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$$(k/\pi)\, AF\, (p - t) = kr^2F\, (p - k'r/\sqrt{2})$$

where k and k' are constants of proportionality, $A = \pi r^2$ is territory area, F is food density, p is the proportion of budget that goes for feeding the young, and t is traveling time. When the species become interspecifically territorial, N_2 is given by $kr^2F(1 + \alpha)(p - k'r/2)/2$. Thus interspecific territoriality is profitable when $N_2/N_1 > 1$, or when $1 < [K - (r/2)]\,(1 + \alpha)/2[K - (r/\sqrt{2})]$, where K is p/k'.
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Should the History of Science Be Rated X?

The way scientists behave (according to historians) might not be a good model for students.

Stephen G. Brush

An editorial in the Washington *Post*, bemoaning double-talk from both sides during the last presidential election campaign, suggested that public reporting of the campaign, being harmful to the ideals of young readers, might be a proper target for censorship (*1*):

> It is time to consider whether this campaign ought not to be rated X for children, on the grounds that young and inexperienced minds might form the impression that our national politics is mainly composed of hypocrisy and cynicism. Adults know that to be wrong, of course, but there is not much in the current campaign by which to prove it.

Such proposals are equally appropriate to a variety of subjects similarly remote from the realm of sex, which the term "X-rated" connotes (*2*). My concern in this article is with the possible dangers of using the history of science in science education. I will examine arguments that young and impressionable students at the start of a scientific career should be shielded from the writings of contemporary science historians for reasons similar to the one mentioned above—namely, that these writings do violence to the professional ideal and public image of scientists as rational, open-minded investigators, proceeding methodically, grounded incontrovertibly in the outcome of controlled experiments, and seeking objectively for the truth, let the chips fall where they may (*3*).

As is customary, "science" will be identified primarily with physics and early astronomy; these subjects usually furnish the successful examples of the scientific approach to be emulated in other fields.

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The Conventional Description of Scientific Behavior

The introduction of historical materials into science courses is often motivated by the desire to give the future scientist not only facts and technical skills, but also the correct attitude or general methodology. His teachers want him to respect the standards of impartiality, logical rigor, and experimental verification of hypotheses and to refrain from excessive theorizing about new or unexplained phenomena on the basis of metaphysical, mystical, or theological preconceptions. As the philosophers of science put it, he should be able to distinguish between the "context of discovery" and the "context of justification"—scientific hypotheses may come in an undisciplined way from the creative mind, but they must ultimately face the test of comparison with experiment and observation (*4*).

Science textbooks generally place a strong emphasis on the experimental character of science. As Charles Kittel and his colleagues say in *The Berkeley Physics Course* (*5*, p. 4):

> Through experimental science we have been able to learn all these facts about the natural world, triumphing over darkness and ignorance to classify the stars and to estimate their masses, composition, distances, and velocities; to classify living species and to unravel their genetic relations. . . . These great accomplishments of experimental science were achieved by men of many types. . . . Most of these men had in common only a few things: they were honest and actually made the observations they recorded, and they published the results of their work in a form permitting others to duplicate the experiment or observation.

Obviously any historical materials that might raise doubts in the students' minds as to whether their heroes—men like Galileo Galilei, John Dalton, or Gregor Mendel—really did follow these precepts would undermine the purpose of the course and would therefore not be appropriate.

Another virtue often mentioned in textbooks is skepticism about established dogma. The scientist must be brave enough to question and criticize anything his teachers or his society may tell him, at the risk of ostracism, denial of financial support, or worse. Only in this way can a scientist hope to make a positive contribution to his subject. Obviously, if an historian of science were to suggest that most scientists, most of the time, are simply working out routine problems according to agreed procedures, his help would not be welcome in teaching science. There is only one established dogma in science—that scientists do not blindly accept established dogma (*6*).

Nevertheless the emphasis on teaching "scientific method" is not very noticeable nowadays in courses for the science major, especially in physics courses. Statements by such physicists as P. W. Bridgman, that there is no such thing as scientific method (*7*), have apparently had some impact, and the usual approach is to assume that the student will absorb the correct attitude as he learns the subject matter and works in the laboratory. But in courses for the nonscience major, in which there is not much subject matter and perhaps no laboratory work at all, the discussion of methodology is much more explicit (*8*). Indeed, many educators feel that the only justification for requiring all students to take a science course is to show them how legitimate (that is, physical and biological) scientists work, in order that they may learn a method they can apply in their own disciplines in the social sciences and humanities. Thus Daniel Bell writes that "as part of a general education all students should be aware of the nature of . . . hypothetical-deductive thought" as it has been developed in science (*9*, p. 248). Bell, a sociologist, quotes biophysicist John Platt's statement that some fields in science move more rapidly than others, in part because "a particular method of doing scientific research is systematically used and taught" (*10*, p. 347). This method relies on "devising alternative hypotheses for any problem, devising crucial experiments, each of which would, as nearly as possible, exclude one or more hypotheses" (*9*, p. 251). The procedure is said to have been particularly effective in the Watson-Crick determination of the structure of DNA and in more recent research in molecular biology, as well as in physics.

History in Science Teaching

The history of science has always exerted a strong attraction on some scientists, and they have often advocated the historical approach in teaching science. For some, this means no more than an extension of the usual technique of "searching the literature" pertaining to a research topic. In extreme cases, this can lead to the publication of series of annotated sources or of gigantic bibliographies (*11*). Such projects, initiated by scientists who maintain that they have primarily in mind the needs of other scientists and students, often turn out to be of considerable value to historians of science, in spite of occasional snide remarks that the publications are "not really history."

One occasionally encounters claims that new discoveries have been inspired by reading about much earlier work that was not successful at the time and hence did not make its way into the textbooks (*12*). Ernst Mach, a physicist who devoted considerable time to historical studies, wrote (*13*):

> They that know the entire course of the development of science, will, as a matter of course, judge more freely and more correctly of the significance of any present scientific movement than they, who, limited in their views to the age in which their own lives have been spent, contemplate merely the momentary trend that the course of intellectual events takes at the present moment.

In spite of such statements, most science teachers have not been willing to make more than a superficial use of history in training scientists. (Perhaps they realize that Mach's own faulty judgment of physical theories in the early decades of the 20th century is an eloquent refutation of his statement.) They apparently agree with J. B. Conant's argument that, while knowledge of the history of science may help a scientist to function better outside the laboratory, it has nothing to teach him about the methods of research he will need in order to make new discoveries (*14*). F. S. Allen, an historian who has surveyed the changing opinions of scientists on this subject, concludes that "since the 1950's, most scientists have not viewed a history of science course as a legitimate subject in the curriculum" (*15*, p. 270).

It has even been argued that historical readings would actually be harmful to a science student. As Thomas S. Kuhn has pointed out, in the great classics of science the student "might discover other ways of regarding the problems discussed in his textbook, but . . . he would also meet problems, concepts, and standards of solution that his future profession has long since discarded and replaced" (*16*, p. 344). Thus he might be led to waste his time doing work that would not be acceptable for publication in scientific journals. (This seems to be the fate of many bright people who try to break into a scientific discipline from the "outside," without having gone through the orthodox training process.)

Conant and other influential science educators have nevertheless strongly urged the use of history in science courses designed for nonscience majors, and many such courses and textbooks have met with some success (*17*). Moreover, in recent years the publication of some exceptionally penetrating articles and books on 20th-century physics has caused many teachers to conclude that such historical studies might indeed challenge their brightest students. The debates of Albert Einstein, Niels Bohr, Erwin Schrödinger, Werner Heisenberg, Paul Ehrenfest, and others on the fundamental problems of relativity and quantum mechanics have attracted renewed attention, especially now that these theories are no longer regarded as beyond criticism.

One symptom of the revival of interest in this subject is the International Working Seminar on the Role of the History of Physics Education, organized by Allen King and held at the Massachusetts Institute of Technology in July 1970, under the sponsorship of the Commission on Physics Education of the International Union of Pure and Applied Physics (*18*). Supposedly, the participants were all agreed that history of science is useful in teaching, and the purpose of the seminar was to organize concrete steps toward collecting and preparing materials, guidelines, and so forth for teachers who might like to use history but know very little about it. The participants made considerable progress along these lines, and the proceedings of the seminar have now been published.

The proceedings include discussions and recommendations; listings of books, films, translations of classic papers; and so forth (*19*). But some doubts were raised at this meeting about whether history and science teaching really go together as well as many participants assumed. Martin Klein played the devil's advocate by pointing out that history and science are inherently different kinds of disciplines; bringing them together is likely to do violence to one or the other. The scientist wants to get at the essence of a phenomenon, and to do so he must strip away all complicating features or contingencies peculiar to time, place, and the personality of the observer. Yet for the historian those are the essence of history; if the detail of past events were to be eliminated, nothing significant would be left. Again, when the science teacher introduces historical materials, he must do so in a very selective way, since his real purpose should be to teach modern theories and techniques more effectively; he can only take from the past that which seems to have significance in the present. The result may be a series of fascinating (and often mythical) anecdotes, but it is surely not history as the historian understands it.

Subversive Aspects of the History of Science

It can be argued that the historical approach, while it may distract students by loading a course with superfluous information, does give the instructor an opportunity to discuss conceptual problems that are often overlooked in conventional teaching (*20*). Yet the science teacher may be justified in following his instincts to ignore history, especially if his purpose is to train scientists who will follow the currently approved research methods.

By "history" I mean simply the most accurate or authoritative account that an historian of science can give of the way a discovery was made, a theory was developed and accepted or rejected by scientists, and the mutual influences of research in different areas of science or of science and other kinds of human activity. History is not merely an unchanging record of facts, but also the interpretations proposed by each new generation of historians.

Many people still believe [with George Sarton, who called Auguste Comte the founder of history of science (*21*)] that the purpose of the historian is to record the process of cumulation of positive knowledge, not forgetting that there have been errors and confusion along the way, but also not forgetting to identify them clearly as such. (Sarton also specifically stated that what may appear as error to one generation of historians might be seen as neglected truth by the next.) A modern historian of science, Charles C. Gillispie, has eloquently argued the thesis that science progresses by using "objectivity" to separate truth from error (*22*). Sarton and Gillispie may be taken as representatives of the traditional view, which is probably still held by a majority of scientists.

In recent years, some historians of science have been moving toward another conception of their role, based on the notion that scientists often operate in a subjective way and that experimental verification is of secondary importance compared to philosophical arguments, at least in some of the major conceptual changes that have occurred in science. If this notion is correct, then the historian must do more than document the application of objectivity to scientific problems. He must be prepared to analyze the philosophical, psychological, and sociological aspects of scientific work, to explain how certain problems came to be considered "scientific" and how particular standards happened to be accepted for evaluating solutions to those problems. He may also have to account for scientific change in terms other than those of linear progress from error toward truth.

This reorientation of the historical interpretation of science is usually considered to have begun with the publication of Alexandre Koyré's Galilean studies in the 1930's (*23*). Here one finds Galileo described not as the first modern experimental physicist, but as a Platonist who helped to replace the Aristotelian world view by a mechanistic one, employing primarily the tools of logical argument, rhetorical persuasion, and mathematical deduction. From the narrowly empirical viewpoint, Galileo could hardly be said to have made any positive contributions to knowledge (apart from his telescopic observations)—his theories did not even explain everyday experience as well as those of the Aristotelians. But Koyré argues that (*24*):

> . . . [W]hat the founders of modern science, among them Galileo, had to do, was not to criticize and combat certain faulty theories, [but] to correct or to replace them by better ones. They had to do something quite different. They had to destroy one world and to replace it by another. They had to reshape the framework of our intellect itself, to restate and reform its concepts, to evolve a new approach to Being, a new concept of knowledge, a new concept of science —and even to replace a pretty natural approach, that of common sense, by another which is not natural at all.

This picture of Galileo was already beginning to infiltrate textbooks two decades ago, for I have taken this quotation from the first edition of Gerald Holton's *Introduction to Concepts and Theories in Physical Science*, published in 1952. In this book, Holton also pointed out that the supposedly objective "facts" with which the scientist deals are useless without some interpretation, and the latter is inevitably linked to theory and to metaphysical preconceptions. These sentiments were not new, but they had rarely been expressed so forcefully in a science textbook.

Koyré's interpretation of Galileo was not universally accepted, but it did acquire considerable support among other historians of science. For example, A. Rupert Hall wrote in 1964 of the battle between the geocentrists and heliocentrists in the 17th century (*25*, p. 71):

> There was no proof for either side: there was only the choice between *this* way of regarding things which belonged to antiquity, and *that* which belonged to Galileo and modern science . . . the number of instances of his offering a precise piece of experiment in support of his notions is small indeed. Even the positive assertions of experimental verification made by Galileo have been doubted. . . . Sufficient-reason considerations and mathematical arguments were of greater effect in winning support for the law of falling bodies than any experiments. . . . Galileo is above all aware that the senses must be educated and assisted to perceive realities. Thus one could *know* the true nature of the moon without the telescope: that instrument simply makes reality easier to discover. . . . A Platonist, a Copernican, a mechanical philosopher could not possibly be a naive empiricist; and it was hardly more possible for him to be, systematically, a follower of the hypothetico-deductive logic.

By 1970, such views had become so widespread that philosopher of science Paul Feyerabend could make the following statement with scarcely any documentation (*26*, pp. 64–65):

> The reader will realize that a more detailed study of historical phenomena like these may create considerable difficulties for the view that the transition from the

pre-Copernican cosmology to Galileo consisted in the replacement of a refuted theory by a more general conjecture which explains the refuting instances, makes new predictions, and is corroborated by the observations carried out to test these new predictions. And he will perhaps see the merits of a different view which asserts that while the pre-Copernican astronomy was in trouble (was confronted by a series of refuting instances), the Copernican theory was in even greater trouble (was confronted by even more drastic refuting instances); but that being in harmony with still further inadequate theories it gained strength, and was retained, the refutations being made ineffective by *ad hoc* hypotheses and clever techniques of persuasion.

Other examples of historians' debunking of the discoveries of Nicolaus Copernicus, Galileo, Antoine Laurent Lavoisier, Dalton, Mendel, and Robert A. Millikan might be cited, but Richard Westfall's article on Isaac Newton (*27*) should suffice: "If the *Principia* established the quantitative pattern of modern science, it equally suggested a less sublime truth—that no one can manipulate the fudge factor quite so effectively as the master mathematician himself" (*27*, p. 752). Westfall, being quite familiar with Newton's work, had no difficulty in shooting down the suggestion that Newton's ploys could be considered acceptable scientific procedure (*28*).

Meanwhile, the suggestion that scientific change may result primarily from theoretical arguments or subjective factors was being generalized into a new description of scientific revolutions by Kuhn (*29*). Kuhn's scheme undermines conventional ideas of scientific behavior in two ways. First, he argues that the (proper) function of scientific education is *not* to produce skeptics who will continually challenge existing dogma, but rather to train highly competent "puzzle-solvers" who will be content to work within the agreed framework of rules and theories—the current "paradigm" governing "normal science" (*30*, p. 341). Second, he describes revolutions as changes from one paradigm to another by a process that is more like a "conversion experience" than a reasoned debate based on objective evidence (*29*, p. 151). Since the paradigm includes not only a theory, but also a set of criteria for determining what problems are worth solving and how one recognizes a solution when he has it, there may not be any mutually agreed basis for determining whether the new paradigm is better than the old. Thus, successive paradigms tend to be "incommensurable," and doubt is cast on the cherished idea that science makes cumulative progress. Moreover, according to Kuhn, scientists do not test theories by a hypothetico-deductive process at all: "Once it has achieved the status of a paradigm, a scientific theory is declared invalid only if an alternative candidate is available to take its place" (*29*, p. 77), and then the choice is made on at least partially subjective grounds. Although the revolution may be initiated by the failure of the old paradigm to account for some crucial piece of experimental data or observation, its outcome may be a new paradigm that fails to account for other data or observations that were explained quite well by the old paradigm. (For example, the failure to observe stellar parallax in the 17th century was a strong argument against the heliocentric theory and a confirmation of the geocentric theory.)

There is no need to review here the controversy generated by Kuhn's thesis (*31*); I will mention only one publication to illustrate the considerable discomfort it has caused those concerned with science education and their failure (in my opinion) to come to grips with the basic problems it raises. Israel Scheffler, professor of education and philosophy at Harvard University, views with alarm the tendency I have just been describing (*32*):

> That the ideal of objectivity has been fundamental to science is beyond question. The philosophical task is to assess and interpret this ideal: to ask how, if at all, objectivity is possible. This task is especially urgent now, when received opinions as to the sources of objectivity in science are increasingly under attack. The notion of a fixed observational given, of a constant descriptive language, of a shared methodology of investigation, of a rational community advancing its knowledge of the real world—all have been subjected to severe and mounting criticism from a variety of directions.
>
> The overall tendency of such criticism has been to call into question the very conception of scientific thought as a responsible enterprise of reasonable men. The extreme alternative that threatens is the view that theory is not controlled by data, but that data are manufactured by theory; that rival hypotheses cannot be rationally evaluated, there being no neutral court of observational appeal nor any shared stock of meanings; that scientific change is a product not of evidential appraisal and logical judgment, but of intuition, persuasion, and conversion; that reality does not constrain the thought of the scientist but is rather itself a projection of that thought. Unless the concept of responsible scientific endeavour is to be given up as a huge illusion, the challenge of this alternative must, clearly, be met. . . .

What Scheffler characterized as the "extreme alternative" is really not an unfair description of the conclusions that have emerged from some recent historical studies. In addition to the reinterpretation of Galileo's work mentioned above, it has been alleged that Dalton and Mendel "cooked" the supposedly experimental data they presented in support of their theories of chemical atomism and heredity, respectively (*33*). Einstein refused to let experimental "facts" shake his belief in the validity of relativity theory, and in this he was supported by H. A. Lorentz (*34*). Paul Dirac stated that a theorist should prefer beautiful equations to uglier ones that yield closer agreement with experimental data (*35*), and Max Planck stated that new theories rarely get accepted by rational persuasion of the opponents—one simply has to wait until the opponents die out (*36*).

Einstein identified the fallacy in assuming that scientific theories are tested by observations when, in 1926, he replied to Heisenberg's statement that only observable magnitudes must go into a theory. After all, asked Heisenberg, didn't you stress this requirement in formulating the theory of relativity? Einstein replied, according to Heisenberg (*37*, p. 63):

> Possibly I did use this kind of reasoning, but it is nonsense all the same. Perhaps I could put it more diplomatically by saying that it may be heuristically useful to keep in mind what one has observed. But on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe.

A year later, when he formulated his indeterminacy principle, Heisenberg recalled Einstein's assertion that "It is the theory which decides what we can observe" and realized that, once that principle had been deduced from quantum mechanics, it was no longer vulnerable to experimental disproof. The processes involved in an experiment or observation must themselves satisfy the laws of quantum mechanics, hence "experiments are unlikely to produce situations that do not accord with quantum mechanics" (*37*, p. 78).

Scheffler argues that scientists simply should not behave this way, and he proposes an alternative philosophy of objectivity. But when the scientists who have laid the foundations of modern physics confess that their theory is not controlled by data, the prospects for enforcing objectivity on the scientific community are substantially diminished (*38*).

Theory and Experiment: Additional Historical Examples

I now turn to three examples of scientific behavior which I have uncovered in my own research in the history of science. In the first case, an apparently well-established theory of the nature of heat was rejected, not because of any new experimental evidence or theoretical calculations pertaining directly to heat, but because of new experimental and theoretical work in optics. In the second case, a theory of the nature of gases was clearly refuted by experimental tests (according to at least one proponent of the theory) but was accepted anyway. In the third case, a particular interatomic force law, derived by fitting experimental data, was abandoned in favor of another force law, which was in worse agreement with the data, primarily because of theoretical calculations. While these decisions were made in the first instance by individual scientists, their colleagues did not protest the irrationality of the decisions but simply followed the leader; hence these cases provide legitimate evidence for the behavior of the scientific community. Moreover, they do not involve major conceptual revolutions such as those mentioned earlier; they pertain, rather, to the less spectacular kind of theory change that is more typical of ordinary scientific activity.

The wave theory of heat (*39, 40*). Any physics textbook will report that the equivalence of heat and mechanical energy was established by the experiments of James Prescott Joule in the middle of the 19th century. Some will also state that Joule thereby overthrew an earlier theory, the "caloric theory," which identified heat as a substance rather than a form of energy. Here, it might seem, is a classic historical illustration of the essential role of quantitative experimentation in exposing fallacious theories, an example eminently suitable for pedagogical purposes.

Unfortunately for the physics teacher, this traditional account of the origin of thermodynamics leaves out one very important fact. At the time Joule did his work, in the 1840's, the caloric theory had already been abandoned by most physicists. The reason was not the earlier experiments of Count Rumford and Humphry Davy (circa 1800), showing that heat can be generated by friction; those experiments were indeed well known, but they failed to shake the faith of early 19th-century scientists in the materiality of heat. Instead, the crucial experiments were those on radiant heat and light. The investigations of William Herschel, Macedonio Melloni, and others showed that radiant heat has all the qualitative properties of light: it can be reflected, refracted, even polarized. Thus, by the 1820's, it was generally accepted that whatever theory might be adopted to explain the physical nature of light, a similar theory must be adopted for heat. I say "heat," not just "*radiant* heat," for at that time the modern distinction between radiant and other kinds of heat was not recognized. In fact, many theorists believed that all heat is essentially radiant.

Before 1820, the conviction that heat and light are essentially the same kind of phenomenon was favorable to the caloric theory of heat, since light was assumed to be composed of material particles rather than of the motions of particles. But when Augustin Fresnel succeeded in establishing the wave theory of light, by a combination of brilliant theoretical and experimental arguments, physicists were quick to draw the obvious conclusion that heat must also have a wave nature—that is, it must consist of vibrations of the same ethereal medium that was then considered responsible for the propagation of light.

The transition from the caloric to the wave theory of heat took place gradually, but inevitably, in the 1830's. Most of the phenomena that had been explained in terms of caloric could be explained in terms of *vibrations* of caloric, or ether, as it had come to be called. The theory was enunciated in its most explicit form by André-Marie Ampère. He did not try to show that the new conception of heat had any marked advantages over the old one in explaining thermal phenomena. The change was motivated solely by the desire for a unified theory of both heat and light. It is easy to demonstrate the widespread acceptance of the wave theory of heat by examining almost any physics textbook or encyclopedia article on heat published between 1830 and 1845 (*41*).

Supporters of the wave theory of heat insisted at first that their hypothesis was distinct from the older ideas of heat as atomic motion, since the "waves" involved only the vibrations of the ether. But once the association of heat with motion had been revived and made respectable, it was not long before the further assumption emerged that ether vibrations could induce or interact with atomic vibrations. There is some evidence that it was just this line of reasoning that led Sadi Carnot, C. F. Mohr, W. R. Grove, Hermann von Helmholtz, W. J. M. Rankine, and J. J. Waterston to conclude that heat can be described as a form of mechanical energy.

The kinetic theory of gases (*42*). By the middle of the 1850's, the adoption of the principles of energy conservation and thermodynamics had created a strong presumption in favor of the ancient idea that heat is simply the energy of motion of atoms in a vacuum. This motion would be especially simple in a gas, if one could assume that the atoms (or molecules) moved in straight lines at a constant speed until they encountered other atoms or the sides of a container. Of course this assumption required that the wave theory of heat be consigned to oblivion, or at least limited to radiant heat phenomena, so as not to complicate the description of atomic motion with ether-drag corrections. Yet it was not at all obvious that the ether could be legitimately ignored, so the early kinetic theorists—Waterston, Rudolf Clausius, and James Clerk Maxwell—hedged their bets by treating the kinetic theory as a hypothesis that might be refuted.

It was in this cautious spirit that Maxwell wrote his first paper on the kinetic theory of gases (*43*). His caution was justified, for he discovered that two deductions from the theory were in conflict with known experimental facts about gases. First, there was the remarkable theoretical prediction that the viscosity of a gas of elastic spheres is independent of density and increases with temperature. Maxwell wrote to G. G. Stokes, who had done considerable work in hydrodynamics, to ask about experimental evidence on this point and learned that the only data known at the time (an observation Edward Sabine made in 1829) suggested that the viscosity of air does vary with density. This nonconfirming instance was duly noted by Maxwell in his paper.

The second difficulty of the kinetic theory was the disagreement of theoretical and experimental values of the ratio of specific heats. In this case, there was no doubt that the kinetic theory was refuted; as Maxwell told the British Association for the Advancement of Science in 1860, "This result of the dynamical theory, being at variance with experiment, overturns the whole hypothesis, however satisfactory the other results may be" (*44*, p. 15).

Fortunately for the progress of physics, Maxwell did not take seriously such a naive version of the hypothetico-deductive method. Instead, he continued to develop the kinetic theory and inspired others to follow him. He also conducted a series of experiments on gas viscosity himself and found that viscosity does not change appreciably over quite a large range of densities. This result was confirmed by several other experimenters and became one of the strongest arguments in favor of the validity of the kinetic theory. Stokes later admitted that the analysis of the Sabine experiment had implicitly involved the assumption that the viscosity of air vanishes at low densities; this assumption is so natural that it might have survived indefinitely had it not been for Maxwell's theory. In this case the theory refuted the experiment.

The other discrepancy was not so easily eliminated, and Maxwell refused to accept the artificial models of diatomic molecules proposed by Ludwig Boltzmann and others to explain the experimental specific heat ratios. Yet he would not abandon an otherwise plausible and successful theory simply because it failed to account for *all* the experimental facts. Those scientists who did suggest that the theory be abandoned, later in the 19th century, did so not because of this difficulty, but because of more deep-seated philosophical objections. For those who believed in a positivist methodology, *any* theory based on invisible and undetectable atoms was unacceptable, regardless of how well its predictions had been confirmed by experiments (*45*).

Interatomic forces (*46*). The specific heats discrepancy and much else was cleared up by the development of quantum mechanics in the first part of the 20th century. During the same period, elaborate techniques for solving the Maxwell-Boltzmann equations of kinetic theory were worked out by David Enskog and Sydney Chapman (*47*). These techniques made it possible to compute the gas transport coefficients (viscosity, heat conduction, and diffusion) for any of a large class of hypothetical interparticle force laws. It was thus natural to suppose that by detailed comparison of experimental and theoretical coefficients one could determine the true law of force, at least for the simplest case of spherically symmetric rare gas atoms. A comprehensive program along these lines was undertaken by J. E. Lennard-Jones, beginning in 1924.

Lennard-Jones proposed to represent the interatomic force law by a function of the general form

$$F(r) = -ar^{-n} + br^{-m}$$

where negative values of F correspond to attractive forces and positive values to repulsive forces. It was expected that the exponent m would be greater than n, corresponding to a combination of a short-range repulsive and a long-range attractive force. By combining data on gas viscosity and virial coefficients with some information on the crystalline lattice spacing for the solid state, Lennard-Jones was able to show that n should be about 5 for helium, argon, and neon. Values of the index m varied from 9 to 21 for these gases, depending on the relative weights given to gas and crystal data.

The conclusion that long-range attractive forces between neutral rare-gas atoms vary inversely as the fifth power of the distance was no sooner established by experiment than it was overturned by theory. Quantum-mechanical calculations by S. C. Wang, J. C. Slater, R. Eisenschitz, and F. London led to the conclusion that this force should vary inversely as the seventh power of the distance. Accordingly, Lennard-Jones proposed in 1931 that his indices should be assigned the values $n = 7$ and $m = 13$. The latter value was not based on quantum mechanics at all, but simply on the fact that it is easier to do certain theoretical calculations in gas theory if m^{-1} is just twice n^{-1}. (Specialists will know that n^{-1} and m^{-1} are the exponents of $1/r$ in the corresponding potential energy function; this is the origin of the "6, 12" potential.) In fact, quantum mechanics leads to a different functional form, e^{-kr}, for the repulsive component of the force law.

Another surprising aspect of this story is that the Lennard-Jones force law (with $n = 7$ and $m = 13$) continued to be described and used as the "most realistic" function in many works on statistical mechanics for the next 30 years, despite the absence of any experimental basis for this claim. Within the last decade, it has finally been realized that neither this nor any other simple function can give an accurate representation of the interatomic force law in a manner satisfactory to both theorists and experimentalists.

The conclusion I would draw from these examples is not that experiments are unimportant in the choice of theories, but that direct experimental tests of hypotheses are often given less weight than the conformity of the hypothesis with a general theoretical superstructure or with more prestigious theories in related branches of science. One might still argue that the superstructures and prestigious theories are themselves established by objective experimental tests—but that is precisely what many historians of science now deny.

The Science Teacher as Whig Historian

The problem of objectivity is closely associated with another issue now being debated by historians of science—the so-called Whig interpretation of history. This phrase was introduced about 40 years ago by historian Herbert Butterfield to characterize the habit of some English constitutional historians to see their subject as a progressive broadening of human rights, in which good "forward-looking" liberals were continually struggling with bad, "backward-looking" conservatives (*48*). In the last few years, historians of science have applied the term to the accounts of scientific progress that tended to judge every scientist by the extent of his contribution toward the establishment of modern theories. Such an interpretation looks at the past in terms of present ideas and values, rather than trying to understand the complete context of problems and preconceptions with which the earlier scientist himself had to work (*49*).

My favorite enunciation of the Whig attitude in the history of science is the one found in the Marquis de Laplace's *Mécanique Celeste*: "When we have at length ascertained the true cause of any phenomenon, it is an object of curiosity to look back, and see how near the hypotheses that have been framed to explain it approach towards the truth" (*50*).

One might say that Whig history is precisely what the science teacher wants—he is interested only in those earlier developments that led up to today's established theories and laws. And, just as the Whig historian assumes that anyone who opposed a liberal reform must have been motivated by selfish interests rather than concern for human rights, so the science teacher assumes that anyone who fails to move toward modern (that is, correct) ideas when the path has been pointed out to him must be acting nonobjectively—he has not accepted the true scientific method.

The rejection of Whig history is made quite explicit in writings such as H. F. Kearney's recent book on the scientific revolution of the 16th and 17th centuries (*51*). Kearney describes, not a progressive change from primitive to modern theories, a replacement of error and confusion by truth and clarity, but a complex interaction between three traditions, or paradigms: the "organic," the "magical," and the "mechanical." [These correspond to what are sometimes called Aristotelian, Hermetic, and Newtonian viewpoints, except that Newton himself may have been influenced by the magical tradition, as were Copernicus, Johannes Kepler, Giordano Bruno, and William Gilbert, according to some historians (*52*).] In their enthusiasm for relating scientific theories to the philosophical and cultural movements of earlier centuries, historians of science have begun to de-emphasize the technical content of those theories that makes them significant in modern science. The result is a widening gap between the goals of the historian and of the science teacher.

Spare the Objectivity and Spoil the Student?

I do not want to give the impression that subjectivism has been generally accepted by historians of science (there are too many outstanding counterexamples) or that future historians can never return to the old notion that scientists are governed by objective standards. My point is that, if science teachers want to use the history of science, and if they want to obtain their information and interpretations from contemporary writings by historians of science rather than from the myths and anecdotes handed down from one generation of textbook writers to the next, they cannot avoid being influenced by the kind of skepticism about objectivity which is now so widespread. They will find it hard to resist the arguments of the historians, especially if they bother to check their original sources. [Once it has been pointed out that in Galileo's statement, "I have discovered by experiment some properties of [motion]," the words "by experiment" were added in an English translation and do not appear in the original Italian version, it is hard to maintain the traditional faith in Galileo's empiricism (*53*). Of course, this kind of historical debunking can go too far; Koyré's suggestion that many of Galileo's experiments were imaginary, because he could not possibly have obtained the results he reported, has been refuted in several cases by Thomas Settle, Stillman Drake, and James MacLachlen (*54*).]

I do not know how science teachers are going to respond to the new historical interpretations. So far, most teachers seem to have ignored them. One way of dealing with unorthodox but occasionally successful behavior is to argue that it is permissible only for those scientists whose intuition is good enough to lead them to the right answer, regardless of the experimental evidence. Thus, the authors of *The Berkeley Physics Course* quote Dirac's statement (*35*, p. 47):

> It seems that if one is working from the point of view of getting beauty in one's equations, and if one has a really sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one's work and experiment, one should not allow oneself to be too discouraged, because the discrepancy may well be due to minor features that are not properly taken into account and that will get cleared up with further developments of the theory.

Galileo would have applauded such a statement, but the authors of the textbook add a caution to the physics student: " . . . most physicists feel the real world is too subtle for such bold attacks except by the greatest minds of the time, such as Einstein or Dirac or a dozen others. In the hands of a thousand others this approach has been limited by the inadequate distribution among men of "a sound insight" (*5*, p. 6). Thus the student is urged to assume realistically that he is not going to be an Einstein or Dirac, but merely another soldier in the ranks, who must learn the established rules for puzzle-solving within the framework of the current paradigm. His systematic labors will lead to the cumulative growth of normal science and may even, if he is lucky, uncover an anomaly that could be seized on by a rare genius to initiate a scientific revolution. But the good soldier should go no further since he will not know how to find or establish a new paradigm.

By adopting this approach, one implies that there are two kinds of scientists: the average scientist, who must obey the rules, and the genius, who will know when to break them. This may indeed be a realistic description of the scientific community, but I wonder what would happen to the morale of this community if such a description were taught to students. Is the occasional Galileo or Einstein to be considered an expectant father who will not get a ticket if he races the stork to the hospital at 100 miles per hour? Or a millionaire who escapes paying income taxes? And one must not forget that experimental results have also been twisted to support false doctrines, such as the caloric theory of heat, by first-rate scientists who apparently thought they had a license to give priority to their own insights rather than to the data (*55*, p. 140).

On the basis of the examples I have studied, I suspect that improper behavior is not peculiar to a handful of great scientists but is characteristic of a much larger group. Indeed, the burden of proof would seem to be on anyone who claims that a majority of scientists habitually use the hypothetico-deductive method in the strict sense (that is, rejecting a theory if it fails to agree with all experimental facts).

If my interpretation of current historical thinking is correct, the science teacher who wants to use historical materials to illustrate how scientists work is indeed in an awkward position. Perhaps one must finally ask: Are the standards of objective scientific method worth preserving, even as ideals that are rarely attained in practice? Or do we distort our understanding of the nature of science by paying lip service to such standards?

Conclusions

I suggest that the teacher who wants to indoctrinate his students in the traditional role of the scientist as a neutral fact finder should not use historical materials of the kind now being prepared by historians of science: they will not serve his purposes. He may wish to follow the advice of philosopher J. C. C. Smart, who recently suggested that it is legitimate to use *fictionalized* history of science to illustrate one's pronouncements on scientific method (*56*). On the other hand, those teachers who want to counteract the dogmatism of the textbooks and convey some understanding of science as an activity that cannot be divorced from metaphysical or esthetic considerations may find some stimulation in the new history of science. As historian D. S. L. Cardwell has argued (*57*, p. 120):

> . . . [I]f the history of science is to be used as an educational discipline, to inculcate an enlightened and critical mind, then the Whig view . . . cannot do this. For it must emphasize the continuities, the smooth and successive developments from one great achievement to the next and so on; and in doing so it must automatically endow the present state of science with all the immense authority of history.

He suggests that the critical mind might be inhibited by seeing the present as the inevitable, triumphant product of the past. The history of science *could* aid the teaching of science by showing that "such puzzling concepts as force, energy, etc., are man-made and were evolved in an understandable sequence in response to acutely felt and very real problems. They were not handed down by some celestial textbook writer to whom they were immediately self-evident" (*57*, p. 120).

The past may give some hints on how to survive the most recent recurrence of public hostility to science. Rather than blaming historians such as Kuhn for encouraging antiscientific attitudes, as one physicist did in a public address in 1972 (*58*), one might consider this criticism of the older style of science history, published in 1940 by W. James Lyons (*59*, p. 381):

> The historians of science are responsible, it would appear, for the unpopularity of science among those most acutely affected by the depression. In their clamor to enhance the scientific tradition, and hoard for science all credit for the remarkable and unprecedented material advances which studded the century and a quarter preceding 1930, these historians have been more enthusiastic than accurate . . . science emerged [in the popular mind] as the most prominent force responsible for making this modern world so startlingly different from all preceding ages. Thus when, for many people, the modern world, in spite of all its resources, began to slip from its role of "best of all imaginable worlds," science came in for a proportionate share of blame. Had a more accurate picture of the part science has played been presented, science would not now be the object of so much suspicion and resentment.

In more recent times, hostility to science has been intensified by the image of the "objective," robot-like scientist lacking emotions and moral values. If the new approach to the history of science really does give a more realistic picture of the behavior of scientists, perhaps it has a "redeeming social significance." Then, rather than limiting the conception of science to the strict pattern allowed by traditional local standards, one might try to change those standards in such a way as to reflect the freedom that the boldest natural philosophers have always exercised.

References and Notes

1. See The Washington *Post* (24 September 1972), p. B-6.
2. I know of only one case in which a statement about the sexual behavior of a scientist was expunged from a book intended to be read by students. When the Physical Science Study Committee reprinted a part of Arthur Koestler's *The Sleepwalkers* as a volume in the "Science Study Series" one sentence was eliminated from Kepler's recollections of his adolescent experiences. See *The Watershed* (Doubleday, Garden City, N.Y., 1964), p. 25.
3. At the end of an enthusiastic review of Giorgio de Santillana's *Reflections on Men and Ideas* [*Isis* **62**, 105 (1971)], A. Rupert Hall warned his colleagues in the history of science: "Do not buy this book for second-year physics majors" (p. 106). I don't know if his reasons were the same as the ones presented in this article.
4. H. Reichenbach, *Experience and Prediction* (Univ. of Chicago Press, Chicago, 1938), pp. 6–7 and 382–384.
5. C. Kittel, W. D. Knight, M. A. Ruderman, *The Berkeley Physics Course*, vol. 1, *Mechanics* (McGraw-Hill, New York, 1962).
6. It might be suggested that there is another established dogma: Scientists are not supposed to be concerned about personal priority rights in a discovery. I omit this topic since it has been thoroughly treated by Robert Merton [*Am. Sci.* **57**, 1 (1969)].
7. P. W. Bridgman, *Reflections of a Physicist* (Philosophical Library, New York, 1955); the statement is frequently quoted—for example, in *The Project Physics Course, Reader 1* (Holt, Rinehart & Winston, New York, 1970), pp. 18–19.
8. O. P. Puri, G. H. Walker, E. Burt, C. S. Kiang, J. D. Wise, W. P. Thompson, H. Rogers, *Concepts in Physical Science* (Addison-Wesley, Reading, Mass., 1970), pp. 9–10; J. G. Reilly and A. W. Vander Pyl, *Physical Science: An Interrelated Course* (Addison-Wesley, Reading, Mass., 1970), pp. 2–8; J. A. Ripley, Jr., and R. C. Whitten, *The Elements and Structure of the Physical Sciences* (Wiley, New York, ed. 2, 1969), pp. 5–10. Discussions of scientific method tend to be more elaborate in biology texts. See, for example, P. B. Weisz, *Elements of Biology* (McGraw-Hill, New York, ed. 3, 1969), pp. 3–12.
9. D. Bell, *The Reforming of General Education* (Anchor, New York, 1968).
10. J. R. Platt, *Science* **146**, 347 (1964).
11. W. Ostwald, *Klassiker der exakten Wissenschaften* (Engelmann, Leipzig, 1889–); J. R. Partington, *A History of Chemistry* (Macmillan, London, 1961–1971); D. ter Haar, *Selected Readings in Physics* (Pergamon, Oxford, 1965–). See the comments on "what happens when a scientist turns to history" by L. P. Williams [*Victorian Stud.* **9**, 197 (1966)].
12. G. P. Thomson, *J. J. Thomson and the Cavendish Laboratory in His Day* (Nelson, London, 1964), p. 54 (on the discovery of the Zeeman effect); C. Truesdell, *Essays in the History of Mechanics* (Springer-Verlag, New York, 1968), pp. 305–333; G. Sarton, *The Life of Science* (Schuman, New York, 1948), pp. 43–44; R. B. Lindsay, *The Role of Science in Civilization* (Harper & Row, New York, 1963), pp. 120–122.
13. E. Mach, *The Science of Mechanics*, trans. by T. J. McCormack (Open Court, La Salle, Ill., ed. 6, 1960), pp. 8–9.
14. J. B. Conant, *Am. Sci.* **48**, 528 (1960). For some reasons that "history of science bores most scientists stiff," see P. B. Medwar, *The Hope of Progress* (Doubleday Anchor Garden City, N.Y., 1973), p. 101.
15. F. S. Allen, *Educ. Rec.* **48**, 268 (1967).
16. T. S. Kuhn, in *Scientific Creativity, Its Recognition and Development*, C. W. Taylor and F. Barron, Eds. (Wiley, New York, 1963), pp. 341–354. Similar fears are suggested by the title (although not the text) of the article by J. L. Synge, "Is the study of its history a brake on the progress of science?" [*Hermathena* **91**, 20 (1958)]. (I am indebted to F. Tipler for this reference.)
17. In his autobiography, Conant describes the origin of the Harvard committee on general education, which produced a report advocating the use of history in teaching science. Since Conant thought that the professors who were not involved in war work might feel left out of things, he decided to give them the job of drawing up plans for education in the post-war world [J. B. Conant, *My Several Lives* (Harper & Row, New York, 1970), p. 364].
18. For a report on the recommendations of the seminar and further details, see S. G. Brush, *Phys. Teach.* **8**, 508 (1970).
19. S. G. Brush and A. L. King, Eds., *History in the Teaching of Physics* (University Press of New England, Hanover, N.H., 1972); S. G. Brush, Ed., *Resources for the History of Physics* (University Press of New England, Hanover, N.H., 1972); H. Kangro, Ed., *Classic Papers in Physics* (Taylor & Francis, London, 1972–).
20. S. G. Brush, *Phys. Teach.* **7**, 271 (1969).
21. G. Sarton, *The Life of Science* (Schuman, New York, 1948), p. 30.
22. C. C. Gillispie, *The Edge of Objectivity* (Princeton Univ. Press, Princeton, N.J., 1960); see the extract from Maxwell's 1871 inaugural lecture at Cambridge University, which Gillispie uses as the foreword to his book.
23. A. Koyré, *Études Galiléennes* (Hermann, Paris, 1966), reprint of three articles published separately, 1935–1939.
24. Quoted in G. Holton, *Introduction to Concepts and Theories in Physical Science* (Addison-Wesley, Reading, Mass., 1952), pp. 21–22, from A. Koyré, *J. Hist. Ideas* **4**, 400 (1943), p. 405.
25. A. R. Hall, in *Galileo: Man of Science*, E. McMullin, Ed. (Basic Books, New York, 1967), pp. 67–81. A good critical analysis of the historical problem of proving that the earth moves was published about this time [R. Palter, *Monist* **48**, 143 (1964)].
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29. T. S. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, ed. 2, 1970).
30. ———, in *Scientific Creativity*, C. W. Taylor and F. Barron, Eds. (Wiley, New York, 1963), pp. 341–354.
31. For a concentrated dose of this controversy, see I. Lakatos and A. Musgrave, Eds., *Criticism and the Growth of Knowledge* (Cambridge Univ. Press, London, 1970). Comments by scientists are discussed, for example, in W. J. Fraser, *Science* **173**, 868 (1971).
32. I. Scheffler, *Science and Subjectivity* (Bobbs-Merrill, Indianapolis, Ind., 1967), pp. v–vi.
33. L. K. Nash, *Isis* **47**, 101 (1956); J. R. Partington, *Ann. Sci.* **4**, 245 (193 G. de Beer, *Notes Rec. R. Soc. Lond.* **19**, 192 (19(L. C. Dunn, *Proc. Am. Philos. Soc.* **109**, 189 (1965); B. L. van der Waerden, *Centaurus* **12**, 275 (1968).
34. On Einstein's attitude, see G. Holton, in *Science and Synthesis* (Springer-Verlag, New York, 1971), pp. 45–70. See also the letter from Einstein to Max Born, 12 May 1952, in *The Born-Einstein Letters* (Walker, New York, 1971), p. 192. H. A. Lorentz, in an address at Leiden in 1926, mentioned the results of D. C. Miller, which some people thought would refute the theory of relativity, but which Lorentz maintained would, even if verified, only "indicate the existence of some unknown cause"; "relativity will be quite safe" [*Collected Papers* (Nijhoff, The Hague, 1935), vol. 8, p. 415].
35. P. A. M. Dirac, *Sci. Am.* **208**, 45 (May 1963). See also the statement, "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," attributed to Eddington by G. S. Stent [*The Coming of the Golden Age* (Natural History Press, New York, 1969), p. 31]. Stent says this is "a realistic description of the psychological dynamics that obtain at the frontier of scientific advance."
36. M. Planck, *Philosophy of Physics* (Norton, New York, 1936), p. 97; *A Scientific Autobiography and Other Papers* (Williams & Norgate, London, 1950), pp. 33–34.
37. W. Heisenberg, *Physics and Beyond* (Harper & Row, New York, 1971).
38. In a private communication, I. Scheffler has pointed out that he locates objectivity not in the individual behavior of scientists, but in the "institutionalized controls by which science itself evaluates theoretical novelty" [see also (*32*, p. 72)]. I suppose one must then ask whether, in the historical cases cited in this article, other scientists followed the leader and thus collectively failed to exert these institutional controls. My opinion is that this is indeed what happened, but there is not enough space to debate the point here. Scheffler has published a further criticism of Kuhn's theory in *Philos. Sci.* **39**, 366 (1972).
S. G. Brush, *Br. J. Hist. Sci.* **5**, 145 (1970).
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44. J. C. Maxwell, *Rep. 30th Meet. Br. Assoc. Adv. Sci.* (1860), p. 15. This short but important summary of Maxwell's conclusions was not included in his *Scientific Papers*, published by Cambridge University Press in 1890 and reprinted by Dover in 1952 and 1965.
45. S. G. Brush, *Synthese* **18**, 192 (1968); J. T. Blackmore, *Ernst Mach* (Univ. of California Press, Berkeley, 1972).
46. ———, *Arch. Ration. Mech. Anal.* **39**, 1 (1970).
47. See S. G. Brush, *Kinetic Theory* (Pergamon, Oxford, 1972), vol. 3, which includes an English translation of Enskog's monograph, and reprints of Chapman's shorter papers.
48. H. Butterfield, *The Whig Interpretation of History* (Bell, London, 1931), pp. 1–13.
49. See J. Hexter, *Reappraisals in History* (Northwestern Univ. Press, Evanston, Ill., 1961).
50. P. S. de Laplace, *Celestial Mechanics*, trans. by N. Bowditch (Chelsea, New York, 1966), vol. 4, p. 1015.
51. H. F. Kearney, *Science and Change 1500–1700* (McGraw-Hill, New York, 1971).
52. The suggestion that Copernicus was an adherent of Hermetism, deriving from the writings of Frances Yates on Bruno, has been denounced by Copernican scholar Edward Rosen. Rosen asserts that "out of Renaissance magic and astrology came, not modern science, but modern magic and astrology." See his article in *Historical and Philosophical Perspectives of Science*, R. H. Stuewer, Ed. (Univ. of Minnesota Press, Minneapolis, 1970), pp. 163–171; see also M. Hesse, *ibid.*, pp. 134–162. The importance of the role of mysticism and pseudoscience in the origin of modern science in Western Europe, as well as in ancient Chinese science, has been stressed by other historians of science: see, for example, O. Neugebauer, *Isis* **42**, 111 (1951); J. Needham, *The Grand Titration, Science and Society in East and West* (Univ. of Toronto Press, Toronto, 1969), p. 162. See ; J. E. McGuire and P. M. Rattansi, *Notes Rec. R. Soc. Lond.* **21**, 108 (1966).
53. A. Koyré, *Isis* **34**, 209 (1943); but see also P. P. Wiener, *ibid.*, p. 301.
54. T. Settle, *Science* **133**, 19 (1961); S. Drake, *Isis* **64**, 291 (19 J. MacLachlan, *ibid.*, p. 374.
55. T. S. Kuhn, *Isis* **49**, 132 (1958).
J. C. C. Smart, *Br. J. Philos. Sci.* **23**, 266 (1972). Herbert Feigl has admitted that "A few of us [philosophers of science] . . . for some time rather unashamedly 'made up' some phases of the history of science" [*Historical and Philosophical Perspectives of Science*, R. H. Stuewer, Ed. (Univ. of Minnesota Press, Minneapolis, 1970), p. 3].
57. D. S. L. Cardwell, *Mem. Proc. Manchester Lit. Philos. Soc.* **106**, 108 (1963–64).
58. L. Eisenbud, "Science for antiscientists," speech given at the American Physical Society meeting, Washington, D.C., 26 April 1972. Kuhn has protested "the description of my views as a defense of irrationality in science," but to no avail; see his article in *Boston Studies in Philosophy of Science*, R. C. Buck and R. S. Cohen, Eds. (Reidel, Dordrecht, 1971), vol. 8, pp. 137–146, and (*29*), p. 186.
59. W. J. Lyons, in *Science in America*, J. Burnham, Ed. (Holt, Rinehart & Winston, New York, 1971), pp. 377–384.
60. This article is based in part on research supported by the National Science Foundation. The author thanks Peter Bowman for his helpful suggestions.

NEWS AND COMMENT

Uranium Enrichment: Rumors of Israeli Progress with Lasers

Rumors have been circulating through the classified research community for the past several weeks that two Israeli scientists have succeeded in enriching uranium with a cheap but sophisticated new laser process. The rumors—which appear to have started with some casual inquiries among U.S. scientists by the Central Intelligence Agency (CIA)—represent an exaggeration, according to one of the two Israeli researchers. There is, nevertheless, an important kernel of truth in the tale—enough to suggest that Israeli researchers are not far behind their American counterparts in developing a technology that promises to greatly reduce the cost and difficulty of obtaining enriched uranium, both for nuclear power plants and for nuclear weapons.

"We have demonstrated the feasibility of laser enrichment, but not the economic feasibility," Isaiah Nebenzahl, a physicist with Israel's Ministry of Defense, told *Science* by telephone from Haifa. Nebenzahl was reluctant to discuss details of the research and took pains to play down the scale of the effort, which he said was "very small."

Last October, Exxon Nuclear, Inc., revealed to a congressional committee that a joint research venture with Avco Everett Research Laboratories had successfully enriched small amounts of uranium by laser, and that the process "is practical today on a laboratory scale." The two Israeli scientists appear to have duplicated this feat. Nebenzahl, however, indicated that he and his colleague, Tel Aviv University physicist Menahem Levin, had not yet produced gram amounts of fissionable material, as was rumored. "We are not near a macroscopic separation," he said.

Nevertheless, some U.S. authorities regard even this small success as a "very significant" indicator both of Israel's technical sophistication and of its interest in what promises to be an extraordinarily cheap method of enriching uranium.

"Enrichment" is a term used to describe any of several ways of artificially concentrating the fissionable isotope ^{235}U, which makes up only 0.7 percent of natural uranium. To make the fuel for conventional, light-water cooled reactors, this concentration is increased to between 2 and 3 percent. Fission weapons normally require an enrichment of more than 90 percent.

The sheer difficulty and expense of enriching uranium have worked for 30 years as effective restraints on the availability of nuclear fuel and weapons. So far only the United States, U.S.S.R., Britain, France, and presumably China have seen fit to build the enormous gaseous diffusion plants necessary to produce large amounts of even modestly enriched uranium. The expense of this process has motivated a continuing search for cheaper and less conspicuous techniques; the leading contender now is the gas centrifuge.

In diffusion plants, uranium hexafluoride gas is pumped at high pressure through porous barriers that preferentially pass the lighter ^{235}U. Thousands of successive steps are required to reach high levels of enrichment. At Oak Ridge, Tennessee, and two other locations in the southeastern United States, "cascades" of diffusion cells fill cavernous buildings as large as 60 acres and half a mile long. One of the hardest things on earth to hide is a