Sociology and Climate Change after Kyoto: What Roles for Social Science in Understanding Climate Change?

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Current Sociology 2009 57: 389
DOI: 10.1177/0011392108101589

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abstract: This article focuses on the comparatively neglected role of the social sciences (including economics) and of assumptions about the social functioning of the scientific community in projections about climate change and about societies' responses to changing climates and related environmental phenomena. Using an approach informed by social constructionism and science and technology studies, it examines the part played by the social sciences and the social institutions of science in making knowledge about the future of humankind in relation to the changing ecosphere. Using a small series of case studies focused on the way that social science features in the shaping of climate knowledge – for example, how value is attached to economic activities in different countries in the course of attempts to calculate the most 'rational' global response to the myriad threats of changing climates – the article shows that there is a need for (1) greater understanding of the social dimensions of the scientific community that studies climate change and (2) more social science reflection on the roles of social science in climate-change models and projections.

keywords: adaptation ✦ climate change ✦ judgement ✦ peer review ✦ social construction

Introduction: Global Warming and Humanly Induced Climate Change

In June 2007, international headlines were taken up with the story that China's annual carbon dioxide (CO_2) emissions had, for the first time, surpassed those of the USA. A small decline in US emissions, associated with

Current Sociology ✦ May 2009 ✦ Vol. 57(3): 389–405

SAGE (Los Angeles, London, New Delhi, Singapore and Washington DC)
DOI: 10.1177/0011392108101589

a downturn in economic growth, combined with continuing expansion of China's use of coal-burning power stations and a further leap forward in its cement production led the Netherlands Environmental Assessment Agency (Milieu- en Natuurplanbureau; MNP) to estimate that, in 2006, China's emissions had exceeded those of the US by around 8 percent. The surprise was not that China's CO_2 emissions should one day overtake those of the US; such an eventuality had been predicted for several years. What was startling, according to the MNP,[1] was that China had risen to world-leading emitter so quickly and by such a significant margin. Climate analysts feared that this rapid rise in emissions would mean that forecast climate changes might come about more quickly than had been anticipated or even that atmospheric CO_2 levels might reach new, higher peaks before atmospheric greenhouse gas levels could be stabilized; in turn, this would leave policy-makers less time than had been assumed to respond to global warming and to reach international agreements.

The importance of this story for the present article is that it shows – in a simple but also dramatic way – that the business of predicting greenhouse emissions, climate futures and policy responses is critically dependent on social variables such as the choice of technology, regional development policies, consumers' behaviour and the performance of the economy. I argue that the focus of analyses of the debate over climate change has – understandably – been fixed on the natural scientific aspects of the issue as represented in models of the climate, oceans and atmosphere operated by scientists associated with the IPCC (Intergovernmental Panel on Climate Change) and others. In many ways, these aspects have been the more epistemologically and technologically interesting ones, but it has often been the case that the social science side of the equation (including social scientific assumptions about how scientific institutions operate) has outweighed in its implications the natural science side. This orientation has led to a neglect of the importance of the ways that economic and social scientific aspects of global warming have entered into the business of forecasting, understanding and trying to manage the changing climate. I thus propose to set out and exemplify the case for refocusing attention onto the social science aspects of climate change.

The 'Constructedness' of Climate Change

Despite the long history of arguments about the utility of viewing environmental problems as 'constructed' (Murphy, 1995; Yearley, 1991, 2005a), there is a simple sense in which knowledge about climate change and specifically about the future climate is undeniably (and uncontroversially) constructed. First, it is constructed in the sense that it is a projection about the future behaviour of an enormously complex system about which, everyone agrees, there is imperfect knowledge. Accordingly, scientists and modellers are obliged to try to estimate the future behaviour of a system without knowing all the variables in play. Some insight into these efforts is available from Lahsen, who has helpfully carried out ethnographic work on the climate modelling community, examining how the models (known as GCMs or general circulation models) gain credibility (Lahsen, 2005b; see also Bloomfield, 1986; Sundberg, 2005: 166–84). By their nature, such models cannot be tested against the future since we cannot wait for the future to arrive before making decisions about whether today's models are right. Nor can they really be adequately tested against data about past climates since they are constructed precisely in the light of information about the past and thus are more likely to be accurate under past circumstances than unprecedented new ones (see Edwards, 2000: 232). Accordingly, the models are inevitably to some extent conjectural. This perception is also widespread within the modelling community, as one of Lahsen's respondents states: 'As atmospheric scientist Kevin Trenberth has noted, "All models are of course wrong because, by design, they depict a simplified view of the system being modeled" ' (Lahsen, 2005b: 919, citing Trenberth, 1997). One way to test the models is therefore to run them against each other; Lahsen tracks how modellers manage the tension between their conceptions of the real word and the modelled world. There are also practical constraints on modelling; it remains very time-consuming and expensive: 'Despite vast increases in computer power, full runs of today's state-of-the-art GCMs still require hundreds of supercomputer hours, since modelers add complexity to the models even more rapidly than computers improve' (Edwards, 2000: 232; see also Edwards, 1996).

Given that the climate science community is not homogeneous, Shackley (2001) argues for the existence of contrasting 'epistemic lifestyles' within the modelling community. Some modellers are concerned with developing the most comprehensive model they can, arguing that this is a necessary route to meaningful climate prediction. Others are concerned to establish as quickly as possible models capable of addressing general long-term trends so that projections can be made and fed into the policy process (see also Sundberg, 2005: 136–7). The latter group has tended to be dominated by thermodynamicists who argue that the climate system can be treated as a black-box exchanging energy with the rest of the universe. With different ambitions for their models and different views of the most important task at hand, these analysts literally construct their models differently.

A second way in which climate projections are constructions is that they depend on assumptions about what people and governments, corporations and householders will do. Just as climate models require simplified versions of the atmosphere and the oceans, climate projections demand simplified versions of societal activity. The key point here is that people's behaviour is by no means fully separable from the business of the GCMs. It is not as though there is a separate carbon system and a distinct social system; social systems can relate directly to various climate-change mechanisms. Growth in aviation, for example, will not only add more carbon to the atmosphere but may stimulate the development of cirrus clouds and other phenomena associated with the 'contrails' from plane engines. Social choices affect climate futures in complicated ways. This second aspect of construction has an additional level of complexity since the behaviours of governments, consumers and other actors will be affected by the various climate-change projections produced (by the IPCC, climate sceptics, pressure groups and others) and how well publicized and persuasive the experts' views are. For this reason, the futures produced by the IPCC and other modellers are not to be regarded as forecasts of what will happen (in the way that an ordinary weather forecast is an estimate of what tomorrow's weather *will* be). They are consciously offered as estimates of what would be expected to happen given certain circumstances. Most importantly, if governments, corporations and consumers pay attention to those calculations, then the circumstances will change and the 'forecast future' will never come. It was this thinking that led the IPCC to move away from something like predictions (which it issued in the 1990s) to numerous possible scenarios for its Third Assessment Report (in 2001); the range of scenarios reflected the range of societal and technical responses as well as the range of acknowledged uncertainties within the climate science.

There is a third distinct sense in which climate-change projections are constructed and that has to do with the design and constitution of the institutions within which the projections are legitimately generated. Models in this area are not produced by lone academics; modelling capacity is highly expensive and projections are produced in relatively small numbers in a few centres worldwide. The results are agreed through an elaborate process of negotiation within the IPCC and, though academics may write up their results in numerous journals and other outlets, the most mainstream publications are the IPCC reports. These result from a hybrid process of scientific discussion and diplomatic negotiation where country representatives have a large say in writing chapter summaries. Given the interests at stake and the importance of trying to achieve an international consensus, this is no doubt sensible. However, it does mean that who gets to write the results, what is presented and how it is summarized are all things regulated in a different way from the standard academic model. The fact that climate science and authoritative climate projections have come to be organized in this way is itself an element of its construction – a construction at the level of the sociology of the scientific community.

To conclude this introductory section, I propose that in specific and demonstrable ways claims about climate change are clearly a 'social construction'.

This is not to say that they are fictions, mere conventions or conclusions arrived at in tendentious ways. In this area there can only be constructions. But there are social scientifically interesting questions about the precise ways in which such knowledge has been socially constructed. Let us now turn to the reasons why the first of these three sense of construction has tended to predominate in the literature on climate change.

Identifying 'Construction' in the Natural Science of Climate Science

At least two factors appear to have encouraged commentators interested in the generation of knowledge about climate change to focus principally on its natural scientific aspects (on construction in the first sense above). There is first the fact that such a major scientific initiative is founded on models and simulations run on enormously powerful and expensive computers. Of course, the study of climate records – human records and ice cores – and the analysis of possible mechanisms for warming and temperature regulation are important too, but the models seem to capture much of what is distinctive in this effort. Second, there is the sheer complication of the science and the sheer diversity of the information that needs to brought to bear, making this a highly interdisciplinary endeavour.

Mention has already been made of the work of Edwards and Lahsen. A series of papers by Shackley and Wynne (1995, 1996) provide further examples since they examine how modelled knowledge is produced, made credible and rendered serviceable for the policy community (see also Shackley et al., 1998, 1999). Thus, writing with two Dutch colleagues (van der Sluijs et al., 1998), they investigated the strikingly consistent nature of estimates of climate sensitivity over a series of models and policy reviews. Their puzzle was that 'The estimated range of the climate sensitivity to CO_2-doubling of 1.5°C–4.5°C has remained remarkably stable over two decades, despite the huge growth of climate science' (van der Sluijs et al., 1998: 315). Their interpretation was that it was factors within the sociology of this community that tended to make changes in the policy prescriptions much less likely than continuity. Additionally, the estimate was broad enough to admit of numerous different interpretations with little friction among the scientific contributors, even if the estimate tacitly excluded more catastrophic scenarios. Despite practitioners' use of highly technical models, sociological factors specific to this community seemed to influence the knowledge it produced. This and other research (for example, that on the 'epistemic lifestyles' spoken of by Simon Shackley: the choice between 'best available model' and 'best available prediction') are examples of attempts by sociologists to examine the construction of climate knowledge in an even-handed, symmetrical way. They lay out the choices available to actors and see what differences arise from the various ways those choices could be (and have been) made. There is a great deal about the natural science approaches adopted in climate science and climate modelling that can be examined from this perspective.

As well as analysts who are interested in setting out the practicalities of science under these new and challenging circumstances, there are also those who use a similar approach with the intention of contesting climate science. Sceptical critics (including the famous ones such as Bjørn Lomborg [2001]) have had some success using a similar kind of focus. Typically, they concentrate on topics such as the treatment of clouds and of water vapour in trying to deconstruct the connection between the build-up of CO_2 and the resulting temperature increase.

The favourite examples in deconstructing climate models work in the following way: the role of clouds in the Earth's energy balance is known to be important but, depending on the type of clouds and their location, clouds may tend to reflect the sun's heat or to cause insulation to be increased. In other words, they may lessen or exacerbate climate change. Clouds are difficult to model closely since they vary on a smaller scale than that which models can handle. Thus cloud impacts are not directly analysed in the principal models but are represented by general parameters (see Lomborg, 2001: 270–3). Critics then suggest that the clouds are treated by scientists worried about climate change in a way which implies that clouds have been discovered to enhance the greenhouse effect; critics contend that this is not a result of the model but the consequence of a decision made by the modellers when setting the parameters in the first place. Water vapour features prominently also since it is a major greenhouse gas and a source of potential positive feedbacks. It is feared that rising temperatures will lead to more water vapour in the atmosphere, which, in turn, will intensify warming. The amount of water vapour is linked to the temperature not just of the Earth's surface but of the lowest stratum of the atmosphere. Lomborg claims that measurements of atmospheric temperature do not easily fit the model's assumptions since the surface warming seems to be far ahead of the atmospheric warming. Potentially, therefore, water vapour may not build up as rapidly as the models propose and thus the temperature may not increase as anticipated. Lomborg suggests that this mismatch between the models' projections about water vapour and direct measurements of the vapour is typically overlooked when assessing the validity of the models.

I include these examples here not to endorse – nor indeed specifically to query – Lomborg's claims, but to indicate that the focus among those interested in the construction of claims about climate change has tended to be on the construction of natural scientific claims, mostly notably claims arising from the climate models.

Studying 'Construction' beyond the Natural Science of Climate Science

I have argued that there are at least three senses in which knowledge about climate change is uncontroversially constructed, and noted that most of the sociological work and public controversy focuses only on the first of these ways: the natural scientific construction. In the following sections, I propose to make the case for the importance of the other two forms of construction: the construction of claims about societal responses to climate change, and the construction of the social institutions within which climate forecasts and projections circulate and are legitimated. In brief, I suggest that the importance of these other forms can be demonstrated through four distinct examples of construction that lie beyond the realms of arguments over detailed natural science points:

- The issue of peer review;
- The issue of scientific judgement;
- The issue of economic valuations;
- The issue of the conceptualization of social science in climate modelling.

At the risk of pre-empting my own arguments, the links between these four themes and the two forms of construction just outlined can be set out as in Table 1.

In the following subsections I review each of these briefly.

Issue 1 – Peer Review

In the relationship between the IPCC – indeed the whole climate-change regulation community – and its critics, not only the science but the various ways in which the science is legitimated have come under attack (see Lahsen, 2005a).

Critics have been quick to point to the supposed vested interests of this community. Its access to money depends on the severity of the potential harms that it warns about; hence – or so it has been argued – it inevitably

Table 1 *Comparatively neglected forms of social science analysis of climate change*

Forms of social scientific analysis	Typical points of empirical focus
social science studies of the scientific community that studies climate change	analyse peer review study scientific judgement
social science reflection on the roles of social science in climate-change models	critically analyse the role of economic valuations monitor the way social science features in the models

has a structural temptation to exaggerate those harms. This highlights one of the outstanding features of the IPCC: though there have been other mass scientific projects (including the human genome project), the IPCC is unusual in that the science with which it had to deal was more controversial and complex than the obvious comparators (see Nolin, 1999). With the human genome, for instance, there was a high level of agreement within the profession about what the answer should look like and no organized lobby denying its basic premises. By contrast, the IPCC was trying to offer policy relevant analyses that many other policy advisers, including some respected scientists, were explicitly trying to junk.

As it was working in such a fraught and multidisciplinary area, the IPCC attempted to extend its network widely enough so as to include all the relevant scientific authorities; it was important that the IPCC should not be dominated exclusively by meteorologists or atmospheric chemists. But this meant that the IPCC ran into problems with peer reviewing and perceived impartiality; there were virtually no 'peers' who were not already within the IPCC (for an analysis of the accusations that could be levelled on this basis, see Edwards and Schneider, 2001). Conventional peer reviewing relies on there being few authors and many (more or less disinterested) peers; the IPCC effectively reversed this situation. When just one chapter in the 2001 Third Assessment Report has 10 lead authors and over 140 contributing authors,[2] then it is clear that this departs from the standard notion of scientific knowledge production.

If challenged, the IPCC tended to fall back in line with the classic script of 'science for policy' (see Yearley, 2005b: 160–2); the IPCC legitimated itself in terms of the scientific objectivity and impartiality of its members. But critics were able to point out that the scientific careers of the whole climate change 'orthodoxy' depended on the correctness of the underlying assumptions. Worse, the IPCC and policy-makers largely selected who was in the club of the qualified experts and thus threatened to be a self-perpetuating community with a vested interest in continuing to find evidence for the importance of the phenomenon to which its members' careers were shackled (this line of attack is described in Boehmer-Christiansen, 1994: 198).

Arguments about peers and peer review may get even more complex and self-referential. Thus, in 2004, US social studies of science scholar Naomi Oreskes published an article in the leading journal *Science*: using bibliometric techniques, she claimed to demonstrate that the overwhelming majority of scientific articles published worldwide agreed on the reality of climate change. The appearance of consensus was not, she implied, manufactured by the IPCC but was genuinely global. In some contexts, her results passed as a 'social fact'. Just as an example, Demeritt (2006: 453) writes that '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.' He invokes Oreskes as though she had documented an indisputably robust scientific consensus. But the climate sceptics are equally interested in deconstructing Oreskes' social facts. There have been challenges to her methodology; for example on the grounds that the climate-science papers used in her final sample from the literature were derived from a fairly limited set of bibliometric search terms; different keywords could have produced a different result.[3] But, aside from any such specific counter-arguments, there is a logical difficulty with this style of argument. To find that the people who publish in the mainstream literature are overwhelmingly in favour of the reality of climate change is not self-evidently to prove that access to the literature is not skewed in favour of those views. It is only when access to publishing opportunities is agreed to be unbiased that one may assume that patterns of publication reflect underlying attitudes; one cannot draw the inference in the opposite direction.

To conclude this subsection, let me repeat that I am not arguing against the reality of climate change nor in favour of climate sceptics. I am not even wanting to demur from Oreskes' substantive assertion about what the broad scientific consensus likely is. My point is more about the process. The IPCC faces twin challenges: to include as wide an interdisciplinary community as possible in its work and yet to legitimate its views in terms of the standard guarantors of scientific impartiality. These twin demands inevitably drive peer review up against its very limits. The peer review system is a socially constructed system and its suitability for new jobs in new contexts is itself a matter for negotiation and demonstration. Its suitability cannot be taken for granted.

Issue 2 – Scientific Judgement

The work of the IPCC is a collective work of judgement. Given the huge scale of the IPCC and its novelty both as an institution and in terms of the phenomena it tries to assess, a key issue was how it would reach judgements distilled from all the detail. The IPCC has to arrive at summary judgements and these judgements – as van der Sluijs et al.'s (1998) study outlined above indicates – are not narrowly determined by the vast array of scientific results in the reports (see also van der Sluijs, 1997). It is clear that sociological and social psychological considerations enter into the formulation of these judgements. Moreover, the IPCC reports are characterized by a further level of judgement since each report volume is introduced with a summary for policy-makers (see, for example, Bruce et al., 1996) that has to be approved in detail by the countries' representatives; it is 'thus an *intergovernmentally negotiated* text' as the Preface makes clear (Bruce et al., 1996: x; emphasis added).

The key social science issue here is an ironical one. Trends in the philosophy and sociology of science over the last three decades have tended to deny that there is any such thing as scientific method; this idea is helpfully expressed in Collins' distinction between an enculturational and an algorithmical view of scientific practice (Collins, 1992: 159–63). In their work scientists do not follow an algorithm (such as Popper's famous idea of generating multiple hypotheses that are to be weeded out by decisive falsifications); science cannot be automated. Rather, they learn a culture and become skilled at making judgements within that culture.

Though this overall argument has been very persuasive within the history of science and science studies, it has not led to as much detailed analysis of scientific judgement as one might have expected. And, in particular, by presenting *all* scientific practice as a matter of judgement, it has left philosophers and sociologists of science with rather little to say about cases of explicit judgement, as with the IPCC.

Consequently, there is still an ambivalence about judgement that leads to similar problems to those detected around peer review. Given the explicit role of expert judgement in presenting periodic assessments of the state of the art in climate science – as the IPCC publications do – the report authors cannot fall back on algorithm-like justifications for the warrantability of their views. However, to take the other route and to explicitly acknowledge that the reports are based on judgements opens them to the charge that judgements have been made in tendentious ways or that the reports are not compelling since a different set of authors might have reached a different set of judgements about the conclusion. To avoid this unattractive dilemma, an explicit acknowledgement of the role of judgement is required. In courts and diplomacy sound judgement is admired. The understanding of shrewd judgement in science can benefit from studying these extra-scientific examples.

Issue 3 – The Role of Economic Valuations

The third detailed issue about the constructedness of climate knowledge relates to the role and interpretation of economic factors within the IPCC process. One specific and highly informative case here was the question of the economic valuation of lives threatened by climatic change. In terms of policy responses to global warming, there appear to be two broad possibilities: either one tries to limit the build-up of greenhouse gases (by reducing emissions or boosting sequestration and so on) or one takes steps to adapt to a changed climate by building better sea defences, relocating homes, increasing provision for cooling buildings and associated measures. In order to work out a reasonable balance somewhere between 'all abatement' or 'all adaptation', one needs to know the relative pros and cons. Both strategies have costs and benefits, and economists working on the 1995 assessment argued that one could not evaluate the various policy paths unless one had a worldwide analysis of these advantages and costs. After one had completed such an analysis, one could then solve the equations to get a mix of policies that provided the greatest net benefit at lowest cost (see Fankhauser, 1995, and, for critical consideration, Demeritt and Rothman, 1999).

In short, they wanted to work out both the economic costs associated with greenhouse gas abatements (direct costs and lost earnings opportunities) and those associated with the adaptation route, including the price of the adaptations themselves (such as sea defences) and the costs to people arising from the changed climate (to which people were adapting). Among other things,[4] this entailed putting a price on the typical life income of people from the various countries since some people would be victims whichever route was chosen. And it turned out, for example, that each South Asian (many of whom are likely to suffer from sea level rises) were calculated to 'cost' their country much less than each westerner whose income might be lost. The economists argued that they were not evaluating the worth of people's lives, only putting a price on the forgone earnings of typical individuals, but the procedure appeared to value the life of a South Asian at about one-fifteenth of the worth of a Northern citizen. The valuations were critical since the relative cheapness of South Asians meant that the 'rational' global policy orientation was for relatively little abatement (since that was costly as it tended to impact high-earning Northerners) and a good deal of adaptation (mostly in the developing world); the adaptation appeared relatively inexpensive because it tended to impact people with low incomes.

This line of reasoning, though retained in Chapter 6 of Volume III of the 1995 Assessment Report (see Pearce et al., 1996), was widely criticized among NGOs (notably the Global Commons Institute, which was founded precisely around this issue). In the end, the economistic argument was largely disavowed in the summary for policy-makers with which the volume began. In the section on the social costs of humanly caused climate change, the summary asserted that:

> The literature on the subject of this section is controversial. . . . There is no consensus about how to value statistical lives or how to aggregate statistical lives across countries. Monetary valuation should not obscure the human consequences of anthropogenic climate change damages, because the value of life has meaning beyond monetary considerations. (Bruce et al., 1996: 9–10)

Within the one report there were two competing and incompatible approaches to valuation. And while this revealed deep philosophical divisions over the very conceptualization of the issues (O'Riordan and Jordan, 1999), the difference was not only philosophical since the way that one construed the fundamental question of pricing dictated, in large measure, the outcome of the policy review. The construction of the issue overwhelmingly shaped the outcome.

This kind of explicit cost–benefit approach was much less in evidence in later assessments. This prompted economics enthusiast Lomborg to lament that 'it is regrettable that [such economic issues are] not rationally assessed in the latest [i.e. the subsequent] report' (Lomborg, 2001: 301). But a measure of continuity with the rational pricing approach can be seen in the recently published Stern Review (Stern, 2007), which again tries to frame policy choices around a 'neutral' economic evaluation. This time there was less deconstructive work from environmentalist authors since Stern arrived at figures suggesting that climate change was well worth addressing even in economic terms alone. It was not until the following year that commentators such as George Monbiot (writing in *The Guardian*, 19 February 2008) engaged with the method by which the Stern Review arrived at its conclusion.[5] Monbiot finally sees that the method is 'just' cost–benefit calculation:

> His report shows that the dollar losses of failing to prevent a high degree of global warming outweigh the dollar savings arising from not taking action. It therefore makes economic sense to try to stop runaway climate change. But what if the result had been different? What if he had discovered that the profits to be made from burning more fossil fuels exceeded the social cost of carbon? We would then find that it makes economic sense to kill people.

The only real surprise here is that Monbiot can muster any surprise and indignation. Conceptually, the issue is identical to the conflict over the 1996 IPCC report.

Issue 4 – The Role of Social Science in Climate Models

The IPCC stood out from many other 'science for policy' organizations through its commitment to include in its core activities the economic, social scientific and policy aspects of its scientific theme. Though, according to the self-understanding of the IPCC, these disciplines could not have the precision and exactitude to which the physical sciences aspired, it was clear that global climate change could not be studied in the absence of societal analyses, for two reasons. On the one hand, the things that worry us about climate change are chiefly the implications for people, commerce, cities and to some extent wildlife. The actual impacts that will arise clearly depend on how people respond. Without expert advice on these policy matters, there could be no sensible modelling of the 'output' side of the climatologists' work. On the other hand, possible policy responses to climate change again depend on people's willingness to accept the policy prescriptions – to forgo air travel or to put up with climate risks and so on. The IPCC handled this issue by dividing its procedures into three parallel tracks dealing with the physical sciences, the socioeconomic impacts and possible policy responses.

The aspect of the 'construction' that is of particular interest here is the construction of social science's role. The IPCC's involvement of social science is on the 'downstream' side, dealing with the consequences and impacts of climate science. That the issue could be framed in a different way is indicated by a four-volume work, edited by Rayner and Malone (1998), in which social science scholars were invited to turn the question round. They were invited to focus on the 'climate impacts of global human change', not only on the 'human impacts of global climate change'.

This innovative enterprise was clearly aimed to mirror the IPCC's work and to highlight the disciplinary orientations overlooked by the IPCC. Alterations in greenhouse gas concentrations are largely due to emissions from people and from their activities, and thus the rate of such atmospheric change depends on the speed and nature of economic growth, people's reproductive behaviour and the size of future populations, the technologies chosen by people, the cultures of consumption and leisure they develop and so on. The institutional assumption of the IPCC is that the most relevant social science is economics; many of the contributors to Rayner and Malone focus on the centrality of culture and thus boost the potential role for social anthropology and sociology, notably in understanding patterns of consumption and the meanings that consumption holds.

My main argument here is not about the specific claims of the authors in Rayner and Malone's volumes; it is that there are clearly different views on the explanatory primacy of the various disciplines needed to understand global climate change. A key part of the way that climate science is currently constructed is the nature of the role assigned to the social sciences and to the implicit hierarchy among those sciences.

Concluding View

Fred Buttel – whose legacy is commemorated in this collection of articles – wrote that, without denying the ability of science to capture how the natural world operates:

> . . . the crucial role of science often lies in how it is 'represented' and how it is employed within social movements, interest groups, regulatory agencies, epistemic communities, international organizations and 'regimes', and so on. Scientific knowledge thus often tends to be enmeshed with social symbols, political ideologies and discourses, social movement 'frames'. How this occurs makes an enormous difference in terms of environmental policy and politics. (Buttel, 2000: 28)

What I have argued in this article fits closely with this view.

However, it seems to me that Buttel's comment also implicitly reflects the majority practice, namely to concentrate on the role of natural scientific knowledge in 'environmental policy and politics'.

I hope to have shown that – at least in the case of climate change – the important question of how knowledge is 'represented' applies with equal significance to social scientific and economics aspects of the knowledge and also to the design and operation of the institutions through which scientific knowledge is warranted in the first place. Climate change has proven to be a major area for research in the sociology of science in the last decade because of the complexity of the relationship between knowledge and policy advising. Even so, it would only count as a highly complex case study in the sociology of science were it not for two novel factors critical to sociological audiences. There is first the way in which the IPCC process runs science-advising up against the very limits of legitimation through peer review. Second, there is the fact that, through its own deployment of social science, the IPCC inevitably raises a 'reflexive' question about the role of the social sciences (see Jasanoff and Wynne, 1998).

Accordingly, I suggest that the conclusions of this article can be summed up by returning to Table 1. Further research of many sorts on climate change is clearly required, but two relatively neglected aspects deal with the social science of climate change. As shown in the left-hand column, there is a need for studies of the scientific community that studies climate change as well as a need for more social science reflection on the roles of social science in climate-change models. In the table these broad types are linked to the examples discussed in this article earlier. I believe that a broadly constructionist approach to climate-change knowledge pays dividends in alerting analysts to the myriad and subtle roles that social scientific knowledge plays in diagnosing, forecasting and planning our climate futures.

Notes

1. For example, this was the lead story in the UK's *Guardian* newspaper (20 June 2007); for details, see the MNP report, at: www.mnp.nl/en/dossiers/Climatechange/moreinfo/Chinanowno1inCO2emissionsUSAinsecondposition.html (accessed 16 November 2008).
2. My example is Chapter 2, 'Observed Climate Variability and Change'.
3. As an example, see 'The Letter *Science* Magazine Refused to Publish' (as though that were unusual!); at: www.globalwarmingheartland.org/Article.cfm?artId=19213 (accessed 16 November 2008).
4. As Demeritt and Rothman (1999: 392) point out, another key issue was the 'discount rate' used in assessing the 'worth' of future benefits/costs but my chosen focus here is on other issues to do with ascribing values.
5. Monbiot starts his piece by suggesting that the Stern Review was so big that it took most commentators a long while to read enough to appreciate its methodological basis; he reports that 'by the time I reached the end I was horrified'.

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