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Sir — In his review of the book *Intellectual Impostures*, Richard Dawkins makes the astonishing claim that the authors, Alan Sokal and Jean Bricmont, "expose Bruno Latour's confusion of relativity with relativism" (*Nature* **394,** 141–143; 1998).

Here is what Sokal and Bricmont say about this matter in their book: "Finally, Latour draws an eminently sensible distinction between 'relativism' and 'relativity': in the former, points of view are subjective and irreconcilable; in the latter, space-time coordinates can be transformed unambiguously between reference frames."

Perhaps, after Dawkins explains how "Latour draws an eminently sensible distinction" became "Latour's confusion", he will tell us why he feels entitled to claim that there is a confusion in an essay of Latour he shows no sign of having read. He could hardly have done so without noticing that Latour, in fact, belabours the distinction between relativity = objectivity (good) and relativism = no objectivity (bad). Is Dawkins really talking about Bruno Latour?

The review is shot through with misinformation. I believe Dawkins is sincere. But he has failed badly to take the measure of the texts and people he talks about, both those he favours and those he does not. For example, an unhurried reading of page 8 of *Strange Weather* shows that he badly misunderstands Andrew Ross's ironic reference to how his lack of scientific training ("the science teachers I never had") determined what book he wrote ("it could only have been written without them"). Referring to arguments about the cultural authority of intellectuals that he developed in an earlier book, Ross explains: "My plan was to explore the reach of these arguments into the world of science and technology. As I lacked the training of a scientific intellectual and the accompanying faith, however vestigal or self-critical, in the certainties of the scientific method, it became clear that my point of identification could not be with 'high' scientific culture. My position, then, became that of a cultural critic examining the power and authority of the claims made for science and technology [and] the responses to these claims in the popular culture."

An equally attentive reading of page 113 of Sandra Harding's *The Science Question in Feminism* refutes the canard repeated by Dawkins about "a notorious feminist description of Newton's *Principia*" as a rape manual. The sexual metaphor in question — forced penetration of a female entity — is not twentieth-century feminist but, on Harding's reading of the literature, early modern scientific. She believes it probably was fruitful for early modern scientific enquiry and conceptualization, hence her ironic observation that "rape manual" seems as apt a metaphor for the early modern conception of Newton's laws as is "mechanics".

Dawkins would like works such as *Intellectual Impostures* to reach a wider audience. So would I. But, unlike Dawkins, I want them to be read sceptically and held to the strict standards of evidence and logic that he and the authors of these works invoke even as they abuse them. By these standards, "it makes me laugh" and "I don't get it" are not arguments. *Nature* should start enforcing these standards too. "Science as a cultural construct", and the letters it generated, came close to meeting these standards (*Nature* **386,** 545–547; 1997). This made it possible for important issues to be discussed in a thoughtful way. The same cannot be said of Dawkins' review or the works that he recommends in it.

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Dawkins replies — It is surely only in a postmodern climate that anyone could seriously feel the *need* to distinguish relativity from relativism. But, yes, Latour does indeed state the obvious explicitly. The trouble is that the good work is then all undone by his stated intention to show that "relativity itself could be said to be social", and his reading of Einstein's text as "a contribution to the sociology of delegation" (Sokal and Bricmont, page 115).

I'm sorry if I misunderstood Latour, Ross and Harding. But, for heaven's sake, isn't being misunderstood precisely what these people do for a living?

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Sir — Once again, you have not hesitated to expose my 'errors' to your readers, devoting significant space to beating the drum about my second Ig Nobel prize, awarded by ignorant self-appointed guardians of the purity of science (*Nature* **395,** 535; 1998). But your readers deserve an opportunity to judge for themselves our results and their theoretical consequences.

I refer them to our website, which contains a simple protocol for those who wish to duplicate our experiments (http://www.digibio.com). Put briefly, the current short-range electrostatic theory of molecule interaction–recognition via random collision cannot help us understand how biological reactions really work. By contrast, the presence in water of long-range electromagnetic fields, as proposed by quantum electrodynamics, sheds light on key features of biological systems. We can now record specific electromagnetic signals.

Isn't it time to open the door to genuine scientific debate? Based on my own painful ten-year experience, we may as well start by throwing out the peer- (or is it pyre?) review system, which has become, behind a facade of excellence, the main antibody blocking the nearly deceased corpus called scientific free exchange, which once was the cornerstone of progress. It condemns any advance that is "hard to reconcile with what we know..." (Dudley Herschbach), a position which is the negation of scientific research. Surely this attitude, reflected in the scientific press, is the clearest indicator of the urgent need for uncensored media.

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Sir — As they try to dispel the "myth of market prices", Don Fullerton and Robert Stavins argue that economists are not exclusively concerned with the financial value of things ("How economists see the environment", *Nature* **395,** 433–434; 1998).

They say, for example, that the value of damages to health from environmental pollution not only includes healthcare costs and wage losses, but also "pain and suffering". The goal of economic analysis being to measure the total value of the loss that individuals incur, "economists insist on trying to convert all these disparate values into monetary terms because a common unit of measure is needed to be able to add them up".

But, unfortunately, neither a disappeared species nor a deceased beloved one can be bought back to life. What is the point of pricing things that no one can buy or sell? Doing so is erroneous and dangerous. It is easily understandable how the need for rational policies leads to the kind of analysis described by Fullerton and Stavins. However, the world does not fit entirely in the concepts of economic science, and sometimes rational decisions are simply not possible. In those cases, clearly stated moral principles, not shaky science, should guide decision-makers.

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