The Construction of Global Warming and the Politics of Science

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Having outlined a theory of heterogeneous social construction, this article describes the scientific construction of climate change as a global-scale environmental problem caused by the universal physical properties of greenhouse gases. Critics have noted that this reductionist formulation serves a variety of political purposes, but instrumental and interest-based critiques of the use of scientific knowledge tend to ignore the ways in which a politics gets built into science at the upstream end. By retracing the history of climate modeling and of several scientific controversies, I unmask the tacit social and epistemic commitments implied by its specific practices. The specific scientific framing of global climate change has reinforced and been reinforced by the technocratic inclinations of global climate management. The social organization of climate change science and its articulation with the political process raise important questions about trust, uncertainty, and expertise. The article concludes with a discussion of the political brittleness of this dominant science-led and global-scale formulation of the climate change problem and the need for a more reflexive politics of climate change and of scientific knowledge based on active trust. *Key Words: climate change, social constructionism, social studies of science.*

In little more than a decade, global warming has been transformed from an obscure technical concern into a subject of widespread public anxiety and international regulatory interest. Only a dozen years separate the World Meteorological Organization's 1985 Villach conference, at which years of previous scientific research and technical debate crystallized into one of the first widely publicized warnings about an anthropogenically enhanced greenhouse effect due to rising concentrations of carbon dioxide (CO_2) and other radiatively sensitive greenhouse gases (GHGs), from the recent Kyoto Protocol to United Nations Framework Convention on Climate Change (UNFCCC), at which politicians hammered out a package of legally binding targets for reducing the GHG emissions of industrialized countries. The speed with which scientific knowledge of climate change has been translated into an international diplomatic consensus is remarkable, if not unprecedented. It is testimony to the authority of science to provide legitimacy for political action.

However, science cannot be given all the credit for spurring the rapid political response to climate change. The 1988 heat wave and drought in North America were arguably as influential in fostering public concern as any of the more formal scientific advice, such as NASA scientist James Hansen's infamous 1988 declaration to Congress that he was "99 percent sure" that global warming was already happening (quoted in Ungar 1992, 491–92; cf. Mazur and Lee 1993). The relationships among the diverse expert and lay understandings of climate change are complex, as are their connections to climate change politics and policy-formation (Jasanoff and Wynne 1998).

A variety of powerful political interests served as midwives to the birth in the mid- to late 1980s of climate change as a pressing global environmental problem. Since the leading cause of increasing atmospheric GHG concentrations is fossil fuel consumption, which is rapidly increasing worldwide, the politics of climate change are closely intertwined with the politics of energy (Boehmer-Christiansen 1990; Levy and Egan 1998; Newell and Paterson 1998; Rowlands 2000) and the politics of development (Rajan 1997; Grubb, Vrolijk, and Brack 1999; Sachs 1999).

Political analysis has focused primarily on the powerful interests competing to set climate change policy. Political scientists and geographers have charted the fate of local climate change mitigation and energy conservation initiatives (Hinchliffe 1996; Lambright, Changnon, and Harvey 1996; Bulkeley 1997; Collier 1997) and intrastate struggles to fashion coherent national energy and environmental policy responses to global warming (Hatch 1993; O'Riordan and Jäger 1996; Sewell 1996; Pleune 1997; Rajan 1997). Students of international relations have studied the geopolitics of negotiating international environmental agreements on climate change (Hecht and Tirpak 1995; Rowlands 1995; Paterson 1996; Yearley 1996; Grubb, Vrolijk, and Brack 1999). This

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work has highlighted the difficult issues of international political economy—differential North-South responsibility for and vulnerability to climate change, international and intergenerational equity, technology transfer and property rights, national sovereignty and treaty enforcement, and economic competitiveness—that have bedeviled negotiators trying to forge international agreements to reduce GHG emissions.

In these discussions, science has been imagined as independent of the political process and feeding information into it. Indeed, geographers have largely adopted this as their role: providing policy makers with scientific assessments of the potential impacts of climate change (National Research Council 1997). This vision of the scientific advisor depends upon an absolute distinction between fact and value and an associated division of labor between scientists and policy makers. "[O]nly scientists can grasp the intricate interactions . . . of the global environment," explains Bert Bolin (1994, 29), former chairman of the Intergovernmental Panel on Climate Change (IPCC). Their role is to "present available knowledge objectively" to policy makers, who are in turn responsible for making political decisions "based on a combination of factual scientific information as provided by the IPCC and [their own] value judgments" (Bolin 1994, 27).

One consequence of this rather conventional view of science as hermetically sealed off from politics is that very little attention has been paid to the cultural politics of scientific practice and its consequential role in framing and, in that sense, constructing for us the problem of global warming. Thus, for epistemic community theorists such as Haas (1990, 1992), the substance of scientific consensus on global climate change is not as important as the fact that agreement among an international community of scientific investigators has enabled them to enroll governments around the world to binding GHG emission reductions. Similarly, realist and liberal institutional approaches to international relations (Young 1994; Sell 1996; Soroos 1997), like those inspired by political economy (Paterson 1996), take both the nature of global climate change itself and our scientific knowledge of it for granted in their study of the state, nongovernmental, and other actors shaping international climate change policy.

Those who have addressed the politics of scientific knowledge have largely done so in an instrumental way, arguing that political interests of one sort or another have distorted the science to serve their own ends. Such conspiratorial charges have come from both the political right and the left. Boehmer-Christiansen (1994a, b) provides a relatively sophisticated version of the argument, now popular in the United States among the largely right-wing opponents of the Kyoto Protocol, that the threat of climate change has been exaggerated by scientists "with a financial stake in adopting an alarmist attitude about global warming" (Singer 1996). The chairman of the George Marshall Institute (Seitz 1996, A16), a conservative think tank, wrote a widely quoted editorial in the *Wall Street Journal* accusing scientists involved with the IPCC second assessment report of "major deception" and "a disturbing corruption of the peer-review process." Seitz's spurious charges were comprehensively rebutted (Edwards and Schneider 1997). However, thanks to generous funding from the fossil fuel industry (Gelbspan 1997), Singer, Seitz, and a handful of other self-proclaimed climate skeptics have become the darlings of the conservative talk-radio circuit and the Republicans in control of the U.S. Congress (Brown 1996).

Whereas these right-wing critics have tried to refute scientific knowledge of global warming as socially constructed and politically biased balderdash, a few instrumental critiques of climate science have also come from the political left (Buttel, Hawkins, and Power 1990; Agarwal and Narain 1991; Taylor and Buttel 1992; Shiva 1993; Yearley 1996; Redclift and Sage 1998; Sachs 1999). These critics assert that the dominant scientific approach to climate change serves the interests of a new, technocratically inspired "environmental colonialism."[1] Such instrumental and interest-based analyses concentrate upon the uses of scientific knowledge "downstream" in the political process. In so doing, they discount the ways in which a politics—involving particular cultural understandings, social commitments, and power relations—gets built "upstream" through the technical practices of science itself. Given the heavy involvement of the discipline of geography in the scientific assessment of climate change and its potential social and environmental impacts, some critical reflection on the politics of science and its construction of global warming is long overdue.

In this article, I reconsider the relationships between the science and the politics of climate change. Although commonly thought of as separate domains, the two are linked in some important ways. Not only has the science of climate change largely driven the national and international politics of climate change, the politics in turn have also influenced the practice of that science. It is my contention that the demand for and expectation of policy relevance has subtly shaped the formulation of research questions, choice of methods, standards of proof, and the definition of other aspects of "good" scientific practice. This pattern of reciprocal influence belies the categorical distinction so often made between science, based purely on objective fact, and politics, which involves value-laden decision making that is separable from and downstream of science.

The permeability of this divide between science and politics is perhaps most clear in the hybrid, trans-scientific realm of applied regulatory science, for which questions about acceptable risks can be asked of science but not answered definitively by it (Weinberg 1972; Jasanoff 1990; Funtowicz and Ravetz 1993). Recent work in science studies suggests that all science, even the very "hardest" varieties, involves contingent social relations (Collins and Pinch 1993; Hess 1997; Golinski 1998). How to conduct this experiment or measurement? Whether to trust that datum or result? Whose interpretation to believe? Such questions are the stuff of everyday scientific practice, and they depend on trust and professional judgment. Try as we may to be scrupulously impartial and open-minded, these decisions remain socially saturated. To insist, therefore, that science is also political, in the broadest sense of that word, is not to say that science is *only* political and thereby collapse entirely the distinction between the two. It is to recognize how problematic this distinction is. The social relations that science involves necessarily influence both the character of scientific understandings upstream and the particular political outcomes that may result from them downstream in legislation or administrative law rulings.

Unfortunately, public representations of science seldom acknowledge the irreducibly social dimension of scientific knowledge and practice. As a result, disclosure of the social relations through which scientific knowledge is constructed and conceived has become grounds for discrediting both that knowledge and any public policy decisions based upon it. This political strategy of social construction as refutation has been pursued by the so-called climate skeptics and other opponents of the Kyoto Protocol. It is premised upon an idealized vision of scientific truth as the God's-eye view from nowhere. Rather than accepting this premise and being forced to deny that scientific knowledge is socially situated and contingent, the proper response to it is to develop a more reflexive understanding of science as a situated and ongoing social practice, as the basis for a more balanced assessment of its knowledge.

A richer appreciation for the social processes of scientific knowledge construction is as important for scientists themselves as it is for wider public credibility of their knowledge. In the particular case of climate change, heavy reliance upon diverse, highly specialized, and multidisciplinary bodies of scientific knowledge highlights the problem of trust in knowledge and the expert systems that produce it. As phenomena, the global climate and anthropogenic changes to it would be difficult even to conceive of without sophisticated computer simulations of the global climate system. Although satellite monitoring systems as well as instrumental records and paleoclimatic evidence have also been important, particularly in the identification of historic changes in the climate to date, it is these powerful computer models that have been decisive in identifying the problem of *future* anthropogenic climate change and making it real for policy makers and the public.[2] Ordinary senses struggle in the face of phenomena so extensive in space and time and incalculable in their potential impacts. For the social theorist Ulrich Beck (1992), this dependence upon science to make tangible otherwise invisible environmental risks is characteristic of what he calls the modern risk society.

Although Beck may exaggerate the inability of nonexperts and lay publics to make sense of climate change and other risks for themselves, it is undeniable that science and in particular the practice of climate modeling have figured centrally in the emergence of global climate change as perhaps the leading environmental problem of our time. Although their underlying technical details are understood only by the modelers themselves, these complicated computer models provide the basis not just for sweeping public policies but also for impact assessments and other scientific research.[3] Thus, 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. The extension of their trust greatly magnifies the political stakes of the microsocial relations involved in constructing and interpreting the models.

To that end, this article addresses, in particular, the assumptions and practices of climate modeling. My sources for this discussion come from a close historical reading of scientific debates about climate models and their construction of global warming. This method of using controversies to explore the social processes of scientific knowledge construction was pioneered by historians and sociologists of science (Latour 1987; Collins and Pinch 1993; Hess 1997; Golinski 1998), some of whose specific empirical studies of climate change research I rely on here (Hart and Victor 1993; Boehmer-Christiansen 1994a, b; Kwa 1994; Zehr 1994; Shackley and Wynne 1995a, b; Edwards 1996b, 1999; Agrawala 1998a, 1998b, 1999; Shackley et al. 1998, 1999; van der Sluijs et al. 1998). My empirical account of the scientific construction and interpretation of climate models also draws on a year and a half of personal experience working alongside climate scientists and policy advisors at the Atmospheric Environment Service of Environment Canada.

Before turning to the specific analysis that these sources and methods inform, I begin with a theoretical discussion of the philosophical implications of my constructionist argument. This discussion is necessarily abstract, and readers not schooled in social theory may find it difficult and wish to skip on to the more substantive discussion that follows. However, this philosophical preface is necessary for defining precisely what I mean by the construction of global warming.

In the sections that follow I review the instrumental and interest-based critiques of reductionist climate science before addressing the more tacit political and epistemic commitments implied by its specific practices. By retracing the history of climate modeling and the controversial craft of model construction, I call attention to their social contingency and unmask the ways in which the scientific framing of climate change as a global-scale problem caused by the universal and hence predictable physical properties of GHGs has reinforced, and been reinforced by, the technocratic inclinations of an emergent international regulatory regime. The article concludes with some wider reflections on the difficulties of this dominant science-led and global-scale formulation of the climate change problem and its solution.

Theories of Social Constructionism

One of my intentions in this article is to show how the technical practices of science have constructed the problem of global warming for us in materially and politically significant ways. This goal requires some discussion of the philosophical implications of such a constructionist argument. Demystifying scientific knowledge and demonstrating the social relations its construction involves does not necessarily imply disbelief in either that knowledge or the phenomena it represents. Given its vital role in helping to make sense of environmental problems such as climate change, there simply can be no question of doing without science. Rather, the challenge is how to understand and live with it better.

In this regard, constructionist accounts of science are important but incomplete (Demeritt 1996). By calling attention to the social relations involved in producing scientific knowledge of the natural world, theories of social construction challenge empiricist, positivist, and realist epistemologies.[4] The practical and political implications of this philosophical critique have not always been articulated clearly.

Two principal difficulties have plagued debates about social construction. First, there is the contentious philosophical question of nature's ontological status and its implications for the objectivity and epistemological authority of scientific knowledge. Impressed by science's spectacular capacity to represent, simulate, and construct nature through such practices as computer modeling and genetic engineering, some social constructionists, following Baudrillard (1983), have posited the total eclipse of the real and the natural by the virtual and artificial within a new, hyper-real society of the simulacra (Woolgar 1988; Myers 1990; Doyle 1997). Proceeding from different theoretical traditions but arriving at many of these same polemical conclusions, many sociologists of science insist that nature and the environment are epiphenomenal and that scientific knowledge of them is entirely explicable by how they are socially constructed (Collins and Yearley 1992; Collins and Pinch 1993; Hess 1997). These theoretical moves have provoked a fierce backlash from critics, many of them practicing scientists, who condemn social constructionism as an irrational and relativist denial both of the truth of scientific knowledge and of the ontologically objective reality it faithfully represents (Gross and Levitt 1994; Sokal 1996; Gottfried and Wilson 1997).

The ensuing debates about science and social construction—the so-called science wars (Ross 1996)—have been marred by a widespread failure to recognize the different varieties of construction talk, the different objects to which they apply the construction metaphor, and thus the important differences between social construction as refutation of science's truth and as unmasking of the inevitable partiality of its formulation (Demeritt 2001a).[5] This distinction, drawn from Mannheim (1952), is nicely explained by Ian Hacking (1998, 1999). The first, Hacking argues, accepts the philosophical presumptions of scientific objectivity and seeks to falsify a particular scientific claim by showing how belief in its truth was mistakenly (and thus, by definition, socially) constructed. Hacking (1998, 63) notes that "[t]he ghost of Karl Popper is at work in this . . . denouncing bad science. That ghost is untainted by all-purpose constructionism." By contrast, he suggests, social construction as unmasking has metaphysical aims. By unmasking the heterogeneous and contingent social relations involved in the practice of science, this form of social construction is directed against "certain pictures of reality, truth, discovery, and necessity" and the scientistic "ideology of . . . pious reverence" for science these metaphysics produce (Hacking 1999, 60, 62; cf. 1998, 65). Although provisionally helpful, this distinction between social construction as refutation and as unmasking is also somewhat simplistic. For instance, Hacking (1998, 1999) vastly underestimates the degree to which social construction as unmasking can be political as well as philosophical in its aims. Indeed, it is the hotly contested politics of climate change that make philosophical questions of how the social construction and warranting of scientific knowledge should be understood so politically contentious.

The second problem with social construction debates is a consequence of their heavily philosophical flavor. So much attention has focused on the philosophical question of whether science might be said to construct socially the nature it studies that little has been paid to the practical relations between science and society. How does the specific articulation of scientific knowledge and practice constitute "the social"? How, in turn, do scientific knowledges depend upon particular social relations? Hung up on the social construction of scientific knowledge and nature, the debate has tended to ignore these questions and take the character of society and human subjectivity for granted. This oversight has had two implications. First, it has served to reinstall, rather than deconstruct, the dominant binary oppositions—nature/society, objective/subjective, science/politics—organizing the now sterile social construction debates. Second, it has meant that questions about the broad cultural politics of science and the role of such politics in reshaping society and what it means to be human have not always received the critical consideration they deserve.

One way out of this dualistic dead end is to think about the mutual construction of nature, science, and society. Rather than taking these phenomena as given, this approach is concerned with how they are constructed through the specific and negotiated articulation of heterogeneous social actors. I call this variety of social constructionism "heterogeneous constructionism," to signal that the facts of nature are not given as such but emerge artifactually as the heterogeneously constructed result of contingent social practices (Demeritt 1998). Such heterogeneous constructionism is indebted to the work of Donna Haraway (1991, 149–82; 1992, 1997) and the actor-network theory of Bruno Latour (1987, 1999), among others (Bernstein 1983; Rouse 1987; Hayles 1991; Butler 1993; Pickering 1995; Escobar 1996; Sismondo 1996; Castree and Braun 1998). Notwithstanding important theoretical differences among them, what these proponents of heterogeneous constructionism share in common is the insight, drawn from the work of Martin Heidegger (1962, 1977), that nature and the other things-in-the-world are disclosed to us as objects through practical engagements that configure them in ways that are recognizable *for* us and transforming *of* us. Heterogeneous constructionism does not deny the ontological existence of the world, only that its apparent reality is never pregiven; "reality" is only ever realized as such through the configuration of practices that make existence manifest, throwing human subjects into a particular world of order and intelligibility.

This Heideggerian insight is a difficult one. Heterogeneous constructionism is ontologically realist about entities, but epistemologically antirealist about theories (what we designate as "electrons" has an ontologically objective existence, but our conception and classification of it are socially contingent). Thus, heterogeneous constructionism bears some similarity to nominalism and the doctrine that concepts are merely linguistic constructions without any essential relationship to the class of material objects to which they refer (Loux 1998). However, heterogeneous constructionists depart from nominalists in their insistence that the process of construction is not just semantic but also practical and that it shapes the phenomena of human perception in ontologically significant ways. The crucial difference between such heterogeneous constructionism and an even stronger idealist, or neo-Kantian, constructionism that is antirealist about both theories and entities (what we designate as "electrons" has no independent ontological existence; it is only our belief in the existence of "electrons" that gives them any substance and constructs them, such as gender, as conventional and ontologically subjective social objects) is that heterogeneous constructionism calls into question the absolute and interlocking distinctions between knowing and being, subjects and objects, nature and society, that make it possible to imagine reality as something distinct from and prior to representation. Heterogeneous constructionism provides a way of acknowledging that the world "matters" without taking for granted either the particular configuration of its matter or the processes by which it may be realized for us. As Joseph Rouse (1987, 159–60) explains:

> Practices are not representations that can be understood abstractly. They are always ways of dealing *with* the world. The ontological kinds they make manifest are determinable only through our purposive interactions with things of those kinds, and thereby with the other things that surround us. And those other things are as essential to the existence of meaningful ontological possibilities as our practices are . . . [F]or there to be electrons, there must be such things as atoms, on the one hand, and cathode-ray tubes on the other. That is, there must be the things that they interact with and the equipment that enables us to interact with them. Another way to put this is that for there to be things of any particular kinds, there must be a world to which they belong. But the reality of that world is not a hypothesis to be demonstrated; it is the already given condition that makes possible any meaningful action at all, including posing and demonstrating hypotheses.

In this Heideggerian sense (1962, 97–98), equipment is not simply an inert tool but also the interdependent languages, conceptual categories, and ways of being-in-the-world through which it becomes a tool-for something. Similarly, the "real" world is not independent of but inseparable from the particular constellation of social practices through which its form is enframed along with our own. For the heterogeneous constructionist, nature is artifactual and its understanding an active and ontologically transformative practice. The practical engagements understanding involves reshape the way subjects and objects are thrown together as beings-in-the-world.

An example may help clarify what I mean. Consider "climate." Defined as the "average weather conditions of a region over a period," conventionally 30 years (Mayhew and Penny 1992, 37), "climate" is a statistical abstraction. The apparently matter-of-fact existence of what we recognize as the climate is an artifact of certain social practices and conventions that make it possible to construct this universal out of so many observed particulars (O'Connell 1993; Porter 1994). Whereas a nominalist might regard the "climate" as merely a linguistic construction that is instrumentally useful for designating a class of real phenomena, an idealist, neo-Kantian constructionist would go further by claiming that the atmospheric phenomena we call climate are themselves socially constructed (and therefore ontologically subjective) through our conventional belief in their existence. The heterogeneous constructionist denies the absolute distinctions between word and thing made by the nominalist and between nature and society by the idealist, neo-Kantian constructionist. For the heterogeneous constructionist, neither the idea of a "global climate" nor the phenomena that it designates are conceivable apart from the world-shaping network of social practices, standardized instruments, orbiting weather and communications satellites, and computer models through which they are made manifest. By unmasking the socially contingent relations of its appearance for us, heterogeneous constructionism neither questions the ontological existence of climate as such nor refutes our knowledge of it.

Heterogeneous constructionism acknowledges the constitutive role of science in disclosing for us the reality of climate change without reducing that reality to some phantasmic science fiction. Thus, heterogeneous constructionism dispenses entirely with the dead-end debate over the truth of scientific representation and whether scientific knowledge corresponds to a pregiven, external, and therefore ontologically objective natural world. Instead, it calls attention to the consequences of scientific practices for ways of being-in-the-world. In the particular case of the global climate, the conditions of its scientific intelligibility are also deeply implicated in the emergence of more reflexive understandings of human nature and subjectivity. The computer models, satellites, and associated scientific practices that make the global climate manifest to us also help to position us as reflexive subjects with a specifically planetary consciousness of the earth's environment as a whole (Cosgrove 1994). No longer fatalistic in the face of incalculable climatic hazards, we feel increasingly able to predict those risks scientifically and therefore to fashion ever more of our individual biographies reflexively on the basis of knowledgeable choices about an open future (Beck 1992). In turn this reflexive subjectivity, with its decision-oriented belief in the possibility of managing life's contingency through rational choice, infuses the science of global climate change with some tacit beliefs about determinacy, prediction, and rational control.

Global Climate Change and the Instrumentalist Politics of Scientific Reductionism

From the very outset, global climate change has been constructed in narrowly technical and reductionist scientific terms. Like the notorious ozone "hole," the problem of an anthropogenically enhanced greenhouse effect first came to the attention of atmospheric scientists concerned with the physics and chemistry of the climate system. From their scientific perspective, what is interesting and important about GHGs are their universal physical properties and the effects of increasing atmospheric concentrations of diffuse anthropogenic GHGs on the planet's radiation budget and thus on the climate system of the planet as a whole. Accordingly, the IPCC and the other national and international scientific bodies studying climate change have tended to regard it as a universal and global-scale problem of atmospheric emissions. They have tried as much as possible to divorce the scientific study of this problem from the social and political contexts of both its material production and its cognitive understanding (Agrawala 1998b).

It should be clear what a dramatic simplification this scientific conception of GHGs and the problem of their emission represents. Scientific projections of future global warming depend upon estimates of future GHG concentrations and thus upon some conception of future paths of social and economic development. For the most part, climate model projections have been driven by highly simplistic business-as-usual scenarios of human population growth, resource consumption, and GHG emissions at highly aggregated geographic scales.[6]

This construction of global warming appeals to the common and undifferentiated interests of a global citizenry in averting what the International Geosphere-Biosphere Programme (IGBP 1990, 1–3) of the International Council of Scientific Unions describes as "unprecedented," "rapid and potentially stressful changes" to "the Earth's life-sustaining environment." This representation of climate change is not untrue—everyone everywhere *is* reliant on the atmosphere (albeit in different ways), and the slow and diffuse build-up of GHG concentrations does give climate change a global dimension, in terms of both its immediate causes and potential effects, quite unlike urban smog, which in these respects is much more localized. However, that scientific conception of global warming is a partial one.

The specifically global scaling of climate change highlights more general concerns about the effects of increasing GHG concentrations on the earth's radiation balance at the expense of other ways of formulating the problem, such as the structural imperatives of the capitalist economy driving those emissions, and indeed of other problems, such as poverty and disease. In recent years scientists have paid increasing attention to the potential regional impacts of global warming. The development of regional impact assessment models is an improvement on previous models, whose coarse scale was unable to resolve how future climate changes will affect different places in different ways. However, it does not satisfy critics who charge that the overall concern with so-called global environmental problems such as climate change is an essentially "Northern" one. They contend that the threat of future climate change holds little meaning for developing nations and the poor people in them struggling daily in the face of crippling structural-adjustment policies with more basic and immediate needs of sanitation, health, and hunger. From this perspective, the environment is not self-evidently or exclusively global in nature. If not refuted altogether as mere social constructions, long-term threats to the global environment are not regarded as immediate concerns (Athanasiou 1991; Taylor and Buttel 1992; Middleton, O'Keefe, and Moyo 1993; Shiva 1993; Redclift and Sage 1998; Sachs 1999).

This global scaling aids and is underwritten by a second way in which climate scientists universalize the objects of their knowledge ontologically. Physical sciences represent GHGs in terms of certain objective and immutable physical properties. They treat CO_2 and other GHGs as interconnected parts of global radiation budgets and nutrient cycles. Such analytical abstractions, I want to emphasize again, are not untrue, but they are partial. There is no denying, for instance, the sense in which the atmosphere is profoundly indifferent to the source, social context, and meaning of GHG emissions—but the same is not true for us humans, so it is important to unmask the effects of this partiality. A narrowly scientific focus on GHGs dissociates their physical properties from the surrounding social relations producing them and giving them (particular) meaning(s). Though widely recognized as politically important, such issues are often treated as analytically separable from, if not in fact irrelevant to, the technical question of "stabiliz[ing] . . . greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system" (UNFCCC 1992, Article 2).

The globally scaled and universalizing physical abstractions of climate science foreground these technical dimensions of GHGs and climate change over more contentious and obviously value-laden ones. This analytical division of labor between science and politics serves some obvious political functions. Critics have focused particular attention on the instrumental uses of scientific representations of climate change. Their global scaling and universalizing appeal conceal the uneven political economy of GHG emissions by divorcing the problem of their accumulation in the atmosphere from related social and economic matters. Thus, "luxury" emissions of GHGs from fossil fuel use in developed countries are analyzed in the same abstract and universal scientific terms as "survival" emissions from agriculture in developing countries (Agarwal and Narain 1991; Shue 1993). These universalizing abstractions can then be used to legitimate the specific political program of international emissions trading and other climate change mitigation measures in the warm and fuzzy glow of global citizenship. The scientific focus on the global climatic effects of future GHG concentrations has tended to sideline political discussion of the uneven pattern of past emissions and the attribution of responsibility for their accumulation in the atmosphere. This further reinforces the undifferentiated appeal of global warming talk to a sense of global citizenship. Of course, appeals to universal human interests are not in themselves necessarily illegitimate, as the example of human rights campaigns suggests. However, they do tend to steer attention away from the difficult politics that result from differentiated social groups having different interests in causing and alleviating environmental problems (Taylor and Buttel 1992).

Still, blindness does have its benefits, even for a progressive environmental politics. Although it is fashionable in many circles to bemoan the reductionism of science as an unmitigated evil, it is important to recognize where we would be without it.[7] Physically reductionist computer-simulation models have been crucial in identifying the physical effects of continued GHG emissions on the climate system. Their alarming red-orange visualizations of a future hothouse earth have played a vital role in bringing these risks to widespread public attention. To be sure, troubling exclusions are built into this epistemic community. The discipline and expertise required to participate meaningfully in its scientific debates restrict not only who is authorized to speak but also what and how things can be spoken about.[8] Important as it is to be reflexive about the exclusions that abstraction necessarily entails, there can be no escaping them entirely, for knowledge is always situated, partial, and incomplete (Haraway 1991, 183–201). Thus a climate model, no matter how sophisticated, can only ever provide a partial window on a much more complicated reality that it must, as a form of abstract reasoning, reduce to some analytically simplified set of physical processes. One way to distinguish the practice of abstraction involved in this kind of physical reductionism from a more general sense of Reductionism is to say that Reductionism commits the "epistemic fallacy" (Bhaskar 1978, 36). It loses sight of the fact that its abstractions are merely analytical constructions, conveniently isolated from the flux of totality, and reduces reality to the terms of its own analytical abstractions.[9]

This distinction between pernicious Reductionism and the physical reductionism of science has occasionally been lost on science critics within cultural studies and critical human geography. All too often, social constructionist critiques of particular scientific abstractions come across, whether intended as such or not, as rejections of science and refutations of its specific knowledge claims. Such antiscience polemicism can be as sweepingly Reductionist as the very thing it opposes. Although the particular abstractions of global climate modeling may not tell us everything that we need to know, they deserve more credit than they sometimes receive from their critics.

Physical process modeling has certain undeniable advantages. For one thing, the physically reductionist abstractions that it involves render the world analytically manageable. Only by dramatically simplifying the messy social relations driving GHG emissions and focusing narrowly on their physical and chemical properties have scientists been able to understand the effects of increasing GHG concentrations on the climate system. Oversimplistic as this way of seeing may be, it is still probably something that we cannot do without. Climate models provide one of the most important tools for exploring the physical relationships among GHG emissions, concentrations, and climate changes. The task, therefore, is to better appreciate the partial insights that these models provide without falling into Reductionism and losing sight of the limitations of physical process modeling.

The trouble with instrumental and interest-based critiques of reductionist climate science is that their exclusive focus on the uses of science downstream in the political arena ignores the ways in which the specific practices of science also involve a politics at the upstream end. In the next two sections, I explore the upstream politics of global climate modeling. First I consider the history of climate modeling and the social relations influencing the contexts of scientific discovery and the socially contingent form in which scientific knowledge of the climate has developed. Then I turn to the craft of model construction and the tacit assumptions and political commitments constructing the contexts of scientific justification and thus the specific content of scientific knowledge of global warming.

A Social and Political History of Global Climate Modeling

The prevailing scientific construction of global warming is a contingent social outcome. The particular commitment of climate modelers to physical reductionism is just one of many epistemic styles in which scientists have historically studied the climate. In the United States, for example, the first systematic network of weather stations was established in the early nineteenth century, in part, to help resolve debate about whether the clearing of forests and agricultural settlement of land were changing the climate, as Benjamin Franklin, Thomas Jefferson, and others had speculated they might (Thompson 1981; Demeritt 1991; Fleming 1998). Although the rapid public acceptance of contemporary scientific theories of anthropogenic climate change doubtless trades on age-old concerns about human disturbance of the environment (Glacken 1967; Thompson and Rayner 1998), the theories themselves have a rather different intellectual genealogy.

It is common to cite the work of Arrhenius (1896), who first hypothesized about an enhanced greenhouse effect brought on by the atmospheric accumulation of CO_2 from fossil fuel consumption. However, like Gregor Mendel's role in the history of genetics, Arrhenius's theory made no institutional or intellectual impact on the atmospheric sciences at the time. Instead, contemporary scientific concerns with global warming come out of the much more recent practice of mathematical modeling and the combination of oceanographic modeling of the global carbon cycle with numerical modeling of the atmosphere (Hart and Victor 1993). Their legacy is the general circulation model (GCM). GCMs simulate the behavior of the climate system by dividing the earth into a three-dimensional grid and using supercomputers to solve mathematical equations representing exchanges of matter and energy between the grid points. In terms of the comprehensive number of processes explicitly incorporated and the level of abstraction and complexity at which they are represented, GCMs sit atop a hierarchy of related mathematical simulation models of ocean and atmospheric dynamics.[10]

Like so much postwar science, in the United States (Kleinman 1995) as elsewhere, the initial justification for this research was military (Kwa 1994).[11] Both the early oceanographic studies of the global carbon cycle and the first efforts to model the atmosphere numerically depended heavily on Cold War research funding (Smagorinsky 1983; Thompson 1983; Hart and Victor 1993). Likewise the justification for the GCMs, the first three-dimensional climate models, was also military rather than environmental, though, significantly, both rationales are consistent with what Paul Edwards (1996a) has called the "closed world discourse" of Cold War America: a language of integrated systems, an image of global containment (of communism and environmental problems) and apocalypse, and a practice of technologically centralized management, communication, and command-and-control.

This predominantly military basis of support for climate system modeling began to change in the late 1960s. With military research funding increasingly directly towards tangible research outputs and applications (Kleinman 1995; Demeritt 2000), scientists tapped into growing public concerns about human impacts on the environment. Atmospheric scientists were able to secure continued funding for basic modeling research, as well as for international scientific cooperation and environmental monitoring, through projects such as the Global Atmospheric Research Programme of the World Meteorological Organization (Boehmer-Christiansen 1994a; Hecht and Tirpak 1995; Agrawala 1998a). Sparked in part by public fears about air pollution and the effects of supersonic jet transportation, new research into human impacts on the atmosphere helped enroll atmospheric chemists into the modeling program, as it became clear that sulfate aerosols, CFCs, and other anthropogenic emissions, as well as CO_2, were radiatively significant (Hart and Victor 1993). It also marked a shift in the climate modeling community, away from an assumption of climatic equilibrium and a theoretical interest with radiative forcing of the atmosphere as an analytical tool for model development and comparison to the present concern with it as a potential mechanism of anthropogenic climate change (van der Sluijs et al. 1998).

Climate Modeling and the Politics of Interdisciplinary Approaches

It is somewhat ironic that climate modeling has become established as the primary means by which global warming is scientifically understood. The modeling community was initially somewhat slow to become interested in the possibility of climate change on historical time scales (Lamb 1977). When the dangers of climatic change received their first widespread attention in the mid-1970s, it was the prospect of global cooling and the onset of another Ice Age, rather than global warming, that sparked public concern and research support (SMIC 1971; Ross 1991). Scientific evidence for these fears came primarily not from the theoretical and physics-based atmospheric sciences, but from the empirical-descriptive traditions of glacial geology and cognate disciplines concerned with the changing history of the earth's climate (National Research Council 1975; Damon and Kunen 1976; Bryson and Murray 1977).

Relations among these scientific disciplines are political in a nontrivial sense. In the early 1970s, when the historical climatologist Hubert Lamb left the British Meteorological Office to set up the Climate Research Unit at the University of East Anglia, he was actively opposed by members of the British meteorological establishment. They lobbied hard against government funding for the study of historic climate changes. Lamb's Research Unit survived only thanks to support from private foundations (Lamb 1997). This was not simply a case of petty infighting; it was also about what kind of science would be practiced and how knowledge would be evaluated (Lamb 1977, 19). With their training in atmospheric physics, many meteorologists branded Lamb's paleoclimatological methods as unsystematic, impressionistic, and unscientific. For them, true science was not descriptive but predictive and law-finding (Nebeker 1995). As Lamb was assembling indirect indications of dramatic climatic fluctuations within the Holocene period, prominent British meteorologists tried to refute his findings as mere local fluctuations in a noisy but otherwise stable global climate system (e.g., Mason 1976).

Now, of course, the intellectual climate has changed. Lamb's old-style descriptive climatology may have won the argument about changes in the climate on a historical (as opposed to geological) time scale, but in some sense his scientific approach has lost out to mathematical modeling.[12] During the planning stages for the first IPCC scientific assessment report, the empirical-statistical and paleoclimatic analogue approaches pioneered by Lamb and advocated within the IPCC by Soviet scientists were marginalized within IPCC Working

Group I, as computer simulation modeling was adopted as the preferred method for projecting future climate changes (Boehmer-Christiansen 1994b, 190; Hecht and Tirpak 1995, 386–87; cf. Brocoli 1994).

Shackley and Wynne (1995b) contend that physically reductionist mathematical modeling now reigns supreme in the overlapping specialties of climate change research. Their claim may overstate the case somewhat, applying more to the U.K., where the majority of government research funds go to support the GCMs (Department of Environment 1995), than elsewhere. Especially in the heterogeneous research culture of the U.S., climate modeling has not displaced other more empirically based and less (or, perhaps more accurately, *different*) physically reductionist forms of climate research. In the U.S., at least, these research traditions have enjoyed the enthusiastic support of scientific authorities since the very dawn of scientific concern about global warming (National Research Council 1975). Indeed, more than two thirds of United States Global Change Research Program (USGCRP) funds have been devoted to the earth observation program (Townsend 1997; Asrar 1999; USGCRP 2000, 57), whose four space shuttle launches and fifty-six surveillance satellites U.S. Space Program officials would otherwise have struggled to justify now that the Cold War is over (Boehmer-Christiansen 1994a, 145).[13] Paleoclimatology also receives generous funding from the USGCRP and the many other governmental and nongovernmental agencies supporting climate change research. Rather than being competitive and mutually exclusive pursuits, such empirical research complements theoretically based climate modeling by providing observational data for model validation and improved parameterization of poorly resolved physical processes.

Increasingly, though, support for those other traditions of climate science is justified in terms of their service to the GCMs (MacCracken 1996; Asrar 1999; Krebs 1999; USGCRP 2000). The design of the next generation of GCMs, so-called earth systems models, is being expanded to incorporate detailed hydrological, ecological, and glaciological submodels so as to "includ[e] all components of the climate system in a fully interactive model" (Hadley Centre 1998, 5). The development of these new earth system models is helping to foster greater collaboration among the different disciplines and research traditions concerned with climate change by enrolling them to the physics-based atmospheric science program of comprehensive global modeling (*Ambio* 1994; Shackley et al. 1998). However, this project also reinforces the authoritative position of the GCM as the hub around which other scientific and policy making communities must revolve. Tensions attend this implicit research hierarchy. In addition to resentments over resource allocations, issues arise about the terms on which such interdisciplinary cooperation is negotiated. The objective of comprehensive earth-system modeling leaves the physical reductionism of the GCMs unchallenged as "the organising principle about which other disciplines have to develop. . . . [by] translating their esoteric knowledge into parameterizations for GCMs" (Shackley 1996, 547). The result, as Shackley (1996, 547) notes, is that cooperating "ecologists, atmospheric chemists, and polar region specialists have to 'travel' further in translating their knowledge in the form of parameterizations for GCMs than GCM modellers 'travel' in adjusting the form of the GCM to accommodate these new processes."

Physical Reductionism and the Political Assumptions of Climate Modeling

Climate models are physically reductionist in two distinct senses. First, their analytical abstractions consider only the physical properties of GHGs, such as atmospheric residence time, radiative signature, and photochemical reactivity. The social context and meaning of these apparently objective and universal facts are ignored by scientists and treated as value-laden questions to be dealt with downstream from the science by policy makers within the political process. Marxist, feminist, and developing country critics complain that, in practice, this form of physical reductionism amounts to Reductionism, if perhaps not always in the way that experts themselves understand their own analytical abstractions, then usually in the way that they are represented publicly (Haraway 1991, 183–201; Ross 1991; Taylor and Buttel 1992; Shiva 1993; Macnaghten and Urry 1998). These critics charge that, by treating the objective physical properties of GHGs in isolation from the surrounding social relations that produced them and give them meaning, the physical reductionism of the models serves to conceal, normalize, and thereby reproduce those unequal social relations.

This claim was powerfully illustrated by Agarwal and Narain's (1991) critique of the politics implicit in scientific estimates of the global warming potential (GWP) of GHGs.[14] This CO_2-denominated measure is calibrated from climate models and used to compare the direct effects of different GHGs on the earth's radiation budget.[15] Agarwal and Narain (1991) argue that any physically reductionist measure of GWPs—even a fully comprehensive one that included all the indirect radiative effects of a particular GHG on clouds and other feedback processes—would still be politically biased, because the rate of GHG removal depends on atmospheric concentrations and thus on gross emissions, which vary hugely among nations. By failing to account for this uneven political economy, scientific calculations of GWPs are, in effect, tacitly allocating rights to the amount of GHG reabsorption in proportion to gross emissions. This allocation greatly favors the U.S.—the world's largest GHG emitter, with one of the highest emission rates per capita—over poorer developing countries. Thus, rather than inoculating science against value-laden political judgment, the physical reductionism of the GCM-calibrated GWP is in fact making controversial (if often unacknowledged) political commitments.

Radical science critics have paid much less attention to the second and substantially more technical sense in which climate models have been constructed to be physically reductionist. The models are premised on the belief that complex environmental systems can be decomposed into their constituent parts and the totality of a system modeled from the bottom up, based on endogenous mathematical representations of the first-order physical principles governing the interactions of its parts and explaining the dynamic behavior of the system as a whole (Kiehl 1992; Root and Schneider 1995). The actual practice of climate modeling does not live up to this ideal. Instead of being modeled from the bottom up, many underlying physical processes are parameterized: that is, they are represented by exogenously specified parameter values, selected either from empirically observed statistical relationships (sometimes called physically based parameterization) or more problematically on an ad hoc basis, such as so-called flux adjustments, which I discuss below. Thus, parameterization is a far cry from the bottom-up physical reductionism idealized by the physics culture of climate modeling. Nevertheless, the modeling of "[v]irtually all physical processes operating in the atmosphere" involves parameterization of some sort, because it provides a computationally economical way to account for important subgrid-scale processes that are either not well enough understood or too computationally demanding to be resolved endogenously by reducing them to dynamic mathematical terms (Kiehl 1992, 336). The work of generating and improving these parameterizations makes up the largest part of actually constructing a climate model. Different models parameterize the same processes differently, and there is animated debate within the modeling community about the merits of various parameterization schemes.

The physical reductionism of simulation modeling has become far and away the most authoritative method for studying the climate system. Speaking in public contexts, advocates of climate modeling repeatedly emphasize that its grounding in the fundamental laws of physics makes physical process modeling the most credible—indeed even the most "rational" (National Research Council 1975, 201)—method for understanding, predicting, and thereby managing global warming (IPCC 1990, xxv; McInnes 1990; USGCRP 1995; IPCC 1996, 31; MacCracken 1996, 8; Harvey et al. 1997, 3; Mahlman 1997; Grassl 2000). For instance, the latest report of the U.S. Global Change Research Program (USGCRP 2000, 46, emphasis added) explains:

> Computer simulation models are the *primary* tools by which knowledge of the workings of the Earth System can be integrated, and the results of these models are, in many ways, one of the major payoffs of the USGCRP. *Only* through Earth system models can we, for example, predict future climate variability and change, including the possible effects of human activities on the global climate system. The long-term objective of Earth system modeling and simulation is to create and apply models that provide credible predictions (including levels of certainty and uncertainty) of changes and variations in climate on regional-to-global scales, along with useful projections of potential environmental and societal consequences.

This passage deserves careful scrutiny. Like many public discussions of climate modeling (e.g., NOAA 1997, 22–23; Patrinos 1997; Krebs 1999; DETR 2000, 3; Hollings 2000, 2), it does not distinguish carefully enough between simulation of the climate system and a much stronger and more seductive suggestion that GCMs can (or will soon) forecast and predict the actual state of the system at some specific future date. Forecasting involves initializing a GCM with actual observational data and running it so as to predict the likely evolution of the system from that initially observed state. Despite dramatic improvements in the sophistication of weather forecasting models, chaos and the sensitivity of the climate system to initial conditions mean that the state of the modeled climate very quickly drifts away from the actual one. As a result, GCMs used for weather forecasting require continual reinitializing with updated observational data to keep their predictions of the future state of the climate system in line with its rapidly evolving actual course (Lorenz 1982). By contrast, climate simulation involves running a GCM without reference to any specific observed state. This practice poses important philosophical questions about the epistemology of simulation, the ontological status of model phenomena, and their relationship to what they represent (Sismondo 1999; Winsberg 1999).

Climate modelers are somewhat equivocal in their interpretation of GCM "experiments"—a usage that, as Dowling (1999) argues, suggests an epistemologically realist attitude to simulation based on the close correspondence of model phenomena to the real-world ones they represent. On the one hand, it is sometimes claimed that, although GCMs may not be able to forecast the actual state of the climate with any certainty, GCM experiments can provide what Lorenz (1975) has termed predictions of the second kind, that is, predictions of the statistical probability of particular system states (Harvey et al. 1997, 5; Hulme et al. 1999; Mitchell and Hulme 1999). In the absence of a long and detailed observational record, the chaotic pattern of natural climate variability—so crucial for distinguishing the "fingerprint" of global warming from the noise of the system (Schneider 1994)—is now being estimated theoretically by using "the internal variability atmosphere-ocean general circulation models . . . as a substitute for instrumental observations" (Hegerl 1998, 759). However, this internal variability is difficult to assess because of the enormous computational demands of the GCMs, which can require weeks of supercomputing time to complete a single run.

Unlike the stochastic tradition of numerical modeling common in economics, engineering, and other disciplines, GCMs are mathematically deterministic. That is, they calculate a unique solution for a given set of initial conditions. As a result, the probability of any particular system state outcome must be estimated from its frequency within a population of GCM runs from slightly different initial conditions. These ensemble techniques are in their infancy within the GCM community (Katz 1999). Probability estimates are based on ensemble populations much smaller than would be acceptable within other modeling cultures (Shackley et al. 1998). Modeling pragmatists recognize these difficulties, but do not regard them as so insurmountable as to prevent the immediate application of GCM outputs to scientifically credible impact assessments or policy advice.

On the other hand, modelers also offer much more modest assessments of the merely heuristic understanding to be gained from exploration of the virtual parameter space of a climate model. Purists believe their "models are simply functional expressions of hypotheses." As abstract entitities, "model phenomena" are qualitatively different from the real ones they simulate (Reifsnyder, quoted in National Research Council 1995, 431). Accordingly, the virtual dynamics exhibited by climate models are regarded as merely analogous to and suggestive of those of the much more complex climate system (Manabe and Stouffer 1988; Gates et al. 1993, 128; Palmer 1993). These differing assessments correspond to some extent to differences within the modeling community between thermodynamicists, who have developed the more comprehensive coupled atmosphere-ocean GCMs for modeling long-term climate changes due to changes in the earth's radiative balance, and dynamicists working with more complex and higher resolution atmospheric GCMs to model detailed circulation patterns for short term weather forecasting (Shackley et al. 1999).

The availability of these alternate scientific rhetorics—one authoritative and pragmatically applied and the other more cautious and concerned with the basic science of model development—has the overall effect of legitimating the use of GCMs to project future climate changes. The promise of prediction seduces downstream users of GCM projections even as potential criticisms are preempted by open acknowledgment of continuing scientific uncertainty and the promise that it will be reduced through future research and model development (cf. Maxwell, Mayer, and Street 1997; DETR 2000; USGCRP 2000). However, valuable as it may be, the understanding gained from climate simulation is very different from actual predictions about the specific future state of the climate, which are what USGCRP (2000, 46) seems to promise. Insofar as science policy advisors and modelers themselves do not always make clear the distinction between prediction and simulation, they reinforce the popular view that GCMs are scientific "crystal balls" (McInnes 1990), or should be and thus can be ignored until they actually deliver such predictive certainty.

Indeed, given the indeterminacy of future emissions scenarios, the promise of scientific prediction is downright deceptive.[16] Uncertainty about future emissions scenarios and economic development paths is rarely mentioned in discussions of the "predictive capacity" of climate science. This silence is symptomatic, I would suggest, of a tacit environmental determinism running through global warming discourse. Although climate change will likely be just one of many changes the future holds in store, it has largely been studied in isolation. Indeed, the vast majority of research funding has been devoted to trying to resolve physical-science uncertainties about projected climate changes. Efforts to understand their broad human dimensions have been appended to this GCM-driven juggernaut like cabooses to a freight train. Taylor and Buttel (1992, 410) argue that this privileging of "the physical over the life and social sciences" amounts to "environmental determinism: the physics and chemistry of climate change set the parameters for environmental and biological change; societies must then adjust as best they can to the change in their environment."

Such a deterministic understanding of climate change is both politically and scientifically impoverished. GCM projections of future climate change have been driven by variations on business-as-usual emission scenarios that assume present emissions trends will continue more or less uninterrupted into the future. This analytical abstraction of climate change science from related social and economic matters promotes a misleading baseline mentality that naturalizes the existing economic structures and cultural imperatives driving those emissions and artificially constrains the range of conceivable alternative development paths to an open and indeterminate future (Cohen et al. 1998). Insofar as a few business-as-usual economic scenarios drive the GCMs projections that in turn inform virtually all of the political debate over climate-policy responses, that political discussion has been constrained by an artificially narrow, spuriously scientific, and therefore misleading view both of the options available and of the importance of the social context within which climate change is understood scientifically and addressed politically.

The Reciprocal Construction of Modeling Science and Policy Application

The USGCRP (2000) is far from unique in identifying the applications of GCM simulations as the reason for favoring their further development (Gates et al. 1993; McGuffie and Henderson-Sellers 1997, 65–66; Harvey et al. 1997, 7; National Research Council 1999, 445). As Martha Krebs (1999, 6), Director of the U.S. Department of Energy's Office of Science, recently testified to Congress, "It is only through such general circulation models that it is possible to understand current climate and climate variability and to predict future climate and climate variability, including prediction of the possible effects of human activities on the global system." Though somewhat more equivocal about the predictive capability of the *present* generation of GCMs, Gates and colleagues (1996, 274) make similarly bullish claims in their contribution to IPCC (1996) about the potential of *future* GCMs to deliver accurate predictions of climate change: "The development of more complete, more efficient, and more accurate coupled models has long been the aim of the climate modeling community, since it is generally believed that it is only through such models that we can gain a scientific understanding (and hence a reliable predictive capacity) of climate and climate change."

This rhetorical slippage between simulation and prediction, commonly made by policy makers and modelers alike, is symptomatic of some other commitments wrapped up with the physical reductionism of the GCMs. One of the principal attractions of the GCMs over simpler models and more empirically based and therefore retrospective approaches to studying climate change, such as paleoclimatology, is the seductive promise that a future generation of more complex and comprehensive GCMs will provide highly detailed predictions of transient and regional-scale climate changes (Shackley and Wynne 1995b). This objective and the rapid pace of progress towards its achievement are repeatedly emphasized both in science policy documents (Maxwell, Mayer, and Street 1997; Patrinos 1997; DETR 2000; Lane 2000, 4, 8; USGCRP 2000) and in the public statements of leading modelers (Boer, quoted in McInnes 1990; USGCRP 1995; MacCracken 1996; Harvey et al. 1997, 7; Mahlman 1997; Hadley Centre 1998; Grassl 2000).

The capacity to simulate regional-scale climates more realistically is thought to be important for two reasons. First, as Gates et al. (1993, 112) note in IPCC (1993), "the horizontal resolution" of GCMs is "too coarse to provide the regional-scale information required by many users of climate change simulations." Significantly, the identity of these "users" is left vague here. The implication is that policy makers and perhaps even the general public might somehow "use" GCM simulations, though exactly how is not at all clear. Nor is the question of whether their needs are best served through the development of more complex and comprehensive GCMs. In this way, modelers' tacit beliefs about downstream needs and identities legitimate their assumption about the need for intensified physical reductionism and continued GCM development.

In practice, the largest immediate consumer of GCM outputs is the scientific impacts assessment community, which uses them to construct regional climate change scenarios. Impact assessment scientists have repeatedly emphasized the importance of understanding future climate changes at a regional scale. They regard the regional scale as the one most meaningful for environmental policy and management decisions as well as ordinary citizens, whose continued support is crucial to sustaining potentially expensive near-term policy changes to mitigate and adapt to future climate changes (Cohen 1990; Easterling 1997). Globalization makes that assumption about the symbolic and socioeconomic primacy of the regional scale with downstream decision makers and the general public somewhat debatable.

Scenarios for regional assessments of climate-change impacts can be generated in a variety of ways. Given the indeterminacy of future emissions scenarios and the uncertainty of regional GCM projections, it is by no means clear that the added precision offered by GCM-based scenarios is a substantial improvement over scenarios constructed through other methods, such as statistical downscaling, historical analogues, or the best guesses of experts. Arguably, the application of complex GCMs to the generation of regional climate change scenarios for impact assessment is tantamount to using a laser guided missile to swat a fly: a fly swatter might do just as well without suggesting a degree of precision unwarranted by unknown levels of modeling uncertainty and future indeterminacy. Modelers have found that large uncertainties in the GCMs mean that for many purposes “statistical models . . . produce considerably better simulations than” the bottom-up physical reductionism of the GCMs (Anderson et al. 1999, 1349). Nevertheless, impact assessment experts prefer to base their scenarios on the most sophisticated models available because the GCMs are regarded as “the most credible” (IPCC 1998b, 23; cf. IPCC 1994, 22–23; Taylor 1997; DETR 2000, 16).

Experts do not always explain the reasoning behind their preference for GCM-based scenarios (Maxwell, Mayer, and Street 1997; DETR 2000). Some cite the complexity and comprehensiveness and thus the realism of GCM simulations of the climate system (Taylor 1997), others their foundation in first principles and hence the internal “consistency” of GCM-based scenarios (Viner and Hulme 1997, 7; IPCC-TGCIA 1999, 25–29). Others point to “the transient nature of climate change, which only GCMs can address,” a rationale suggested by one anonymous reviewer of this article. That these reasons are themselves logically inconsistent—model comprehensiveness is provided through statistical parameterizations, which are at odds with the preference for grounding in the first principles of physics that is the reason for preferring GCM-based scenarios over historical analogues and statistical downscaling—suggests that some other influences are also involved. One consequence of the application of GCM outputs to regional impact assessment is the reinforcement of the hierarchical distinction between the separate GCM modeling and the impact assessment communities. By citing the need of impacts experts for more certain regional GCM projections, modelers reinforce their own identity as the providers of these data to downstream users and further legitimate the upstream project of physically reductionist GCM development. In turn, the application of upstream outputs from state-of-the-art GCM outputs by impact-assessment experts enhances the credibility of their own work (and by extension also the GCMs themselves) to outside audiences.

A second reason for the scientific concern with improving the certainty of regional-scale climate change projections—and thus the complexity and comprehensiveness of the GCMs—involves a more political judgment by modelers and other climate scientists that such knowledge is necessary for reaching climate change policy decisions. For instance, Ronald Prinn (1997, 4), director of the MIT Center for Global Change Science, observed in his testimony before the House Science Committee that “the needed policy response [to global warming] is uncertain because the science is uncertain” and recommended the development of improved climate modeling capacity to provide the certainty he thought necessary for policy. Likewise, Root and Schneider (1995, 339) advocate an integrated program of multiscaled modeling because they believe that “[b]efore most policy makers would be willing to endorse a particular policy they would likely require estimates of the possible consequences.” These beliefs are widespread among climate scientists. Shackley and Wynne (1995b, 225) argue that scientists’ understanding of the policy desire for “locally or regionally specific environmental resource management” has helped consolidate “the dominance of a particular style and future programme of climate science—GCMs and their enlargement into ever more inclusive models” thought capable of providing information for that purpose.

Recent changes within the USGCRP bear out these claims about the mutual construction of climate science and policy application. In response to persistent criticisms about the policy irrelevance of its global climate change science (Brunner 1996; Pielke 2000a, 2000b), the USGCRP has substantially reoriented both its public rhetoric and its substantive research funding around the understanding of present-day climatic variability. The showcase achievement of the remodeled USGCRP is regionally precise El Niño forecasts, which are useful for a variety of short-term planning and environmental management applications. This development depends heavily on improvements in the basic sciences of climate-system modeling and monitoring, especially of the small-scale ocean eddies so important in the prediction of El Niño events (Shukla 1998). These poorly understood physical processes pose interesting research puzzles, which is why so many scientists have been drawn to study them. However, their scientific choices of particular research problems and approaches cannot be entirely disentangled from the social context in which they are made. Scientists and program managers alike have repeatedly emphasized the practical and political importance of the predictive capacity of models for environmental management and policy purposes (MacCracken 1996; Krebs 1999; USGCRP 2000). Such applications provide skeptical and impatient policy makers with reasons to continue supporting the USGCRP and its $1.74 billion program of GCM modeling and remote sensing, without which the development of an El Niño forecasting capacity would not have been possible (Hollings 2000).

In my own experience at the Atmospheric Environment Service of Environment Canada, the interaction of research scientists and policy makers was premised on largely tacit understandings of each other's respective needs and capabilities. These mutually constructed expectations had important implications both for the negotiation of upstream research agendas and for the presentation of results to downstream users. I recall one particular meeting with a senior government policy advisor. She was writing a Memorandum for Cabinet about the monetary costs of climate change, based on a report my colleagues and I had been asked to prepare for her. As I wrote in my notes of the meeting, she stressed to us the importance of providing her not only with "scientifically credible information about the costs of impacts and adaptations" but also enough geographically specific detail "so that Ministers can see themselves and their ridings in it." With the Canadian Cabinet divided in the run-up to Kyoto, her goal was to write a memo that would show ministers the "costs of inaction" and thus the importance of pursuing GHG emission reductions at Kyoto. Naturally, my colleagues and I wanted to be helpful. Aside from our personal feelings of support for her political objective, we were employed precisely to provide such expert advice. Moreover, our boss made it clear that our cooperation would help our group in the upcoming negotiations within the Ministry over the next round of budget cuts. For all these reasons, we felt under some pressure to give the ministerial policy advisor what she wanted, despite our misgivings that the question we were being asked about the monetary costs of climate change could not be meaningfully answered and ran the considerable danger of subsequent misinterpretation by policy makers downstream. We feared that whatever figure we came up with would be radically decontextualized from any caveats we might include about the heroic assumptions and politically contentious valuation practices that it involved (Demeritt and Rothman 1999).

Senior researchers in our group told us that our concerns about the applications of our report were political objections, and as such inappropriate to our task of scientific assessment. As recent work in science studies has emphasized (Gieryn 1994), this kind of boundary making is one of the most important ways in which science is socially constructed. Since economists had published a number of cost estimates, it was our duty, after the fashion of the IPCC, to report them. Our report dutifully repeated the published estimates, while trying, much less successfully judging from the uncritical way in which the numbers seem to live on in plans for a second phase of the Canada Country Study, to explain the moral and intellectual poverty of the kind of scientific estimates of "the costs of inaction" this ministerial advisor seemed to assume we could provide (Rothman et al. 1998). This kind of mutual accommodation can serve to reinforce the impression of scientists and science advisors that a particular kind of scientific information is necessary for policy making and to persuade policy advisors and politicians that it is scientifically possible to produce such information.

I would suggest that such policy demands for greater certainty about regional-scale climate impacts, imparted to research scientists indirectly through their involvement in the IPCC and other national-level science advisory processes, may have reinforced scientific preferences for the GCMs in an analogous way. In addition to its real scientific merits, the development of even more complex and comprehensive integrated earth system models also promises the certainty about possible regional climate change impacts thought necessary for policy. In this way, scientific judgments about the importance of using the GCMs and increasing their comprehensiveness in order to predict future regional-scale climate changes both reinforce and are reinforced by political considerations about managing those changes. However, the history of climate science suggests that GCMs are by no means the only way to understand climate change scientifically. Other approaches are available, but these alternatives have been downplayed, at least in part because of a largely tacit assumption by scientists that GCM predictions are necessary for policy purposes and more useful for impact assessment than less formalized expert assessments or best guesses.

The Craft of GCM Construction and Its Implicit Politics

It would be possible to read my argument thus far as pertaining only to the contexts of scientific discovery and the particular form in which scientific knowledge of climate change has been developed, without making any stronger claims about the construction of the actual contents of scientifically accepted knowledge. This distinction between the contexts of discovery and of justification was central to Popper's influential attempt to distinguish science from other kinds of belief. For Popper (1959), the social commitment to skepticism and the continual testing of belief defined science, whereas the method of empirical falsification guaranteed the credibility of the resulting scientific knowledge, if not—at least for a committed positivist such as Popper—its metaphysical truth. From my heterogeneous constructionist perspective, I am skeptical about these categorical distinctions between science and nonscience, upstream and downstream, form and content, discovery and validation. Even if, like Popper, I ultimately put my faith in the social processes of science, I understand the status of its contingent knowledge rather differently. I want to insist that even the most technical details of science and scientific knowledge are heterogeneous constructions, artifacts of historically specific and ontologically transformative practices.

In this next section I discuss several scientific controversies about the construction and validation of climate models. These problems are well recognized within the modeling community, whose ongoing discussions provide the primary sources for my analysis. However, nonmodelers are much less familiar with these problems or their implications. These debates are also worth revisiting because they unveil a number of debatable modeling assumptions and practices that have emerged out of the interactions of climate modelers within a wider epistemic community of research scientists, impact assessment experts, science advisors, policy makers, and political interests interested in predicting and managing global warming.

The Selection and Construction of Model Feedbacks

Many important physical processes within the climate system are not yet fully represented by state-of-the-art GCMs. These models are an outgrowth of earlier weather forecasting models, the short time scale of which meant that longer-term processes, such as those relating to biogeochemical cycles, could be ignored altogether or—as in the case of snow and ice albedo feedbacks—simplistically incorporated as fixed boundary conditions that influence atmospheric behavior but are not influenced by it in turn (Kiehl 1992, 319–20). These practices are problematic for the longer time scales involved in simulating future climate changes because initial boundary conditions, such as the amount of snow reflecting incoming solar radiation, will likely change along with and in response to the changing climate. Accordingly, great effort is being directed at explicitly representing physical processes that were not previously included in GCMs, or were resolved only simplistically, for historically specific reasons having to do with the imperatives of weather forecasting.

Among the most important feedbacks initially ignored by the GCMs were anthropogenic aerosols, which have significant but poorly understood radiative effects. By reflecting solar radiation, aerosols have a direct effect on the earth's radiation balance, but they also affect it indirectly by serving as nuclei for the formation of clouds, which—depending on their particular characteristics—may result in either positive or negative radiative forcing. The effects of aerosols were at the center of debates about global cooling in the early 1970s. Using simple one- and two-dimensional models, scientists inferred negative radiative forcing from increasing aerosol concentrations, particularly in the northern hemisphere, where fossil fuel use and thus short-lived aerosol haze is concentrated (SMIC 1971; Schneider 1994). However, these specific effects were not incorporated by the more comprehensive three-dimensional atmospheric GCMs developed in the 1980s. The GCM projections of global warming cited by the influential IPCC reports (1990, 1993) did not include the negative radiative forcing from aerosols alongside the positive forcing from increasing GHG concentrations.

Several reasons account for the initial focus of the GCMs on modeling the much better understood effects of GHGs. Scientists believe that the direct radiative forcing from aerosols is much less significant than that from GHGs (Schimel et al. 1996, 117, Figure 2.6). Covey (2000, 409) also points to the importance of aesthetic considerations "of elegance" and the influence of referees on the decision of GCM modelers to avoid the "difficult task of calculating aerosol distributions consistent with model-simulated meteorology. It was always possible to crudely simulate aerosol effects by altering a GCM's surface albedo, but a crude parameterization of an important phenomenon is a frequent point of attack on a GCM study. Reviewers are sometimes kinder to papers that ignore the phenomenon altogether."

Recent efforts to incorporate the climatic effects of aerosols in GCMs are both scientifically and politically significant. Aerosols are an important scientific uncertainty in GCM projections of climate change. Modeling research is one of several important approaches to improving scientific understanding of their climatic effects. Beyond its research value, the addition of aerosols to recent GCM simulations also rebuts a major criticism leveled by so-called climate skeptics against the GCMs and GHG emission reduction policies these models inform. This political context was an important, if not explicitly acknowledged, factor in scientific judgments about when and how to model aerosols. Mitchell and colleagues (1995, 501) began the first published results from a GCM to include aerosols by noting, "Climate models suggest that increases in greenhouse gas concentrations in the atmosphere should have produced a larger global mean warming than has been observed in recent decades." For many climate skeptics, this incongruity between the theoretical simulation models and the observational record falsifies the models and their projections of global warming (Balling 1996; Michaels 1997; Singer 1997). By adding the negative radiative forcing from sulphate aerosols to the HadCM2 GCM of the U.K. Meteorological Office, Mitchell and colleagues (1995, 501) "significantly improve[d] the agreement [of their model output] with observed global mean and large-scale patterns of temperature in recent decades." Mitchell et al. (1995) did not discuss any further the implicit political significance of their findings, but it was made abundantly clear in an accompanying *Nature* editorial by Wigley (1995, 464): "So far, climate modelers have had limited successes and have had to bear the brunt of criticism from those who are concerned about the role of models in the greenhouse policy debate. At last, however, it seems that the door is opening and the light of credibility is filtering through."

The critical question is: credible to whom? As Covey (2000) notes, the particular technique Mitchell and colleagues (1995) chose—adjusting the parameterization of surface albedo to represent the backscattering from aerosol haze—is so simple that it could have been tried many years before the incongruity of model projections and observations became so politically contentious. This implies that public credulity was as much an issue as scientific credibility. Modelers had long been aware of the model errors caused by their failure to account for aerosols and had judged them accordingly. The claim of the IPCC (1990, xxviii; cf. Gates et al. 1993) that GCM "simulation of present climate is generally realistic" reflects the largely tacit and informal judgment of modelers not to take model outputs at face value but instead to "subjectively . . . correct for known errors in the models" (Mitchell, quoted in Schlesinger 1988, 878). In the face of growing "criticism from those who are concerned about the role of models in the greenhouse policy debate" (Wigley 1995), Mitchell and colleagues (1995) used recently published global aerosol data to account more explicitly for aerosols through a physically constrained parameterization. By providing an explanation for the slower than previously predicted onset of global warming, their paper lent scientific weight to the politically symbolical and intergovernmentally negotiated conclusion of the IPCC summary for policy makers that "the balance of evidence suggests a discernible human influence on the climate" (IPCC 1996, 4).

Climate skeptics now charge that the addition of aerosols to the models, ex post facto, is a desperate and politically motivated attempt to salvage an otherwise empirically falsified hypothesis about global warming (Michaels 1997). In the particular case of Mitchell and colleagues (1995), Rodhe, Charlson, and Anderson (2000) deny that there were any problems with faulty or circular logic. Mitchell et al. (1995) used independent data on aerosol concentrations to parameterize the effects of aerosols on temperatures in different grid boxes of their model, rather than, as sometimes has been the case, using the observed pattern of global temperatures to infer aerosol parameterizations and then using those parameterizations to simulate those same empirically observed temperature patterns. However, Rodhe, Charlson, and Anderson (2000) concede that the widespread practice of constructing and tuning GCMs to match observations can create problems of circular logic.

Validating Complex Models

As the example of aerosols suggests, this routine practice of tuning model parameterizations has important implications for the validation and credibility of complex GCMs. Traditionally, modelers have tested their models by "eyeballing the differences between observed and simulated maps of a particular variable" (Santer, quoted in Schlesinger 1988, 875).[17] Such informal practices are symptomatic of the relative intimacy that has long characterized the modeling community. The tacit "personal knowledge" circulating semiprivately within the GCM community through email, word of mouth on the conference circuit, and the shared practical experience of modeling can be as important for modelers' judgments of the GCMs as the knowledge disclosed publicly to outsiders through formal reports and peer-reviewed publications (Polanyi 1958). Fellow modelers, complained Henderson-Sellers (quoted in Schlesinger 1988, 876–77), "are well aware of what's wrong with many aspects of the models . . . They just don't want to say so in print. There are certain groups that are worse at this than others" (cf. Mitchell in Schlesinger 1988, 874; Phillips 1995; Anderson et al. 1999, 1354). While personal familiarity with other models and modeling scientists enables modelers to know how much they can trust the subjective eye-balling of their peers, the basis for such judgments is "not well known to people outside the modeling community" (Wigley, quoted in Schlesinger 1988, 877).

Modelers are making increasing use of more formalized and systematic procedures of quantitative model testing (Wigley and Santer 1988; Gates 1992). The culmination of these internationally formalized GCM tests are the various Model Intercomparison Projects sponsored by the World Climate Research Program. GCMs are run with standardized forcing scenarios and provide outputs in standard formats suitable for systematically comparing the magnitude of model errors, such as those introduced by the controversial practice of flux adjustment (AMIP 1996; CMIP 1997). "[P]art of the motivation" for this more systematic testing "arises from the IPCC process in that many of the coupled models are relied on for high visibility climate change simulations" (CMIP 1997).

However, as with the drive in the 1950s towards objective methods of weather forecasting (Nebeker 1995, 127–32), the appeals of more formalized and quantitative evaluation methods are social and political as much as they are technical and scientific. Often statistical testing does not tell modelers anything about their models that they do not already know intuitively from long practical experience. Thus, there is some debate about whether "modeling groups [actually] need better ways of analyzing their results [statistically]" (Henderson-Sellers, quoted in Schlesinger 1988, 87). It is widely believed that "modelers know better than anyone else the problems with their models" (Wigley, quoted in Schlesinger 1988, 877). The problem is that their richly nuanced understanding is vulnerable to politically motivated charges of bias, idiosyncrasy, and dissimulation. Like doctors, auto mechanics, and other specialized practitioners, modelers rely heavily on tacit personal knowledge, craft skill, and uncodified practical experience in performing the work of model diagnosis. Quantification makes the basis for these technical judgments more transparent and less contingent upon individual skills, informal understandings, and private negotiations of experimental competence, credibility, and empirical adequacy (Porter 1995; Demeritt 2001b). In this sense it also makes them more publicly credible, insofar as adherence to rigidly uniform and impersonal and in that sense "procedurally objective" (Megill 1992, 310) rules limits the scope for individual bias or discretion and thereby guarantees the vigorous (self-)denial of personal perspective necessary to make knowledge seem universal, trustworthy, and true.

Although agreement between model output and empirical observations might be taken as an indication that the GCMs have got it right, it might also be, as one modeler explained, that the models are "agreeing [with one another] now simply because they're all tending to do the same thing wrong" (anonymous source, quoted in Kerr 1997). This is a serious concern because historically much of the underlying FORTRAN code for different GCMs has been shared among modeling groups, so as to avoid unnecessary duplication of effort (Edwards 1999).

Philosophers of science have long noted the logical impossibility of verifying a scientific model. In response to an influential critique (Oreskes, Shrader-Frechette, and Belitz 1994) of the fallacy of affirming the consequent and assuming that the agreement of model output with observations logically proves the model's ability to predict any future observations of an open system, modelers increasingly refer to the process of model testing as evaluation (Gates et al. 1996, 235; Harvey et al. 1997, 8) instead of validation (Wigley and Santer 1988; Gates 1992; Kiehl 1992). However, the philosophical modesty about truth implied by this fallibilist usage is not always carried over either into modelers' own descriptions of the epistemic implications of a good fit between model outputs and observations or into representations made subsequently by and for policy makers and the public (AMIP 1996, 2–5; Hedger, Hulme, and Brown 2000, 7).

Recent work in science studies has shown that, in practice, the falsification principle of Popper's (1959) logical positivism turns out to be no less problematic than verification (Hacking 1992; Collins and Pinch 1993; Hess 1997). For Popper, the hallmark of good science is the construction of bold yet potentially falsifiable hypotheses. In the case of GCMs, however, even experienced modelers, conceded one modeler at a National Research Council (1995, 198) workshop, find it "hard to tell whether the forcing is wrong or the model is poor when results don't agree with observations." Harry Collins (1985, 2) has dubbed this problem the "experimenter's regress." With so many potentially adjustable parameters, no purely logical grounds exist for deciding whether an inconsistency between a model and some empirical test of it is due to an inadequacy of the test or an error in the model. At a NATO Advanced Study Institute on Physically-Based Modeling and Simulation of Climate and Climatic Change, one modeler expressed the concern that "one is more likely to get the right answer for the wrong reason in a more complicated model than in a simpler one because you have so many more knobs to turn in the big model" (Salzman, quoted in Schlesinger 1988, 879). Others disagreed with this suggestion that added complexity made the GCMs harder to test than simpler models:

> There are really not that many knobs [in complex GCMs]. This knob-turning criticism has been voiced many times. I wish I had as many knobs as you seem to think there are, because we would be furiously turning those knobs, doing thousands of experiments trying to simulate the climate better. This is not the case. In fact, it's perhaps the other way around. There are fewer knobs in these physically based GCMs than there are in the simpler, highly parameterized climate models. (Schlesinger, quoted in Schlesinger 1988, 879)

This debate highlights some unresolved disagreements within the modeling community about whether more complicated GCMs in fact constitute the best science (Randall and Wielicki 1997; Shackley et al. 1998; Henderson-Sellers and McGuffie 1999). "An assumption of research involving . . . GCMs is that the realism of climate simulations will improve as the resolution increases" (Harvey et al. 1997, 7; cf. McGuffie and Henderson-Sellers 1997; Crowley, quoted in Schlesinger 1988, 873–74). Accordingly, modelers have sought to improve their models by increasing the models' complexity and comprehensiveness. However, these improvements have come at the cost of greatly increasing the size, intricacy, and computational requirements of the models, which in turn have made state-of-the-art GCMs vastly more costly and cumbersome to test. Scientific calls for more research with simpler and more easily falsifiable models, such as the ones I have quoted above (cf. Randall and Wielicki 1997; Shackley et al. 1998), are driven by the belief that the prevailing strategy of GCM improvement has tended to exacerbate as much as resolve the experimenter's regress.

Scientific debates about GCM construction and validation do not occur in a political vacuum. Reporting to its members on the results of the international Model Evaluation Consortium for Climate Assessment, the research arm of the American public utilities industry, the Electric Power Research Institute (1998b), noted that "the complexity of the ocean-land-atmosphere system makes it very difficult to identity specific process algorithms that are performing poorly." By exposing these outstanding scientific uncertainties to the oxygen of publicity, the fossil fuel industry and other downstream political interests can prolong or even prevent the resolution of these uncertainties upstream by scientists. Cross-examination in adversarial legal contexts offers a potent mechanism for deconstructing the expert judgments that scientists routinely make in order to settle unresolved issues and press ahead at the cutting edge of science (Jasanoff 1995).

If normal science is based on the falsification of hypotheses, the highly politicized scientific debates over global warming operate by some rather different norms than the scientific values of universalism, openness, disinterestedness, and organized skepticism heralded by Merton (1973). Competing groups seek to advance their interests by putting forward their own selected experts and discrediting opposing claims. The Electric Power Research Institute (1998a), for instance, brags that its "research programs generate major paybacks" such as highlighting the importance "of sulfate aerosol processes that can counterbalance greenhouse warming." It is not entirely clear whether the "payoff" for its industry sponsors is the magnification or resolution of scientific uncertainty about global warming; perhaps it is both. Covey (2000, 410) implies that another reason why modelers shied away from modeling aerosols for so long, "incorporating only well quantified effects in simulations," was their awareness of how brittle parameterizations based only on "our best estimates for factors that are uncertain" can sometimes be in such politically contested circumstances. One strategy for defending the credibility of scientific knowledge is to draw sharp boundaries between technical matters of scientific fact, which they alone are competent to decide, and those of publicly debatable, value-laden political judgment (Gieryn 1994). Such defensiveness, however, seldom engenders much open and reflexive debate about the location of this boundary or the expert judgments it may involve.

Tuning Out Extremes?

The influential work of Mearns, Rosenzweig, and Goldberg (1997) has emphasized to modelers the importance of modeling changes in statistical variance. However, until recently "most cases [of model] validation extend[ed] only to average values of variables" (Gates 1992, 11). As a result, modelers did not explicitly consider the ability of models to simulate extremes of temperature and other values that are often the most biologically and economically significant. By tuning their models to improve the simulation of observed means, modelers were making the tacit assumption that it is prudent to concentrate first on simulating what seem to be the likely outcomes. Shackley and Wynne (1995b) suggest that this notion of sensible planning is something that climate scientists picked up from their interactions with policy makers in the IPCC. Doubtless certain practical considerations, such as the need to conserve computing power during model runs by saving only averaged temperature and other values, were also important.

Whatever its origin, this practice embeds a subjective judgment about risk tolerance into climate models, where it may suppress the degree of system variability they consider. These models provide the basis for the scientific risk assessments of the IPCC. In turn, these scientific assessments provide the basis for the risk management decisions of policy makers and the general public. These groups are unlikely to appreciate the full significance of the informal judgments of modelers about how best to represent the variability of the climate system and their uncertainty about it. In this way, the political decision to run the unknown risk of extreme climate changes is legitimated by a form of scientific knowledge about that risk that is underdetermined by nature and is reciprocally validated by an unacknowledged risk tolerance on the part of the dominant scientific culture.

Paleoclimatic research has provided another important test of GCM skill at simulating extreme climate states very different from the present-day averages to which they have been tuned (Gates 1992; Grassl 2000). Because of the paucity and heterogeneity of paleoclimatic data, as well as the incongruities of scale between empirical observations from single sites and globally gridded model outputs, such comparisons tend to be rather generalized and to require considerable interpretive skill. However, they have highlighted the difficulty of simulating the kind of large and abrupt climatic changes suggested by recent research with ice cores and other high-resolution paleoclimatic records (Dansgaard, White, and Johnsen 1989; Taylor et al. 1993; Adkins et al. 1997). Despite improvements in GCM simulations of a breakdown in North Atlantic thermohaline circulation, which drives the Gulf Stream and keeps northwest Europe warmer than other high latitude regions (Broecker 1997; Wood et al. 1999), a recent assessment concluded:

> Climate models have a tendency to be quite stable as compared to observed paleo records. This may partly be a consequence of tuning the models in such a way that they remain numerically stable during the integration . . . In this respect it is relevant to investigate, among others, what the effect is of the very large artificial flux corrections in state of the art climate models. (Opsteegh 1998, 60–61)

The potential for abrupt and nonlinear responses to rising GHG concentrations is now a top priority for the IPCC (1998a; Pearce 1998). The fact that the overlapping climate science and policy communities have begun in the last few years to consider much more seriously the risks of catastrophic climate change is an indication both of the responsiveness of modelers to new research findings from outside disciplines and of the robustness of the science advisory process, which has rapidly fed these concerns into the policy process. However, the first scientific papers about the possibility of abrupt climate change were published more than a dozen years ago (Broecker, Peteet, and Rind 1985; Broecker 1987). This, then, poses some questions about kind of social and epistemic commitments that might explain why, despite the widely acknowledged uncertainties of the models, the potential for surprise was played down for so long within the IPCC. Why were GCM projections of a slow, linear response to increasing GHG concentrations so readily offered to and so gladly accepted by those concerned with managing global warming?

Flux Adjustment

Many coupled atmosphere-ocean GCMs have relied upon the controversial practice of flux adjustment. This practice is common in global climate modeling, because underlying model errors cause the simulated climate to "drift" unrealistically unless ad hoc and nonphysical adjustments are made to the calculated ocean fluxes of heat and moisture. These flux adjustments keep the simulated climate in equilibrium with the observed climatalogical baseline so that GCMs can be run for long enough time-scales to allow the transient climate changes caused by increasing GHG concentrations to become readily apparent (Kattenberg et al. 1996). Climate skeptics point to these ad hoc adjustments as a major reason to discount GCM projections of global warming from rising GHG concentrations (Brown 1996; Michaels 1997). More importantly, perhaps, the underlying flux errors such tuning corrects make it difficult to test assumptions about the stability of the climate system, which depends upon thermohaline circulation, sea ice, sea-level rise, and other atmosphere-ocean feedbacks dependent on those erroneously simulated temperature and moisture fluxes (Wood et al. 1999).

Until modelers can eliminate the underlying errors responsible for model drift, they face a practical dilemma if they wish to provide transient and regional-scale projections of future global warming. On the one hand, some modeling purists have constructed coupled GCMs without making flux adjustments, but the onset of unrealistic drift in such models "normally excludes precisely those [long] time-scales that one would like to study with a coupled model" (Sausen, Barthel, and Hasselman 1998, 146). Thus these unadjusted models were not very useful for the purposes desired by and promised to policy makers and other users of model outputs. These problems have been partly addressed by recent model developments that reduce the need for flux adjustments. More complexly resolved parameterizations now allow some unadjusted models to be run without unrealistic drift for several centuries, thereby providing the kind of long-term projections formerly possible only by using flux-adjusted models (Kerr 1997; Gordon et al. 2000).

On the other hand, other modelers have preferred to couple their GCMs using flux adjustments, in order to fix their calculations of sea-surface temperatures, sea ice, and other fluxes more closely to observed values and to run realistic looking long-term climate simulations. Despite concerns about the massive "fudge factor" involved and the resulting logical difficulties of distinguishing any "real" climate change from artifacts of the adjustment (Kerr 1994), flux adjustment was until recently the technique of choice for simulating future global warming. It continues to be widely used (CMIP 1997; Hulme et al. 1999), notwithstanding the improvements in unadjusted models, because for many scientific purposes, such as exploring feedbacks between future climate change and sea ice albedo, flux adjustment is preferable to using unadjusted models in which the simulation of these parameters is not so realistic or too computationally demanding.

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. The apparent realism of flux-adjusted GCMs has been crucial to their credibility with policy makers and the public. Realistic-looking control run simulations are also important for impact assessment experts. Their preference for GCM-based scenarios might be dampened if they could not feed realistic baseline inputs generated from the same source as their climate change scenario into their own impacts models so as to assess the difference that climate change will make. Whereas some climate modelers disapproved of flux adjustment because it "makes your model look better than it really is" (quoted in Kerr 1994, 1528), others thought the practice justified, given the need for long-term projections of transient and regional-scale climatic changes (Shackley et al. 1999). I have already suggested that this "need" is debatable, since equally plausible scenarios can be generated in other ways, and is at least partly constructed through the impression of downstream users that such GCM projections were scientifically possible, if not immediately then very soon. In turn, this impression of modelers on the part of nonmodelers was reinforced by the way in which the practice of flux correction, resorted to by modelers partly because of their perception of its policy need downstream, covered up underlying model errors and made the practice seem more credible to consumers. Thus, as with the choice of the GCM itself, the selection of particular GCM modeling techniques was also socially negotiated. It was influenced by scientific perceptions of political desirability, which were in turn informed by the belief of policy makers of its technical practicality.

Towards a More Reflexive Understanding of Science

I have tried in this essay to reconsider the politics of global warming by examining the social relations involved in its scientific construction. Such constructionist arguments are often dismissed as attempts to refute scientific knowledge claims by suggesting how the contexts of their discovery and validation are socially constructed and politically influenced. By contrast, my heterogeneous constructionism seeks to refute neither the existence of global warming nor our socially contingent knowledge of it. Instead, my objective is to unmask the ways that scientific judgments about the GCMs as a method for understanding climate change have both reinforced and been reinforced by certain political considerations about managing it.

An international epistemic community of scientists and policy makers is coalescing around the world picture produced by the GCMs. To date, however, there has been little public discussion of the commitments embedded within and advanced through its technical practices. This closure has occurred in part because of the enormous complexity of climate change and of the highly specialized multi- and interdisciplinary bodies of expert scientific knowledge through which it has been understood.

Open and reflexive debate has also been stifled because of the way this knowledge has been organized and communicated. The linear model of upstream science feeding into the downstream policy process has been replicated within the scientific research community. This hierarchical division of labor tends to sharpen intergroup boundaries between specialized expert competencies and downstream technical ignorance. Whereas the experts building climate models are qualified to judge their problems and uncertainties, subsequent users are less likely to be thus qualified. Indeed, users often regard model output with much greater confidence than do the modelers themselves. In turn, the needs and understandings of these downstream users, although perhaps informally acknowledged, are not the formal responsibility of upstream research scientists, who as a consequence are more likely to misinterpret them. In practice, of course, lines of communication in science for policy are more blurry and two-way, but one persistent danger of this top-down model of linear communication is that policy makers and others relying on GCM output may not fully appreciate—let alone endorse—all of the local understandings built into them.

This uneven geography of expert knowledge production and dissemination underscores the importance of redoubling the efforts of the IPCC to communicate levels of confidence in its scientific assessments across the science-policy interface. Its reports couch predictions in three levels of confidence: high, medium, and low (IPCC 1996). By helping to bridge the technical knowledge gap between experts and lay publics, this practice provides a better foundation for public debate about how to respond to the incompletely understood risks of global warming.

At the same time, however, this overarching emphasis on scientific uncertainty and its communication also serves to reinforce the authority of expertise. This is a second, much less widely acknowledged danger of the highly technocratic, linear view of environmental policy making. Much has been made of the important distinction between the falsifying imperative of normal science to avoid Type I errors of accepting a false positive and the precautionary imperative of public policy to avoid Type II errors of rejecting a true positive (Harman, Harrington, and Cerveny 1998). However, uncertainty is by no means the only important aspect of scientific knowledge that has a bearing on its understanding. The prevailing scientific construction of global warming embodies other important and yet also potentially contentious judgments, assumptions, and practices that I have tried to highlight in this essay:

- Anthropogenic climatic change is a global-scale, environmental (as opposed to political or economic) problem.
- It is caused by the universal physical properties of GHGs (as opposed to underlying political structures or moral failings).
- These objective entities have universal meanings that can be discovered scientifically by experts.
- The best way to understand global warming scientifically is to model it mathematically.
- An important objective of climate science should be the construction of more complex, comprehensive, and physically reductionist models.
- Model simulations provide the basis for future climate predictions.
- Rational policy is (or should be) founded on GCM projections about the regional-scale impacts of climate change.
- The regional scale is the most meaningful one for policy making.
- Model parameterizations adequately simulate the climate system variability, or soon will.
- Modelers should focus first on (what they perceive to be) the most likely outcomes, as opposed to the most extreme.
- Experts are best placed to decide the legitimacy and credibility of these practices.

Many of these assumptions are informal and negotiated by relatively small communities of investigators. Others are not formally acknowledged because they emerge out of the interactions of scientists within a wider epistemic community of research scientists and policy makers. In this way, a socially contingent form of scientific knowledge is being shaped by an emergent international policy regime that, in turn, is being constructed and legitimated by this same body of scientific knowledge. Judgments about the physical nature of climate change and the best way of understanding and responding to it are debatable, but the process of mutual construction tends to reinforce and naturalize them. The challenge is to remain open about the often tacit social commitments built into the technical details of scientific knowledge and practice.

The difficulty is what happens when very real scientific debates get translated from the relatively exclusive confines of journal pages into the wider public sphere. For Beck (1992), the prospect of sweeping public scrutiny of science represents the final achievement of the Enlightenment's emancipatory potential. Although I want to share in these utopian hopes, I am not so confident that the present tenor of debate about global warming, risk, and science is as reflexive or as enlightened as Beck suggests. In the highly adversarial contexts of this debate, the values of openness, disinterestedness, and good faith upon which science depends are quickly suspended. Personal motives are subjected to corrosive scrutiny and expert judgments discounted out-of-hand as competing groups seek to advance their interests by deconstructing opposing claims in the media or on the witness stand. Fortunately, interested skepticism about the science of climate change—paid for by the fossil fuel industry—is largely restricted to the political culture of the United States. It is difficult to see how this very cynical and deeply interested campaign to discredit the science of climate change is either reflexive or enlightened.

The response to these political attacks has been to emphasize the sound scientific basis for climate policy decisions and to downplay the inevitably partial interpretations and professional judgments that scientific understanding involves. Ironically, these efforts to win public trust by basing policy on scientific certainty can actually increase public skepticism and make the resulting policy decisions more politically uncertain. They invite political opponents to conduct politics by waging war on the underlying science (and scientists!), which in turn breeds scientific defensiveness rather than reflexive engagement in the face of criticism and debate.

Since it is the personal motives of scientists that are subjected to public assault, science for policy is based increasingly on narrowly technical problem formulations and mechanical decision making procedures such as cost-benefit analysis. These elaborate exercises in Reductionism are attractive because they offer the seductive promise of unimpeachably scientific solutions to contentious political problems. However, such technically sophisticated methods do not eliminate the need for value-laden judgment; often they simply conceal the particular judgments involved in a haze of technical detail. Even the much less immediately "policy relevant" GCMs incorporate a set of debatable and value-laden political commitments.

I am concerned that the dominant science-led politics of climate change rests on a weak foundation.

Given the immensely contentious politics, it is tempting for politicians to argue that climate policy must be based upon scientific certainty. This absolves them of any responsibility to exercise discretion and leadership. This science-led politics is also attractive to some scientists since it enhances their power and prestige. However, this political reliance on the authority of science is deeply flawed: it provides neither a very democratic nor an especially effective basis for crafting a political response to climate change. It enshrines in apparent scientific objectivity the particular judgments and values embodied by the IPCC assessment. If these presumptions are not acknowledged more openly, the risk is run that, when they are eventually exposed through open debate, the resulting acrimony will make negotiation and mutual accommodation even more difficult.

Even worse, perhaps, is that the highly technical and undifferentiatedly global basis of its appeal simply turns people off. Appeal to the universal interests of a global citizenry is founded on scientific certainty, rather than the more difficult work of making global warming meaningful to a differentiated international public. As a result, continued scientific uncertainty has become the principal rationale for continued inaction. The narrowly scientific focus on global climate change addresses itself to an undifferentiated global "we" and relies exclusively on the authority of science to create this sense of global citizenship. In the absence of some other basis of appeal, "we" are likely to act more as spectators than participants in the shaping of our related but different futures (Taylor and Buttel 1992, 406).

In place of this emphasis on scientific truth and (un)certainty, I would like to suggest the practical value of relying more on the rhetoric of social trust and solidarity in trying to construct a political response to climate change. Trust in the social institutions of science makes for a very different and much less authoritative rhetoric than the objective scientific truth so often invoked as the self-evident justification for political action. As Rorty (1991) notes, the trouble with the rhetoric of objectivity is that it suggests that science somehow stands above and outside the fray as a uniquely privileged vehicle to Truth. This understanding of scientific truth leaves us with an inflexible, take-it-or-leave-it approach to scientific knowledge: either true, objective, and therefore undeniable, or false, subjective, and thus unworthy of any credence. It contributes to the starkly dichotomous public reactions to technical expertise: on the one hand, unqualified faith in and craven deference to science, and on the other, outright rejection of and alienation from scientific knowledge and institutions.

This, of course, is precisely what has happened with climate change science. While the climate skeptics have sought to refute climate change science by exposing the socially negotiated assumptions and uncertainties of the climate models, advocates of GHG reduction have responded by denying them altogether. Neither response is very helpful. What is needed instead is a more reflexive understanding of science as a social practice. Scientists complain that media coverage tends to distort and polarize scientific debate by turning it into a simplistic story with "two sides." Journalistic objectivity and balance demand that each side's story be reported without editorial comment or analysis of its credibility (Henderson-Sellers 1998). However, when the issue is precisely the competing factual claims of differing experts, nonexperts can hardly be expected to judge the scientific facts for themselves. Instead they base their judgments about environmental risks on both the perceived credibility of the scientists in question and wider criteria about the kind of social and political commitments those risks involve (Jasanoff and Wynne 1998).

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. Science does not offer the final word, and its public authority should not be based on the myth that it does, because such an understanding of science ignores the ongoing process of organized skepticism that is, in fact, the secret of its epistemic success. Instead scientific knowledge should be presented more conditionally as the best that we can do for the moment. Though perhaps less authoritative, such a reflexive understanding of science in the making provides an answer to the climate skeptics and their attempts to refute global warming as merely a social construction.

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Notes

1. It is not entirely clear whether these critics intend, as right-wing climate skeptics certainly do, to refute scientific knowledge of global warming by showing it to be socially constructed and therefore untrue. Another interpretation is that their critiques simply unmask the limits of an exclusively scientific understanding of climate change and the social interests served by that partiality.
2. This is a somewhat contentious claim. In a personal communication, Diana Liverman suggested that "different scientists and policy makers have been convinced by different aspects of the science—some by computer simulations (because they believe the models are credible or because the results are dramatic)—but many others by the evidence that the climate is changing (temperature records, glacier retreat, etc.)" (Liverman 2000). Although I acknowledge that these empirical records have also been persuasive, I would still insist that the models have been decisive, because modeling results are the principal basis for distinguishing natural climatic fluctuations from the "fingerprint" of anthropogenic global warming (Schneider 1994). Moreover, because they are theoretical, the climate models are able to provide two other telling lines of evidence. The models can simulate not only the *future* climate changes caused by continued GHG emissions but also the effects of their successful mitigation. This second function, as Edwards (1996b, 156) notes, is crucial, if also not widely acknowledged. The near-term costs of mitigation are likely to be substantial, making clear demonstration of their benefits essential to maintaining public support for them. However, these benefits are not only distant in time but counterfactual in nature. If the GHG emission reductions set in train by the Kyoto Protocol ultimately achieve the objectives of the UNFCCC (1992), they will prevent "dangerous anthropogenic interference with the climate system." Thus, the only way to know that these policies have succeeded is by using climate models to simulate what would have happened without them.
3. Indeed, as Edwards (1999) emphasizes, even the apparently independent empirical records of global climate change from terrestrial weather stations and orbiting satellites are to some extent dependent on climate models, which have been used variously to help filter, correct, interpolate, and grid otherwise variable and incomplete data sets.
4. These three epistemologies differ crucially on such matters as the roles of verification and falsification, the nature of observation, and the existence of unobservable structures and entities. All too often, however, their social constructionists ignore these differences and refer (disparagingly) to them all as realists (see, for example, Rorty 1991).
5. Sismondo (1996), Demeritt (1998, 2001a), and Hacking (1999) provide contrasting typologies of the different uses of the term "social construction."
6. To be fair, the IPCC has begun to respond to criticisms of the narrowness of its future emission scenarios as well as to other concerns about their complexity and comprehensiveness. It is now considering a more diverse range of scenarios that also include greater geographical resolution so as to account for GHG emissions from anticipated land-use changes and anthropogenic aerosols, which have important regional climate impacts (Nakicenovic and Swart 2000). Whether for practical reasons of limited computational resources or their own tacit social judgments about the probability of business-as-usual emissions trajectories, climate modelers have tended to base their global warming projections on "business as usual" scenarios. As a result, the political discussion based on those model outputs is driven by a misleadingly narrow picture of the potential for quite different sets of future conditions. For a discussion of the problems of this baseline mentality, see Cohen et al. (1998).
7. I am indebted to Steve Rice for this insight.
8. State-of-the-art climate models require such hugely expensive supercomputing facilities that there are only a dozen or so leading research centers worldwide. The uneven geography of climate modeling science has had important political implications. Without their own modeling capacity, leaders of developing countries have sometimes been reluctant to put much faith in the scientific pronouncements of unfamiliar experts. Partly in response to this confidence gap, the UNFCCC and the IPCC have both made increasing scientific capacity in developing countries a priority so as to enroll them in the epistemic community growing up around the general circulation models and the IPCC assessment process (Agrawala 1998b; Kandlikar and Sagar 1999).
9. In a response to an earlier version of this essay, Cindi Katz (1998) emphasized to me the importance of the distinction between Reductionism and abstraction, though she is not responsible, of course, for the particular way I have rendered it here.
10. Atmospheric GCMs, which three-dimensionally model only the atmosphere at a fine grid scale for short-term weather forecasting purposes, are sometimes distinguished from coarser-scaled but more comprehensive atmosphere-ocean GCMs that couple separate three-dimensional models of the global ocean and of the global atmosphere so as to simulate climate dynamics over long time scales. The basic structure of all these three-dimensional models is essentially the same. For simplicity sake I will refer to them generically as GCMs to distinguish them from one- and two-dimensional radiative-convective and energy balance climate models, which are often (though not always) less complex and less comprehensive. For an overview of these different types of climate model and their uses, see Harvey et al. (1997) and McGuffie and Henderson-Sellers (1997).
11. Although the U.S. Army and Navy Research Offices financed climate modeling research at a number of American universities and National Laboratories (Hart and Victor 1993), the military connection was much more direct in the United Kingdom and Canada, where meteorological services were originally attached to Ministries of Defence (Fitzpatrick 1992). As with Bjerknes' development of the meteorological theory of the polar front during World War I (Friedman 1989), this military connection and the obvious military applications of numerical climate models for weather forecasting were important in winning early support for the establishment in those countries having national climate modeling centers, which have since become world leaders in the field. By contrast, the heterogeneous system of research funding in the U.S. fostered a number of competing climate modeling centers and GCMs. The most prominent of these are NOAA's Geophysical Fluid Dynamics Laboratory at Princeton University (created in 1955), the National Center for Atmospheric Research (NCAR) in Boulder, Colorado (established in 1960)—sponsored by the U.S. National Science Foundation, which also financed important early modeling research at UCLA and the RAND

Corporation—and NASA's comparatively recent Goddard Institute for Space Studies in New York City (Hecht and Tirpak 1995; Nebeker 1995). A member of the U.S. National Research Council's Committee on the Human Dimensions of Global Change recently told me that, in the face of the now widely recognized superiority of the British and Canadian national GCM models, which were used in preference to their more numerous and competing American counterparts in the congressionally mandated U.S. national assessment study (Kerr 2000), federal science managers have begun to reconsider their funding policy and to encourage the centralization of U.S. modeling research at NCAR. At the same time, American science managers are also lobbying Congress to overturn the ban on Japanese supercomputing imports, which is another factor in American modeling centers' falling behind in the GCM race (Reichhardt 1999).

12. It is perhaps significant that a number of the more vocal skeptics of the GCMs and the physics-based theory of an enhanced greenhouse effect, such as geographers Robert Balling (1996) and Tim Ball (1997), hail from this empirical tradition of descriptive climatology. With their knowledge of the observational record, they have insisted both that the case for *anthropogenic* climate change is unproven and that the GCMs have been effectively falsified by the incongruity between their predictions of global warming and the observational record. These claims are not widely accepted. They are disputed both by other climatologists familiar with the anomalies and uncertainties of the observational records put forward by Balling and others as falsifying the models and by modelers themselves, who insist that the resolution of subgrid processes is not an adequate test of the models (Mahlman 1997; Trenberth 2000).
13. The nature of the interagency accounting within the USGCRP makes cost estimates difficult. The figures given in USGCRP (2000, 57) suggest that satellite-based earth observation systems soaked up as much as 75 percent of the USGCRP budget in fiscal year 1998, but program director Michael MacCracken (1996, 7) reported that in fiscal year 1995, the USGCRP "devoted about 60 percent of its budget to observations and data management." In fiscal year 2001, the USGCRP is requesting $897 million, or just over 51 percent of its $1.74 billion budget, "for the space-based observation component" of the USGCRP (2000, 57). In his recent testimony to the Senate Science Committee, Neal Lane (2000, 8), Assistant to the President for Science and Technology, celebrated this relatively reduced expenditure on the satellites as proof of "the progress that has been made over the last five years in increasing the proportion of USGCRP funding for scientific research and analysis."
14. Because GWPs fail to account for indirect forcing, many scientists also regard it as an oversimplistic and unscientific *policy* abstraction (Smith and Wigley 2000), though for rather different reasons than Agarwal and Narain (1991). The point of their critique is that, even if these scientific uncertainties were fully resolved, GWPs would still be a politically biased measure because their physical reductionism ignores important social facts.
15. The recent Kyoto Protocol depends fundamentally upon scientific calculations of GWP. The Protocol's flexible, market-based implementation mechanisms of emissions trading, sink enhancement, and "clean development" require GWP to make the emissions of six different—and differently located—GHGs fully interchangeable in terms of their CO_2-denominated GWP. Moreover, the Protocol itself is meaningless without GWP as the currency of a universal economy of greenhouse gas exchange. National compliance with the Protocol is determined by comparing nationally comprehensive GHG emissions totals, which are calculated from GWP standardized emissions of six different GHGs against a similarly standardized 1990 baseline of that country's emissions and then adding any credits gained through emissions trading or other flexible mechanisms.
16. For this reason, the IPCC prefers the term "projection," because "it acknowledges that the model and the characteristics of the climate change experiments have major limitations and that it is not possible, therefore, to attach a probability to the results of such a description" (Viner and Hulme 1997, 15). Significantly, the uncertainty that Viner and Hulme (1997) refer to here is uncertainty in the *physical sciences* about the representation of climate system dynamics, so my wider criticism of this implicit environmental determinism of its business-as-usual baseline thinking still applies.
17. Since this test focuses on model outputs, as opposed to the representation of component parts, it is somewhat at odds with the commitment of modelers to the physically reductionist ideal that "when all the physics is adequate, regional fidelity should come as a matter of course" (Wigley, quoted in Schlesinger 1988, 872). Modelers also test GCM parameterizations of particular component processes, but one of the biggest modeling challenges is understanding the often nonlinear responses caused by feedbacks between component parts of a coupled GCM. In practice, therefore, the physically reductionist impulse to focus first on modeling the parts rather than whole is considerably tempered.

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