

a naïve realist, philosophically speaking, and getting together into communities (for warranting or any other purposes) only makes them more so.

The "realist" scientific method is to ask which view is more effective. No contest here, either, in my opinion; the realist program never lets you down as a way of increasing knowledge, does it? Whereas history is littered with catastrophic failures to make things true by institutional fiat.

Of course these arguments only apply to science itself. I am quite prepared to believe that thought in sociology is entirely culturally determined. Berger illustrates this rather neatly when he asserts that "trust" and "credit" are financial metaphors. Only in the U.S. of A.!

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**Response:** The letters columns of *Science* seem hardly the place for extended epistemological debate, so I will try to be brief. To Hagan let me say, first, that my reference to "culture wars" was not to one between natural scientists and social scientists (if there is such a war, there is no good reason for it), but to the one between academic traditionalists and academic avant-gardes, regardless of discipline. Second, I did not say, or even imply, that the extreme views of Gross and Levitt are representative of mainstream science. I have no reason to believe they are and, like Hagan, I hope they are not.

Bauer says that *Science's* readers deserve a "second opinion," which he provides, and there are third and fourth opinions by Bunge and Coulson. None of these, however, does much more than restate what Gross and Levitt have already argued more forcefully. There is no issue between us of the credibility of scientific findings, only about the foundations of the credibility. The issue is epistemological, and as in Gross and Levitt's book, no epistemological arguments are made in these several letters. It may surprise, even comfort, Bauer to learn that, like him, I believe that a real world (physical and social) exists out there that "constrains what we can do," but this "realism" (?) of mine in no way weakens the skeptical relativism that sees in these constraints sources that not only enable but also obstruct our efforts to obtain reliable information about the world. This "relativist critic" sees little or no "happencstance" in the achievements of science; the social world, like the physical one, is real in its constraints.

Nor is the question of hypocrisy, or bad faith (raised explicitly by Bauer and implied by Coulson), relevant here. I thought I explained clearly enough in the review it-

self how a belief in the credibility of empirical findings could be sustained with a relativist epistemology. Unlike Bunge, whose letter indicates a sensibility immune to dialog, Coulson has some wit working for him. It may surprise him to learn that there are sociologists whose realism matches his and the rigor of whose research methods would win his approval, just as it surprises me to learn that "trust" and "credit" have no financial meanings in Scotland.

Still, it's good to discover that there are real readers (even when hostile) out there. Sociologists are not often so fortunate with feedback when publishing in our own journals. But it amazes me that scholars (those in science studies, for example) with relatively low prestige in university hierarchies have been able to evoke such threatened responses from those so much more powerfully placed in the academic order of precedence. In a sense it's sort of flattering that we little guys should be perceived as dangerous by so much bigger fish in the academic sea. Yet, as that king of Siam said, it's a puzzlement why this group of science studies researchers—with its very small constituency and its utter failure to have any impact on working scientists—should be found to be so threatening. Could it have something to do with the somewhat lowered prestige of science (like that of most established institutions) in recent years and its severely cut funding? Bad times and tight budgets often generate irritable dispositions and the search for scapegoats. In fact, we academics are in a business that trades in knowledge and prestige and in the prestige of knowledge. Our differences are minor, trivial, compared with the criteria (logic, evidence, and other rhetorics) that sustain us in the common enterprise of finding truths. The enemies of this enterprise reside less often within the academy than outside of it. Letters complaining in the name of science about a moderate critique of an extremist book might better be directed at newspapers that daily print astrology columns or at scientists whose work for certain companies (oil, tobacco, chemical, and so forth) has done far more damage to the credibility of science than the piddling efforts of a few professors of literature, history, sociology, and philosophy.

**Bennett M. Berger**

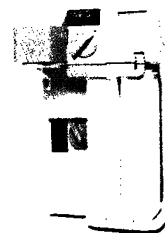
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#### Corrections and Clarifications

Marcia Barinaga's 1 July Research News article "Knockout mice: Round two" (p. 26), did not mention that Alexandra Joyner's collaborator on the brain cell fate mapping experiment is Eric Mercer, in David Anderson's laboratory at Caltech.

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