

LETTERS

Relativistic 'Reference Frame' Leads to Another Skirmish in the Science Wars

My puzzlement about David Mermin's defense of Bruno Latour as a thinker on relativity, and his rebuke of Steven Weinberg and Alan Sokal as critics of what is now called science studies (PHYSICS TODAY, October 1997, page 11), is not much reduced as I read that his daughter Liz "has been in cultural studies for some years, is now in anthropology, and once taught a class at Harvard on relativity for nonscientists. . . ." I respect respect for a clever daughter, and will continue to respect my own, even

should she say something silly from time to time to her Harvard colleagues.

On the other hand, I find Liz's quoted exegesis of Latour as enigmatic as the rest of her father's argument. She describes relativism (to be sure, "in the old-fashioned anthropological sense," as she says) as a search for absolutes. He thinks science studies is a discipline "where objects and aims of inquiry have . . . an ambiguous and uncertain character." I thought I had learned, from philosopher Susan Haack, who learned it from C. S. Peirce, that real inquiry excludes just such objects and aims. After all, if the aims are ambiguous and uncertain, can the conclusions be far behind?

Anyway, Latour on relativity is not understood in science studies simply as commentary on social science. It is taken as deep thought on relativity and on physics. I've taught many students, fresh from courses in the sociol-

ogy of science, who think and say so. Nor has Latour, or anyone else I know of, said that he is just being funny, and not interpreting or criticizing scientific knowledge. If, as Mermin writes (after mining the Latour paper for meaning), "Latour's first two sentences [of a passage quoted by Mermin] provide an exemplary encapsulation of the essential core of relativity," that does not excuse the mistakes. Such errors give physics a bad name with people who don't know physics (that is, most of them). And these days, almost any serious reader can state "the essential core" of relativity in a sentence or two. Such statements are thick upon the ground. Professors like Latour do not rise to fame by repeating what was laid out long, long ago in *Popular Science*.

Mermin says that scientists are taking potshots at science studies

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without understanding its language. I must note, therefore, that the languages of constructivist, postmodernist, feminist and multiculturalist writing about literature—and the social sciences too—are pretentious, imprecise and misleading. If you doubt it, have a look at the new volume, *Literature Lost*, by John M. Ellis. It encapsulates “the essential core” of what hundreds of humanists are now decrying: the silliness of those languages. Such denunciations are emerging into print, albeit some humanists and social scientists suffer still in silence and fear of an academic juggernaut—of which the trendier kind of science studies is a small part.

As for Sokal’s “famous spoof” (in Mermin’s phrase), is it possible that Mermin thinks of it as a mere pot-shot, ineffective in showing that politicized, *haute-theoretical* science studies denigrates science in aid of politics or hermeneutics? If so, then I commend to his attention *Impostures intellectuelles*, by Sokal and Jean Bricmont and recently published in Paris, on the misreadings, misunderstandings and derogations of science by some of the heavy hitters in cultural studies: Jacques Lacan, Gilles Deleuze, Julia Kristeva, Jean Baudrillard, Paul Virilio and Latour. The resulting rage of the Paris papers should also be instructive.

It’s too bad that the academy, in its frenetic search for novelty (and dissertation topics), was seduced by such stuff 20 years ago; too bad also for our daughters. Still, it didn’t matter to scientists as long as science studies commandos were knocking off the humanistic old guard and taking over academic departments. But it should matter, and it has begun to matter, to science that putative experts—some (but not all) of whom are hostile to science, distort its history and fail to understand its content—now present arguments to nonscience students (and ultimately thereby to the public) that science enjoys an undeserved and dangerously high standing in the public mind.

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Judging by N. David Mermin’s column, one could conclude that he has become persuaded, after all, by those who initiated the so-called science wars. His previous PHYSICS TODAY columns (see, for example, April 1996, page 11) exposed authors who distorted science to further what were

essentially ideological viewpoints. It is ironic to see that Mermin is now rallying to the defense of Bruno Latour’s paper entitled “A Relativistic Account of Einstein’s Relativity,” which has been substantially criticized by Alan Sokal, Steven Weinberg and others. I believe that Mermin gives far more credit to Latour than is deserved.

Mermin’s fundamental objection to Latour’s scientific critics is that, even though Latour may be guilty of “misconstruing the content of relativity and [making] elementary technical mistakes,” those critics have misinterpreted the chief aim of his paper. Mermin invokes the reported opinion of his daughter, now an anthropologist, that Latour is really trying only to extract a lesson for sociologists, based on a concept of relativism that Latour believes Einstein’s text inspires.

Although it is true that Latour tries to draw conclusions about the practice of sociology, he also attempts to demonstrate from his reading of Einstein’s book that even a “formal science” is “social through and through.” Upon careful study, it becomes evident that Latour is saying rather plainly that one of his aims is to show that even a theory as abstract as relativity can be described as a “social” construct, along the lines of the “strong programme” advocated by the Edinburgh school of the sociology of science. This discussion occupies roughly half of Latour’s paper and, in my opinion, cannot be idly dismissed. Mermin’s implication that the paper is primarily intended as a lesson for sociologists is misleading, as Latour himself asserts that this is only half his aim.

Although, as Mermin has pointed out (through his daughter), Latour has indeed attempted to extract a sociology lesson from special relativity, a substantial fraction of Latour’s paper is aimed at the “social content” of special relativity. Sokal’s and Weinberg’s criticisms are directed at this other aim of Latour’s endeavor, and, very appropriately, they address Latour’s evident misunderstandings of physics, which, in turn, infect his whole analysis.

Mermin defends Latour’s statement that “Einstein takes the instruments [clocks and rulers] to be what generates [Latour’s emphasis] time and space” as being part of what Mermin calls “an exemplary encapsulation of the essential core of relativity.” In point of fact, Latour’s phrase is not an accurate reading of Einstein’s text. A more appropriate translation from the German of Einstein’s original statement is that Einstein took the “placement of clock hands” (*Zeiger-*

stellung) to be “time indicators” (*Zeitangabe*). It is quite a distortion to extract from this that a clock “generates” time. As far as I can tell, the form used by Latour is an attempt to reinforce his assertion about the “social” nature of relativity. It also begs the question of what meaning one can draw about the “social content” of a formal theory from a semiotic reading of a lay text in translation (Latour worked from a translation of Einstein’s original).

Another point raised by Mermin in Latour’s defense is the issue of “fun.” He quotes Latour’s playfulness with words at various points, and I guess we are supposed to infer that the harmless puns exempt Latour from a serious critique of his ideas. Yet, Mermin doesn’t bring to light any examples of how Latour’s “fun” is being misinterpreted by physicists. Nor, upon a careful reading, is it difficult to distinguish between Latour’s occasional attempts at humor and the parts of his paper in which he tries to make serious points. So the issue of wordplay is also a diversion.

In sum, I am highly puzzled by Mermin’s attempt to both rescue Latour and denigrate Latour’s critics. Mermin’s arguments in defense of Latour are quite unconvincing, whereas Sokal and Weinberg have pointed out serious problems with Latour’s interpretations. In trying to bridge the evident gap between scientists and sociologists of science, Mermin would have done better to have found a more defensible position. I fear that this column of his will only intensify the existing schisms between the science and science studies cultures.

Reference

1. B. Latour, *Social Studies of Science* 18, 3 (1988).

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David Mermin chides some of us who would defend science from the onslaught of the “New Sociology of Science” (NSS) and more generally from postmodern ideology. Unfortunately, his criticism places him as a virtual fifth columnist in what are now being called the science wars.

The NSS arose mostly in the 1960s and 1970s as an attempt to give a coherent account of the progression of scientific knowledge, with emphasis on its being a highly nonlinear process involving occasional jumps (paradigm shifts, scientific revolutions and so forth). However, in the process of explaining this epistemic evolu-

tion, the NSSers confused it with epistemic relativism and ended up questioning the possibility of science attaining any degree of objective truth. Rather than viewing the progress of science as a nonlinear process yielding a converging series of partial truths about the world, the present-day NSSers view science as little more than a kind of superstructure—a highly subjective human construct in which reality has all but evaporated.

Clearly, if the NSSers in general and Mermin's example of Bruno Latour in particular have managed to reduce science to a subjective experience, they have cleverly exploited important philosophical weaknesses in much of scientific writing. Especially damaging is the continued presence of the dusty philosophy of positivism—even of its extreme form, operationalism—which introduces subjectivity (clocks, measuring rods, observers) right into the heart of physical theory. No matter how heuristically useful these subjective means may be in describing or even in developing new physical insights and theories, such subjectivism must ultimately be eliminated and theories must be formulated in purely objective—that is, physical and observer-free terms.

Relativity is a theory neither about clocks nor about observers. Rather, it is theory about objective events in spacetime. Mario Bunge's *Foundations of Physics*¹ is a classic example of how the axiomatization of physical theories can systematically achieve the elimination of subjectivism. When Mermin praises Latour for asserting that in relativity "Einstein takes the [measuring] instruments to be what generate space and time" (Latour's emphasis), the science wars have already been lost. If Mermin and Latour are right, and the spacetime underpinnings of reality are indeed subjectively "generated" in this way, then the physicists may as well go home and let the sociologists get the superstructure right.

Reference

1. M. Bunge, *Foundations of Physics*, Springer, New York (1967).

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MERMIN REPLIES: All three writers disparage the Latour passage I praised. Paul Gross says it is a banal truism—but I have been teaching relativity to nonscientists since 1965 and can assure him that comparably eloquent formulations are rare indeed in the popular literature. John Huth

says it is a mistranslation from the original German—but Latour was giving his own reading of Einstein's story, not paraphrasing him. And Shaun Lovejoy says that if it is accurate, then subjectivity has triumphed and the science wars are lost—but there is nothing subjective about a clock or a measuring rod.

In commenting on my April 1996 PHYSICS TODAY column, Huth is misreading me if he thinks I accused Harry Collins and Trevor Pinch of distorting relativity for ideological purposes in their book *The Golem*. Although I did suggest that they had used certain rhetorical tricks to bolster their case, my primary criticism was that their account of the acceptance of special relativity supported their theory of scientific knowledge only because that account was incomplete. I did not suggest (and do not believe) that they had tried deliberately to deceive their readers.

Latour is harder to criticize, since he glories in word play and ambiguity. Indeed, it is hard to imagine that Gross's students could have come away from Latour's essay with any sense at all of what relativity might be. We have a strong program in science studies at Cornell, but in teaching relativity to hundreds of nonscientists, I have never found any of them to have been infected by exposure to Latour.

And I am puzzled by Gross's recommendation to take seriously only those disciplines in which the objects and aims of inquiry are certain and unambiguous. That would make for a pretty dull life of the mind. I wouldn't impose that constraint even on scientific inquiry.

The danger to science comes not from academics whose convoluted epistemological games lead to journal articles expounding complex counterintuitive claims about the character of scientific knowledge. It comes from those politicians and scientists whose oversimplified public visions of science inspire others to write op-ed pieces asserting, for example, that \$30 billion has been wasted on a war on cancer because the disease is still with us.¹ That is the kind of threat we ought to be worrying about, as Collins and Pinch admirably argued in another (better) part of their book.

Neither Huth nor Gross understand why Latour's wit ought to be mentioned in a critical assessment of his article. If he had written a poem about relativity, his scientific critics would surely have remarked upon his choice of genre. His essay is closer to poetry than to *Physical Review Letters*. Not to note that he delights in playing verbal games with

Einstein's little text is to mislead those who have not read him themselves, and to lose the confidence of those who have.

Appropos of genre, although it would be inappropriate in a scholarly text to speak through a fictitious colleague named Mozart or hand the page over, with an outpouring of fatherly pride, to one's daughter, PHYSICS TODAY's "Reference Frame" columns are a different art form. I share Gross's interest in the education of our children. How Latour is actually read by one who knows a little relativity and a lot of cultural studies is germane. My daughter's reading of Latour is neither obscure nor befuddled. Gross gives us a fine demonstration of a technique he practices in his book,² making her sound silly by reducing her "although things appear differently from different perspectives, certain things do remain the same" to "she describes relativism . . . as a search for absolutes." He seems unaware that her formulation of cultural relativism is also an accurate characterization of relativistic physics, where invariant quantities play a fundamental role.

Gross commends Sokal and Bricmont to me. In fact, a preprint of their chapter on Latour solidified my worry that we scientists are stating our concerns counterproductively. Whom are we trying to convince? If only each other, then we can chuckle over amusing errors or absurd misunderstandings, without regard to context and without making the effort to ascertain what points these people might be trying to make, before we dismiss them as frauds. But if we want to have a serious exchange with scholars or students of intellect and integrity who do not deplore the ascendance of "theory" in the humanities, then we will have to do better than that. Otherwise the people we ought to be educating will answer us as Socrates answered Polus: "What's this, Polus? You're laughing? Is this yet another kind of refutation, which has you laughing at ideas rather than proving them wrong?"³

References

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3. Plato, *Gorgias* 473E, Robin Waterfield, trans., Oxford U. P., New York (1994).

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