LETTERS

Discussion of Nature of Science Provokes Hit-or-Myth Debate

The exchange in "Letters" (July 1996, page 11) between David Mermin on the one hand and Harry Collins and Trevor Pinch on the other, triggered by Mermin's two columns on their book *The Golem: What Everyone Should Know about Science* (March, page 11; April, page 11), brings to mind Augustine of Hippo's famous confrontation with the followers of the Skeptics, who held that man can know nothing with certainty. "Are you certain of that?" Augustine asked.

No modern-day consensus on the nature of science will be reached until we agree that what we are talking about is neither sociology nor science, but philosophy. Accordingly, we should evaluate Collins and Pinch's arguments by treating them as philosophy, rather than trying to subject them to the methods used in science to discover truth.

In their response to Mermin's columns, Collins and Pinch appear to re-express the basic world view of the Skeptics that because all knowledge (including scientific knowledge) enters the mind through the fallible senses, no knowledge can ever be certain, only highly probable. (In the 18th century, David Hume gave the same argument against knowledge based on induction ever being certain.) In this view, they are correct: Certainty is a property of the mind, and not of the facts. Once the probability of something (like a scientific theory) being true becomes high enough, we close our minds to the possibility of the opposite occurring and we say we are "certain" of it. Collins and Pinch point out that the discriminator level at which scientists become certain is a subjective judgment, and tends to vary with external circumstances such as length of time and other people's opinions.

Mermin's retort is essentially "So what?" It is a giant leap from certainty being a subjective judgment to the conclusion apparently implied in *The Golem* that the scientific method of discovering truth is biased and not a reliable guide to reality. Mermin's strongest counterargument is the cry of Archimedes: "It works!"

The successful use of science to predict future events correctly is the best argument that scientific procedures and protocols are a valid guide to objective reality. And for anyone who really does not believe that we can be certain of anything in the external world, have I got a wager for them about the time of sunrise tomorrow.

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Having read the interaction between Collins and Pinch on one hand and Mermin on the other, I would like to present another point of view—namely, that neither side seems to have quite gotten the point of earlier work on the study of science (although I do support the strong points made by Mermin in his brilliant response).

Facts are inherently social; even simple observations must be communicated and, if important, necessarily confirmed.

Science is an intensely social activity, as is any activity that involves many people and immense and diversified efforts. That is, science could not be other than a social activity and have any success whatever. However, as a reasonable person, I depart from the "social constructivist" view of science represented by Collins and Pinch's implicit assertions and declare that science does not just produce biased outcomes but can produce "results" or "facts." The title of their book gives away their agenda; a golem, according to my dictionary is "an artificial human being in Hebrew folklore endowed with life." That's rather instant derogation!

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Beginning with Paul Feyerabend and the University of Edinburgh group in the 1970s, an argument was developed that the necessary outcome of a social process is subjective. Science is, of course, a social activity. Accordingly, in their terms, there is no authority whatever in science—which is precisely what Feyerabend and company wanted to conclude in the first place.

However, that view went against an earlier tradition of curiosity and wonder that imperfect humans could arrive at nearly perfect analyses of some parts of a complex universe. The resultant conflict—human cap acities versus the exactitude of knowledge—created the agenda for the work of Robert King Merton, Thomas Kuhn (guided by James Conant), Derek Price, John Ziman and now-aging "youngsters" such as Nicholas Mullins (prematurely deceased), Warren Hagstrom, Henry Small, Lowell Hargens and myself.

Mermin has gone along unwittingly with Collins and Pinch's metaphor of carpentry, a discipline that can easily be practiced by a single individual. However, consider a truly sophisticated automobile—a combination of many sciences and technologies. Or consider the means used to get a jet plane off and onto the ground and to change its in-flight configuration from that of a high-lift object to that of a low-drag object—all the outcome of applied mathematics. Or consider the computer on which I wrote this letter.

If painstaking and logical proof were required to create a "fact," rather than the consensual process identified by Ziman and discussed by Peter Medawar and others, we would travel in buggies and write on parchment. However, although the sciences do not seem to meet the standards of Collins and Pinch or the school of thought they represent (a school I have long regarded as espousing nonsense, rather than heresy), the cars and planes and computers actually exist.

There really are results and facts—all created within the internal standards of the technical disciplines, and they are the bases for extremely painful and highly discriminating processes of evaluation and selection. Such processes were developed completely outside the rather theological standards that Collins and Pinch would wish to impose.

Mermin's accurate assertions about their misinterpreting the status of documents and arguments within science should be taken very seriously. And perhaps some sense of reality could be imparted to Collins and Pinch by putting them on jet planes that lack flaps or spoilers.

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I have read *The Golem*. Basically, I agree with Mermin's criticism of Collins and Pinch, but I am still sympathetic to the metaphor of science as golem: We commit hubris when we forget the human origins of physics.

However, misunderstandings about the subtle workings of physics are not strictly the fault of those, like Collins and Pinch, who study it from a distance. It is unlikely that any account from the outside can ever capture what is really happening on the inside, even down to our subjective experiences, our "stubbornness," our "convictions." Yet, we physicists generally recoil from the philosophical task of reflecting on the limits of our knowledge, and, as long as we do so, we can expect others to do it for us.

This unfortunate state of affairs has not always been the case. Earlier in the 20th century, Einstein engaged in a dialogue with philosophy that was intellectually fruitful (but was not entirely conclusive), and he made significant contributions by pursuing the epistemological and social implications of his physics. He recognized the benefits of such discussion when he affirmed that "the reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is . . . primitive and muddled."[1]

Perhaps it is time for physicists to pick up Einstein's project where he left off.

Reference

1. *Albert Einstein: Philosopher–Scientist*, P. A. Schilpp, ed., Open Court, La Salle, Illinois (1970), p. 683.

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No social phenomenon as complex as science can possibly exist without mythology, despite what Collins and Pinch, as well as Mermin, apparently assume. Surely historical events are not part of introductory science teaching because science students are failed historians, but because heroic narratives define the field's situation within the social context. This is the essential function of myth. In fact, Collins and Pinch's implication that the world would be a better place if all scientists committed

themselves to a supposedly nonmythical instrumentalist and social constructivist view of science is in itself part of a pernicious mythology. Collins and Pinch's view is pernicious because it is just as intolerant as the scientistic mythology they criticize. Fortunately, mythology is neither always pernicious nor, contrary to Collins and Pinch, necessarily intolerant.

Scientific mythology is constrained, however, by a specific requirement that does not equally apply to other narratives. Because of the special role that facts play in science, the stories about science must themselves be reasonably accurate. But their function is nonetheless to tell us something about the here and now. It is less relevant how contemporaries judged the Michelson–Morley experiment than that, in hindsight, it was a significant piece of evidence leading to special relativity. In this regard, a science teacher approaches the topic necessarily with a different emphasis than a historian of science, though neither one can claim to be more accurate than the other.

Whether storytellers should be allowed to fib a little for the sake of those who can't take the whole truth has been debated at least since the 17th century, when mythology was first edited *ad usum Delphini* at the French court. The commitment of science to factual accuracy would suggest that, at least in this area, bending the truth should not be acceptable. Mermin is right about the temptations of expediency, although I think he underestimates the significance of falsehood and lies in science teaching. But that simply means that science is not taught as well as it should be, not that lies are an integral part of any mythology, least of all the inevitable mythology of science.

Alfred Kracher
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Collins and Pinch have performed a useful service, not in explaining what science is, but in showing us that it is a poorly understood concept.

Science is thought to be the application of mathematics to explain or describe hard facts. As such, physics, chemistry and biology are required to share the same description of being called a science with economics, psychology, sociology, medicine and much else, leaving some sciences to be promoted to core sciences when those particular ones do a better job of being a science—that is, when they are more experimental (rigorous) or more theoretical (clever at "using" mathematics), depending on the emphasis of the day.

We feel giddy if we hear that there are no hard facts, but that is true in real science if "hard" is taken to mean rigid. A fact in real science is not a raw-sense datum but a sophisticated artifact, and it is all the more real for this, its ability to change making it as "hard" as possible in the process of discovery.

What is essential to investigations of reality is that they hang together. Mermin writes that "Michelson–Morley was only a small part of an intricate network of theory and experiment to which relativity brought clarity and coherence." The last word deserves much exploration because correspondence theories generally require rigid facts.

What is special to and peculiar about real science is that it develops mathematics and does not merely apply it. Mathematicians are real scientists and vice versa—not as individuals, perhaps, but insofar as the essence of their subject is concerned. As a result, the edifice is unusual. It incorporates, subsumes, illuminates, refines and enlarges upon its earlier theories and facts. It doesn't pick them up and put them down as flavors of the month. This is so contrary to Collins and Pinch's description of science as being no more than a "craft and a body of cultural achievements" that we must thank them for their useful description of pseudoscience. And their description may have an element of historical reference too, for it is not surprising that, given the complex makeup of human intelligence, we have gone about trying to do science by a confusing plethora of means.

But now that we see where we have got, there will be physicists who know their subject is a special one, and they need not be too afraid of being fundamentalist. To say that one has precise knowledge of something is not to think that it is (in Collins and Pinch's words) "the royal road to all knowledge." Only the royal road to more!

Roger L. Burnley
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Collins and Pinch reply: It is nice to know that Belver Griffith is a reasonable and unbiased person; that he knows lots of people who do the sociology of science better than we do is also reassuring.

We are tempted to collapse in the face of the nearly uniform phalanx of criticism in the five letters, but, fortu-

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nately, the coincidence of opinion turns out on close inspection to be a "tapestry" of different colored threads. Dolan thinks the touchstone of science is prediction, Griffith thinks it is application (and reasonableness, of course), Kracher thinks it is factual accuracy and Burnley thinks it is the application of mathematics.

We do not want to deny any of these beliefs. That, taken together, they add up to a description of science and technology is beyond dispute. Also, we agree that we prefer to fly on airplanes with flaps and spoilers, though we have never actually checked with the pilot and cabin crew before taking off.

Our problem is that we never meet disputing scientists who change their scientific views when confronted with such criteria of good practice and good sense; they all agree with the criteria but continue with the same scientific ideas. That is why such notions do not work in the history of science (just as Griffith's invocation of reasonableness does not work in this debate).

We do agree with Dolan that we are engaged in a form of skepticism, but to show that there is more going on here, we spent time trying to work out the consequences of our view in the conclusion of *The Golem*. Kracher's astute comments on myth are well worth thinking about more deeply; if we recognize that it is a matter of preferring one myth to another, we will have started to make progress.

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MERMIN REPLIES: I agree with Joseph Dolan that probability is of central importance, but unlike him I don't believe the social constructivist view takes adequate account of the fact that knowledge, though never certain, can be overwhelmingly probable. There is a lot of territory between the realization that absolute certainty is impossible and the claim of Collins and Pinch that "scientists at the research front cannot settle their disagreements through better experimentation, more knowledge, more advanced theories, or clearer thinking." To understand that intervening ground, you have to take into account the role of probability. In *The Golem*, as far as I can tell, Collins and Pinch don't.

I hope never again to hear anyone declare that even social constructivists expect the sun to rise (Dolan), airplanes to fly (Griffith) and unsupported objects to fall (innumerable writers of letters to newspapers). But until some sociologist provides a plausible account of how they reconcile these private expectations with their professional view of scientific knowledge, such shots will continue to sail across their bows. If they would unambiguously acknowledge that view to be nothing more than a methodologically crucial constraint on their sociological investigations, as Collins and Pinch now come close to doing, we could move on to the substantive issue of whether such a constraint is too intellectually confining.

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Sociologists, Scientist Pick at Threads of Argument about Science

We are delighted that David Mermin and PHYSICS TODAY are providing an opportunity to make some progress in this usually ill-informed and fruitless argument about the social science view of the natural sciences.

In his July 1996 reply to us in "Letters," Mermin advances a theory of scientific progress, saying that progress is made by the weaving together of many strands of evidence, each insufficient in itself. It is a reasonable theory and can be found in other historical treatments. It is almost certainly not the whole story, however, since strands of evidence can be woven in different ways. Thus, one still needs an explanation of why a group of scientists interprets a set of strands of evidence one way rather than another, and one needs to set this explanation in the context of an analysis that shows how different kinds of weaving could have been done. In the chapter on gravitational radiation in *The Golem*, we tried to show how it was that the weaving was done in one particular way. In the last couple of pages of the chapter on relativity, we gestured toward the kind of explanation needed in the case of relativity without trying to provide a proper historical treatment; backing for our preferred treatment can be found in the wider historical literature. Of course, the history of relativity has a huge literature in its own right and, as in other social sciences, few explanations remain unchallenged for long. Luckily, as Mermin implies, the main purpose of our relativity chapter was to lay the myth of the crucial experiment, and we are content if we have achieved that.

We are glad that we and Mermin can agree to separate scientific fundamentalism from science as a way of understanding the natural world. We understand his defense of the crucial-experiment myth as a way of simplifying life for students, but we think the price is too high. Such ideas hide the craft work of experiment and encourage the notion of the scientist as someone who can resolve a disagreement with a superhuman flourish—an invitation to fundamentalism. That this is just an "early scaffolding" cannot be right, or the sociology of science would engender fewer passions than it does. If it is to maintain its position as a discipline that treasures truth, science should not misrepresent its history.

The opening of Mermin's July 1996 response to our letter is crucial if we are to unpick the knot of mutual misunderstanding. Mermin is right: His views of science and ours differ in the way in which the rightness and wrongness of science are handled. He says, correctly, "The rightness–wrongness axis is not a relevant dimension in [our] kind of sociological analysis of science." But that is not an accident or something we have overlooked; it is, on the contrary, a methodological requirement of our work, for two reasons. First, it would be hubris for sociologists to take physical rightness and wrongness into account. Sociologists are not physicists, and it is no more their business to offer opinions about the findings of physics than it is the business of physicists to make better tables. Second, since the early 1970s, good sociology and history of science has studiously avoided explaining the emergence of truth by reference to its truthfulness because such explanations are circular—like ascribing the power of opium to its dormative properties.

The mistake that is made by most scientists (and some social scientists), is to think that this approach competes with or undermines the findings of science. It does not. But, in shedding a tangential light on the origins of scientific truth, it does compete with the fundamentalists. Thus, this approach says nothing about any particular truth but something about the nature of scientific truth in general. We would not expect scientists at the laboratory bench to find these ideas particularly useful (except when they become involved in unexpected controversy). However, we would expect them to find the ideas useful