Steven Weinberg

Physics and History

I AM ONE OF THE FEW CONTRIBUTORS to this issue of *Dædalus* who is not in any sense a historian. I work and live in the country of physics, but history is the place that I love to visit as a tourist. Here I wish to consider, from the perspective of a physicist, the uses that history has for physics, and the dangers both pose to each other.

I should begin by observing that one of the best uses of the history of physics is to help us teach physics to nonphysicists. Although many of them are very nice people, nonphysicists are rather odd. Physicists get tremendous pleasure out of being able to calculate all sorts of things, everything from the shape of a cable in a suspension bridge to the flight of a projectile or the energy of the hydrogen atom. Nonphysicists, for some reason, do not appear to experience a comparable thrill in considering such matters. This is sad but true. It poses a problem, because if one intends to teach nonphysicists the machinery by which these calculations are done, one is simply not going to get a very receptive class. History offers a way around this pedagogical problem: everyone loves a story. For example, a professor can tell the story (as I did in a book and in courses at Harvard and Texas) of the discovery of the subatomic particles—the electron, the proton, and all the others.[1] In the course of learning this history, the student—in order to understand what was going on in the laboratories of J. J. Thomson, Ernest Rutherford, and our other heroes—has to learn something about how particles move

Steven Weinberg is Josey Regental Professor of Science at the University of Texas at Austin.

under the influence of various forces, about energy and momentum, and about electric and magnetic fields. Thus, in order to understand the stories, they need to learn some of the physics we think they should know. It was Gerald Holton's 1952 book *Introduction to Concepts and Theories of Physical Science* that first utilized precisely this method of teaching physics; Holton told the story of the development of modern physics, all the while using it as a vehicle for teaching physics. Unfortunately, despite his efforts and those of many who came after him, the problem of teaching physics to nonphysicists remains unsolved. It is still one of the great problems facing education—how to communicate "hard sciences" to an unwilling public. In many colleges throughout the country the effort has been given up completely. Visiting small liberal-arts colleges, one often finds that the only course offered in physics at all is the usual course for pre-medical students. Many undergraduates will thus never get the chance to encounter a book like Holton's.

History plays a special role for elementary particle physicists like myself. In a sense, our perception of history resembles that of Western religions, Christianity and Judaism, as compared to the historical view of other branches of science, which are more like that of Eastern religious traditions. Christianity and Judaism teach that history is moving toward a climax—the day of judgment; similarly, many elementary particle physicists think that our work in finding deeper explanations of the nature of the universe will come to an end in a final theory toward which we are working. An opposing perception of history is held by those faiths that believe that history will go on forever, that we are bound to the wheel of endless reincarnation; likewise, particle physicists' vision of history is quite different from that of most of the sciences. Other scientists look forward to an endless future of finding interesting problems—understanding consciousness, or turbulence, or high temperature superconductivity—that will go on forever. In elementary particle physics our aim is to put ourselves out of business. This gives a historical dimension to our choice of the sort of work on which to concentrate. We tend to seek out problems that will further this historic goal—not just work that is interesting, useful, or that influences other fields, but work that is historically progressive, that moves us toward the goal of a final theory.

In this quest for a final theory, problems get bypassed. Things that once were at the frontier, as nuclear physics was in the 1930s, no longer are. This has happened recently to the theory of strong interactions. We now understand the strong forces that hold the quarks together inside the nuclear particles in terms of a quantum field theory called quantum chromodynamics. When I say that we understand these forces, I do not mean that we can do every calculation we might wish to do; we are still unable to solve some of the classic problems of strong interaction physics, such as calculating the mass of the proton (the nucleus of the hydrogen atom). A silly letter in *Physics Today* recently asked why we bother to talk about speculative fundamental theories like string theory when the longstanding problem of calculating the mass of the proton remains to be solved. Such criticism misses the point of research focused on a historical goal. We have solved enough problems using quantum chromodynamics to know that the theory is right; it is not necessary to mop up all the islands of unsolved problems in order to make progress toward a final theory. Our situation is a little like that of the United States Navy in World War II: bypassing Japanese strong points like Truk or Rabaul, the Navy instead moved on to take Saipan, which was closer to its goal of the Japanese home islands. We too must learn that we can bypass some problems. This is not to say that these problems are not worth working on; in fact, some of my own recent work has been in the application of quantum chromodynamics to nuclear physics. Nuclear forces present a classic problem—one on which I was eager to work. But I am not under the illusion that this work is part of the historical progress toward a final theory. Nuclear forces present a problem that remains interesting, but not as part of the historical mission of fundamental physics.

If history has its value, it has its dangers as well. The danger in history is that in contemplating the great work of the past, the great heroic ideas—relativity, quantum mechanics, and so on—we develop such reverence for them that we become unable to reassess their place in what we envision as a final physical theory. An example of this is general relativity. As developed by

Einstein in 1915, general relativity appears almost logically inevitable. There was a fundamental principle, Einstein's principle of the equivalence of gravitation and inertia, which says that there is no difference between gravity and the effects of inertia (like centrifugal force). The principle of equivalence can be reformulated as the principle that gravity is just an effect of the curvature of space and time—a beautiful principle from which Einstein's theory of gravitation follows almost uniquely. But there is an "almost" here. To arrive at the equations of general relativity, Einstein in 1915 had to make an additional assumption; he assumed that the equations of general relativity would be of a particular type, known as second-order partial differential equations. This is not the place to explain precisely what a second-order partial differential equation is—roughly speaking, it is an equation in which appear not only things like gravitational fields, and the rates at which these things change with time and position, but also second-order rates, the rates at which the rates change. It does not include higher order rates, for instance, third-order rates—the rates at which the rates that are changing are changing. This may seem like a technicality, and it is certainly not a grand principle like the principle of equivalence. It is just a limit on the sorts of equations that will be allowed in the theory. So why did Einstein make this assumption—this very technical assumption, with no philosophical underpinnings? For one thing, people were used to such equations at the time: the equations of Maxwell that govern electromagnetic fields and the wave equations that govern the propagation of sound are all second-order differential equations. For a physicist in 1915, therefore, it was a natural assumption. If a theorist does not know what else to do, it is a good tactic to assume the simplest possibility; this is more likely to produce a theory that one could actually solve, providing at least the chance to decide whether or not it agrees with experiment. In Einstein's case, the tactic worked.

But this kind of pragmatic success does not in itself provide a rationale that would satisfy, of all people, Einstein. Einstein's goal was never simply to find theories that fit the data. Remember, it was Einstein who said that the purpose of the kind of physics he did was "not only to know how nature is and how her transactions are carried through, but also to reach as far as possible the utopian and seemingly arrogant aim of knowing why nature is thus and not otherwise. . . ." He certainly was not achieving that goal when he arbitrarily assumed that the equations for general relativity were second-order differential equations. He could have made them fourth-order differential equations, but he did not.

Our perspective on this today, which has been developing gradually over the last fifteen or twenty years, is different from that of Einstein. Many of us now regard general relativity as nothing but an effective field theory—that is to say, a field theory that provides an approximation to a more fundamental theory, an approximation valid in the limit of large distances, probably including any distances that are larger than the scale of an atomic nucleus. Indeed, if one supposes that there really are terms in the Einstein equations that involve rates of fourth or higher order, such terms would still play no significant role at sufficiently large distances. This is why Einstein's tactic worked. There is a rational reason for assuming the equations are second-order differential equations, which is that any terms in the equations involving higher order rates would not make much of a difference in any astronomical observations. As far as I know, however, this was not Einstein's rationale.

This may seem rather a minor a point to raise here, but in fact the most interesting work today in the study of gravitation is precisely in contexts in which the presence of higher-order rates in the field equations would make a big difference. The most important problem in the quantum theory of gravity arises from the fact that when one does various calculations—as, for instance, in attempting to calculate the probability that a gravitational wave will be scattered by another gravitational wave—one gets answers that turn out to be infinite. Another problem in the classical theory of gravitation arises from the presence of singularities: matter can apparently collapse to a point in space with infinite energy density and infinite space-time curvature. These absurdities, which have been exercising the attention of physicists for many decades, are precisely problems that involve gravity at very short distances—not the large distances of astronomy, but distances much smaller than the size of an atomic nucleus.

From the point of view of modern effective field theory, there are no infinities in the quantum theory of gravity. The infinities are cancelled in exactly the same way that they are in all our other theories, by just being absorbed into a redefinition of parameters in the field equations; but this works only if we include terms involving rates of fourth order and all higher orders. (John Donaghue of the University of Massachusetts at Amherst has done more than anyone in showing how this works.) The old problems of infinities and singularities in the theory of gravitation cannot be dealt with by taking Einstein's theory seriously as a fundamental theory. From the modern point of view—if you like, from my point of view—Einstein's theory is nothing but an approximation valid at long distances, which cannot be expected to deal successfully with infinities and singularities. Yet some professional quantum gravitationalists (if that is the word) spend their whole careers studying the applications of the original Einstein theory, the one that only involves second-order differential equations, to problems involving infinities and singularities. Elaborate formalisms have been developed that aim to look at Einstein's theory in a more sophisticated way, in the hope that doing so will somehow or other make the infinities or singularities go away. This ill-placed loyalty to general relativity in its original form persists because of the enormous prestige the theory earned from its historic successes.

But it is precisely in this way that the great heroic ideas of the past can weigh upon us, preventing us from seeing things in a fresh light. Said another way, it is those ideas that were most successful of which we should be most wary. Otherwise we become like the French army, which in 1914 tried to imitate the successes of Napoleon and almost lost the war—and then in 1940 tried to imitate the 1916 success of Marshall Petain in defending Verdun, only to suffer decisive defeat. Such examples exist in the history of physics as well. For instance, there is an approach to quantum field theory called second quantization, which fortunately no longer plays a significant role in research but continues to play a role in the way that textbooks are written. Second quantization goes back to a paper written in 1927 by Jordan and Klein that put forth the idea that after one has introduced a wave function in quantizing a theory of particles, you should then quantize the wave function. Surprisingly, many people think that this is the way to look at quantum field theory; it is not.

We have to expect the same fate for our present theories. The standard model of weak, electromagnetic, and strong forces, used to describe nature under conditions that can be explored in today's accelerators, may itself neither disappear nor be proved wrong but instead be looked at in quite a different way. Most particle physicists now think of the standard model as only an effective field theory that provides a low-energy approximation of a more fundamental theory.

Enough about the danger of history to science; let us now take up the danger of scientific knowledge to history. This arises from a tendency to imagine that discoveries are made according to our present understandings. Gerald Holton has done as much as anyone in trying to point out these dangers and puncture these misapprehensions. In his papers about Einstein he shows, for example, that the natural deduction of the special theory of relativity from the experiment of Michelson and Morley, which demonstrated that there is no motion through the ether, is not at all the way Einstein actually came to special relativity. Holton has also written about Kepler. At one point in my life I was one of those people who thought that Kepler deduced his famous three laws of planetary motion by studying the data of Tycho Brahe. But Holton points out how much else besides data, how much of the spirit of the Middle Ages and of the Greek world, went into Kepler's thinking—how many things that we now no longer associate with planetary motion were on Kepler's mind. By assuming that scientists of the past thought about things the way we do, we make mistakes; what is worse, we lose appreciation for the difficulties, for the intellectual challenges, that they faced.

Once, at the Tate Gallery in London, I heard a lecturer talking to a tour group about the Turner paintings. Turner was very important, said the guide, because he foreshadowed the Impressionists of the later nineteenth century. I had thought Turner was an important painter because he painted beautiful pictures; Turner did not know that he was foreshadowing anything. One has to look at things as they really were in their own time. This also applies, of course, to political history. Consider the term "Whig interpretation of history," which was invented by Herbert Butterfield in a lecture in 1931. As Butterfield explained it, "The Whig historian seems to believe that there is an unfolding logic in history." He went on to attack the person he regarded as the archetypal Whig historian, Lord Acton, who wished to use history as a way to pass moral judgments on the past. Acton wanted history to serve as the "arbiter of controversy, the upholder of that moral standard which the powers of earth and religion itself tend constantly to depress. . . . It is the office of historical science to maintain morality as the sole impartial criterion of men and things." Butterfield went on to say:

> "If history can do anything it is to remind of us of those complications that undermine our certainties, and to show us that all our judgments are merely relative to time and circumstance. . . . We can never assert that history has proved any man right in the long run. We can never say that the ultimate issue, the succeeding course of events, or the lapse of time have proved that Luther was right against the pope or that Pitt was wrong against Charles James Fox."[2]

This is the point at which the historian of science and the historian of politics should part company. The passage of time has shown that, for example, Darwin was right against Lamarck, the atomists were right against Ernst Mach, and Einstein was right against the experimentalist Walter Kaufman, who presented data contradicting special relativity. To put it another way, Butterfield was correct; there is no sense in which Whig morality (much less the Whig Party) existed at the time of Luther. But nevertheless it is true that natural selection was working during the time of Lamarck, and the atom did exist in the days of Mach, and fast electrons behaved according to the laws of relativity even before Einstein. Present scientific knowledge has the potentiality of being relevant in the history of science in the way that the present moral and political judgements may not be relevant in political or social history.

Many historians, sociologists, and philosophers of science have taken the desire for historicism, the worry about falling into a Whig interpretation of history, to extremes. To quote Holton, "Much of the recent philosophical literature claims that science merely staggers from one fashion, conversion, revolution, or incommensurable exemplar to the next in a kind of perpetual, senseless Brownian motion, without discernible direction or goal."[3] I made a similar observation in an address to the American Academy of Arts and Sciences about a year and a half ago, noting in passing that there are people who see scientific theories as nothing but social constructions. The talk was circulated by the Academy, as is their practice, and a copy of it fell into the hands of someone who over twenty years ago had been closely associated with a development known as the Sociology of Scientific Knowledge (SSK). He wrote me a long and unhappy letter; among other things, he complained about my remark that the Strong Program initiated at the University of Edinburgh embodied a radical social-constructivist view, in which scientific theories are nothing but social constructions. He sent me a weighty pile of essays, saying that they demonstrated that he and his colleagues do recognize that reality plays a role in our world. I took this criticism to heart and decided that I would read the essays. I also looked back over some old correspondence that I had had with Harry Collins, who for many years led the well-known Sociology of Scientific Knowledge group at the University of Bath. My purpose in all of this was to look at these materials from as sympathetic a point of view as I could, try to understand what they were saying, and assume that they must be saying something that is not absurd.

I did find described (though not espoused) in an article by David Bloor, who is one of the Edinburgh group, and also in my correspondence with Harry Collins, a point of view that on the face of it is not absurd. As I understand it, there is a position called "methodological idealism" or "methodological antirealism," which holds that historians or sociologists should take no position on what is ultimately true or real. Instead of using today's scientific knowledge as a guiding principle for their work, the argument goes, they should try to look at nature as it must have been viewed by the scientists under study at the time that those scientists were working. In itself, this is not an absurd position. In particular, it can help to guard us against the kind of silliness that (for instance) I was guilty of when I interpreted

Kepler's work in terms of what we now know about planetary motion.

Even so, the attitude of methodological antirealism bothered me, though for a while I could not point to what I found wrong with it. In preparing this essay I have tried to think this through, and I have come to the conclusion that there are a number of minor things wrong with methodological antirealism: it can cripple historical research, it is often boring, and it is basically impossible. More significantly, however, it has a major drawback—in an almost literal sense, it misses the point of the history of science.

Let us first address the minor points. If it were really possible to reconstruct everything that happened during some past scientific discovery, then it might be helpful to forget everything that has happened since; but in fact much of what occurred will always be unknown to us. Consider just one example. J. J. Thomson, in the experiments that made him known as the discoverer of the electron, was measuring a certain crucial quantity, the ratio of the electron's mass to its charge. As always happens, he found a range of values. Although he quoted various values in his published work, the values he would always refer to as his favorite results were those at the high end of the range. Why did Thomson quote the high values as his favorite values? It is possible that Thomson knew that on the days those results had been obtained he had been more careful; perhaps those were the days he had not bumped into the laboratory table, or before which he'd had a good night's sleep. But the possibility also exists that perhaps his first values had been at the high end of the range, and he was determined to show that he had been right at the beginning. Which explanation is correct? There is simply no way of reconstructing the past. Not his notebooks, not his biography—nothing will allow us now to reconstitute those days in the Cavendish Laboratory and find out on which days Thomson was more clumsy or felt more sleepy than usual. There is one thing that we do know, however: the actual value of the ratio of the electron's mass to its charge, which was the same in Thomson's time as in our own. We know, in fact, that the actual value is not at the high end but, rather, at the low end of the range of Thomson's experimental values, which strongly suggests that when Thomson's measurements gave high values they were not actually more careful—and that therefore it is more likely that Thomson quoted these values because he was just trying to justify his first measurements.

This is a trivial example of the use of present scientific knowledge in the history of science, because here we are just talking about a number, not a natural law or an ontological principle. I chose this example simply because it shows so clearly that to decide to ignore present scientific knowledge is often to throw away a valuable historical tool.

A second minor drawback of methodological antirealism is that a reader who does not know anything about our present understanding of nature is likely to find the history of science terribly boring. For instance, a historian might describe how in 1911 the Dutch physicist Kamerlingh Onnes was measuring the electrical resistance of a sample of cold mercury and thought that he had found a short circuit. The historian could go on for pages and pages describing how Onnes searched for the short circuit, and how he took apart the wiring and put it back together again without any success in finding the source of the short circuit. Could anything be more boring than to read this description if one did not know in advance that there *was* no short circuit—that what Onnes was observing was in fact the vanishing of the resistance of mercury when cooled to a certain temperature, and that this was nothing less than the discovery of superconductivity? Of course, it is impossible today for a physicist or a historian of physics not to know about superconductivity. Indeed, we are quite incapable while reading about the experiments of Kamerlingh Onnes of imagining that his problem was nothing but a short circuit. Even if one had never heard of superconductivity, the reader would know that there was something going on besides a short circuit; why else would the historian bother with these experiments? Plenty of experimental physicists have found short circuits, and no one studies them.

But these are minor issues. The main drawback of methodological antirealism is that it misses the point about the history of science that makes it different from other kinds of history: Even though a scientific theory is in a sense a social consensus, it is unlike any other sort of consensus in that it is culture-free and permanent.

This is just what many sociologists of science deny. David Bloor stated in a talk at Berkeley a year ago that "the important thing is that reality underdetermines the scientists' understanding." I gather he means that although he recognizes that reality has some effect on what scientists do—so that scientific theories are not "nothing but" social constructions—scientific theories are also not what they are simply because that is the way nature is. In a similar spirit, Stanley Fish, in a recent article in the *New York Times*, argued that the laws of physics are like the rules of baseball. Both are certainly conditioned by external reality—after all, if baseballs moved differently under the influence of Earth's gravity, the rules would call for the bases to be closer together or further apart—but the rules of baseball also reflect the way that the game developed historically and the preferences of players and fans.[4]

Now, what Bloor and Fish say about the laws of nature does apply while these laws are being discovered. Holton's work on Einstein, Kepler, and superconductivity has shown that many cultural and psychological influences enter into scientific work. But the laws of nature are not like the rules of baseball. They are culture-free and they are permanent—not as they are being developed, not as they were in the mind of the scientist who first discovers them, not in the course of what Latour and Woolgar call "negotiations" over what theory is going to be accepted, but in their final form, in which cultural influences are refined away. I will even use the dangerous words "nothing but": aside from inessentials like the mathematical notation we use, the laws of physics as we understand them now are nothing but a description of reality.

I cannot prove that the laws of physics in their mature form are culture-free. Physicists live embedded in the Western culture of the late twentieth century, and it is natural to be skeptical if we say that our understanding of Maxwell's equations, quantum mechanics, relativity, or the standard model of elementary particles is culture-free. I am convinced of this because the purely scientific arguments for these theories seem to me overwhelmingly convincing. I can add that as the typical background of physicists has changed, in particular, as the number of women and Asians in physics has increased, the nature of our understanding of physics has not changed. These laws in their mature form have a toughness that resists cultural influence.

The history of science is further distinguished from political or artistic history (in such a way as to reinforce my remarks about the influence of culture) in that the achievements of science become permanent. This assertion may seem to contradict a statement at the beginning of this essay—that we now look at general relativity in a different way than Einstein did, and that even now we are beginning to look at the standard model differently than we did when it was first being developed. But what changes is our understanding of both why the theories are true and their scope of validity. For instance, at one time we thought there was an exact symmetry in nature between left and right, but then it was discovered that this is only true in certain contexts and to a certain degree of approximation. But the symmetry between right and left was not a simple mistake, nor has it been abandoned; we simply understand it better. Within its scope of validity, this symmetry has become a permanent part of science, and I cannot see that this will ever change.

In holding that the social constructivists missed the point, I have in mind a phenomenon known in mathematical physics as the approach to a fixed point. Various problems in physics deal with motion in some sort of space. Such problems are governed by equations dictating that wherever one starts in the space, one always winds up at the same point, known as a fixed point. Ancient geographers had something similar in mind when they said that all roads led to Rome. Physical theories are like fixed points, toward which we are attracted. Starting points may be culturally determined, paths may be affected by personal philosophies, but the fixed point is there nonetheless. It is something toward which any physical theory moves; when we get there we know it, and then we stop.

The kind of physics I have done for most of my life, working in the theory of fields and elementary particles, is moving toward a fixed point. But this fixed point is unlike any other in science. That final theory toward which we are moving will be a theory of unrestricted validity, a theory applicable to all phenomena throughout the universe—a theory that, when finally reached, will be a permanent part of our knowledge of the world. Then our work as elementary particle physicists will be done, and will become nothing but history.

ENDNOTES

[1]Steven Weinberg, *The Discovery of Subatomic Particles* (San Francisco: Scientific American/Freeman, 1982).

[2]Herbert Butterfield, *The Whig Interpretation of History* (New York: Scribners, 1951), 75.

[3]Gerald Holton, *Einstein, History, and Other Passions* (Reading, Mass.: Addison-Wesley, 1996), 22.

[4]Stanley Fish, "Professor Sokal's Bad Joke," *New York Times*, 21 May 1996, Op-Ed section.