

LETTERS (continued from page 15)

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 Mer-

min 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

when they see what happens to science in the courtroom, in the classroom and in technological controversies and disasters. To make sense of these things, science must be understood as a craft. In exploring science as a craft, the strange sociological approach—which temporarily sets aside what we all know to be true—is useful, if uncomfortable.

In the scientific and technological debates in which citizens become embroiled—for example, over health priorities, the safety of British beef or the risks of nuclear power generation—the scientific and technological truth of the matter is still an issue. Unreflective scientists, as well as laypersons, will miss the point if their model of science has been generated with the aid of nothing other than 20/20 hindsight. To get the point, one must go back to when the truth was still unknown. In the case of relativity, this very exchange in *PHYSICS TODAY* reveals how hard it is to do that.

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MERMIN REPLIES: Collins and Pinch make several points. Let me address them one by one.

▷ **Multiple Explanations.** By the time a subject has developed to the stage where many strands of evidence are available, the actual challenge is not to distinguish among several different ways of weaving them together, but to find even a single clear and convincing synthesis. When there does remain more than one way, we invent experiments to help distinguish among the alternatives. We do this fully aware that such tests may be difficult to perform and hard to interpret, but it has been our experience that remarkably often the difficulties are overcome and the ambiguities resolved. If no such tests can even be imagined, then the alternatives are equivalent and it is a matter of taste which to use. As far as I know, the historical literature does not record an empirically distinguishable rival to special relativity of comparable simplicity, power and coherence in the 1930s.

▷ **Science Pedagogy.** I was not defending the use of dubious historical fables in the teaching of science. Such “history” is usually part of an oral tradition bearing only a vague resemblance to what really happened. Furthermore, the issues introduced by the pseudohistorical approach

often have little if any current scientific interest. Worst of all, approaching the subject historically can make it harder, not easier, to understand the contemporary state of affairs. What I was actually suggesting was that the use of such myths in the teaching of science might be a fruitful area of sociological study. I also offered my own guess why Michelson–Morley had been so widely incorporated into the pedagogy of relativity. It certainly does not simplify the subject. Having to wade through Michelson–Morley at the outset makes the learning process substantially harder. The only useful purpose I see it serving is to help motivate the exploration of an initially highly counterintuitive proposition.

▷ **Truth and Falsehood.** I do understand that the irrelevance of physical rightness or wrongness is not an oversight, but a methodological imperative for Collins and Pinch, and I said so in my “Reference Frame” column of March 1996 (page 11). But if followed too rigidly, the imperative can lead to conclusions about the nature of scientific consensus that overlook essential parts of the process. It is one thing to set aside our current knowledge of what is true and false as an impediment to gaining a clear understanding of the social process by which those distinctions first became broadly accepted. But it is quite another thing (and this is my major criticism of *The Golem*) to transform that methodological tool of the sociologist into an unqualified characterization of the nature of scientific truth. To do so is to confuse one of the instruments used in the study with the object of study itself.

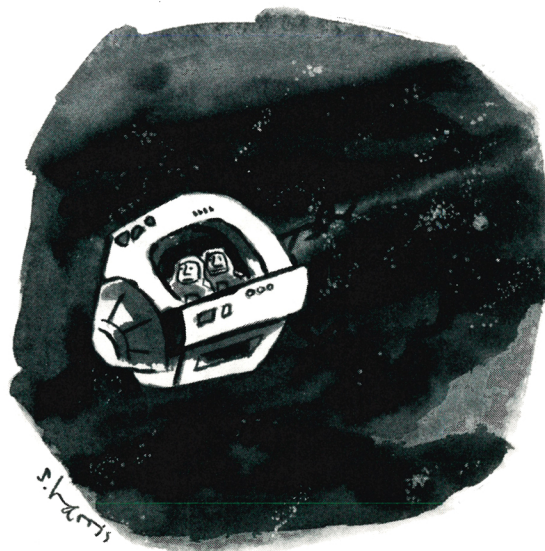
So, although you may miss much of interest and importance if you focus too strongly on objective truth in trying to understand the process by which one theory wins out over its rivals, you have to be careful.

You must not become so enamored of the strategem that you entirely ignore what we all now know to be true, letting

Collins and Pinch’s “temporarily” shade into “permanently.” Doing so obscures the difficult but important sociological question of what distinguishes the growth of a genuine science from that of a pseudoscience. Indeed, it makes it impossible to frame the question at all, and can have the pernicious effect of leading you to proclaim to the general public that, at some fundamental level, there is no difference.

▷ **The Neutrality of the Sociologist.** I do not understand Collins and Pinch’s claim that they are unqualified to offer opinions about the validity of any of the findings of physics. The essays in *The Golem* show them to have a sophisticated understanding of some quite subtle areas of science. While I agree that they ought to refrain from taking sides on issues that are still unresolved, why should it be beyond their sphere of competence to have an opinion on the strength of the evidence that moving clocks do indeed run slowly? In examining how such truths are established, they are far too sophisticated to run the risk of falling into the kind of trivial circularity they warn against.

▷ **Undermining the Findings of Science.** Few scientists object to the scholarly examination of the often messy and confusing process whereby human knowledge is advanced. Indeed, those who pay attention find the case studies fascinating and often in accord with their own experience. The claim that a better understanding of the origins of the findings of science undermines those very findings was not put forth by scientists.



"IF EINSTEIN IS CORRECT, WHEN WE GET BACK, MY CAR WILL HAVE BEEN DOUBLE PARKED 320 YEARS."

It was stated, among the conclusions of their own studies, by certain sociologists. This is the claim that many scientists so heatedly challenge. I accept Collins and Pinch's declaration that they are not among the sociologists who take this view, though much of what they say in *The Golem* about the general character of science would seem to place them squarely in that camp. Perhaps it is just a case of the rhetoric getting in the way of the message, in which case the moral is clear. We should all pay more attention to explaining clearly where we stand, and steer away from extravagant and sometimes inflammatory flourishes.

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ITER Debaters Reply to Pro and Con Fusion Comments from Readers

We welcome the letters from Janis Lawyer, Igor Fodor and Bruno Coppi (PHYSICS TODAY, December 1996, page 11) commenting on our debate about the pros and cons of proceeding to the actual construction of the International Thermonuclear Experimental Reactor (ITER) following completion of the six-year Engineering Design Activities (EDA) phase in mid-1998 (PHYSICS TODAY, June 1996, page 21). We hope that the science and engineering communities will continue to offer critical comments on the future directions of fusion research—that is, on learning how to use thermonuclear energy in a benevolent fashion to generate electric power.

Lawyer suggests a similar debate on the merits of fusion research itself. Underlying this suggestion is the point, recognized by the fusion research community, that even its best efforts may not lead to a full scenario for economically competitive fusion power. However, along with many others, the members of this community believe that fusion does have a great potential for environmentally safe power generation—a potential that mandates exploration and understanding.

We concur with Fodor's comment that the time scale for fusion power could well stretch to the year 2050. But our society will probably not be able to continue for another century as an economy based on fossil fuels. A crisis is coming and there are a limited number of possible responses, none of them free of risk and proven to be economically and environmen-

tally viable. All of them—including fission, fusion and solar (biomass, direct conversion and heating)—need more support now, and the cost for even the most vigorous research program would be only a minor fraction of 1% of the nation's current annual spending on energy consumption. A far greater cost will be incurred down the road if we postpone the hunt for a benign energy source. Magnetic and inertial fusion in particular will take a long time to develop or even realistically assess. Clearly the completion date is a strong function of funding levels. To lose the present infrastructure and multinational momentum would add many years to the timetable.

Fodor also brings up other long-standing issues for fusion prospects, as have been identified elsewhere.¹ Here, we respond briefly to three of his specific points.

First, it is premature to guess what the ultimate cost and reliability of a fusion reactor will prove to be. Studies with present-day "credible" assumptions find fusion reactors to be competitive with other nonfossil energy sources, but only further scientific exploration and technological experience will make it possible to develop realistic answers.

Second, a number of studies have indicated that liquid lithium can be employed safely, as in other liquid metal-cooled reactors. A more conservative approach for tritium breeding could utilize nonreactive solid pellets formed of oxides or zirconates of lithium.

Third, the production of long-lived radionuclides will be orders of magnitude below that in comparable fission reactors, particularly if low-activation structural materials such as vanadium are used. Still, as Fodor points out, there will be large quantities of low-level waste. It is thought that this waste can be recycled for reactor reuse. Nevertheless, the building of a successful fusion reactor will require a much more comprehensive knowledge of low-activation material technology than is now available, and we encourage continued research in this area.

Coppi points to the important ideas for a burning-plasma experiment pioneered in the Ignitor concept. Our June 1996 dialogue specifically cited Ignitor, which is now being prototyped in Italy, as one example of an affordable ignition or near-ignition experiment. Constructed soon, Ignitor could provide unusually worthy insights pertaining to the characteristics and performance of a fusion plasma, including ITER, and could guide

scenarios for plasma control, heating and diagnostics.

Our PHYSICS TODAY debate last June also cited the growing accuracy of computer modeling and simulation. Since then, this approach has started to yield a computer-theoretic understanding of the turbulence suppression discovered earlier in experiments using reversed magnetic shear. (See PHYSICS TODAY, January 1996, page 9.) Support for this increasingly powerful tool in plasma research has not kept pace with its potential, however, and we strongly endorse a much-increased level of funding for fusion-relevant computation.

Finally, we wish to point out that subsequent to the completion of the EDA in mid-1998, a determination is to be made by the present parties to the ITER agreement (the US, the European Union, Japan and Russia) regarding the construction of ITER. As of mid-December 1996, it appears likely that if the parties were to elect to construct, ITER would be sited in Europe or Japan, with the bulk of the cost to be borne by the host. We recognize that many factors will enter into the decision process and that the numerous and complex issues could be resolved in many different ways. In any case, these negotiations may be expected to define the character of a continuing ITER process that would consist of large-scale multinational collaboration in fusion research and technology. We believe that planning for an effective post-1998 fusion research program for the US must take place now and must contemplate the full range of possible outcomes of these discussions.

In summary, we believe that energetic research directed toward economically competitive fusion power is an essential component of today's long-range energy strategy, and we encourage a robust multinational program of fusion experimentation, theoretical analysis, computer-aided analysis and materials technology.

Reference

1. President's Committee of Advisers on Science and Technology, *Report of the Fusion Review Panel* (July 1995), and references therein.

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