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Science studies, climate change and the prospects for constructivist critique

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Abstract

Starting from the debates over the 'reality' of global warming and the politics of science studies, I seek to clarify what is at stake politically in constructivist understandings of science and nature. These two separate but related debates point to the centrality of modern science in political discussions of the environment and to the difficulties, simultaneously technical and political, in warranting political action in the face of inevitably partial and uncertain scientific knowledge. The case of climate change then provides an experimental test case with which to explore the various responses to these challenges offered by Ulrich Beck's reflexive modernization, the normative theory of expertise advanced by Harry Collins and Robert Evans, and Bruno Latour's utopian vision for decision-making by the 'collective' in which traditional epistemic and institutional distinctions between science and politics are entirely superseded.

Keywords: reflexive modernization; expertise; politics; actor-network theory.

One small and somewhat perverse indication of the enormous significance of global warming is that the issue has gotten dragged into academic debates about science studies and the implications for critique of social constructionist theory. Notwithstanding the robust scientific consensus to the contrary (Oreskes 2004), a small but vocal band of self-styled 'climate sceptics' continues to deny the risks of anthropogenic climate change. Their denials have been greatly amplified by the deep pockets of multinationals with vested interests in the consumption of fossil fuels, which are the largest anthropogenic

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source of the greenhouse gases (GHGs) implicated in global warming. These special interests are responsible for a slick public relations campaign seeking to discredit the Intergovernmental Panel on Climate Change (IPCC 1996: 4) and its conclusion that 'the balance of evidence suggests that there is a discernible human influence on global climate'. Bypassing the normal scientific process of peer review through newspaper editorials and advertising, a host of conservative think tanks and industry-funded political action committees have spent millions disseminating the counter-claim that global warming is an incredible hoax foisted upon us by crazed eco-zealots and grasping bureaucrats trying to choke us in red tape (McCright and Dunlap 2000; Lahsen 2005).

Such special interest organized scepticism is particularly influential in the United States, which, under President George W. Bush, formally withdrew from the 1997 Kyoto Protocol. Wrapping up a two-hour address on the senate floor, Oklahoma Senator James Inhofe, the Republican chair of the Senate Committee on Environment and Public Works, which oversees the US agencies funding climate research, recently declared:

> With all the hysteria, all the fear, all the phony science, could it be that manmade global warming is the greatest hoax ever perpetrated on the American people? I believe that it is. And if we allow these detractors of everything that has made America great, those ranging from the liberal Hollywood elitists to those who are in it for the money, if we allow them to destroy the foundation, the greatness of the most highly industrialized nation in the history of the world, then we don't deserve to live in this one nation under God.
>
> (Inhofe 2003: S10022)

Likewise the Bush administration has repeatedly denied the scale and potential risks – even the very existence – of anthropogenic climate change. 'A report put out by the bureaucracy' (quoted in Seelye 2002) was how the President airily dismissed the US Climate Action Plan and its matter-of-fact endorsement of the view, expressed both by the IPCC (2000) and by the National Research Council (NRC 2001), that recently observed climate changes 'are likely due mostly to human activities' (US State Department 2002: 4). Under the heading 'Winning the global warming debate', a leaked Republican election strategy document counselled candidates to 'challenge the science' because if 'the public come to believe that the scientific issues are settled, their views about global warming will change accordingly' (EWG 2003: 138, 137). White House officials insisted on such extensive changes to the EPA's (2003) state of the environment report that EPA scientists deleted almost all reference to climate change rather than let the Administration, as one internal EPA memo put it, insert so many qualifying caveats as to 'create uncertainties . . . where there is essentially none' (quoted in UCS 2004: 6).

This first controversy about the scientific facts of climate change intersects with a second about the academic field of science studies. This otherwise heterogeneous body of academic scholarship is united by its agnostic stance

towards scientific truth and its emphasis on the socially contingent manner in which the objects of science are constructed and knowledge about them is socially validated. Different scholars account for this contingency in different ways, and there is an extensive debate within science studies about the philosophical, methodological, and political implications of different approaches to the social construction of science, knowledge, and nature (for a recent review, see Sismondo 2004). What is important for my purposes here is that, notwithstanding their disciplinary and methodological diversity, science studies scholars share a common hostility to the logical positivist project of discovering a unique and incontrovertibly rational method to demarcate science from non-science.

Although logical positivism is now largely exhausted as a philosophical project, the pious reverence for science it encouraged lives on. With its sensitivity to the influence of partisan special interests, American political culture has long idealized science as a highly disciplined and rule-bound way of ensuring the objectivity and disinterestedness of decision-making (Porter 1995). Thus, in the same speech in which he called climate change a 'hoax', Senator Inhofe self-righteously intoned:

> We are going to make our decisions not on a political agenda, but on sound science. . . . To the average person hearing, 'all you want is sound science,' that sounds perfectly normal. Why would we *not* want sound science? Why predicate decisions on something that has nothing to do with sound science? But leftwing environmental communities insist sound science is outrageous. For them a pro-environment policy can only mean top-down command-and-control rules dictated by bureaucrats; science is irrelevant. Instead for extremists politics and power are the motivating forces for making public policy. Sadly that is true in the current debate over many environmental issues. Too often, emotions stoked by irresponsible rhetoric rather than facts based on objective science shape the contours of environmental policy.
>
> (Inhofe 2003: S10013)

Inhofe's image of 'sound science' rests on a simplistic correspondence theory of truth and a crude understanding of empirical data as unproblematically available to observation free from any prior theoretical commitments. This kind of crude naturalism leaves little room for any trace of the experimental contingency, expert interpretation, or active intervention, manipulation, and creation of scientific phenomena that science studies has shown to be central to scientific knowledge and practice. Instead, it encourages a sharp, and in practice very difficult to maintain, differentiation between the empirically proven facts of science and political values or mere theories. American conservatives are now mobilizing this discourse of sound science to question the scientific basis for concerns about climate change and other environmental problems. For example, at the behest of high-level Bush Administration officials (Gelbspan 2004: 57–8), the Competitive

Enterprise Institute (2003) recently launched a legal challenge against the publication of the National Assessment on Climate Change (USGCRP 2001) on the grounds that 'The basic rule of science is that hypotheses must be verified by observed data before they can be regarded as facts. Science that does not do this "junk science", and at minimum is precisely what [the Data Quality Act] is designed to bar from the policymaking process' (quoted in Herrick 2004: 426).

A similarly idealized image of 'science' underwrites much of the academic backlash against science studies. For instance, the physicist Alan Sokal offered this justification for his notorious spoof of science studies:

> What concerns me is the proliferation ... of a particular kind of nonsense and sloppy thinking: one that denies the existence of objective realities. ... [M]y concern about the spread of subjectivist thinking is intellectual and political. Intellectually, the problem with such doctrines is that they are false (when not simply meaningless). There *is* a real world; its properties are not merely social constructions: facts and evidence do matter. What sane person would contend otherwise? And yet, much contemporary academic theorizing consists precisely of attempts to blur these obvious truths. ... Politically, I'm angered because most (though not all) of this silliness is emanating from the self-proclaimed Left. ... For most of the past two centuries the Left has been identified with science and against obscurantism. ... The recent turn of many 'progressive' or 'leftist' academic humanists and social scientists toward one or another form of epistemic relativism betrays this worthy heritage and undermines the already fragile prospects for progressive social critique. Theorizing about the 'social construction of reality' won't help us find a cure for AIDS or devise strategies for preventing global warming.
>
> (Sokal 1996: 63–4)

For Sokal, the political problem with science studies and the constructionist talk it has encouraged is that it leaves no epistemologically secure foundation from which to speak truth to power. Many on the traditional Left fear that the powerful stand to gain the most from epistemic scepticism about scientific truth. Other critics, however, insist that construction talk is an essential tool for critique, though, as I will try to show, the parameters of constructionist critique are understood in some very different ways. Whereas Bruno Latour's vision for a new 'cosmopolitics' depends on abandoning the absolute distinctions between 'the rational and the irrational, between the indisputable "fact" of nature and the merely social "representation"' used, in the modern constitution, 'to abort politics ... [and] dictate moral conduct in the place of ethics' (2004a: 190, 19), Andrew Sayer insists that losing these anchor points would be 'disastrous for emancipatory movements' (2000: 98).

Such charges and counter-charges have become familiar from debates across a number of academic fields ranging from feminism to environmental studies. Not only has the table thumping bravado associated with the performance of such bottom-line arguments become tiresome (Ashmore *et al*. 1994), it also

tends to polarize debate and oversimplify the rich variety of constructionist understandings of science and its politics.

In this paper I want to bring the controversies about climate change science together with those about academic science studies in hopes of thereby clarifying what might be at stake politically in constructivist forms of critique. The example of climate change shows the difficulty of identifying constructionist science studies with any consistent philosophical or political position. Whereas Sokal (1996), like Paul Gross and Norman Levitt (1994) before him, matter-of-factly identified science studies with the 'Academic Left', it was not long before other critics connected it with the specific efforts by the climate sceptics to deconstruct the scientific basis for GHG emission reductions (e.g. Murphy 1995; Redclift and Woodgate 1997). Riley Dunlap and William Catton, for instance, rejected the 'constructivist orientation' of environmental sociology precisely because of its political implications: 'if global change is seen as primarily a social construction rather than an objective (albeit imperfectly understood) condition, then it poses little threat to the future of our species' (1994: 23). Confronted by the climate sceptics and other 'dangerous extremists . . . using the same argument of social construction to destroy hard won evidence that could save our lives' even that arch iconoclast Bruno Latour has rhetorically asked whether 'I was wrong to participate in the invention of science studies' (2004c: 227).

This is a serious question, but the philosophical and political implications of constructionism are not as straightforward as critics have sometimes assumed. Indeed, a major problem with the term has been a lack of precision about its use. Sokal, for instance, mocks those 'who believe that the laws of physics are mere social conventions' by inviting them 'to try transgressing those conventions from the windows of my [twenty-first floor] apartment' (1996: 62). In so doing, however, he dismissively conflates a number of different possible uses of the construction metaphor – the construction of *social* reality (i.e. gender as a social convention about sexual difference opposed to sex itself, the biologically given, immutable material reality of those differences), the institutional construction of the social institutions and traditions of science (i.e. the historical emergence of physics as a discrete scientific discipline), the construction of knowledge and standards of proof (i.e. how certain beliefs about what we call 'gravity' came to be considered 'laws', itself an interesting juridical metaphor), the social construction of objective reality (i.e. the idealist claim that belief in gravity is what makes it real). Each of these constructionisms implies somewhat different philosophical and political commitments, and they cannot all be tarred (or lauded) with the same brush (Hacking 1999; Demeritt 2002).

These controversies over climate change and science studies raise important questions, at once epistemological and normative, about the relationship between science and politics. While 'sound science' has, in the United States especially, become something of a codeword for conservative obstructionism, environmentalists, along with many others on the traditional Left, are no less committed to science as the antidote to false consciousness, ideological

misrepresentation and our deep 'irrational tendencies' (Weinberg 1996). Thus one month after the right-wing George C. Marshall Institute published *Politicizing Science* (Gough 2003), detailing how science was being manipulated by American liberals to advance their political agenda, Democratic Congressman Henry Waxman called a press conference to publicize the release of a report entitled, *Politics and Science in the Bush Administration*, detailing 'numerous instances where the Administration has manipulated the scientific process and distorted or suppressed scientific findings' (2003: i) to serve its policy agenda. Waxman's report was soon followed by another from the Union of Concerned Scientists (UCS 2004) exposing 'the Bush Administration's Misuse of Science'.

The case of climate change exemplifies many of the practical difficulties highlighted by science studies research with this ideal of sound science as a foundation for policy. But what kind of politics are we left with, then, if 'the closure of scientific and technical debates' about matters such as climate change (Collins and Evans 2002: 239) is to be regarded as a socially contingent (and thus deconstructable) achievement? To what extent does such a constructivist stance leave room for speaking truth to power? Or should the warrant for political critique, as indeed for epistemology, rest on something else altogether?

Reflexive modernization and science

To work through these questions I want to begin with Ulrich Beck's theoretical account of reflexive modernization, because it suggests an explanation for the increasing urgency of these concerns about science in politics. Beck (1992b) contends that we are now living in a transitional period. Industrial society, based on the unquestioned application of science to the production and distribution of goods, is giving way to a 'risk society' in which questions about the management and minimization of 'bads' such as global warming are becoming central to the organization of society. Public recognition that such risks are not external givens but instead the contingent by-products of 'techno-economic development itself' has engendered a process Beck terms 'reflexive modernization'. No longer taking science at its word, citizens are now beginning to subject it to the same searching scrutiny as the traditional superstitions of old. Beck (1992b: 165) sees science studies and academic theories of social construction as one manifestation of this new reflexivity about science and technology. Abandoning the scientific deference of 'industrial society', embodied by Karl Mannheim's (1954) sociology of knowledge, science studies scholars have questioned 'what comes to count as scientific knowledge and ... how it comes so to count' (Collins 1983: 267). Reflected in controversies ranging from genetic engineering and GM foods to cancer and chemical carcinogens, such public reflexivity about science marks

the final achievement of the Enlightenment's emancipatory promise. Beck writes:

> The public sphere, in co-operation with a kind of 'public science', would be charged as a second centre of 'discursive checking' of scientific laboratory results in the crossfire of opinions. Their particular responsibility would comprise all issues that concern the broad outlines and dangers of scientific civilization and are chronically excluded in standard science. The public would have the role of an 'open upper chamber'. It would be charged to apply the standard, 'How do we wish to live?' to scientific plans, results, and hazards.
>
> (Beck 1992a: 119)

This utopian vision would be more attractive if Beck were more explicit about precisely what it entails for the relationship between science and politics. What kind of '"discursive checking" of scientific laboratory results' does Beck hope the public will perform in his 'upper house'? At times Beck's account of the 'generalization of scientific argumentation' whereby 'more and more people are able to play the role of assessors of science' (1992b: 168) seems to imply that the role for public participation is to double-check the adjudications of fact and truth performed by the lower house of science. We can call this vision of the epistemic, truth-determining role of the public in science 'Beck$_1$' (Figure 1a). 'Only a strong competent public debate, "armed" with scientific arguments is capable of separating the scientific wheat from the chaff', Beck (1992a: 119) writes. To liberal political theorists like John Dewey, who lamented the technocratic 'eclipse of the public' (1984: 307), which maintained only the right, but not the actual ability, to participate in policy deliberations, Beck counters that the proliferation of 'dissenting voices [and] alternative experts' is enabling 'the institutions for directing technology – politics and law – to reconquer the power of their own judgment' (1992a: 119) by

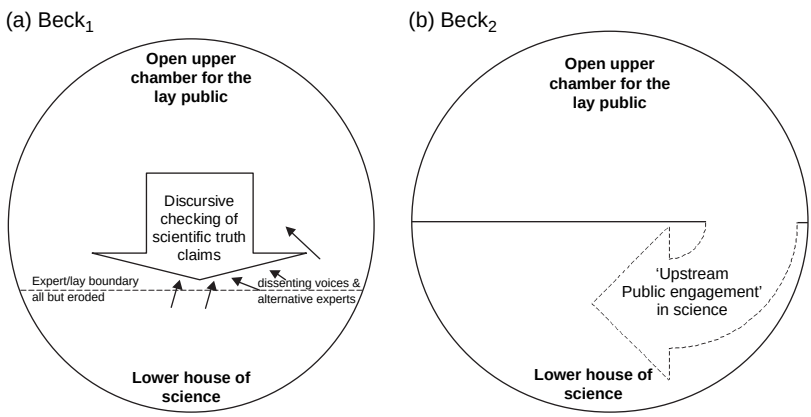

Figure 1 Discursive checking and the upper house according to (a) Beck$_1$ and (b) Beck$_2$

dissolving the sharp epistemic and institutional distinctions of industrial society between the amoral technical deliberations of scientific experts and the morally charged but often irrational and ignorant politics of the public sphere. The parliament in $Beck_1$ is practically unicameral because the public house, like the UK House of Commons under the Parliament Act, can override the decisions of the second chamber. But insofar as its say depends on appropriating the scientific knowledge of 'dissenting voices [and] alternative experts', the vision in $Beck_1$ is not too dissimilar to Dewey's liberal lament in which political authority draws its warrant from cognitive authority (Turner 2001). Ironically this same relationship between knowledge and power also features centrally in the deficit model of public (mis)perception of risk (Wynne 1995) that Beck otherwise so categorically rejects on the grounds that it overlooks the 'cultural premises of acceptability contained in scientific and technical statements on risk' (1992b: 58).

Thus another way to read Beck's vision for the public upper house is as filling a largely normative rather than epistemic function. We might term this '$Beck_2$' (Figure 1b). By applying the normative 'standard ''How do we wish to live?'' to scientific plans, results, and hazards' (Beck 1992a: 199), the public upper house would be responsible for regulating techno-scientific innovations made by the lower house. Insofar as $Beck_2$ depends on already established distinctions between the scientific work of discovering new facts and the political work of agreeing on the values to regulate them, it may sound more like a description of the status quo than some new, more reflexive modernization. This apparent contradiction might be resolved if $Beck_2$ were read as a plea for 'upstream public engagement in science' (Wilsdon and Willis 2004). For its advocates, citizen juries, consensus conferences, and other forms of upstream public engagement provide a mechanism for 'expos[ing] to public scrutiny the values, visions, and assumptions that usually lie hidden' in scientific practice (Wilsdon and Willis 2004: 24). In this way the public upper house would not simply apply political controls to regulate whatever scientific innovations emerge from an autonomous lower house but would also be actively involved *beforehand* in steering the course of future scientific research. Thus, rather than dissolving entirely the distinctions between science and politics, as in $Beck_1$, the kind of upstream public engagement called for by $Beck_2$ would make the institutional boundary between them more porous while at the same time preserving the epistemic distinction between facts and values.

(De-)constructing the hockey stick

The controversy over climate change suggests, however, that public 'exposure of scientific uncertainty' is not always the politically progressive and reflexive 'liberation of politics, law and the public sphere from their patronization by technocracy' that Beck (1992b: 109) sometimes imagines. With millions in fossil fuel money behind them, a small group of climate sceptics have been able

to organize and sustain public scepticism in the United States about the 'reality' of global warming. Much of their recent efforts have been aimed at deconstructing the so-called 'hockey stick' reconstruction of northern hemispheric temperatures (Figure 2).

First published in *Nature* and then subsequently modified and extended in several further publications (Mann *et al*. 1998, 1999), this dramatic figure assembles the best available estimate of hemispheric scale temperature fluctuations over the last thousand years. It provides one (but by no means the only) basis for the claim, in the summary for policy-makers of the third IPCC assessment report, to have found 'new and stronger evidence that most of the warming observed over the last 50 years is attributable to human activities' (2000: 100). As such the 'hockey stick' has assumed an almost iconic status in recent debates in the United States about global warming.

Very different from simply reading a thermometer, estimating the average annual temperature of the hemisphere as a whole over a millennium is quite literally an act of reconstruction. Behind the hockey-stick graph stands the congealed labour of hundreds, perhaps even thousands, of scientists involved in assembling, manipulating, and transforming various recalcitrant materials annually laid down in tree ring and ice cores, laminated lake and ocean sediments, and ocean corals (to name just the finest resolution sources of proxy palaeoclimatic data). Through careful measurements and complicated statistical techniques scientists relate various quantitative properties of those materials to each other and to various other temperature and precipitation records so as to reconstruct individual records of average annual temperatures. Combining these

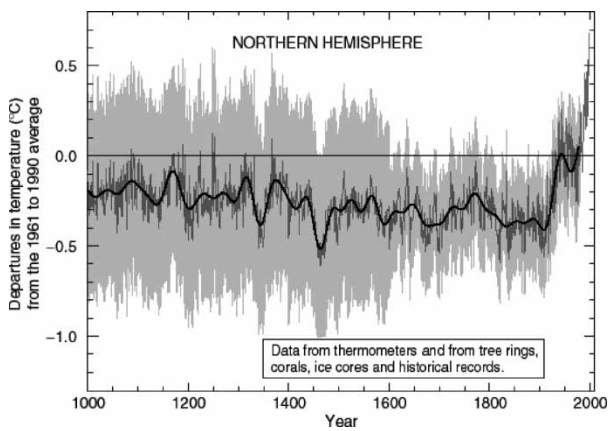

Figure 2 The Mann *et al*. (1999) 'hockey stick' diagram of average annual temperature anomalies in the northern hemisphere since 1000 A.D. published as figure 2.20 in IPCC (2000). The light grey represents the 95% confidence range for the annual data shown by the dark jagged line. The smooth black line is a 40 year running mean.
Source: Intergovernmental Panel on Climate Change. Printed with permission.

heterogeneous proxy records for various places and regions into a spatially homogenized annual time series for the hemisphere as a whole then involves further calibrations against instrument and other records (themselves, of course, also the result of previous rounds of statistical purification) to ensure that the final palaeo-climatic reconstruction represents average annual temperatures over the hemisphere as a whole. Each step in this process of assembling a reliable witness to historic climate changes in the northern hemisphere over the last millennium involves potentially contestable judgements and practices (Bradley 1999).

While science studies scholars have become adept at disentangling this mangle of practice and identifying the contingent constructions involved, so too have climate sceptics. The hockey stick and its lead author, Michael Mann, have become the object of intense and often highly personal attack. Two of the most prominent climate sceptics, Willie Soon and Sallie Baliunas (2003), drew selectively on some of the same proxy palaeo-climatic data in the hockey stick to counter the conclusion that recent warming is unprecedented. Their counterclaims were fiercely denied by Mann and colleagues, who took the unusual step of publishing a rebuttal in *Eos*, the newsletter of the American Geophysical Union (Mann *et al*. 2003), as well as by many of those whose data were used by Soon and Baliunas (2003).[1] Despite such criticisms, these new 'findings' were quickly touted in press releases by the American Petroleum Institute, which helped finance the study, and in a barrage of op-ed pieces put out by the smoothly oiled anti-climate change PR machine (Gelbspan 2004).

Using the power of the majority, Republicans in Congress fanned the hockey-stick controversy still further. In July 2003 the Inhofe-chaired Senate Committee on Environment and Public Works subpoenaed public testimony from Mann as well as several of his critics, noted for their prominence more among right-wing think tanks than in the climate research community. Dismissing the hockey stick as a 'Biased Record Presented by the IPCC and National Assessment', David Legates, a University of Delaware geographer and co-author, along with Baliunas and several other sceptics (Soon *et al*. 2003), of another critique of Mann, published in *Energy and Environment*, an obscure social science journal edited by Hull University geographer Sonja Boehmer-Christiansen, erroneously testified to Congress – as is plain from Figure 2 – that 'Mann et al. replace the proxy data for the 1900s by the instrumental record and no uncertainty characterization is provided. This too yields a false impression that the instrumental record is consistent with the proxy data and that it is "error free"' (2003). Legates went on to make a less implausible complaint about the contestable judgements and uncertainties involving in constructing a hemispherically averaged temperature from so many individual palaeo-climatic records (themselves of course highly processed inscriptions retracing the performance of successive rounds of enrolment, transformation, and purification of various heterogeneous elements and materials):

> Where we differ with Dr. Mann and his colleagues is in their construction of the hemispheric averaged time-series, their assertion that the 1990s are the warmest

> decade of the last millennium, and that human influences appear to be the only significant factor on globally averaged air temperature. Reasons why the Mann et al. curve fails to retain the fidelity of the individual proxy records are detailed statistical issues into which I will not delve. But our real difference of opinion focuses solely on the Mann et al. curve and how we have concluded it misrepresents the individual proxy records. In a very real sense, this is an important issue that scientists must address before the Mann et al. curve is taken as fact.
>
> (Legates 2003)

Most recently, in June 2005, Representative Joe Barton of Texas, the Republican chair of the House Energy and Commerce Committee, wrote personally to Mann and his co-authors demanding an immediate response to a two-page list of questions about the sources, methods and analytical judgements behind their climate reconstructions, details of all the research and other funding they have ever received, the contractual terms of any previous federal grants or contracts, and about their precise roles in the IPCC process. The pretext for this investigation was a publication by Stephen McIntyre, a former mining engineer, and an economist Ross McKitrick (2005) charging that the statistical techniques used by Mann distort the palaeo-climate record and tend to produce anomalous 'hockey-stick' shapes even from randomly generated time series. Although these claims are hotly disputed, they received extensive media coverage, including a front-page article and subsequent editorial in the *Wall Street Journal* (Regaldo 2005). Though even the Republican chair of the House Science Committee has condemned Barton's letter as a 'misguided and illegitimate . . . attempt to intimidate' the scientists involved (quoted in Eilperin 2005), the controversy, like the Barton Committee investigation, continues.[2]

Debate over the hockey stick nicely illustrates what sociologist of science Harry Collins has termed 'the experimenter's regress' (1985: 2). Because 'experimentation is a matter of skillful practice' it is very difficult to perform a critical test of theory in the way that simplistic ideas of sound science insist, since 'it can never be clear whether a second experiment has been done sufficiently well to count as a check on the results of a first'. Whereas conservative advocates of sound science insist upon definitive empirical proof, science studies scholars have found that, in controversial science, more data and experiments tend to raise new questions as much as resolve old ones. As Collins and Pinch provocatively put it, 'experiments tell you nothing unless they are competently done, but in controversial science no one can agree on a criterion of competence. Thus in controversies . . . . scientists disagree not only about results, but also about the quality of each other's work' (1993: 3). In his testimony Mann responded that, unlike 'experienced paleoclimate researchers', Soon *et al*. (2003) failed to make important distinctions between proxy records for moisture and those for temperature. Furthermore, by refusing to aggregate to the hemispheric scale or to compare against a temporally normalized benchmark, they confuse short-term regional fluctuations (which

can be explained by the wave-like character of the jet-stream, making some regions cool while others are warmer) with truly hemispheric, long-term changes, which 'tend to be much smaller in magnitude than those for particular regions, due to the tendency for a cancellation of warm and cold conditions in different regions. . . . What makes the late 20th century unique is the simultaneous warmth evident during this period in Northern Hemisphere average temperatures' (Mann 2003).

Conservative ideologues have systematically ignored these technical qualifications and the other independent lines of evidence for concerns about global warming. In the summer of 2003, the White House demanded that Mann's hockey-stick figure be removed from the EPA's (2003) report on the state of the environment report and be replaced by a reference to Soon *et al*. (2003), which one EPA official characterized as 'a limited analysis that supports the administration's favored message' (quoted in UCS 2004: 6). Referring specifically to the controversy over the Mann *et al*. graph, Senator Inhofe recently repeated his claim that 'global warming is the greatest hoax ever perpetrated on the American people' (2004: S11292). Seizing the sound science high ground, he declared, 'global warming ideology has no place in policy debates regarding scientific issues. Credible, reproducible studies should be our gold standard – our minimum standard. By that standard, carbon restrictions fail the test' (ibid.: S11297). This empiricist framing of sound science disregards the future risks posed by the continuing rapid and anthropogenically driven increases in GHG concentrations and the evidence from black body physics, simulation modelling, palaeo-climatic analogues, and other sources about the climatic changes likely to result from them. Instead, it seeks to focus debate on the technically difficult question of whether or not at *present* GHG concentrations, an anthropogenic 'fingerprint' of global warming, can be empirically detected within the noisy, heterogeneous, and incomplete climate record.

Constructionist science studies and the problem of extension

It is difficult to see how this campaign to discredit the science of climate change is very reflexive or enlightened. Beck himself worried that the demonopolization of scientific authority under a more reflexive modernization would transform 'the sciences . . . into self-service shops for financially well endowed customers in need of arguments', though he also held out hope that 'as a result of successful learning in contacts with the sciences . . . unwelcome results can be blocked professionally' (1992b: 172). Other critics, however, are less sanguine. For them, these special interest attacks on climate science exemplify everything that is wrong with constructivist scepticism about science (e.g. Murphy 1995).

Though there are no substantive connections between science studies scholars and the right-wing climate sceptics close to the Bush Administration,

their arguments exemplify many of the wider claims made in academic science studies about the construction of scientific knowledge. Highlighting the technical uncertainties, evidentiary under-determination, social interests, and contestable judgements and practices involved in producing and evaluating climate science, conservative climate sceptics insist that global warming must not be regarded as 'a proven phenomenon', or even a potential future risk, but rather, as Republican Congressman Dana Rohrabacher provocatively put it, 'at best to be [an] unproven [hypothesis] and at worst to be liberal clap trap' (quoted in Brown 1996: 33).

Harry Collins and Robert Evans see this kind of politicized continuation of scientific debate as the logical outcome of constructionist science studies. Its methodological relativism explained scientific facts as a sociological feat of consensus building. In so doing science studies undermined the epistemic basis for excluding non-scientists from scientific debates and provided the intellectual justification for the now widely accepted idea of 'increased participation by the public to solve the Problem of Legitimacy' (Collins and Evans 2002: 263) in risk management and other areas of science-based decision-making (e.g. UK House of Lords 2000). In the process though, constructionist science studies 'create[d] a new problem of extension' (Collins and Evans 2002: 269), because, 'like the approaches of Ulrich Beck' (ibid.: 249) (or at least Beck_1), it confused the 'role of expertise and the role of democratic rights' (ibid.: 260) in technically informed decision-making. By denying the traditional epistemic justification for heeding the knowledge claims of experts, science studies made it difficult to resolve controversy over matters of fact, because debate can always be extended by dissenters, however ignorant, ill-informed, or duplicitous their claims. Furthermore its 'ready acceptance of the idea that science is politicized through and through rule[d] out the possibility of complaint . . . that certain scientific and technical arguments are hopelessly biased by their sources' (ibid.: 280).

The debate over climate change provides a good example of these problems. To counterbalance the enormous weight of scientific opinion embodied by the conclusions of the IPCC, climate sceptics point to the fact that over 17,000 people, including a number of Nobel prize winners, have signed the Oregon petition (e.g. Inhofe 2004: S11292). This declares: 'There is no convincing scientific evidence that human release of carbon dioxide, methane, or other greenhouse gasses is causing or will cause catastrophic heating of the Earth's atmosphere and disruption of the Earth's climate' (Oregon Institute of Science and Medicine 2005). Its organizer, Professor Frederick Seitz, may be a former president of the US National Academy of Sciences, but his background is in solid-state physics, and therefore, like most signatories, he possesses no special expertise to judging this particular proposition. If the petition's signatories do not find the IPCC reports 'convincing' or find the kinds of climate changes they describe insufficiently 'catastrophic' to warrant political action, those should not be regarded as scientific judgements about the evidence. Rather, they are political judgements about what to do about it. To claim otherwise,

Collins and Evans (2002) contend, is to confuse membership of the scientific community in general with that of the core set of experts relevant to any given technical question.

To solve these problems Collins and Evans propose 'a normative theory of expertise' (2002: 238) (Figure 3). Complicating the simple expert versus lay divide, their theory of expertise recognizes the plurality of the sciences and the fact that having a PhD in one specialist area of research does not qualify you as an expert in others. At the same time, it also acknowledges the diversity of lay knowledges and seeks to recognize the substantive contributions to scientific debates made by uncertified specialists from among what had been previously regarded as a uniformly unqualified lay public. As a result the line separating the upper and lower house in Figure 3 is jagged to take in 'the odd-shaped pockets of expertise found among the lay public' (ibid.: 251) and exclude those members of the scientific community not possessing the core-set expertise necessary to evaluate the technical details of the issue in question. In this way, Collins and Evans seek to reinscribe more precisely the political-epistemological distinction 'between scientific expertise and political rights' (2002: 250), which second wave science studies, like $Beck_1$, dissolved with its agnostic approach to expertise as a socially constructed attribution of epistemic authority. By this logic the Oregon petition signatories have the same democratic rights to participate in the political debate about how to respond to climate change as ordinary members of the public, but, for Collins and Evans (2002), their views are simply not relevant to deciding the scientific question of whether or not anthropogenic GHG emissions are in fact causing global warming. On that proposition, the views of the core set of experts involved in the IPCC scientific assessment should be authoritative, though Collins and Evans are silent about whether the basis for this authority rests on

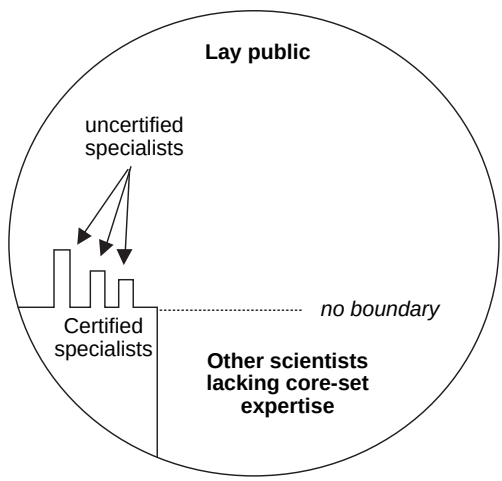

Figure 3 A normative theory of expertise, after Collins and Evans (2002)

a correspondence or conventionalist theory of truth. Both epistemologies are consistent with their political-epistemological claim that matters of fact should be decided by experts alone.

Thus the normative theory of expertise proposed by Collins and Evans (2002) is consistent with the $Beck_2$ vision of reflexive modernization in its desire to remap the institutional boundary between science and the lay public, while still preserving the epistemic distinction between facts and values. Matters of fact about global warming would be the preserve not of scientists *per se* (Nobel prize-winning economists have nothing special to contribute), but of the core set of experts, certified and otherwise, in a lower house. The role of the public upper house would be restricted to reconciling disputes over values, to 'apply[ing] the standard "How do we wish to live?" to scientific plans, results, and hazards' (Beck 1992a: 119). Collins and Evans conclude:

> It seems, then, important to retain a notion, even [if] it is an idealized one, of a core-set community in which expertise is used to adjudicate between competing knowledge-claims and to determine the content of knowledge. The wider society still has a role to play in forming a view about the socially acceptable use of such knowledge and what to do while such knowledge remains contested, but this contribution lies in the political sphere. Lay people as lay people, however, have nothing to contribute to the scientific and technical content of debate. Even specific sets of lay people . . . have a special contribution to make to science and technology only where it can first be shown that their special experience has a bearing on the scientific and technical matters in dispute.
>
> (Collins and Evans 2002: 281)

From matters of fact to matters of concern

These efforts to isolate a space in politics for deciding purely scientific questions of fact face several problems. First, as a number of critics of Collins and Evans (2002) charge (e.g. Jasanoff 2003; Wynne 2003; Rip 2003), defining the scientific questions to be asked, and thus the relevant group of experts to answer them, is itself a value laden and political act. Many developing countries complain that the predominant scientific framing of climate change as a global problem of GHG emissions is an essentially 'Northern' one that ignores important social differences between 'luxury' emissions of GHGs from fossil fuel use in developed countries and 'survival' emissions from agriculture in developing countries (Agarwal and Narain 1991). What's more this purportedly scientific framing serves to legitimates the specific political programme of international emissions trading, which is predicated on the universality and absolute interchangeability of GHG emissions, whatever their source, within a globally homogeneous atmosphere (Demeritt 2001). In this way Brian Wynne has suggested that 'the *intellectual* order of climate scientific prediction, and the *political* order of global management and universal policy control, based as it is

on the promise of deterministic processes, smooth changes, long-term prediction, and scientific control, mutually construct and reinforce one another' (1996: 372). An international epistemic community of scientists and policy-makers is coalescing around the world picture produced by the computer-based general circulation models (GCMs) of the global climate (Miller and Edwards 2001), but there has been relatively little open public acknowledgement of the commitments embedded within and being advanced through this way of seeing climate change. This reticence is at least partly a response to the efforts of climate sceptics to pursue their political agenda by attacking the science and scientists involved. One strategy for defending the credibility of scientific knowledge is to draw sharp boundaries between technical matters of scientific fact, which scientists alone are competent to decide, and of publicly debatable, value-laden political judgement (Gieryn 1994). Such defensiveness, however, seldom engenders much open and reflexive debate about the location of this boundary or the contestable judgements it may involve.

In addition to these difficulties keeping politics out of scientific deliberations, there is a second, and arguably much more serious, issue about the proper role for science in politics. The case of climate change shows how a technically framed and expert-led politics of sound science can be debilitating. In place of the difficult work of making global warming a meaningful matter of concern to a differentiated international public, supporters of the Kyoto Protocol have relied instead on scientific appeals to what Latour (2004a) calls the 'House of Nature' to adjudicate the political dispute about what, if any, restrictions on GHG emissions should be taken. Thus Lord May, President of the Royal Society and former Chief Scientific Advisor for the UK, recently urged the G8 group of leaders 'to initiate a process that arrives at a scientifically-determined target at which the concentration of CO 2 and other greenhouse gases should be stabilised within the atmosphere. . . . Once this target has been agreed . . . all countries will be able to commit to the necessary long-term cuts in global emissions' (May 2005). Apart from whether '[d]efining how much climate change is too much' is even something that can be 'scientifically determined' at all, as the UK environment minister recently asked (Beckett 2005), May's suggestion naively assumes that political consensus about what we ought to do will follow automatically from any prior scientific consensus about what is. This instrumental role of science in legitimating policy invites interest groups to contest political decisions by questioning the science (and scientists) rather than debating the reasons for the policy itself. In this way the problem of extension afflicts our politics as much as our science and will not be resolved, as Collins and Evans (2002) suggest, by distinguishing more precisely between the two realms. Indeed, efforts to do so can sometimes make political resolution more difficult to achieve, if the search for certainty about propositional questions of fact lifts the Rawlsian veil of ignorance behind which policy debate can occur without regard for eventual winners and losers or forestalls discussion of the contested values and ideals of justice central to those deliberations.

This is exactly what happened in the United States. Rather than begin the wide-ranging discussion about who should respond to climate change and how, national policy debate in the United States has become fixated upon the narrow scientific question of whether or not global warming is empirically detectable and real. In an effort to answer that scientific question, the US Global Change Research Program (USGCRP) was created in 1990 by the first President Bush. In its first ten years of existence, it spent more than $16 billion, and since coming to office President George W. Bush has poured a further $8 billion into climate research (Eilperin 2004), making the USGCRP one of largest American investments in scientific research ever. But this accumulating pile of research and data has done little to build any domestic political consensus (Sarewitz and Pielke 2000). Instead, in this highly adversarial political context, researchers have sought to defend themselves against charges of bias by addressing narrow technical questions rather than the more contentious and policy-relevant ones central to advancing political debate. In turn their research has generated new scientific questions and uncertainties to resolve rather than delivering the kind of indisputable foundation for political action often hoped for from sound science. What is worse, perhaps, is that, with all the focus on reducing the uncertainty of scientific predictions about the scale and impacts of future climate change, only a tiny fraction of what was spent on the USGCRP has gone to support the development of renewable energy technologies needed to mitigate further climate changes and even less on adaptation to cope with them. Partly as a result, the United States was woefully unprepared for Hurricane Katrina, while the imminent retirement of its civilian nuclear power plants leaves the US even less well placed now to make the transition to a carbon-free economy than it was twenty years ago when the problem of GHG emissions first came to widespread public attention.

Latour offers a boldly anti-naturalist alternative to this kind of overly scientized politics. He urges us to shift the focus of deliberation from matters of scientific fact and levels of certainty about them to what he terms 'matters of concern'. In Latour's new 'parliament of things', 'the composition of a common world is no longer a given from the outset' (2004a: 62), to be revealed by a singular science so as to provide the fixed and 'mononaturalist' framework for constructing a multicultural political consensus. Instead, Latour insists on the pluralism of the sciences in a multi-naturalist 'pluriverse' where politics is concerned with the very composition of the world in common, a sort of 'experimental metaphysics' based on a never- ending 'explicit, progressive, deliberative process of excluding certain entities for the time being as incompatible with the common world' (2004a: 179).

Despite Latour's (1993) typically provocative insistence that 'we were never modern', his blueprint for a new bi-cameralism extends Beck's vision of a more reflexive modernization in some interesting ways. Latour's new division of powers bisects the old modernist distinction between a 'House of Nature', concerned with scientifically identifying matters of objective fact, and a 'House

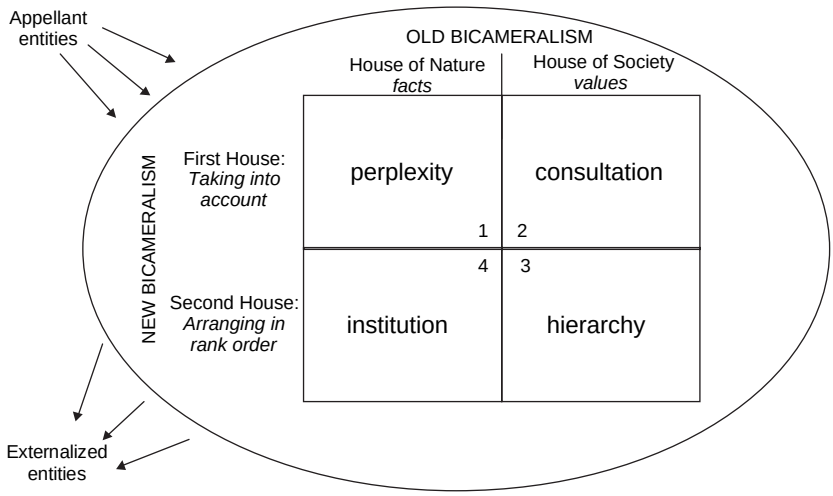

Figure 4 The four collective tasks of Latour's new 'parliament of things'

of Society', focused on deliberating over political values (Figure 4). Charged with the power to take into account, Latour's first, upper house answers the question, 'how many are we?' by remaining open both to new propositions for existence – what Latour calls 'perplexity' – and to all the relevant voices in the articulation of those propositions – what Latour calls 'consultation'. His second, lower house answers the question, 'how can we live together?', by exercising its power to arrange in rank order through 'hierarchy', or deciding upon the importance of new propositions relative to already existing ones, and through 'institution', or closing off of debate about the place of a proposition within the newly reconstituted collective. As Latour explains, every 'new proposition' to the collective must respond to these four requirements:

> It induces perplexity in those who are gathered to discuss it and who set up the trials that allow them to ensure the seriousness of its candidacy for existence; it demands to be taken into account by all those whose habits it is going to modify and who must therefore sit on its jury; if it is successful in the first two stages it will be able to insert itself in the states of the world only provided it finds a place in a hierarchy that precedes it; finally if it earns its legitimate right to existence, it will become an institution, that is, an essence, and will become part of the indisputable nature of the good common world.
>
> (Latour 2004a: 123)

Under this new bi-cameral division of powers all the professions – politicians, scientists, moralists, and economists – have a role to play in performing each of these collective tasks, rather than, as before, scientists and economists being restricted, respectively, to 'perplexity' and 'institution' in the 'House of Nature' and politicians and moralists to 'consultation' and 'hierarchy' in the 'House of Society'.

But in this brave new world it is no longer clear what distinguishes the various professions. How are the contributions of scientists *qua* scientists different from those made by moralists or by scientists in their role as ordinary citizens? Whereas the normative theory of expertise advanced by Collins and Evans offers a carefully nuanced 'reason for using the advice of scientists and technologists in virtue of the things they do *as* scientists and technologists, rather than as individuals or as members of certain institutions' (2002: 236), Latour is much less clear about what distinguishes the role of a scientific expert from other professions. However the professions are to be defined, there is another question about how their various contributions to 'due process' in Latour's new bi-cameralism are to be coordinated in space and time once we abandon linear models of innovation or of mandated science *for* policy (Salter 1988). Whereas James Wilsdon and Rebecca Wilson (2004), in the reforming spirit of $Beck_2$, call for new forms of upstream public engagement with science to make the existing divisions between them more porous and the unfolding of scientific research programmes responsive to explicit, anticipatory, and normative political control, Latour's bi-cameralism alternatively seems to change nothing at all, since each of the professions continues to pursue its own specific calling as before, or to 'map out a public space [so] profoundly different from what is usually recognized under the label of "the political"' (Latour 2005: 15) as to be almost unrecognizable from the present.

Nevertheless Latour's abstract blueprint does highlight the provisionality of any progressive composition of the world. What is externalized or ignored through the lower house's power of rank ordering may always appeal again to perplex the collective, in another iteration, through the upper house's responsibility to take into account. As Latour explains, 'the collective as we understand now, is not a thing in the world, a being with fixed and definitive borders, but a movement of establishing provisional cohesion that will have to be started all over again every single day' (2004a: 147) This insight is not too different from Beck's claim that reflexive modernization is based on a new awareness of contingency and the impossibility any longer of externalizing risk to any extra-social realm of the given, necessary, or natural.

But Latour's cosmopolitics differs from Beck's reflexive modernization in at least three respects. First, whereas Beck takes 'politics to mean the give-and-take in an exclusive human politics' (Latour 2004b: 454), Latour shares the view of Donna Haraway that '[t]he actors are not all "us." If the world exists for us as "nature", this designates a kind of relationship, an achievement among many actors, not all of them human, not all of them organic, not all of them technological' (1992: 297). Latour's disavowal of Beck's humanism is important not simply as an ecocentric principle about human cosmopolitical responsibilities towards creation. '[D]eleting the word "social" [from constructionism] to redistribute the agencies and to add some uncertainty concerning *what* is doing the making' (Latour 2003: 33–4) acknowledges that the challenge we face in building a world in common is not simply the cosmopolitan one of expanding our horizons multiculturally:

> You want to save the elephants in Kenya's parks by having them graze separately from cows? Excellent, but how are you going to get an opinion from the Masai who have been cut off from the cows, and from the cows deprived of elephants who clear the brush for them, and also from the elephants deprived of the Masai and the cows?
>
> (Latour, 2004a: 170)

Second, while both Beck and Latour recognize the centrality of the sciences to any such interrogations, Latour balks at offering any overarching principles for 'telling good from bad fabrications' (2004c: 461). For him this smacks of the narrow-minded fundamentalism of old, whereas Beck's reflexive modernization, like the normative theory of expertise proffered by Collins and Evans (2002), seeks to provide just such guidance.

This points to a final difference between Beck and Latour. Whereas Beck provides an explanatory engine to usher in his new world of reflexive modernization, Latour explicitly disavows utopianism. He modestly insists that he is simply offering a more accurate description of a world 'that already exist[s] everywhere and that only ancient prejudice kept us from seeing' (2004a: 163). This stance begs several questions, as Noel Castree notes:

> If our current governmental arrangements are really as anaemic as Latour maintains, then it seems strange that a uniquely perspicuous individual is needed to identify the fact. . . . Equally, if Latour's alternative constitution is truly a bride in waiting, it seems odd that he can identify no determinate force capable of hastening its arrival at the altar . . . . In light of this, one is left with the impression that Latour has created a set of arguments that, while arresting and clever, are entirely of his own making. Whatever purchase on reality they may have is weakened by his fondness for ideal-typical argumentation, hyperbolic incantation, and rarified generalization.
>
> (Castree 2006: 167)

Some myths to live by?

Latour's critique highlights an important paradox in contemporary environmental politics. The notion of a purely scientific realm of objective facts as distinct from a political one of contestable values is idealized by nearly all participants in debates about climate change, even as it is habitually breached in ordinary practice. Arguably the great success of the IPCC has not been its substantive reports, whose conclusions have been the subject of so much criticism from the climate sceptics, but the IPCC scientific review process itself, which has been instrumental in the emergence of an international epistemic community of scientists, policy-makers, and environmentalists united by their concern about climate change. In order both to expand this community of concern and to build international trust in its soundness, the

IPCC has gone to great lengths to open itself up to participation by non-governmental groups and to build the scientific capacity of developing countries to participate meaningfully in what are otherwise highly technical deliberations. Thus, rather than sticking strictly to matters of fact, the IPCC has actually been involved in bonding together the otherwise separate worlds of science and politics to deliberate collectively on an important matter of concern (Agrawala 1998a,b; Jasanoff and Wynne 1998).

Does it follow then, that the old modernist constitution and its attendant distinctions between science and politics were entirely illusory? That is certainly one way to read the proliferation of constructionist science studies. But, before repealing the old constitution, we might reflect on why, if our science and politics actually involve a good deal of often unacknowledged promiscuity, we continue to invest so much in its norms of due process.

The traditional rationale is epistemic. Minimizing the influence of politics and values over questions of scientific fact is said to promise more truthful and accurate answers. Thus environmentalists have sought to cast the climate sceptics as 'pseudo-scientists' in hock to special interests. They note, for instance, that, for the most part, the sceptics' claims have not been peer reviewed, or, if they have, not by widely recognized journals, and thus do not conform to the norms of good science. Tellingly about the only thing that has remained constant over the years about the sceptics' various counterclaims (McCright and Dunlap 2000) – first that the climate is not changing, then that changes were only minor and caused by natural variability, and now that those changes will be of little consequence or even beneficial – is the steady stream of fossil fuel money seeking to amplify them in the public sphere (Gelbspan 2004; Mooney 2005). Collins and Evans fear that constructionist science studies provide no grounds for dismissing such lies for what they are:

> For example do we never want to say that the tobacco industry has for years falsified the implications of epidemiological studies out of a concern for selling more cigarettes? Do we want to say, rather, that this was just the tobacco industry's point of view and that the only fight there is to be had with them is a political fight, not a scientific fight?
>
> (Collins and Evans 2002: 280)

But, in calling for a more precise demarcation of experts from non-experts to solve the problem of extension, Collins and Evans discount the degree to which this is also a political problem rather than a strictly epistemological one about how to provide authoritative answers to questions of scientific fact relevant to '[t]echnical decision-making in the public realm' (2002: 235).

One reason that science is so often in the firing line in environmental politics is that all too often policy decisions are legitimated in purely technical terms, leaving opponents with only scientific grounds for contesting policies that they oppose for other reasons. When politicians, such as David Cameron (2005), the newly anointed leader of the British Conservative Party, look to science 'to take the politics out of' environmental policy deliberations so as to 'constrain future

governments in the face of the natural tendency to put short-term electoral considerations above the long-term interests of the country and the planet', they are actually encouraging this very extension of debate over the scientific soundness of policy by forestalling the kind of open deliberation necessary to forge consensus about those long-term interests and thus about environmental policy.

There is still an important place for science in those debates, to be sure, but it is hardly the last or only word. The case of climate change shows the difficulties of distinguishing strictly scientific questions for experts alone to decide from associated value-laden and often politicized matters of concern for the lay public to debate. Upholding such a strict quarantine between political and scientific questions may insulate scientific judgements about the hockey-stick temperature reconstruction from unwelcome partisan political interference, but it has also made the scientific research of the USGCRP increasingly irrelevant to the important challenges we will face in mitigating climate change and adapting to its effects, while at the same time discouraging openness about the contestable social assumptions and political effects of the orthodox scientific framing of climate change as a universal and global-scale matter of concern.

In pointing to those inevitable partialities, my aim has not been to denounce climate science because its framing is socially contingent, but in the reflexive spirit of Beck and Latour to explore the wider effects of its construction and political deployment. Despite blurring the traditional demarcations between science and politics defended so fiercely by Collins and Evans (2002), the various constitutional arrangements proposed by $Beck_1$, $Beck_2$, and Latour still provide a basis for denouncing the climate sceptics for failing to observe the norms Merton (1973) famously associated with science. But the reason for observing those norms of due process is no longer the strictly epistemic one of guaranteeing the truth of our claims. Rather, once we acknowledge that the 'the operative question is how to distinguish between good constructions and bad' (Latour 2004b: 459), it becomes possible to recognize that the commitment to reasonableness, honesty, and open deliberation is as important for our politics as for our science. The two, of course, are closely intertwined, as historians of liberal democracy have noted (e.g. Ezrahi 1990; Shapin and Schaffer 1985). To the extent that they seek to hide the reasons for their political opposition to the Kyoto Protocol and other measures to reduce the GHG emissions of the United States behind a veil of technical objections to climate change science, the sceptics can be condemned as much for engaging in bad politics as in bad science.

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Notes

1 The editor-in-chief of *Climate Research* subsequently resigned in protest, along with four members of the editorial board, over the publisher's refusal to publish a retraction of Soon and Baliunas (2003) and reform the review process to reduce the influence of the editor responsible for its original publication (Monastersky 2003). The hostile response from mainstream climate scientists prompted charges of a political witch-hunt from sceptics and their supporters, who like to portray themselves as modern-day Galileo figures facing Inquisition from the Lysenkoist (they also like to mix their metaphors) establishment.

2 At the request of Science Committee Chairman Sherwood Boehlert (R-NY), the National Academy of Science convened an expert Committee on Surface Temperature Reconstructions of the Last 2,000 Years (NRC, 2006). Published in June 2006, its report largely vindicates the Mann *et al*. hockey stick reconstruction. Nevertheless at this writing Barton's House Energy and Commerce Committee has scheduled further hearings for July to raise 'Questions Surrounding the 'Hockey Stick' Temperature Studies'.

References

Agarwal, Anil and **Narain, Sunita** (1991) *Global Warming in an Unequal World*, New Delhi: Centre for Science and Environment.

Agrawala, Shardul (1998a) 'Context and early origins of the Intergovernmental Panel on Climate Change', *Climatic Change* 39: 605–20.

—— (1998b) 'Structural and process history of the Intergovernmental Panel on Climate Change', *Climatic Change* 39: 621–42.

Ashmore, Malcolm, Edwards, Derek and **Potter, Jonathan** (1994) 'The bottom line: the rhetoric of reality demonstrations', *Configurations* 2: 1–14.

Beck, Ulrich (1992a) 'From industrial society to the risk society: questions of survival, social structure, and ecological enlightenment', *Theory, Culture & Society* 9: 97–123.

—— (1992b) *Risk Society: Towards a New Modernity*, trans. M. Ritter, London: Sage.

Beckett, Margaret, Rt. Hon. MP (2005) 'Keynote address of the Secretary of State for Environment, Food and Rural Affairs to UK Stabilisation Conference, Exeter, Tuesday, 1 February 2005'. Available at: < http://www.defra.gov.uk/corporate/ministers/speeches/mb050201.htm > (accessed 20 December 2005).

Bradley, Raymond S. (1999) *Paleoclimatology: Reconstructing Climates of the Quaternary*, New York: Academic Press.

Brown, George E. (1996) *Environmental Science under Siege: Fringe Science and the 104th Congress. A Report by Rep. George E. Brown to the Democratic Caucus of the Committee on Science, U.S. House of Representatives*, Washington, DC: Democratic Caucus, US House of Representatives. Available at: < http://www.house.gov/science_democrats/archive/envrpt96.htm > (accessed 12 June 2000).

Cameron, D.avid (2005) 'Change our political system and our lifestyles', *The Independent* 1 November: 29.
Castree, Noel (2006) 'A congress of the world', *Science as Culture* 15: 159–70.
Collins, Harry M. (1983) 'The sociology of scientific knowledge: studies of contemporary science', *Annual Review of Sociology* 9: 265–85.
—— (1985) *Changing Order: Replication and Induction in Scientific Practice*, London: Sage.
Collins, Harry M. and **Evans, Robert** (2002) 'The third wave of science studies: studies of expertise and experience', *Social Studies of Science* 32: 235–96.
Collins, Harry M. and **Pinch, Trevor** (1993) *The Golem: What Everybody Should Know About Science*, Cambridge: Cambridge University Press.
Competitive Enterprise Institute (2003) 'Letter to Thomas N. Pyke, Jr., Chief Information Officer, United States Department of Commerce, 19 February, re: Petition to cease dissemination of the National Assessment on Climate Change, pursuant to the Federal Data Quality Act'. Available at: <http://www.thecre.com/cei_petition.htm> (accessed 24 November 2004).
Demeritt, David (2001) 'The construction of global warming and the politics of science', *Annals of the Association of American Geographers* 91: 307–37.
—— (2002) 'What is the "social construction of nature"? A typology and sympathetic critique', *Progress in Human Geography* 26(6): 766–89.
Dewey, John (1984) *The Public and Its Problems*, in J. A. Boydston (ed.) *John Dewey: The Later Works*, Vol. 2, Carbondale, IL: Southern Illinois University Press, pp. 235–372.
Dunlap, Riley E. and **Catton, William R., Jr.** (1994) 'Struggling with human exceptionalism: the rise, decline, and revitalization of environmental sociology', *The American Sociologist* 25: 5–30.
Eilperin, Juliet (2004) 'Climate talks bring Bush's policy to fore', *Washington Post* 5 December: A01.
—— (2005) 'GOP chairmen face off over global warming', *Washington Post* 18 July: A04.
EPA (2003) *Draft Report on the Environment*, Washington, DC: US Environmental Protection Agency, Publication number EPA 260–R–02–006. Available at: <http://www.epa.gov/indicators/roe/pdf/EPA_Draft_ROE.pdf> (accessed 24 May 2005).
EWG (2003) 'Luntz Memo on the Environment'. Available at: <http://www.ewg.org/briefings/luntzmemo/> (accessed 24 May 2005).
Ezrahi, Yaron (1990) *The Descent of Icarus: Science and the Transformation of Contemporary Democracy*, Cambridge, MA: Harvard University Press.
Gelbspan, Ross (2004) *Boiling Point: How Politicians, Big Oil and Coal, Journalists and Activists Have Fueled the Climate Crisis – and What We Can Do to Avert Disaster*, New York: Basic Books.
Gieryn, Thomas F. (1995) 'Boundaries of science', in S. Jasanoff, G. Markle, J. Petersen and T. Pinch (eds) *Handbook of Science and Technology Studies*, London: Sage, pp. 393–443.
Gough, Michael (ed.) (2003) *Politicizing Science: The Alchemy of Policymaking*, Palo Alto, CA: Hoover Institution. Available at: <http://www-hoover.standford.edu/publications.books.polscience.html> (accessed 25 October 2004).
Gross, Paul R. and **Levitt, Norman** (1994) *Higher Superstition: The Academic Left and Its Quarrels with Science*, Baltimore, MD: Johns Hopkins University Press.
Hacking, Ian (1999) *The Social Construction of What?*, Cambridge, MA: Harvard University Press.
Haraway, Donna J. (1992) 'The promises of monsters: a regenerative politics for inappropriate/d others', in L. Grossberg, C. Nelson and P. A. Treichler (eds) *Cultural Studies*, New York: Routledge, pp. 295–337.
Herrick, Charles N. (2004) 'Objectivity versus narrative coherence: science, environmental policy, and the U.S. Data

Quality Act', *Environmental Science & Policy* 7: 419–33.
Inhofe, James Senator (2003) 'Science of climate change', *Congressional Record* 28 July: S10012–S10023.
—— (2004) 'The science of climate change', *Congressional Record* 11 October: S11291–7.
IPCC (1996) *Summary for Policymakers: The Science of Climate Change – IPCC Working Group I*, Bracknell: UK Meteorological Office for IPCC/WMO/UNEP. Available at: <http://www.ipcc.ch/pub/sarsum1.htm> (accessed 24 May 2005).
—— (2000) *Summary for Policymakers: A Report of Working Group I of the Intergovernmental Panel on Climate Change*. Available at: <http://www.ipcc.ch/pub/spm22-01.pdf> (accessed 24 May 2005).
Jasanoff, Sheila (2003) 'Breaking the waves in science studies: comment on H. M. Collins and Robert Evans, "The third wave of science studies"', *Social Studies of Science* 33: 389–400.
Jasanoff, Sheila and **Wynne, Brian** (1998) 'Science and decisionmaking', in S. Rayner and E. L. Malone (eds) *Human Choice and Climate Change, Vol. 1, The Societal Framework*, Columbus, OH: Battelle Press, pp. 1–87.
Lahsen, Myanna (2005) 'Technocracy, democracy, and U.S. climate politics: the need for demarcations', *Science, Technology, and Human Values* 30: 137–69.
Latour, Bruno (1993) *We Have Never Been Modern*, trans. C. Porter. Cambridge, MA: Harvard University Press.
—— (2003) 'The promises of constructivism', in D. Ihde and E. Selinger (eds) *Chasing Technoscience*, Bloomington, IN: University of Indiana Press, pp. 27–46.
—— (2004a) *Politics of Nature: How to Bring the Sciences in Democracy*, trans. C. Porter. Cambridge, MA: Harvard University Press.
—— (2004b) 'Whose cosmos, which cosmopolitics? Comments on the peace terms of Ulrich Beck', *Common Knowledge* 10: 450–62.
—— (2004c) 'Why has critique run out of steam? From matters of fact to matters of concern', *Critical Inquiry* 30: 225–48.
—— (2005) 'From realpolitik to dingpolitik, or how to make things public', in B. Latour and P. Weibel (eds) *Making Things Public: Atmospheres of Democracy*, Cambridge, MA: MIT Press, pp. 14–41.
Legates, David (2003) 'Statement of Dr. David Legates, Director, Center for Climate Research, University of Delaware before U.S. Senate Committee on Environment & Public Works, 29 July'. Available at: <http://epw.senate.gov/hearing_statements.cfm?id=212845> (accessed 26 November 2004)
McCright, Aaron M. and **Dunlap, Riley E.** (2000) 'Challenging global warming as a social problem: an analysis of the conservative movement's counter-claims', *Social Problems* 47: 499–522.
McIntyre, Stephen and **McKitrick, Ross** (2005) 'Hockey sticks, principal components, and spurious significance', *Geophysical Research Letters* 32: L03710.
Mann, Michael E. (2003) 'Statement of Dr. Michael Mann, Associate Professor, University of Virginia, Department of Environmental Science before U.S. Senate Committee on Environment & Public Works, 29 July'. Available at: <http://epw.senate.gov/108th/Mann_072903.pdf> (accessed 26 November 2004).
Mann, Michael E., Bradley, Raymond S. and **Hughes, M. K.** (1998) 'Global-scale temperature patterns and climate forcing over the past six centuries', *Nature* 392: 779–87.
—— (1999) 'Northern hemisphere temperatures during the past millennium: inferences, uncertainties, and limitations', *Geophysical Research Letters* 26: 759–62.
Mann, Michael E., Ammann, C., Bradley, R., Briffa, K., Jones, P., Osborn, T., Crowley, T., Hughes, M., Oppenheimer, M., Overpeck, J., Rutherford, S., Trenberth, K. and **Wigley, T.** (2003) 'Comment on "On past

temperatures and anomalous late-20th century warmth'': response', *Eos* 84(44): 473–4.
Mannheim, Karl (1954) *Ideology and Utopia*, New York: Harcourt Brace.
Lord May of Oxford (2005) In 'Ripe for change', *The Guardian* 30 June: 21.
Merton, Robert K. (1973) 'The normative structure of science', in N. W. Storer (ed.) *The Sociology of Science: Theoretical and Empirical Investigations*, Chicago: University of Chicago Press, pp. 267–78.
Miller, Clark and **Edwards, Paul N.** (eds) (2001) *Changing the Atmosphere: Expert Knowledge and Environmental Governance*, Cambridge, MA: MIT Press.
Monastersky, Richard (2003) 'Storm brews over global warming', *Chronicle of Higher Education* 50(2): A16–7.
Mooney, Chris (2005) *The Republican War on Science*, New York: Basic Books.
Murphy, Raymond (1995) 'Sociology as if nature did not matter: an ecological critique', *British Journal of Sociology* 46: 688–707.
NRC (2001) *Climate Change Science: An Analysis of Some Key Questions*, Washington, DC: National Academy Press. Available at: <http://www.nap.edu/books/0309075742/html/> (accessed 24 November 2004).
—— (2006) *Surface Temperature Reconstructions for the Last 2,000 Years*, Washington, DC: National Academy Press. Available at: <http://www.nap.edu/catalog/11676.html> (accessed 15 July 2006).
Oregon Institute of Science and Medicine (2005) 'Global warming petition'. Available at: <http://www.oism.org/pproject/s33p37.htm> (accessed 19 May 2005).
Oreskes, Naomi (2004) 'Beyond the ivory tower: the scientific consensus on climate change', *Science* 306: 1686.
Porter, Theodore M. (1995) *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*, Princeton, NJ: Princeton University Press.
Redclift, Michael R. and **Woodgate, Graham** (1997) 'Sustainability and social construction', in M. Redclift and G. Woodgate (eds) *The International Handbook of Environmental Sociology*, Cheltenham: Edward Elgar, pp. 55–70.
Regaldo, Antonio (2005) 'Global warring: in climate debate the ''hockey stick'' leads to a face-off', *Wall Street Journal* 14 February: A1, A13.
Rip, Arie (2003) 'Constructing expertise: in a third wave of science studies?', *Social Studies of Science* 33: 419–34.
Salter, Liora (1988) *Mandated Science: Science and Scientists in the Making of Standards*, Dordrecht: Kluwer.
Sarewitz, Daniel and **Pielke, Roger, Jr.** (2000) 'Breaking the global warming gridlock', *The Atlantic Monthly* July: 54–64.
Sayer, Andrew (2000) *Realism and Social Science*, London: Sage.
Seelye, Katharine Q. (2002) 'President distances himself from global warming report', *The New York Times* 5 June.
Shapin, Steven and **Schaffer, Simon** (1985) *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life*, Princeton, NJ: Princeton University Press.
Sismondo, Sergio (2004) *An Introduction to Science and Technology Studies*, Oxford: Blackwell.
Sokal, Alan (1996) 'A physicist experiments with cultural studies', *Lingua Franca* 6(May–June): 62–4.
Soon, Willie and **Baliunas, Sallie** (2003) 'Proxy climatic and environmental changes of the past 1000 years', *Climate Research* 23: 89–110.
Soon, Willie, Baliunas, Sallie, Idso, Craig, Idso, Sherwood and **Legates, David R.** (2003) 'Reconstructing climatic and environmental changes of the past 1000 years: a reappraisal', *Energy and Environment* 14: 233–96.
Turner, Stephen (2001) 'What is the problem with experts?', *Social Studies of Science* 31: 123–49.
UCS (2004) *Scientific Integrity in Policymaking: An Investigation into the Bush Administration's Misuse of Science*, Cambridge, MA: Union of Concerned Scientists. Available at: <http://www.ucsusa.org/global_environment/

rsi/page.cfm?pageID = 1641 > (accessed 25 October 2004).
UK House of Lords (2000) *Third Report of the Select Committee on Science and Technology: Science and Society*, London: HMSO. Available at: < http://www.parliament.the-stationery-office.co.uk/pa/ld199900/ldselect/ldsctech/38/3801.htm > (accessed 27 November 2005).
US Department of State (2002) *The United States of America's Third National Communication under the United Nations Framework Convention on Climate Change*. Available at: < http://yosemite.epa.gov/oar/globalwarming.nsf/content/ResourceCenterPublicationsUSClimateActionReport.html > (accessed 24 November 2004).
USGCRP (2001) *Climate Change Impacts on the United States: The Potential Consequences of Climate Variability and Change*, Washington: US Global Change Research Program. Available at: < http://www.usgcrp.gov/usgcrp/Library/nationalassessment/foundation.htm > (accessed 24 November 2004).
Waxman, Henry A. (2003) *Politics and Science in the Bush Administration*, Washington, DC: US House of Representatives, Committee on Government Reform – Minority Staff. Available at: < http://www.house.gov/reform/min/politicsandscience/ > (accessed 24 November 2004).
Weinberg, Steven (1996) 'Sokal's hoax', *New York Review of Books* 48(8 August): 11–15.
Wilsdon, James and **Willis, Rebecca** (2004) *See-through Science: Why Public Engagement Needs to Move Upstream*, London: Demos. Available at: < http://www.demos.co.uk/Seethroughsciencefinal_pdf_media_public.aspx > (accessed 27 January 2005).
Wynne, Brian (1995) 'Public understanding of science', in S. Jasanoff, G. Markle, J. Peterson and T. Pinch (eds) *Handbook of Science and Technology Studies*, Thousand Oaks, CA: Sage, pp. 361–88.
—— (1996) 'SSK's identity parade: signing-up, off-and-on', *Social Studies of Science* 26: 357–91.
—— (2003) 'Seasick on the third wave? Subverting the hegemony of propositionalism: response to Collins & Evans (2002)', *Social Studies of Science* 33: 401–17.

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