: what the most pressing goals of the field are; whether the subject itself is more akin to engineering or to science; how its practitioners should be trained; what constitutes professional publication; and so forth. Physicists look to governments for their major funds, whereas computer scientists look to their own institutions and to the computer industry. Physicists display the patina of an established aristocracy, whereas computer scientists exhibit characteristics of the nouveau riche.

Different Sorts of Science

Young ⟷ Mature
Data-driven ⟷ Theory-driven
Data-rich ⟷ Data-poor
Experimental ⟷ Observational
Quantitative ("hard") ⟷ Qualitative ("soft")

etc.

And Different Bits of Science May Be

Frontier science ⟷ Textbook science
Normal science ⟷ Revolutionary science

And Scientists, as in Other Professions, Vary in Being

Good ⟷ Poor
Competent ⟷ Incompetent
Outstanding ⟷ Mediocre
Creative ⟷ Uninspired
Interesting ⟷ Entirely ignorable

etc.

Figure 3. Scientific activity displays a very wide range of characteristics.

It should be noted, however, that the theories commanded by a mature science are not necessarily more final and true than those commanded by a young science, though the assured behavior of the practitioners of the mature might lead one to think so. Physics for centuries has been the most mature among the sciences; and physicists periodically lapse into the belief that all the important principles of their subject have been discovered and that what remains is only to fill in the details. That was the case, for instance, circa 1870; but it was followed by disillusionment and then some of the most revolutionary bits of science: radioactivity, revealing that atoms are not stable and indestructible; relativity, altering drastically the notions of time and space and gravitation and motion; quantum phenomena, and the paradoxical view that some things could behave at times like particles but at other times like waves. Some of the most fundamental theoretical principles were replaced. (Yet, it should be noted, the vast majority of *knowledge,* in contrast to *theories,* in physics remained intact. A piece of uranium ore will always cause a photographic plate to become exposed, even through its protective black paper covering, and that tells us something definite about the external world; though the terms in which we try to explain that, connecting it with other phenomena, are always subject to some degree of change. That burning means combining with oxygen and not the release of phlogiston was the central realization of the most significant revolution in chemistry; but an enormous amount of what chemists know about combustion—which substances will burn in the presence of each other—is the same now as it was before that revolution. Thus much of the knowledge of what happens in nature, together with a great deal of the explanation that ties those phenomena together, remains essentially unaffected by apparently revolutionary shifts in theory.)

Again, mature science is not necessarily data rich; desired data are not necessarily acquired easily. To a possibly (but by no means certainly) apocryphal chemist is attributed the statement that he could not be bothered wading through the literature because he could get any answer he wanted more quickly by doing the experiment. Though physics is a more mature science than is chemistry, physicists would not venture that jocularity—though they might well say that they do not look it up because they can calculate it. Experiments in much of chemistry are more readily performed than in most of physics.

Much of chemistry is indeed data rich: feasible experiments can deliver a wealth of results in little time, and the chemical literature brims with data about millions of substances, about their preparation and properties. Consequently, chemistry has developed its idiosyncratic judgment about what and how much a chemist ought to accomplish. On the one hand, chemists publish—and are expected by their peers to publish—more articles than, say, geologists; on the other hand, chemists derogate those who do nothing but "turn the crank"—who use a given technique or instrument to derive data from a succession of different substances or reactions—because that is so easy a thing to do. Again because data are generated in such massive amounts, chemistry was the leader in developing techniques for trying to control the exploding literature, through "abstracts" journals and then computerized on-line abstracts. And again because so much can be done, chemists desperately want to have many co-workers to push their research forward, and much about university departments of chemistry can be understood as flowing from the overriding need to recruit plenty of graduate students, "pairs of hands" to carry out the necessary experiments. Thus there is related the story of X, a certain graduate student in chemistry who had the temerity to leave town with the wife of his supervising professor, Y. And X had the further temerity, a year or so later, to ask Y whether he could return to complete the work for his degree. As Y later said to a colleague, "Of course I agreed. He's first rate, and as you know, good graduate students are hard to come by while wives are a dime a dozen." One would not be quite as ready to believe that story if told about an astronomer or a geologist, for many excellent careers in astronomy and in geology, but few in chemistry, have been made without reliance on the labor of co-workers.

In data-rich fields, theory has a certain down-to-earth quality: speculation is fettered by the ease with which it can be disproved. But in data-poor fields, extremely tenuous chains of speculation are indulged. Thus cosmologists are notorious for theorizing whose equivalent on other subjects would be dismissed as science fiction: they are free, for example, to publish about what things may have been like before the big bang that started everything we know about; astrophysicists and cosmologists accept as conceivable the interpretation that certain observations are of objects producing inconceivable amounts of energy by means of inconceivable processes; those searching for extraterrestrial intelligence have published copiously, in almost total absence of data and in complete absence of any direct data; paleoanthropologists construct whole new charts of human evolution whenever a new fossil is found. By contrast, geologists denied continental drift for decades, supported though it was by fitting coastlines and biogeographical distributions. Geologists are always faced with a complex richness of data that offers continuing challenges even to meaningful categorization, let alone explanation, and so geologists are used to waiting and waiting for explanatory schemes to be developed; there is no hurry for that, explanation is not (yet) so salient a part of geology, and they have plenty of useful and time-consuming things to do without indulging in grandiose theorizing.

Some scientists thus do a lot of speculating, whereas others do virtually none, and there is no warrant to call the one approach scientific and the other not. It is just the case that different aspects of nature yield to investigation at different rates and in different ways, and so scientists come to differ in all manner of things. Whenever a generalization is made about science or about scientists, disregarding thereby the fact that there are so many distinct sorts of science, misconception is promulgated. What is true or fruitful for a field that is mature, data rich, and relatively quantitative (thermodynamics, say) is scientific for that specialty even though it may be entirely inappropriate and therefore unscientific for a field that is young, descriptive, and data poor (some bits of planetary science, say).

Again, though we use the single word "science" for both, *textbook* science is a very far cry from *frontier* science. What is in the texts is reliable. It is relatively uncolored by the personalities of those who originally conceived it. It is generally agreed to by almost all the experts. It is unlikely to need to be altered in the future, and in that unlikely event the alteration will likely be of limited extent. By contrast, science at the frontier is very unreliable: today's discovery often turns out tomorrow to have been an error. Frontier science often bears the stamp of its discoverer's persona; and it is often disputed by other experts. Frontier science and textbook science are about as different from one another as any two things can be, within the bounds that both are guesses about the nature of the real world. Our failure to bear these differences in mind has drastic consequences (as illustrated in chap. 6).

Scientists Are Human

Finally, the common view of science as a unitary, monolithic enterprise fails to recognize how varied are the people who do it. Scientists are supposedly trained to judiciousness, objectivity, patience, and careful experimentation and observation; scant attention has been paid to how the practice of science is influenced by the fact that scientists, like all other human beings, vary in ability, competence, dedication, and honesty.

Indeed, thinking of science as using the scientific method portrays science as an activity that is highly unnatural: human beings are not by nature objective, judicious, disinterested, skeptical; rather, human beings jump to conclusions on flimsy evidence and then defend their beliefs irrationally. The widely held myth of the scientific method is one reason that scientists are often stereotyped as cold, even inhuman. Consider, for instance, what a celebrated humanist educator had to say:

> . . . teaching is an art, not a science. It seems to me very dangerous to apply the . . . methods of science to human beings. . . . a "scientific" relationship between human beings is bound to be inadequate and perhaps distorted. . . . to be orderly in planning . . . and precise in . . . dealing with facts . . . does not make . . . teaching "scientific." . . . A "scientifically" brought up child would be a pitiable monster. A "scientific" marriage would be only a thin and crippled version of a true marriage. A "scientific" friendship would be as cold as a chess problem. . . . Teaching is not like inducing a chemical reaction: it is much more like painting a picture or making a piece of music, or on a lower level like planting a garden or writing a friendly letter. You must throw your heart into it, you must realize that it cannot all be done by formulas."

Thus science, arguably the finest exemplar of human intellectual achievement, is made to appear at best a necessary evil. When science is pictured as so impersonal and ascetic an activity, how to understand that scientists *do* throw their hearts into their work, which also cannot and is not all done by formulas? The myth of the scientific method hinders recognition of the wonderful diversity of the sciences. It makes it impossible to understand the history of science and contemporary scientific activity, and it fosters the stereotype of the cold, inhuman, sometimes evil scientist.

Genesis of the Myth

The false view of science as a unity defined by the unitary scientific method was not perversely adopted holus-bolus in the teeth of the evidence. It is just a naive and now-superseded view that congealed, for quite understandable reasons, during the nineteenth century. Moreover, that it was once a plausible view entails that it can still be made to seem plausible, at least under some circumstances and if one emphasizes some things to the exclusion of others. The classical picture is wrong in nuance perhaps more than through and through. Nevertheless, those errors of nuance have portentous consequences.

The roots of modern science, it is widely agreed, largely lie somewhere and somehow in the seventeenth century in western Europe. Though one can also trace continuity in important respects from earlier times than that into contemporary science, nevertheless pre-seventeenth- and post-seventeenth-century science are unlike one another in a striking way. Galileo and Newton epitomize that revolutionary epoch after which science grew at an exponential pace. Trying to understand what happened and why, it was natural enough to note that the Copernican revolution put the Sun rather than the Earth at the center of things, *where it actually is;* so centuries, even millennia, of careful and increasingly accurate observation had, it would seem, made plain *what the facts are,* and that had displaced preconception (or theory) based on (religious) authority. Galileo looked through the telescope; Newton saw the apple fall. Modern science seemed to have begun when theory—belief—came to be based on evidence, not on tradition or revelation.

And modern science began with simple, quantitative relationships among a small number of entities—bodies or particles, and forces.

Chemistry followed physics—albeit a century or so later—in becoming scientific, when the true theory of burning (substances reacting with oxygen) replaced the false notion of phlogiston, again apparently as the result of careful and quantitative observation in well-chosen crucial experiments. Geology followed, as humanity was forced to accept the evidence of strata, erosion, sedimentation, and so forth. Finally, Darwin brought the realm of living nature within scientific understanding by immensely detailed observation that pointed to a straightforward mechanism whose action is capable of yielding the variety of known biological species. So it was natural to see science by hindsight as a systematic progression toward true knowledge about the world, a progress made possible by inducing knowledge from observation; and this view could be bolstered by citing such people as Francis Bacon, who had explicitly advocated this approach.

One can trace over many centuries the intellectual struggle between, on the one hand, those who thought belief should follow authority, a priori reason, revelation, and the like, and, on the other hand, those who—like Bacon—thought that observation, experience, and evidence should be decisive. By the nineteenth century it seemed reasonable enough to most thinkers to believe that science had made triumphant progress by subordinating theory to evidence, and that the same sort of progress could be made in *any* field or form of knowledge—psychology, say, or mediumistic spiritualism—just so long as the evidence was gathered objectively and the theory based faithfully on it. This was the grand age of science, when it seemed to the leading scholars of humanity that the sure road to understanding all things had finally been discovered in science and its Rosetta stone, the scientific method. This was the time when T. H. Huxley preached what he called "lay sermons" in praise of the Church Scientific, and such accomplished scientists as Sir William Crookes, Sir Oliver Lodge, and Alfred Russel Wallace joined in the Society for Psychical Research to bring spiritual matters equally to understanding by means of scientific study. Social Darwinism flourished, Marx made history into a science, and Freud too based his theory on his evidence as he elucidated the workings of human personality.

By and large, the popular view of science remains much the same as that arrived at a century ago. That science is a powerful and progressive path to certain knowledge has been underscored by the proliferation of technology and of high technology, and especially by the harnessing of atomic energy in the 1940s. The classic description of the scientific method—theory based on and decided by evidence—has, despite its now-obvious inadequacies, been taught to and accepted by successive generations including the present one. But circumstances have made the classical view demonstrably obsolete: for one thing, over the past hundred years science itself has changed in many consequential respects; for another, our understanding of the birth and early development of science has become much more realistic.

Classically, scientists have been seen as dedicated truth seekers rather than as people who happen to hold jobs in science much as they might in, or as an alternative to, retail business, manufacturing, banking, or the law. Classically, they have worked as individuals, not in teams. Classically, they have been seen as curious and knowledgeable about the whole of nature even as they actively have studied only a relatively limited area of it. In times past, all these things were by and large true. But nowadays these things are no longer quite so true, at least not for the vast majority of those who work as astronomers, biologists, chemists, and so forth. Specialists in one field are little better than sheer amateurs in most other parts of science, and many of them are not even particularly interested in other areas than their own. Careerism, conflict of interest, and group-think are about as common now in science as in medicine, say, or in engineering or technology-based corporations. Most scientists now have a job more than they have a vocation.

Classically, the vices of scientists were scientific virtue taken too far, an excessive single-minded zeal in truth seeking: Faust bartered his soul for knowledge; Frankenstein's urge for technical accomplishment was not matched by humane understanding; Gottlieb and Arrowsmith (in Sinclair Lewis's novel *Arrowsmith*) suffered because of their very disinterestedness and naivete about human failings. Nowadays, however, the publicly revealed vices of scientists are seen to be quite the same as those of other people, avarice and dishonesty in particular.

As historians delve deeply into details of the past, it becomes abundantly clear that the classical understanding of scientific activity is far from the whole story—so far, in fact, that it needs more than cosmetic modification. The Copernican revolution was no simple triumph of evidence over preconception, for the simple reason that it could not have been: at the time, there were no decisive technical or computational advantages to Copernicus's approach over the long-standing Ptolemaic one. The Copernican venture was significant in subtler but also more deeply far-reaching ways—for example, in daring to make an individual intellectual choice in the face of long-established authority. For a time and to the benefit of a number of people, resistance to Copernicanism from established religion was avoided by emphasizing that this was only a model, not a statement about what the case might actually be in the real world. Attention to historical detail also reveals that it is not so clear why Galileo was condemned by the Church: if for Copernicanism, why did the condemnation come so late, two decades after Galileo had adopted the heliocentric view? As to Newton, some historians have found difficulty in accommodating the view of Newton as the ultimately objective scientist, drawing theories scrupulously from the evidence, with the fact that most of Newton's thinking time was devoted to alchemical and biblical exegesis rather than to mathematics and mechanics, as well as the fact that he quarreled bitterly with others over questions of scientific priority—not to speak of his use of "fudge factors" to improve the appearance of success of his science.

The corpus of science at any stage always includes only what has, up until then, stood the test of time. We see nothing in it of the trial and error, backing and filling, dismantling and rearranging that actually took place in the past, be that centuries ago or just a few years ago. Only when we read the actual accounts written by early students of nature do we begin to realize how many errors and false starts there were that left no traces in modern scientific texts. One can give excellent, objective, rational grounds now for the science in the textbooks, but that does not mean that it was actually assembled in an impartial, rational, steady manner.

The inadequacy of the classical view is not widely enough known, and much public discussion is still, implicitly if not always explicitly, based on it. Our rate of scientific literacy, as we have seen, is even measured by counting as literate those who hold classically mistaken ideas about the scientific method. Most universities specify that all undergraduates must study science, by which is typically meant a year or so of *any* science—as though that could be a useful sample of what science is, let alone what role it plays in culture and society.

The Epitome of Science

So long as science is viewed as monolithic, founded on the scientific method, it is possible (and therefore irresistibly tempting) to label some sciences, or some bits of science, as "more scientific" than others, according to the degree to which the method has been or can be successfully deployed. Hence, in the classical epitome of science, quantification and mathematics rule the roost, because hypotheses can obviously be framed and tested with real precision only when numbers are used. Immediately, then, biology and geology come to be seen as somehow less scientific than chemistry, which in turn is less scientific than physics, the *wholeheartedly* scientific science. Physics becomes the epitome of science.

That easy judgment is also abetted by historical circumstances. The scientific revolution of the seventeenth century was most dramatic in mechanics and gravitation, and until quite recently most historians of science worked actually in the history of physics. And since the history of science provides the grist for the philosophers of science, they too could become further confirmed in the opinion that physics is the ultimately scientific science. The most publicized scientist of the twentieth century, Albert Einstein, was a physicist. When the Nobel Prizes are disbursed in Stockholm, that for physics comes first. And physics, too, has been given the credit for achieving atomic bombs and nuclear power.

Yet if it is granted, among the many possible definitions of science, that science most fundamentally and undeniably means the study of nature, then it is surely a misconception that physics is the epitome of science: why should one think it more scientific to study waves and electrons than rocks or polar bears? Nor is physics less error prone than other sciences: once-accepted beliefs (theories) have been modified or replaced in physics as in other fields of science—indeed, perhaps more dramatically and unexpectedly than in other sciences. It is incidentally also a misconception that atom bombs and nuclear power stand to the credit of physics: many physicists, it is true, worked at these projects, and Robert Oppenheimer, the scientific director at Los Alamos, had been trained as a physicist; but many chemists, engineers, mathematicians, and others worked on the project, and the basic phenomenon on which all else was founded, the fission of uranium, had been discovered and understood primarily by the chemists Otto Hahn, Ida Noddack, and Fritz Strassmann.

Admittedly, physics is the most fundamental of the sciences in the sense that one explicitly needs some physics to do astronomy or chemistry, just as one needs chemistry to do any of the other sciences. Physics is also in a real sense simpler than the other sciences: it deals with phenomena that lend themselves to description in terms of a very small number of forces and things, so that simple relationships emerge. But that physics is fundamental and simple does not in logic make it the epitome of science (though it does offer plausible reason why human beings should have been able to succeed at it before they could get very far in more complex areas of science).

The misconception that physics is the epitome of science has unfortunate corollaries. In public life, one seeming consequence is that presidential science advisors are almost routinely recruited from among the ranks of physicists—and, moreover, no eyebrows, let alone protesting voices, have been raised over that. In other spheres it would be protested as grossly inequitable, not to say undesirable and potentially deleterious. To sense the latent significance for science policy, consider the analogy of defense policy: imagine that the chairman of the Joint Chiefs of Staff were routinely recruited from the ranks of, say, the army. Immediately everyone would recognize the possible conflict of interest and the subtle ways in which one-sided opinion could become too influential. So too with science, though popular lack of understanding masks the fact. Advice about research budgets and priorities would vary—on average only, of course, and in degree only—if it came from a chemist rather than a physicist or a biologist or a geologist. Moreover, no individual can be predicted to be more or less satisfactorily scientific by virtue of having been trained in one specialty rather than another.

In 1989, in the cause of improved economic forecasting, a conference between economists and physicists was mounted, again marking physics as the epitome of science, as though training in physics equipped one to be insightful and wise about anything and everything. In fact, physics might well be the *worst* possible training, within the options available in science, for advising on matters of social policy. The simplicity of the things with which physics deals easily leads physicists to look for single and simple causes and cures, so that they may oversimplify such things as the extinction of dinosaurs, the design of Star Wars systems for defense against missiles, or politics as a whole. Because physics deals with simple relationships, physicists are trained to be simpleminded. That is illustrated, for instance, by the ease with which accomplished physicists have been fooled by purported psychics and mediums, notably in the latter part of the nineteenth century and again in recent decades.

Because physicists, like almost everybody else, still see physics as the epitome of science, they are likely to make recommendations for other sciences that would be appropriate for physics but not elsewhere. Because of their training and the environment in which they have worked, it must be more difficult for a physicist than for another scientist to question the wisdom of dedicating more than six billion dollars (more recently estimated as sixteen billion) to the Superconducting Super-Collider, the next step toward the Grand Unified Theory. Yet that sum of money, if distributed among the other sciences, would treat them to a quite unprecedented sense of luxury; and I wager with great confidence that if it were put to the vote in a scientific senate with equal representation from each of the sciences, excellent other uses for that money would be found. Because the scientific method is universally thought to be universally applicable, recommendations appropriate to physics come to be not only made but also accepted about defense, education, and no doubt other human activities as well.

From Myth to Ideal

That the scientific method is a myth, that it does not explain the success of science and that scientists in practice do not follow the method, does not mean that the method itself should now be ignored or disparaged. Rather, it should be seen as an ideal—an admittedly unattainable ideal—not as a description of actual practice.

Those who hold ideals, no matter that they are unattainable, are likely to behave more in accord with them than will people who do not hold those ideals. Priests who vow chastity and poverty are likely to be more chaste and poor than people who do not make such vows—even though not every priest (and perhaps not even a single one) will be *entirely* chaste or *completely* poor. Politicians who believe that bribes are evil are less likely to accept them than are politicians who see nothing wrong with the practice. And so on. That human beings cannot by nature be entirely objective does not render objectivity an unworthy ideal: far from it, the ideal of objectivity in the form of disinterestedness, impartiality, or fairness is to be found not only in science but also in many aspects of social life. Those who strive to be objective in science can learn to be a bit more objective than they might otherwise be. Further, though it is well known that expertise does not readily transfer from one field to another, at least some of the people who have learned to be somewhat objective through doing science might be helped thereby to learn to be a little more objective in other matters too—just as some lawyers come to be fairly good judges of evidence and of human nature outside the courtroom as well as in it. There is no good reason to discard the scientific method as an ideal; rather, there is good reason to keep it so. Myths, after all, even if not literally true, are stories that embody moral truths.

That questing the grail is taught as an appropriate ideal does not mean that we should train undiscriminating Don Quixotes. It is important to make plain that the scientific method is an ideal, not actual practice. So long as the belief is widespread that science veritably follows the method, the belief will also be widespread, notably among scientists themselves, that *scientists* normally follow the method—if not perfectly, at least well enough. How then will the scientific community react when one of their number is found to have done some serious cheating? By regarding it as an individual aberration, almost certainly resulting from mental imbalance of some sort, an act that can have no significance for science as a whole, because scientists understand that they can make careers only by doing *objectively* sound work, for that is how they are judged, and anyone who tries to cut corners must have a screw loose. Through believing that the impersonal scientific method is actually in practice, scientists are thus kept from the realization that the progress of science—the progress, not the ultimate substantive content—is profoundly affected by the way in which scientists, as a community but also as individuals, behave. The greater the approach to objectivity, the better will peer review work; the greater the degree of honesty, the better will the whole system work; and, by contrast, the more corners are cut, the more students will imbibe the notion that corners are there to be cut and that the race goes to the swiftest, not to the best or the most sure. The myth of the scientific method keeps the scientific community from recognizing that they must have a humanly developed and enforced professional ethics because there is no impersonal method out there that automatically keeps science the way it ought to be.

The unqualified myth encourages hubris. One learns that science is objective. One learns that scientists are trained to be objective and to be skillful in use of the scientific method. Naturally, then, society learns to admire *scientists* as much as science itself, as people who are able to be objective; and scientists themselves, of course, are not immune to that chain of inference. So they may be led to think of themselves as more able to be impartial and free from conflict of interest than other people, like the Nobelist who was queried about the propriety of holding a university position while directing a commercially funded research institute: "I certainly do that to other people; I look at where they would benefit from a position and wonder whether what they're saying is a totally independent judgment. I think people are entitled to ask that of me. But I do think the statements and decisions I make come from the highest sense of integrity."

Instances are common enough in which successful scientists succumb to the temptation to see themselves as authorities not only in their own tiny field but over science as a whole and even beyond that; why not, after all, if application of the scientific method would make things so much better in all spheres of human activity? And because the public and the media also believe the myth of the method, great scientists are apt to be accepted as universal gurus (see chap. 4).

The myth of the scientific method, then, encourages the laity to have an unrealistic view of scientists and therefore also to have unrealistic expectations of them and of science; and it encourages scientists themselves to be unrealistic about themselves and about science, and to neglect the importance of cultivating consciously ethical behavior. It

leads the scientific community to assume that its public credibility is permanent and quite automatically guaranteed—which makes it shocking and inexplicable when the credibility of science is brought into question, as in recent years under such well-publicized instances of misconduct as that of Thereza Imanishi-Kari, which was featured in congressional hearings and brought unwelcome notoriety also to her coauthors—notably Nobelist David Baltimore.