Science and the Understanding of Science: A Reply to Schneider

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Although I do not necessarily agree with everything that Stephen Schneider (2001) has said here, I am very pleased he has made the effort to constructively engage my analysis, "The Construction of Global Warming and the Politics of Science" (Demeritt 2001b). I am pleased also that we seem to agree broadly about the importance of more open public discussion to understanding better the values of climate change science. As a social practice, science involves not only certain normative values, judgments, and social relations "upstream" among its practitioners, but also more obviously political questions about the instrumental values (and material provision necessary to achieve those objectives) that its applications are to serve "downstream" in society. Unlike some scientists, Schneider is quick to acknowledge that science is value-laden in both these senses. Though Schneider and I disagree somewhat about the precise philosophical and political implications of that realization, we do agree that it demands a more nuanced understanding of scientific knowledge than the simplistic oppositions between fact and value, science and politics, objective and subjective, upstream and downstream, that have long structured public debate about global warming and other environmental problems. I am hopeful that this exchange will help spark the kind of public dialogue about science and the understanding of science that we agree is long overdue.

By publishing in the *Annals*, my aim was to foster such a discussion. In these days of academic specialization, the *Annals* provides a rare forum for addressing, not just a broad spectrum of human geographers and others working in the human sciences, but also physical geographers and other natural scientists. Interdisciplinary communication is notoriously difficult. Different disciplinary communities have their own working assumptions and expectations, technical terminologies and shorthands, methods of research, styles of presentation, and standards of evaluation. Without an insider's experience, outsiders often find it difficult to understand and evaluate specialist debates. In my article, I tried to call attention to some of the informal working assumptions and practices of climate modelers by retracing the history of several scientific controversies. The problems at issue in debates about the construction and validation of climate models are much better recognized upstream among climate modelers themselves than downstream by other research scientists, impact-assessment experts, science advisors, policy makers, and political interests interested in predicting and managing global warming. I tried to explore the political implications both of modelers' own modeling practices and of the ways in which those practices are understood, and to some extent shaped, by the interactions of modelers and their models within this wider epistemic community of scientists and policy advisors.

As Schneider's response rightly observes, my own analysis was itself reliant on a repertoire of specialist terms and theories drawn from recent work in critical human geography and in particular from science and technology studies (STS). This somewhat heterogeneous body of work has challenged the self-image of science as an epistemologically objective and value-free study of the self-evident (once discovered) facts of a real and ontologically objective world. This umbrella of metaphysical beliefs is common among practicing scientists, as Schneider himself notes. Scientists have often reacted with hostility to the claims made about science by STS.

One of the key terms in those debates about science is "construction." Along with its various cognates—constructed, constructionism, deconstruction, etc.—construction is often applied, opposed, or both to "science," "objectivity," "facts," "nature," and "reality." These terms describe difficult concepts with long and complex histories (Daston 1992; Demeritt 1998, 2001b), but they have been thrown around in some quite different and often very imprecise ways. Ian Hacking (1999, 22–23) calls words such as "fact," "truth," and "reality" "elevator words" because they tend to work at a number of different levels in philosophical debates. Facts, truths, and reality are not simply things in the world like rocks or trees. They are also used abstractly to describe the nature of the world itself and to characterize our knowledge of it. Thus, the correspondence theory of truth holds that true propositions reflect the world as it really is (Rorty 1979). Notice the circularity that this definition involves. One

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of the problems with elevator words is that they are defined in terms of one another. Thus, the *Webster's New Collegiate Dictionary* (1977) on my desk defines "fact" as "a piece of information having objective reality" and then gives "in fact" as one definition for "reality."

Partly as a result of this reliance on imprecise and polemical "elevator words," participants in debates about science have tended to talk past each other. Indeed, Hacking (1999, 1) suggests that, rather than designating a precise doctrine, the term "construction" is now most often used to declare what "side" one is on: "[I]f you use it favorably, you deem yourself rather radical. If you trash the phrase, you declare that you are rational, reasonable, and respectable." In this context, it is hardly surprising that the tone of recent debate about constructionism and science has degenerated into a "war," with everything that this metaphor implies about the resulting divisiveness, hostility, and destruction. Like me, Schneider is dissatisfied with these bombastic debates, because important issues and significant shades of grey have been lost amidst the artificial polarization into opposing "sides."

Thus my paper had two objectives, the first and more general one concerning the understanding of constructionism and science and the second concerning the specifics of climate change. Where Schneider takes most issue with me is over my more specific empirical claims about the science of climate change. Both to put those disputed claims in context and to outline the substantial areas of agreement between us, I think it is helpful to say something first about my more general objectives. In reviewing various theories of social constructionism, my intention was to clarify what is at stake in different understandings of the term. With the idea of "heterogeneous constructionism," I tried to formulate a middle-ground position between the extreme relativist's denial of the ontological existence of the world and the naive realist's belief in the self-evidence of its appearance through empirical observation.

Such philosophical discussion can be hard going, but it is important because constructionist claims, no less than those made on behalf of science, rest on a philosophical foundation of some sort. Unfortunately, climate scientists have not always been as aware as perhaps they should be about the philosophical commitments their methods and language imply. Despite the well-recognized philosophical criticisms of verificationism (Oreskes, Shrader-Frechette, and Belitz 1994), it is still somewhat common for modelers to describe their models as having been "verified." To some extent these are simply cases of sloppy usage, but they also point to an important philosophical and political dispute about how to understand the warranting of computer simulation models of the climate.

In the case of climate change, as in other areas of scientifically uncertain environmental risk, the Republican opponents of environmental regulation have sought to impose high standards of scientific certainty before requiring preventive action (Brown 1996). In effect, as Schneider notes, this standard involves prioritizing the avoidance of type 1 (false positive) errors, in which action is taken when there is no actual risk, over type 2 (rejecting true positive) errors, where action is deferred for want of definitive proof of its necessity. The scientific and policy preference for one kind of uncertainty over another is value-laden, but—as I tried to emphasize—uncertainty is by no means the only important aspect of scientific knowledge that has a bearing on understanding either that knowledge or any political responses to it. The prevailing emphasis on scientific certainty is often accompanied by a belief in the superiority of scientific knowledge to other ways of understanding and being in the world. Failure to acknowledge the partiality of scientific knowledge can lead to conflict with other people who steer by different lights.

Dogmatically insisting that "the climate projection problem *is* global," Schneider (2001, 342) finds it difficult to understand how other people might not accept the validity, relevance, or legitimacy of his physically reductionist approach to understanding global warming. Global-scale and physically reductionist modeling may well be the "most credible" method for "predicting the climate response to given forcing scenarios." However, given the indeterminacy of the emissions scenarios and the narrow range of possible scenarios considered by modeling experiments to date I remain critical of the somewhat deterministic tone with which modeling results are often presented publicly. Only a few critics of scientific reductionism actually dispute the truth and validity of this method for understanding the potential feedbacks between increasing greenhouse gas concentrations and other components. Instead, most are concerned more with the relevance, legitimacy, and purpose of concentrating so much attention on asking those questions about global warming at the expense of other, more explicitly political ways of formulating the problem.

There are complex issues here, simultaneously epistemological and political, about the basis for warranting scientific claims and taking political action. Traditionally, science and politics have been regarded as absolutely separate domains. Schneider (2001, 339) recognizes that "values are certainly involved in [applied] science for policy" but is more hesitant to acknowledge their influence in the realm of pure science. There he suggests that, given sufficient research resources and time, "heterodox ideas that eventually demonstrate 'ob-

jective reality' surface after some battling" (Schneider 2001, 340). By contrast, I am much more skeptical about any categorical distinctions between pure science, applied science for policy, and politics, not least because of Schneider's caveats about time and research resources, which tend to influence the development of scientific research programs and to make the history of science—to borrow a term from nonlinear dynamics — "path-dependent."

Schneider's three suggestions on how the public should interrogate experts are much closer to my own conclusion about how we should understand science. In a sense, my argument about why we should believe in science and its knowledge of global warming is actually quite traditional. Rather than depending on some conception of objective truth and the vision of an Archimedian point above the fray from which that singular truth might be distinguished from biased and false claims, I tried to say, with Popper (1959) and Rorty (1991), that belief in scientific knowledge might rest instead upon our faith in the social processes of open scientific debate to deliver the best understanding we can do for the moment. If Popper and Rorty seem like strange companions, our surprise is a measure of how distorted the polarized debate about constructionism and science has become.

The second more specific goal of my paper was to explore the political effects of the way that climate science, and in particular the outputs of computer simulation models, are socially constructed and publicly represented. As should be clear from the foregoing remarks, my use of the term "construction" here does not reflect disbelief in model results as merely "constructed." Rather, it signals a concern for the social processes and practices from which they have emerged. For too long, climate models have been heralded as crystal balls that can, or should, yield indisputable answers. This is a problem that Schneider also bemoans, but he complains that I do not acknowledge sufficiently the concerted effort of climate modelers to dispel this misplaced faith with their frequent caveats and qualifications. Whether those caveats have succeeded in communicating the rich understandings of modelers to impact-assessment experts and others farther downstream is another matter.

Schneider finds my description of the heterogeneous world of climate change science to be at variance with his own personal experience. His long-standing experience as a modeler and widely published science advisor is certainly impressive, but cannot claim to be fully representative of the entire, highly heterogeneous science and policy community concerned with global climate change. In support of my claims I can only point to the documentary evidence, culled from several years of research, which I provided in my paper. There is not the time or space to rehearse here the extensive quotations I drew from the published work, informal conference discussions, and congressional testimony of leading climate scientists and policy advisors.

What seems to irritate Schneider most and to lie behind his charges of "overstatement" and "straw men" is his belief that I was charging scientists with having intentionally acted in bad faith. I make no such claim. Schneider consistently misreads my argument about the social process through which global warming was scientifically constructed in instrumental terms. For instance, he says that I intimate that "problems with 'flux corrections' . . . are deliberately obscured" (Schneider 2001, 341). Likewise, he glosses my argument about the political effects of considering climate change solely in terms of the globally scaled and universalizing physical abstractions of climate modeling as "a politically motivated framing by scientists" (Schneider 2001, 342). My primary concern in this article is not with the intentions of climate modelers but with the effects generated by their positioning within wider social and political processes. I tried to show that climate science is not monolithic but is broken up into different groupings—climate modelers, impact-assessment experts, remote sensing and paleoclimatological experts, and various policy advisory scientists. These groups stand in different relationships to each other. They come together bringing somewhat different understandings of, for instance, the meaning and purpose of GCM "experiments" and of their differing needs and expectations. Their interactions are conditioned by those understandings. Thus, my claim about flux correction is not that modelers sought to "cover up" underlying model errors by resorting to ad hoc corrections—modelers knew better than anyone the problems with their own models. Rather, it is that this practice reinforced the false confidence of those farther downstream, who in turn came to expect long-term model predictions that modelers then tried to satisfy, partly by resorting to the controversial practice of flux correction.

Where Schneider and I agree is on the need for more open, public discussion about science and the understanding thereof. A richer appreciation for the social processes of scientific knowledge construction will improve not only the resulting knowledge, by exposing unacknowledged assumptions and practices to critical scrutiny, but also the level of its public understanding and thus the quality of public debate about its implications.

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