A Constructive Deconstruction of Deconstructionists: A Response to Demeritt

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As a natural scientist working at the social interface (climate effects, impacts, and policy issues) and as the founder and editor of the interdisciplinary journal *Climatic Change*, I have long been swayed by the arguments of the Science and Technology Studies (S&TS) community that science is not the value-free, objective enterprise it often prides itself as being. Moreover, I have long accepted that not only is it obvious that policy choice is a highly normative enterprise, so too is analysis infused with values—but less so, since these are more of the "what do we choose to analyze" kind than the "which alternative do we choose" variety. Furthermore, even "objective science," Jacob Bronowski (1965, 62) noted decades ago, needs certain social conditions to flourish. Openness, respect for independence, and willingness to scrap favored preconceptions are among the traits he listed, but he left out a political alliance with those who fund the activities and implicitly—and sometimes too explicitly—choose what gets objectively measured. These strong and weak normative aspects of environmental sciences are hardly news, except for too many practitioners of science who simply do not read the S&TS literature. And when a few of them venture into this arcane world, most come away at best confused and at worst shaking their heads at what they often see as self-cited social theories devoid of real world knowledge of what scientists do. How can this situation get turned around?

This editorial essay is my attempt to address these issues, prompted by my reviewing for the *Annals* an article that appears in this issue (Demeritt 2001). As a three-decade participant in this topic, I eagerly went to work, but in the process found—and let me be very frank here—precisely the kinds of problems I think keep too much of the S&TS enterprise as a marginal intellectual backwater largely ignored by those very scientists who need most to hear the message. Before some readers think it, let me add that it is too simple to just dismiss the bypass of most S&TS literature by scientists as paradigmatic blindness on the part of scientists unaware of their value-laden shadows. The fault can be placed at least as accurately on the deconstructionists as on the deconstructed, even if the latter mostly do not recognize their fallen status.

As the editor of a peer-reviewed journal, I am not interested in giving a public review of this article. I did that in private along with the other referees, as it should be, and the author was quite responsive, even if not sufficiently so, as I will soon argue. Rather, I will use this article as a springboard to make some generalizations about how social studies of science can better influence science. I will base these generalizations on a few examples from Demeritt's work, not because I think it represents some archetypal example of the failings of the sociology of science field, but because I think I know the empirical story of climate science as well as anyone anywhere and thus can use that background and this article to help the S&TS community think about doing a little deconstruction of its own research and outreach style. I apologize in advance if I use too broad a brush, overgeneralize, and offend some in this community. I make no pretense to knowing the S&TS literature as a good practitioner should. So take this essay as it is intended to be: a constructive set of observations from an interested outsider and the perceptions of a sympathizer, and not as the ranting of a defensive paradigm-bound believer in "scientific objectivity."

In this connection, I have had the good fortune to learn from a number of people within the science and technology studies field, in particular Paul N. Edwards at the University of Michigan, Ann Arbor. After mentioning my effort here to him, I was sent several comments. I think it is important to state some of them before I begin my analysis, since he made clear that there are many intellectual cross-currents within the broadly constructed S&TS field and I am primarily addressing a single segment, deconstructionism. In his words (Edwards 2001):

> S&TS borrows and extends historical, sociological, and anthropological approaches. It contrasts with traditional history and philosophy of science, which tends to focus on individuals, their personal backgrounds and discoveries, and analysis of science as a rational enterprise. Instead, S&TS focuses on the *credibility, authority, and social role* of scientific knowledge. It explores the links between science and other sources of authority and knowledge, such as political institutions, cultural traditions, and "common-sense" understanding.

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> Much S&TS analysis has concluded that science acquires its credibility from socially embedded, culturally specific systems of norms and practices for warranting public knowledge (for a recent review, see Jasanoff et al. 1995). "Warranting" refers to the justification or validation of knowledge through such methods as public demonstration of experiments, independent replication of results, and credentialing of professional scientists. Only if warranting techniques achieve broad acceptance can they serve their purpose of justifying knowledge claims. S&TS examines how science and society together have created (and sometimes dismantled) general assent to the effectiveness of particular methods in generating truth (Shapin and Shaffer 1985; Jasanoff 1990; Cole 1992; Jasanoff 1996). In these important senses, S&TS concludes that scientific knowledge is socially constructed. For this reason, the S&TS viewpoint is often identified with a broader, sometimes radical epistemology known as "social constructivism." Although a few people may actually think that social agreement literally produces external reality, I regard such a belief as bordering on the insane. As Wittgenstein famously put it, thinking does not make it so. No social consensus that people can fly by flapping their arms will help anyone survive a swan dive from the World Trade Center. Nor would a social decree that the planetary climate either is or is not warming change anything about the actual future trend of global temperatures. However, this is not what most social constructivists believe.
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> Social constructivism is—or ought to be—a methodological principle for the empirical study of knowledge production. As methodology, it requires an agnostic stance toward the truth or falsity of any given knowledge claim. In other words, it does not begin by asking whether a particular claim is true or false; rather, it questions *why people believe*, or do not believe, that claim. Social constructivism represents an empirical investigation—as dispassionate and objective as any other empirical science, in principle—into the causes of belief. The fundamental argument is this: given that people in all times and places (including ourselves) have believed many things that later turned out to be false, truth itself cannot be the *cause* of any belief's credibility. (Edwards 2001)

I am indebted to Paul Edwards for helping me place at the outset of this essay a context of S&TS work, so that my critique and the suggestions that follow based on Demeritt's (2001) article will be in a broader perspective than I could have sketched from my limited knowledge of the S&TS literature. I will shortly argue that the same is true for S&TS analysts: they too need either to take on board scientists who know their fields well or to become so thoroughly steeped themselves in the applications they study that the empirical base from which theoretical inferences are drawn will be credible, and thus their conclusions will have a chance of influencing the broader scientific community.

Before addressing detailed concerns, let me begin with Demeritt's conclusions. Happily, I usually agree with his views. For example, he says that "a richer appreciation for the social processes of scientific knowledge construction is as important for scientists themselves as it is for the wider public . . ." (Demeritt 2001, 309). And, in discussing the use of climatic models' outputs by impact assessors or policy analysts who work downstream of the complex mathematical models, he rightly asserts that " . . . most scientists stand in a similar downstream relation to climate models as those of policy makers and the lay public: they are forced to put their faith in technical expertise that they do not fully understand" (Demeritt 2001, 309). I feel exactly the same way about my medical test reports. Moreover, Demeritt rightly laments that "[t]he discipline and expertise required to participate meaningfully in [these] scientific debates restricts not only who is authorized to speak but also what and how things can be spoken about" (Demeritt 2001, 314).

However, this lament is really a caricature, as the scientific community is filled with alternative expressions of analysis and conclusion. In fact, the public is often so confused by this cacophony and the media's dutiful reporting of polarized extreme views (or their attempts to "balance" the conclusions of a 500-scientist assessment with a few outlier Ph.D.s who say "It ain't so!") that political leaders ask groups such as the U.S. National Research Council or the Intergovernmental Panel on Climate Change (IPCC) to help society sort out where current consensus really lies. Is that a social construction? Of course! So what? The fact that political leaders and governments support the work of the IPCC, as well as the many national efforts to understand and forecast climate change, does not *by itself* prove that politicians or political agendas strongly influenced scientific results or scientific practice. The presence or absence—and the nature—of such influence are empirical issues that can only be addressed by careful and thorough research. The question is: how—if at all—did the social construction of climate science change the outcome of the answers from the assessment group?

Moreover, what other epistemology has a ghost of a chance of coming close to the credibility of this process? In my view, good S&TS work should compare the efficacy of various epistemologies in producing credible assessments—but I get ahead of myself. A "QED" after proving "values crept in" tells us little we did not already know. Values are certainly involved in the science for policy process (e.g., Ravetz 1986), whereby what to study is at least partially determined by the societal need for certain information (see Moss and Schneider 2000 for examples of this applied to IPCC assessments). However, as Edwards noted above, this fact does not imply that the scientific conclusions obtained were necessarily tainted because what was being analyzed was a social construction. To repeat: what we need is to find out how the construction may have altered what the public would otherwise have heard, and the relative credibility of any other alternative constructions. The mere existence of alternative paradigms that are downgraded in an assessment does not necessarily make the assessment noncredible. Maybe those paradigms have little evidence to support them? I agree that the scientific establishment can indeed be inward-looking, but over time heterodox ideas that eventually demonstrate "objective reality" surface after some battling. However, the scientific community is rightly reluctant to embrace contrarian views when they are supported by little more than assertion.

Nevertheless, Demeritt's point is true: outside of the scientific insiders, few can easily discern the weak normative aspects of assessment conclusions from the strong. That is why I am all for making scientists more aware of their biases, conscious and unconscious. I know firsthand the uneasiness I feel when I—like most of us—cannot easily discern how safe some common medicines or food treatments really are and have to trust assessment committees to tell me the two essential questions all scientifically literate people should ask experts (e.g., Schneider 1997a): "What can happen?" and "What are the odds it might happen?" I like to add a third question: "How do you know?" If one asks that question, it takes more than the typical twenty-second sound bite on the evening news program or five minutes of testimony to a congressional or parliamentary hearing to find out.

I would also like to recommend that specialists and nonspecialists alike ask anyone claiming skill in projecting events what the nature is of the odds being given—i.e., are they "frequentist" or "objective" probabilities, or, rather, Bayesian and "subjective" (e.g., Moss and Schneider 2000)? It is not easy to get many scientists to admit that nearly all "interesting science"—e.g., complex systems science in my prejudice—is way too complex to fully falsify, and that all such assessments about future events are necessarily subjective (but nonetheless expert when certain procedures are used—see, e.g., Edwards and Schneider 2001).

In a final quotation of a conclusion I support, Demeritt ends by saying:

> The proper response to public doubts is not to increase the public's technical knowledge about and therefore belief in the scientific facts of global warming. Rather, it should be to increase public understanding of and therefore trust in the social process through which those facts are scientifically determined. . . . Instead, scientific knowledge should be presented more conditionally as the best that we can do for the moment. (Demeritt 2001, 329)

I could not agree more. I have been writing this since my first popular book twenty-five years ago (Schneider and Mesirow 1976), and I continue it by teaching a freshman seminar on Environmental Literacy (Schneider 1997b). In that course, I stress the process into which science is embedded, not primarily the technical "facts" of science.

What bothers me about Demeritt's article, then, is neither the beginning nor his conclusions, which are reasonable and written in language accessible to even the narrowest natural scientist. It is what comes in the middle of the article, which appears to be typical of the style of many deconstructionist articles, that I believe turns most scientists off about learning from this kind of literature—literature that they, as Demeritt rightly asserts at the outset, need more than any other social group.

Let me organize this next part of my response into a series of topics with a few quotes to illustrate each.

(1) Impenetrable jargon. I concede that natural scientists are as guilty of impenetrable jargon as anyone in the S&TS community. However, when we in science want to reach a popular audience, it is *we* who must change our language to that of the audience; we cannot expect that they will come to us. For example, Demeritt writes: "The crucial difference between such heterogeneous constructivism and an even stronger idealist, or neo-Kantian, constructivism that is antirealist about both theories and entities (what we designate electrons has no independent ontological existence . . ." (Demeritt 2001, 311). After a dozen readings, I just gave up trying to fully understand that sentence. I appreciate Demeritt's deconstruction of many sociologists' "polemical conclusions" that science is wholly socially constructed, a position that encouraged the destructive "science wars." Let me not use polemics either, then: I recognize that the quoted sentence piece above was written for colleagues familiar with the concepts behind the jargon. However, I simply get lost in it, as do many of my colleagues who tell me they have tried to penetrate S&TS literature. There is a double problem here: there are too few attempts to bring S&TS concepts clearly to the natural scientific community in journals they read and in language they can follow without multiple rereads and missed jargon. *Climatic Change* would very much welcome more articles that manage this, such as one I solicited a few years back: Edwards (1996), which did get the attention of a portion of the natural science community. Please, more of that.

(2) Messages that lose credibility by being packaged with too many assertions at variance with most scientists' experience. I am afraid that I had this experience way too many times with Demeritt's article, as it cited nonrepresentative cases or seemed often not to resemble what I have observed for thirty years in the climate business. For example, in several places Demeritt asserts that the GCM (big computer models called general circulation models) community suppressed the alternative views held by empiricists who wanted to use historical analogs to estimate future changes: "the empirical-statistical and paleoclimatic analogue approaches . . . were marginalized within IPCC . . . as computer simulation modeling was adopted as the preferred method for projecting future climate changes" (Demeritt 2001, 315). This is true. However, Demeritt presents empiricism as losing to a socially powerful cabal of computer modelers, and only briefly discusses the actual demerits of the fully empirical approach. Historical analogs simply *cannot* accurately represent the response to unprecedented forcing of the climatic system that includes global greenhouse gases, regional aerosol and land-use patterns, and the feedbacks among all the climatic subsystems at the rapid scale at which they are being perturbed. Historical situations are essential to test the tools used to make future estimates—a systems version of "falsification"—but there are no precise analogs to the time-evolving what-if questions of global-change disturbances. The modeling community takes no glee in this, and the pages of IPCC reports drip with caveats about just such limitations.

Demeritt cites the fact that all models of complex systems must use parametric representations (parameterizations) of process that occur on scales below the "grain size" of the model, often via statistical methods. This is also true. However, he cites parameterization as a logical inconsistency of modelers' assertions about first principles that invalidates their claim to better credibility than wholly empirical methods. Thus, he implies that the rejection of historical analogs over GCMs as the preferred assessment method "suggests that some other influences are also involved" (Demeritt 2001, 320). If scientists did not fall over backwards to emphasize the problems with parameterization, then I would agree that this is a reasonable suspicion, although even a reasonable suspicion needs to be supported by good evidence to make a credible claim. However, anyone who has spent quality time at scientific meetings listening to talks or reading modelers' papers gets tired of all the caveats about sub-grid-scale processes not being explicitly treated. There is little "unmasking" to be done, as Demeritt asserts he is doing, since in my opinion the community already goes overboard doing their own unmasking; politicians have told me dozens of times that if we scientists don't stop caveating everything so much, they won't be able to get political support for strong action.

Demeritt's intimation that problems with "flux corrections" (a parameterization in some models) are deliberately obscured, or that modelers are hypocrites since they denigrate purely statistical approaches while using some embedded statistical relations themselves, would not be considered a balanced or reasonable critique by almost any climate scientist. It smacks of beating the modelers with their own caveats and then using it to support a social theory that does not fit very well the empirical case.

Personally, I want people with the insights of a David Demeritt to poke around climate science and help us to see implicit assumptions and hidden paradigms. However, to get the attention of such a community, the detailed critiques need to be representative and fair and the technical details recognizable, not coming across as caricatures. I realize that this is no trivial assignment. A sufficient empirical base is unlikely to be built spending one or two dozen months talking to a few climatologists, reading a small fraction of the literature, and attending very few actual meetings where climate assessments are constructed. A strong empirical base is essential, not only in order to learn the field well enough to be capable of offering well-grounded deconstructions and to build confident social theories, but in order to do so credibly to the climate scientists who are one of the prime target audiences of the effort. If it is asking too much to have an S&TS analyst not trained in climatology do this for him- or herself, then the analyst might consider coauthors who are empirically well grounded in the discipline. Moreover, publication of results should see editors choosing peer reviewers who know the scientific application as well as those who are familiar with the S&TS literature, if such reviewers are to fulfill their social function of maintaining high community standards in peer-reviewed publications. That is, peer review itself is a social construction, not a "truth machine", as argued by Edwards and Schneider (2001)—but one more likely to produce a closer approximation to "reality" than any other epistemology.

(3) Assertions undocumented empirically. Picking up on the parameterization issue discussed above, Demeritt asserts that GCM modelers use tuning techniques to obscure flaws and with larger purposes in mind: "The practice of flux adjustment also serves wider social functions. Flux adjustment makes the control run simulations of the present climate look realistic by covering up systematic errors" (Demeritt 2001, 326). However, Demeritt gives no empirical evidence than anyone has used flux corrections or other tuning techniques without many ca-

veats, oft-repeated in climate assessments. Furthermore, even less evidence is given to back up Demeritt's charge of pursuing a "wider social function"—presumably one of more credibility with politicians. Perhaps this statement is credible to some social theorists, but it has little empirical justification. In my view it is pure assertion—and a pretty damning one at that. This is just the type of statement that so many scientists find so objectionable in some S&TS literature (my own assertion, I admit, but one backed by considerable anecdotal experience).

Moreover, few scientists I know think very much about downstream "users" of their work, although I wish they did—I started *Climatic Change* twenty-five years ago precisely to foster such consciousness in the context of quality interdisciplinary science. Rather, most conventional scientists worry primarily about "type 1 errors"—will the forecast be wrong?—and often refuse even to provide subjective confidence estimations of their necessarily unfalsifiable-before-the-fact forecasts (see, e.g., Moss and Schneider 2000 for a fuller discussion of the reluctance of many natural scientists to offer subjective estimates of probability for projections of consequential events that are not well tested empirically). Thus, this statement of Demeritt's about cover-ups and social manipulation is also an example inconsistent with most modelers' experience.

He follows it shortly with the flat assertion that the modelers "need" this cover-up so that downstream users will turn to them even though "equally plausible scenarios can be generated in other ways," presumably by historical analogs (Demeritt 2001, 327). Again, he provides zero evidence to back up this very *im*plausible assertion, which ties in with the comparably implausible assertion that "the selection of particular GCM modeling techniques was . . . socially negotiated. It was influenced by scientific considerations of political desirability, which were in turn informed by the belief of policy makers of its technical practicality" (Demeritt 2001, 327). Given the "overcaveating" about which politicians have repeatedly complained to me (and I can give many cites to anyone interested of congressional testimonies where this is evidenced), and the reluctance of many modelers to make subjective probability assessments because they fear the type 1 error more than the type 2 error (society will pay the consequences if their forecast is right), this quoted assertion is simply upside down and backwards. No wonder it can be tough to get natural scientists to pay attention to the social studies of science, when assertions flow with little or no empirical grounding that is consistent with the mainstream experience of those scientists. I am sorry to be so harsh about this, but this kind of analysis will either have to get more credibly empirically grounded or remain marginalized.

(4) Unnecessary straw men. A problem related to Demeritt's overstatements or mischaracterizations of the behavior of most scientists is his use of too many straw men. Because acknowledged uncertainties occur everywhere, especially in the upstream estimates of what society will emit that might alter climate—something acknowledged strongly and often in IPCC assessments—Demeritt argues that forecasts cannot be deterministic, and that the GCM approach thus amounts to "environmental determinism" (Demeritt 2001, 318). Then the straw man is set up: "Such a deterministic understanding of climate change is both politically and scientifically impoverished" (Demeritt 2001, 318). If it were indeed asserted by the bulk of modelers or the IPCC that GCMs could produce credible deterministic single forecasts, such an assertion would be a gross misrepresentation. However, nearly all papers and certainly all assessments explicitly use multiple scenarios of emissions, land use, and other social projections. In fact, there is as much uncertainty in the projections of future warming from the social scenarios as in the spread in the range of projections from physical or biological systems uncertainties. Again, where is the empirical evidence that the community neglects this reality? Why set up such a straw man—except perhaps to support a social theory of the case at hand devoid of solid empiricism?

Demeritt also asserts that the global conception of climate built into the GCM framework is somehow socially driven to highlight the "universal and global-scale problem of atmospheric emissions" in an appeal to "the common and undifferentiated interests of a global citizenry" despite the very heterogeneous reality of who emits—mainly the rich, of course (Demeritt 2001, 312, 313). The latter is indeed true. However, the climate projection problem *is* global, since most greenhouse gasses are globalized within a year and since it has been shown conclusively that modeling the long-term climate and its many feedbacks requires global-scale treatment, as regional models per se cannot handle important global-scale feedback processes. That is simply as objective as the reality of climate science gets. To assert that a global conception of the problem is a politically motivated framing by scientists since it neglects the social injustice of heterogeneous emissions by different countries or income groups—despite the fact that greenhouse gas buildups are indeed global, as Demeritt properly notes—is a stretch beyond elastic limits.

This assertion is false in my experience for two reasons. First, most scientists are woefully unaware of the social context of the implications of their work and are too naïve to be politically conspiratorial. Second, as I have recently returned from the international climate negotiations in the Hague and from several IPCC meetings, I can assure the readers that there is no risk that the heterogeneities in who consumes more resources has been lost on poorer nations or populations within richer nations. Indeed, that is one of the major problems in reaching international consensus: the balance among efficiency, equity, and environmental protection.

So why set up a straw man of global conception when the reality is that most of the scientists involved think primarily about issues such as scales of diffusion and positive and negative feedbacks? Of course there is inequity in the world, but reframing the global warming issue as if the global component were secondary because the proposed alternative framing would call more attention to injustice is itself a social construction with little scientific relevance. Regional models or historical analogs simply will not be able to credibly project what might happen over time. Neither, I readily admit, might GCMs. However, as the dozens of assessments over the past three decades keep reaffirming—assessments increasingly populated with scientists from developing countries—they remain the best way to look at "what ifs."

Given the modeling "what ifs," usually presented as alternative possibilities under various physical, biological, and social assumptions, the political community then has the job of making the strong normative judgments about trading off equity, efficiency, and environment—all in the face of considerable uncertainty in each component (e.g., Schneider 1997a). Those judgments cannot reasonably be made with global average information alone. However, neither can climate projections be credibly approached with any other known framing than one that includes the global components. To assert that alternative frames are comparably credible for *that purpose*—predicting the climate response to given forcing scenarios—is simply to demonstrate ignorance of what is currently well established, and it will not encourage those who need to hear the important opening and concluding messages of this work to pay attention to its analysis. As for deciding what actions to take given the uncertainties and broad context of the issues, of course that needs other framing in addition to a global conception. However, this aspect of Demeritt's "deconstruction" is a straw man, since such multiple considerations are exactly what can be found, for example, in the three IPCC Working Groups' reports.

Conclusion

In this essay, I have tried to show by concrete examples, using the Demeritt (2001) article as a springboard to make specific points, that for the social studies of science community to be heard by most scientists it will have to (1) simplify its language, (2) engage in outreach to the scientists via their journals, (3) be careful to back up all social theoretical assertions with large numbers of broadly representative empirical examples, (4) avoid setting up straw men that suit theory but are at best caricatures of the experience of most scientific practitioners in their profession. In addition to these four points, developed above, I would add a few others for the S&TS community to consider in future work: (a) distinguish between "science for policy" and "policy for science" constructions and show how each particular construction might have affected the outcome, and (b) recognize that objective and subjective probability distinctions have long been discussed by policy scientists but are just beginning to penetrate the awareness of most conventional scientists as they confront "science for policy" assessments such as climate change (see, e.g., Moss and Schneider 2000).

With regard to the latter, many natural scientists still refuse to provide "subjective" determinations of probabilities. Some do not even recognize that seemingly objective methods are often based on debatable assumptions and thus are in essence subjective. I believe this is often due to a narrow cultural construction of conventional scientists based on a greater fear of committing the type 1 error than the type 2 error. That is, society ignores the possibilities of plausible problems, because the forecasts are so heavily hedged and/ or are not expressed as understandable probabilities. The social studies of science community has many insights that I believe would improve both the quality of science and science assessments. To prevent such insights from being further distanced from the scientific community, studies based on them need to be backed up by careful case study empiricism, rather than dominated by assertions that support social theory and primarily cite other theorists.

The conclusion that science is at least partially socially constructed, even if still news to some scientists, is clearly established. If social studies of science are to make an important set of further contributions, then I believe that what needs to be done is to go beyond platitudes about values embedded in science and to show explicitly, via many detailed and representative empirical examples, precisely how those social factors affected the outcome, and how it might have been otherwise if the process were differently constructed. Moreover, the assertion some have made (which Demeritt wisely labels as polemics) that because values creep into science it has no higher claim on reality than other value-laden epistemologies is one very unlikely to gather too many adherents—and rightly so. It would be far better to diagnose the relative implications and credibility of a variety of frameworks, for the job begins—not ends—when some construction is uncovered.

I look forward to more constructive interactions between the S&TS and scientific communities. We need each other, and a better dialog in mutually comprehensible language is long overdue. I hope this critique and set of suggestions from one who is outside the S&TS community can be interpreted in that light.

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