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LETTERS

Parasitology Issues

The extensive coverage given to parasitology in the issue of 24 June (pp. 1857–1886) was an important, positive gesture, and the articles and news items were informative and well presented. The topics discussed are among those which represent major directions of research and development in this field and are well suited to attract the attention of the wider scientific community. What neither the introductory editorial (p. 1827) nor the rest of the issue makes clear to the nonspecialist reader, however, is that parasitology is more than tropical medicine and hygiene, more than the study of molecular biology of pathogens causing tropical diseases. Parasites are also of major public health importance in the temperate zones. Waterborne parasitic infections (giardiasis and cryptosporidiosis, for example) represent significant challenges for municipalities, while toxoplasmosis and microsporidiosis are sadly prevalent in immunocompromised patients with, primarily but not exclusively, acquired immunodeficiency disorder. Research and development in these areas is of critical importance. The significance and economic role of veterinary parasites should also be mentioned. Research on the latter is economically rewarding for the pharmaceutical industry, with spinoffs for human parasitology. The successful fight against African river blindness was made possible by the generous free supply of a drug that has been developed for the veterinary field and has been successfully marketed.

As the special issue makes clear, a formerly unexpected plethora of unusual mechanisms of cellular processes is revealed in parasitic organisms. Study of the biology of parasitic organisms provides an insight into the limits of specialization of eukaryotic cells. While the expression and processing of genetic information in parasitic organisms are of interest, one should be aware that the diversity of many other aspects of their organization is equally pronounced. It remains to be established which of their peculiarities represent adaptive changes elicited by a parasitic mode of life and which are relics of their earlier evolutionary history, necessarily encompassing free living ancestral forms. Some major parasitic protists are probably descendants of the earliest, possibly premitochondrial, branches of the eukaryotic tree. These organisms might harbor clues about what the earliest eukaryotes looked like. Biochemical and cell biological studies clearly show that mitochondria are not obligatory constituents of eukaryotic cells. They also disclose the existence of unusual organelles of metabolism (glycosomes and hydrogenosomes) and unusual metabolic processes in certain groups of parasites. These results demonstrate that the eukaryotic cell mode of life is much less stereotyped than hitherto assumed. Further studies of parasitic organisms thus promise a clearer view of eukaryotic evolution in addition to benefits to human and veterinary medicine.

Miklos Müller
Laboratory of Biochemical Parasitology,
Rockefeller University,
1230 York Avenue,
New York, NY 10021–6399, USA

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"Culture Wars"

Bennett M. Berger's critique (Book Reviews, 13 May, p. 985) of *Higher Superstition: The Academic Left and Its Quarrels with Science* by Paul R. Gross and Norman Levitt (Johns Hopkins Univ. Press, Baltimore, MD, 1994) effectively neutralizes the polemic of authors Gross and Levitt. Nevertheless, the review and its reference to "culture wars" can only exacerbate the perceived discord between social scientists and the scientific disciplines they study. To the extent that he portrays the extreme views of Gross and Levitt as representative of mainstream science, Berger offers a caricature that is as inaccurate as the leftist, antiscience bias attacked by the authors. Rational discourse requires mutual respect born of a desire to unite these divergent cultures.

My dual hard-soft (wet-dry?) graduate training in chemistry and science and technology studies at Rensselaer Polytechnic Institute has made me painfully conscious of the gap that often divides the social sciences and humanities from the physical and biological sciences. Instead of hostility, the prevailing relationship is benign indifference. Natural scientists, barely aware of the existence or content of science studies, do not bother to question the legitimacy of such scholarship; it is considered irrelevant to the practice of science. If history, philosophy, and sociology are ever to be regarded as fundamental rather than "ornamental,"

better communication with the practitioners of science must prevail.

William J. Hagan Jr.
Department of Physical and Biological Sciences, College of St. Rose, Albany, NY 12203–1419, USA

Your readers deserve at least a second opinion, preferably several, about *Higher Superstition* by Gross and Levitt. The review argues against the authors without giving readers an adequate sense, which any review ought to provide, of what is actually in the book: for example, whether there are satisfactory notes and index. In fact, the authors fully document all their criticisms, among them that the authors they criticize do indeed appear to be poorly versed in the science they write about. That would seem to be sufficient grounds for scientists, or for that matter anyone else, to display some outrage and indulge in some polemics.

Among the approaches attacked in the book is extreme relativism. It struck me therefore as more than a little unfair that the book was reviewed by, as the reviewer himself states, a relativist who is the friend of one author criticized in the book and the colleague of another.

The reviewer is careful to remark, quite properly and necessarily, that social constructivism is only "one variety of the relativisms opposing realism." But he himself then lumps together all the disparate varieties of *realisms*, of which there at least as many flavors as there are of relativisms. If there is a single notion common to all realists, it would be that there exists a real physical world that constrains what we can do, and that those constraints enable us to get some unequivocally reliable information about how the real world really works.

Relativists appear to deny that unambiguous knowledge about the real world is available. To social scientists that seems only natural, of course, because their disciplines harbor, as Berger puts it, "plural and diverse" "warranting communities." In plainer English, that means equally competent and distinguished sociologists often disagree with one another over how to understand any given social phenomenon. Relativist critics of the natural sciences would have it that the almost universal consensus enjoyed by the natural sciences is a happenstance brought about by social interactions rather than an inevitability imposed by the dictates of Nature as to what works and what doesn't.

The inconsistency, not to say hypocrisy, of the relativists' position lies in their insistence in general and in theory that the natural sciences have no certainty to offer, while in specifics and in practice their actions expose that they too believe that what textbook science (*1*) says is operationally true.

Henry H. Bauer
Department of Chemistry, Virginia Polytechnic Institute and State University, Blacksburg, VA 24061–0212, USA

References and Notes

1. It is of course necessary to distinguish between frontier science and well-established science; see H. H. Bauer, *Scientific Literacy and the Myth of the Scientific Method* (Univ. of Illinois Press, Chicago and Urbana, IL, 1992), especially chapters 3 and 6.

I was appalled to see Bennett M. Berger's negative review of Gross and Levitt's *Higher Superstition*. This book, written by a scientist and a mathematician, exposes some of the garbage that is presently being manufactured in our universities, in particular the grotesque distortions of science involved in the constructivist-relativist anthropology, sociology, and philosophy of science. The book tells the truth about this fad: that it is produced by people who ignore the ABC's of science and who, moreover, are hostile to it and, in some cases, to reason as well.

The author of the review does not hide his sympathy for this branch of pseudoscience. He even describes Bruno Latour's work as "sober ethnography," when one of Latour's central theses (*1*) is that doing science is just "making inscriptions," which is of course the only thing a nonscientist can see when visiting a laboratory, for he is not equipped to understand what those "inscriptions" mean or why they are being made. Incidentally, one of the feats of that same "sober" scholar is to have "proved," through text analysis, that Einstein's inaugural paper on special relativity should not have been titled, "On the electrodynamics of moving bodies," but rather "New instructions for bringing back long-distance scientific travelers" (*2*).

Mario Bunge
Foundations and Philosophy of Science Unit, McGill University, Montreal, Québec, Canada H3A 1W7

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1. B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Sage, London, 1979).
2. B. Latour, *Soc. Stud. Sci.* **18**, 3 (1988).

In his review of *Higher Superstition*, Bennett M. Berger says that he knows "of no scientific method for 'proving' the preferability of [the realist or relativist] view" of the basis of scientific truth. So let me provide him with one; or rather with two—one for relativists, the other for realists.

If I've got this straight, the relativist would ask what the "warranting communities" prefer. No contest here; there isn't a practicing scientist in the world who is not

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.!

A. F. W. Coulson
Institute of Cell and Molecular Biology, Division of Biological Sciences, University of Edinburgh, Edinburgh EH9 3JR, Scotland, UK

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 "happenstance" 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 itself 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
Department of Sociology, University of California, San Diego, La Jolla, CA 92093–0102, USA

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.