

## **SYMPOSIUM**

### **What Do the Natural Sciences Know and How Do They Know It?**

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### **The Methods of Science... And Those by Which We Live**

*Steven Weinberg*

Standing in a bookshop in Harvard Square a decade ago, I noticed a book on the philosophy of science by a friend of mine. I opened it and found it interesting—my friend was discussing questions about scientific knowledge that I had not thought of asking. Yet, although I bought the book, I realized that it would probably be a long time before I sat down to read it, because I was busy with my research and I knew deep in my heart that this book was not going to help me in my work as a scientist.

This was not a great discovery. I think few philosophers of science take it as part of their job description to help scientists in their research. Wittgenstein and others have explicitly disclaimed any such aim. But then, standing in the bookshop, it occurred to me to ask why this should be? Why should the philosophy of science not be of more help to scientists? I raise this question here not in order to attack the philosophy of science, but because I think that it is an interesting question—even philosophically interesting.

In thinking this over, I realized that a good part of the explanation must be that science is a moving target, that the standards for successful scientific theories shift with time. It is not just our view of the universe that shifts, but our view of what kinds of views we should have or can have. How can we expect a philosopher (or anyone else) to know enough about the universe and the human mind to anticipate these shifts in advance? How could anyone expect Aristotle in his writings on projectiles to imagine the quantitative study of the

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motion of projectiles that began just a generation later in Alexandria? To Aristotle, although he knew about quantitative sciences like arithmetic and astronomy, the sciences appropriate for most of nature were little more than taxonomy. We learn about the philosophy of science by doing science, not the other way around.

A favorite example of mine, one much closer to home, is presented by Albert Einstein's development of the special theory of relativity in 1905. For some years before 1905 a number of physicists had been worrying about why it seemed to be impossible to detect any effect on the speed of light of the earth's motion through the ether. The electron, just discovered in 1897, was then the only known elementary particle, and it was widely supposed that all matter was composed of electrons. So most physicists who worried about the ether, such as Abraham, Lorentz, and Poincaré, tried to develop a theory of the electron's structure, such that the lengths of measuring rods and the rates of clocks made of electrons would change as they moved through the ether in just such a way as to make it seem that the speed of light did not depend on the speed of the observer.

Einstein did not proceed along that line. Instead, he took as a fundamental hypothesis the principal of relativity, that it is not possible to detect the effects of uniform motion on the speed of light or anything else. On this foundation, he built a whole new theory of mechanics. Einstein's theory was widely accepted by the cognoscenti in theoretical physics, including Lorentz. But Lorentz, though a great admirer of Einstein, did allow himself one very mild complaint, that Einstein had assumed what Lorentz and others had been trying to understand. This was quite true. Einstein just assumed that there would be no effect of motion through the ether. What Einstein had done was to set the tone of the twentieth century by taking a principle of symmetry, or invariance—a principle that says that some changes in point of view cannot be detected—as a fundamental part of scientific knowledge, a hypothesis at the very roots of science, rather than something that is unsatisfactory until it can be deduced, as Lorentz was trying to do, from a specific dynamical theory. In other words, Einstein had changed the way that we score our theories. Today, a theory based on an assumed new symmetry, if it also fit in with experiment, would be regarded as very appetizing, a very promising theory. It wasn't so for Lorentz.

The fact that the standards of scientific success shift with time does not only make the philosophy of science difficult; it also raises problems for the public understanding of science. We do not have a fixed scientific method to rally round and defend. I remember that I spoke years ago to a high-school teacher who explained proudly that in her classes they were trying to get away from teaching just scientific facts and instead give the students an idea of what the scientific method was. I replied that I had no idea what the scientific method was, and I thought she ought to teach her students scientific facts. She thought I was just being surly. But it's true, most scientists have very little idea of what the scientific method is, just as most bicyclists have very little idea of how

bicycles stay erect. In both cases, if they think about it too much, they're likely to fall off.

The changes in the way we judge our theories has bothered philosophers and historians of science. Kuhn's early book, *The Structure of Scientific Revolutions*, emphasized this process of change in our scientific standards. I think he went overboard in concluding that there was a complete incommensurability between present and past standards, but it is correct that there is a qualitative change in the kind of scientific theory we want to develop that has taken place at various times in the history of science. But Kuhn then proceeded to the fallacy—much clearer in what he has written recently—that in science we are not in fact moving toward objective truth. I call this a fallacy, because it seems to me a simple *non sequitur*. I do not see why the fact that we are discovering not only the laws of nature in detail, but what kinds of laws are worth discovering, should mean that we are not making objective progress.

Of course, it's hard to prove that we are making objective progress. David Hume showed early on the impossibility of using rational argument to justify the scientific method, since rational argument, that is, appeal to experience, is part of the scientific method. But this kind of skepticism gets one nowhere. One can be equally skeptical about our knowledge of ordinary objects, because the methods of science are not that different, except in degree, from the methods by which we live our lives. But in fact we don't worry very much about whether our knowledge of common objects like chairs is objective or socially constructed.

The physicist who lives with principles of symmetry, like the principles of relativity, or with the more esoteric constructs like quarks or quantum fields or superstrings, gets nearly as familiar with them as with the chairs on which he or she sits. The physicist finds that just as with chairs, these constructs can't be made up as one goes along, that they seem to have an existence of their own. If you say the wrong thing about them, you'll find out about it pretty soon, when experiment or mathematical demonstration proves that you've been wrong.

My experience with the principles of physics is not that different from my experience of chairs. Even with chairs one can raise the question of different levels of knowledge. We mostly know about chairs by sitting in them or by bumping into them, but there are other ways of knowing about chairs. For instance, you can look at photographs of chairs. That's a mode of knowing about chairs that's actually fairly refined. I believe there are primitive peoples—though I'm not sure about this—who do not recognize photographs of objects as representing the objects. I know that my cat doesn't. He is incapable of associating a photograph even of something interesting, like a fish, with the actual object. We can; we are sophisticated enough to possess such a higher mode of knowing about objects like chairs. From that to the methods of modern science I see no philosophically relevant discontinuity.

I think it does no good for scientists to pretend that we have a clear *a priori* idea of the scientific method. But still, we should try to say something about what it is that we think we are doing when we make progress toward truth in the course of our scientific work. There is one philosophical principle that I find of use here. It is, to paraphrase another author, "It don't mean a thing if it ain't got that zing." That is, there is a kind of zing—to use the best word I can think of—that is quite unmistakable when real scientific progress is being made.

Here is an example, a vicarious one, because I was not one of the active players at the time. Back in the mid-1950s, when I was a graduate student, particle physicists were beset with a tangle of problems associated with the properties of certain particles called K mesons. In 1956, two young theorists, Tsung Dao Lee and Chen Ning Yang, pointed out that the whole tangle could be resolved if you just took away one of the assumptions on which all previous analyses had been based, the assumption of what is technically called parity conservation—essentially just the symmetry between right and left. This symmetry principle states that the laws of nature will take the same form whether the x, y, and z axes of your coordinate system are arranged like the thumb, forefinger, and middle finger of your right hand or of your left hand. This had been taken for granted, by that time, for forty years. It was regarded as self-evident. I, in my wisdom as a graduate student, thought it was absolutely absurd to challenge this principle, especially since it had been very successfully used in a wide variety of contexts in atomic and nuclear physics. Yet here were Lee and Yang proposing that this symmetry principle must be given up in order to understand the K mesons. They also proposed experiments that could verify their idea, and within months their idea was verified. Within less than a year the whole world of physics was convinced that Lee and Yang were right and that this forty-year-old principle, which we had all taken for granted, was not universally applicable. In the following two or three years this discovery led to a great clarification in our understanding of the weak nuclear forces, going beyond the question of left-right symmetry. In showing that the question of whether or not a symmetry principle is true must be tested experimentally, Lee and Yang had reversed the tendency, beginning with Einstein, to take symmetries as given, nearly self-evident principles. The sense of excitement, of breakthrough, of accomplishment—in short, the "zing"—was evident to everyone.

Another example is closer to my own work. The 1960s had seen the development of a unified theory of weak nuclear forces and electromagnetism. It was not clear that this theory was mathematically consistent, although Abdus Salam and I argued that it was. Then, in 1971, a previously unknown graduate student, Gerard 't Hooft at the University of Utrecht, showed that theories of this type are, in fact, mathematically consistent. Immediately the world of theorists began to take this seriously and write many papers about it. It had not yet, however, become part of the scientific consensus. This change in the consensus took longer than the one started by Lee and Yang. In 1973, two years after

't Hooft's first work, and six years after my own earlier work, experimental evidence began to emerge showing that the theory was valid. Even so, although the theory was now widely held to be right, there remained some healthy skepticism, which was reinforced in 1976 when some other experiments pointed in the other direction. Finally, in 1978, experiments done at the Stanford Linear Accelerator Center decisively supported the unified theory of weak and electromagnetic forces, and it was from then on generally regarded as correct. From the very beginning to the end, the process of general acceptance had taken about eleven years, of which five were a period of intense experimental effort.

Experiment always has something to do with the fashioning of a scientific consensus, but in ways that can be quite complicated. In this case, the theorists were pretty well convinced of the general idea of this sort of theory after 't Hooft's work in 1971, before there was the slightest new experimental evidence. The rest of the physics community became convinced over a longer period of time, as the experimental evidence became unmistakable. But, by the end of the 1970s, there was a nearly universal consensus that this theory was right.

This story illustrates a few points. First, the interaction between theory and experiment is complicated. It is not that theories come first and then experimentalists confirm them, or that experimentalists make discoveries that are then explained by theorists. Theory and experiment often go on at the same time, strongly influencing each other.

Another point, ignored almost always by journalists and often by historians of science, is that theories usually exist on two levels. On one hand, there are general ideas, which are not specific theories, but frameworks for specific theories. One example of such a general idea is the theory of evolution by natural selection, which leaves open the question of the mechanism of heredity. In the case of the unified theory of weak and electromagnetic forces, the underlying general idea was that the apparent differences between these forces arise from a phenomenon known as "spontaneously broken symmetry," that the equations of the theory have a symmetry between these forces, which is lost in the solution of these equations—the actual particles and forces we observe. These general ideas are very hard to test because by themselves they do not lead to specific predictions. This has sadly led Popper to conclude that because such general ideas can't be falsified, they can't be regarded as truly scientific.

Then there are the specific, concrete realizations of such ideas. These are the theories that can be tested by experiment, and can be falsified. As it happened with the unified weak and electromagnetic theory, the symmetry pattern that had been originally suggested as a specific realization of the general idea of broken symmetry (aspects of which can be found also in work of Glashow and Salam and Ward, that did not incorporate this idea) turned out to be the right one. During the period of the 1970s, theorists were mostly convinced about the general idea, but not about this specific realization of the idea. Of

course, the experimentalists had to prove that some specific theory was right before any of this could become part of the scientific consensus.

Also, when I say that the physics community became universally convinced of something, I am speaking loosely—this is never entirely true. If you had a lawsuit that hinged on the validity of the unified weak and electromagnetic theory, you could probably find an expert witness who is a Ph.D. physicist with a good academic position who would testify that he didn't believe in the theory. There are always some people on the fringes of science, not quite crackpots, often people with good credentials, who don't believe the consensus. This makes it harder for an outsider to be sure that the consensus has occurred, but it does not change the fact of the consensus. The consensus for Lee and Yang's idea about the symmetry between right and left and the consensus for the unified theory of weak and electromagnetic interactions were unmistakable when they happened.

I would also like to point out that, at least within the area of physics, which is what I mostly know about, and within this century, whenever this consensus has been achieved, it has never been simply wrong. To be sure, sometimes the truth turns out more complicated than what had been thought. For example, before 1956 there had been a consensus that there is an exact symmetry between right and left, and then we learned that the symmetry is not exact, it is only a good approximation in certain contexts. But the forty years of theoretical physics research that relied on that symmetry to understand nuclear and atomic problems was not wrong, there were just small corrections that physicists didn't know about. No consensus in the physics community has ever been simply a mistake, in the way that in earlier centuries you might say, for example, that the theory of caloric or phlogiston was a mistake.

Now, all of this is of course a social phenomenon. The reaching of consensus takes place in a worldwide society of physicists. This fact has led to a second fallacy—that, because the process is a social one, the end product is a mere social construct.

A physicist turned author, Andrew Pickering, wrote a book about the conceptual development of the quark called *Constructing Quarks*. The book showed the interesting social process by which the existence of quarks gradually became the consensus among physicists. If I understand it correctly, the book's conclusion was that this is all that quarks are—a social consensus like the rules of contract bridge. Again, I think this is a simple *non sequitur*. The analogy I drew in my book *Dreams of a Final Theory* was to a party of mountain climbers who argue about the path to some peak. Their arguments are conditioned by the social structure of the expedition, but when they find the right path they know it because then they get to the peak. No one would write a book about mountaineering with the title *Constructing Everest*.

Anyway, the social milieu of physics research does not seem to me to be well described by postmodern commentators. It is far less oppressive and hegemonic

than most people would imagine. In many cases the great breakthroughs are made by youngsters like 't Hooft, who no one has ever heard of before, while the famous graybeards who have senior positions in the great universities often get left behind. Werner Heisenberg and (to a lesser extent) Paul Dirac were left behind by the physics community after 1945, as were Einstein and De Broglie after 1925. Heisenberg and De Broglie rather discredibly tried to force their views on the physics communities in Germany and France. Einstein and Dirac, gentler souls, simply went their own ways. But even Heisenberg and De Broglie were not able to damage German or French physics for very long. The exact sciences show a remarkable measure of resilience and resistance to any kind of hegemonic influence, perhaps more than any other human enterprise.

The working philosophy of most scientists is that there is an objective reality and that, despite many social influences, the dominant influence in the history of science is the approach to that objective reality. It may seem that, in asserting the objective validity of what we are doing, scientists are simply trying to protect their own status. It is not easy to answer that criticism. I could say, "I am not a crook," but such arguments only go so far. Perhaps the best answer is "*tu quoque*." It seems to me that much of the comment on science by the social constructivists and postmodernists seems motivated by the desire to enhance the status of the commentator—that he be seen not as a hanger-on or adjunct to science, but as an independent investigator, and perhaps as a superior investigator, by reason of his greater detachment. This is especially true of those who follow the "Strong Program" in the sociology of science, which my friend Sidney Coleman calls the "Strong Pogrom."

This motivation was close to the surface in a recent article in *Isis* by Paul Forman. He described historians of science as preoccupied with their independence from the sciences. He called for a greater degree of independence, because this was important to their work as historians. So far, so good, but he also wanted historians to exercise an independent judgment not just as to how progress is made, which certainly is in their province, but also on whether progress is made. He gave no arguments that such judgments would have any kind of intellectual validity, except that this was the sort of thing that historians have to do as part of being historians.

I think we scientists need make no apologies. It seems to me that our science is a good model for intellectual activity. We believe in an objective truth that can be known, and at the same time we are always willing to reconsider, as we may be forced to, what we have previously accepted. This would not be a bad ideal for intellectual life of all sorts.