SOCIAL THEORY AND THE GLOBAL ENVIRONMENT

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SCIENTIFIC KNOWLEDGE AND THE GLOBAL ENVIRONMENT

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INTRODUCTION

In sociological company it does not need to be argued that 'the global environment' is a social and cultural construct, into whose multivalent articulation are poured many complex and conflicting anxieties and commitments. The discipline of scientific knowledge is seen as the one superordinate discourse which can lend coherence to this incipient anarchism, to identify and describe the real natural problems, account for the underlying processes, and to define reliable and realistic options for societal response. Thus the social authority of science becomes a central issue, and ever more sharply so that the environmental and geopolitical arena over which it is supposed to reign expands to literally global proportions.

Two features of this developing global environment enterprise and its associated problems of global authority beg attention here. The first is the concentration on developing a 'sound scientific basis' (UK White Paper 1990) for internationally agreed policies, notably via the working groups of the Intergovernmental Panel on Climate Change (IPCC). The second is the explicit integration of social science into the framework of science and policy. In principle both of these processes can be taken for granted. However, the particular forms which these principles are being given as they are translated into practice are also taken for granted. Sociology of scientific knowledge has a role to play in exposing some neglected issues in both these aspects.

The way in which social science has been brought into the global environmental change programmes of virtually all the significant national and international research and policy bodies (from the UN to the US National Science Foundation and the Human Dimensions of Global Change programme of the International Geosphere Biosphere Programme, with the exception of the UK Economic and Social Research Council (ESRC), but not of the UK Inter-Agency Committee for Global Environmental Change (IAC)) has been as a subordinate to the precommitments and agenda of the natural sciences. It either provides information to the natural sciences on human activities which perturb the natural processes, or takes the natural science predictions as given and then works out the social and economic consequences.

A further role entirely consistent with this subordination to the basic agenda and epistemology of the natural sciences arises from the authority problem. Here social science is supposed to offer ways of educating global publics into better understanding and appreciation of the 'real' hazards (for example, ASCEND 1992), the questionable assumption being that lack of public uptake of scientific knowledge and prescriptions is based only upon ignorance or misunderstanding, not upon any more fundamental problems of cultural identification or alienation. Some of these deeper difficulties may be associated with the particular 'cultural' properties of science which are exposed by a sociological examination of what is meant by, and automatically assumed to be, 'sound science' for global environmental policies.

Although this subordinate relationship to natural science has been questioned (and rightly so), the reasons for doing so have not always been so clear. If as some including myself have argued, social science ought to be involved more upstream in the currently 'private' processes of constructing scientific knowledge or technological artefacts, how far does this sociological remit properly go, and what are the implications? If environmental scientific knowledge is sociologically deconstructed, what does that deconstruction itself 'reveal'? And what is the relevance of the criticism sometimes aired that sociological treatment of the current scientific 'consensus' that we have a greenhouse warming problem inevitably plays into the hands of the international fossil fuels lobbies and their political friends who are doing their very best to demolish that consensus?

In this chapter I will suggest that there are confusions on these questions, and that these relate to the long-standing differences between interests-based and more culturally rooted social constructionist perspectives on scientific knowledge.

The issues for sociology of science in relation to the global environment can be partly summed up in the question: What social or cultural factors do particular scientific discourses about the global environment tacitly reflect as if natural, and thus exempt (whether deliberately or not), from wider debate, negotiation and responsibility? In his book, *Strange Weather*, Andrew Ross has suggested that the structure of US weather forecasting is 'shaped by a social and political mapping of the world as much as it is determined by the atmospheric map of shifting fronts and air-masses' (Ross 1991: 240). What kind of social and political maps are implicitly represented in global environmental science? Posed from a social constructivist perspective this question also includes a challenge to social science, because the boundaries of the 'natural' and the social or cultural are at issue, and much of social science as currently engaged in the global environment field is obscuring the full scope of the social and cultural issues by default.

REDUCTIONISM AND THE GLOBAL ENVIRONMENT

It is ironic to note that as the geopolitical reach of environmental science has become more and more expansive, its intellectual temper has become more reductionist. Whatever the justifications may be for globalizing the instrumentalist and standardizing culture of science, the problems of diverse local cultural identification and authority for its pronouncements and associated policies only escalate even further as a result. Conventional approaches such as those emanating from scientific institutions see the problems here to be about public ignorance, and *the need to develop more rational* attitudes (for example, ASCEND 1992). However sociological approaches recognize that scientific knowledge reflects social and cultural factors which render it parochial in important respects; just how parochial in the global political and cultural milieux, remains to be charted.

In recent years the most prominent focus of 'official' scientific attention to the global environment has been the IPCC and its scientific working groups attempting to give objective projections of world climate futures and their dependence on anthropogenic carbon and other emissions. Newby, amongst others, has noted the apparent regress from the 1987 UN Brundtland Commission's broad focus on global environmental futures, to that of the more recent IPCC. Whereas Brundtland articulated a basic political, moral and social framework *from which to define* policies for environmentally sustainable global development, including scientific R&D and technology policies, IPCC began from a scientific origin - defining and managing a sustainable climate - from which should be derived the necessary social, economic and other policies for survival.

Newby's observations resonate with wider critiques of both Brundtland and IPCC, that they still, albeit in different ways, reflect rich world agendas and interests, and *limit via* 'natural' expert authority the uncertain, but certainly uncomfortable, extent to which the industrialized rich world needs critically to examine and reconstruct its own deep commitments in order adequately to address the contemporary crisis which is called global environmental change. Various authors have commented on the evident point that the IPCC science-led process has effectively reduced the global change problematique from the wider political economy issues surrounding North–South inequities, the debt burden and the 'environmental poverty trap' in which poor countries are in too desperate an economic state to be able to forgo immediate resource exploitation whatever the long-term implications for sustainability. Whereas the IPCC's discourse is of a global *environmental* crisis, the critical voices assert a global political, economic, moral and cultural crisis. The question of interest here is whether, and how, 'scientific' discourses such as that around the IPCC prevent us from recognizing and responding to the awful challenges of the larger version of the global crisis.

The political perspective on global environmental change leaves the science *as knowledge* untouched by critical analysis. That it is alleged to be handmaiden to political attempts by the rich countries to impose a policy consensus on the developing world implies nothing about the kind of scientific knowledge being developed and deployed. That IPCC equates global environmental change with climate and greenhouse warming, and mainly with carbon emissions, is already a form of reductionism with political implications. There is also some analysis (for example, Lunde 1991) of the management of scientific consensus in IPCC in order to try to achieve greater political authority. However, the more subtle and difficult question is whether the deeper epistemological commitments on which the scientific knowledge is built serve to constrain the vision of what is at stake, socially and culturally.

One useful approach to this question is via historical reflection. In the present scientific enterprise, 'sound science' is taken to mean huge supercomputer models which are only available at six research centres in the world. These mathematical titans involve physical meteorological parameters, equations and sub-models. They are the direct descendants of weather forecasting models which extended their scope from a few days to the outer envelope of viable predictive control of the relevant variables, thought to be about twelve days, and are now extended to try to give credible predictions thirty to forty years hence, based upon given input assumptions about atmospheric carbon emissions.

As many have noted, despite their colossal size and complexity these models leave out several important factors such as cloud behaviour in relation to global warming, and biological processes such as marine algal fixing of atmospheric carbon, natural methane production and release as temperature changes. There is no reason to suppose that these processes are any less basic to global climate change mechanisms than the processes which have been incorporated, it is just that they have not been treated as central in weather forecasting and so have not been previously subjected to intensive data gathering - at least not by recognized scientific research specialities.

The physical-mathematical models are being validated, which also means examining the significance of these omissions, by comparing their outputs of calculated global temperatures from retrospective runs of the models with past data on global temperatures.

These reconstructions of past temperature and climate states, stretching back centuries, have been performed by a range of different research disciplines, from geography and social history to geochemistry and palaeogeology. The kind of evidence adduced ranges from parish records of crops and harvests, to radiocarbon dating of ice-cores to establish dates of inferred measures of temperature and carbon dioxide at different sites. This kind of science is manifestly indirect in its access to its object, namely data on temperature and CO_2 levels and distributions. It relies openly upon indirect and surrogate variables, and composite variables, where control for other factors, known or unknown, is overtly more difficult.

Although it is now dependent upon this non-reductionist scientific knowledge *for validating* its own mathematical models, the meteorologists and physicists who dominate the science-policy climate-change domain have long regarded it as unsound. In the traditional cultural hierarchy, good science is taken to equate with that which allows prediction and control with analytical atomization, high precision and single-variable measurement and manipulation where possible. The Meterological Office, whose Director headed the scientific working group 1 of the IPCC, has been the guardian of this concept of good science in the climate-change field. It is overwhelmingly staffed by mathematicians and physicists, and has in turn dominated UK research on climate. When the geographer Hubert Lamb, who had been on a special fellowship at the Met. Office, left in the early 1970s to establish the Climate Research Unit (CRU) at The University of East Anglia, with the explicit interest in long-term climate changes and reconstruction of past climate states, financial support was not provided by the UK research system. The Met. Office opposition to Lamb's kind of science found resonance in the reductionist culture of 'good science' generally in the UK, and the CRU programme was only rescued thanks to private support from Shell International and The Nuffield Foundation.

There is a particular irony in this situation which is worth emphasis. At this time, in the 1970s, the Met. Office *scientists did not* believe that long-term climate change was an issue. Thus whilst Lamb and colleagues gathered their 'unsound' scientific evidence for reconstructing long-term climate changes, Mason and colleagues from the Met. Office were publishing rebuttals (see, for example, Mason 1976) to the effect that the global climate system contained strong equilibrating factors which made it resilient against perturbations; fluctuations occurred, but only around a stable mean climate condition. Climate change was thus a grossly exaggerated issue, according to the meterologists, and the broader climate researchers such as Lamb were responsible for the exaggeration.

Ironically, therefore, the discipline - and indeed the institution - which now dominates the scientific and policy consensus-making on global climate change was then pouring scorn on other 'unsound' sciences which were advancing the idea that climate change was a problem, even if the causes and directions of change were still obscure. There were even hints that these climate-change scenarios and their associated disciplines were part of a pernicious antiscience movement. Certainly they enjoyed widespread public uptake, as in Nigel Calder's 1970 BBC TV programme, *The Weather Machine*; and the diatribes against the emergent environmental movement as 'antiscience' (for example, Maddox 1972) included reference to the environmentalists' belief in climate change and the anthropogenic greenhouse effect.

Thus the less reductionist perspective of the other sciences on global warming was initially denigrated and denied as 'second-rate science' by the scientific establishment, only to be taken over and transformed when the notion of climate change gained wider credibility, into the reductionist idiom of the powerful supercomputer models of the IPCC.

The dependency of these models upon longer-term historical data to validate them does not alter this state of affairs, indeed reports from IPCC negotiations indicate that these surrounding disciplines are organized as servants to fuel the megamodels. So too it appears are the scientists from institutions in the developing world - and even in the developed countries - who do not have direct access to the few supercomputer models (Liverman 1991). There are important implications for the credibility and purchase of policy proposals seen to be emanating from this narrowly exclusive cadre.

Resistance to the kind of climate research at CRU on the part of the UK research policy establishment was based upon the physics-based notion that sound science equals reductionist, high control, high precision science; thus the science of CRU and Lamb was unsound, and unworthy of support - 'second-rate science' (Beverton 1992).

This same reductionist cultural notion of sound science in environmental policy is institutionalized in UK research policy. For example, a study of UK environmental research (Wynne 1991) found not only that the majority of funding went to physics-based research projects, but that even within biological research as a whole a majority of funding was in laboratory-based molecular biology and genetic manipulation - the biological counterparts of physics. Some researchers recognized that this artificial, high-control research had exhausted its possibilities because of the lack of research actually testing behaviour in (less reductionist) field conditions.

Thus the cultural syndrome identified in the competition between physical meteorology and other less-reductionist disciplines over how to define and develop climate change knowledge was part of a wider pattern (also affecting social science) that begs further research. The recognized disciplinary paradigm conflicts between ecology and physics in science-policy disputes are part of this deeper picture yet to be properly filled out. Robbins and Johnson's (1976) case-study of toxicology versus geochemistry in the environmental led controversy in the early 1970s is a similar example.

For the climate research case these considerations raise questions already aired in relation to other science policy areas, such as environmental and safety regulation, as to how criteria of good science for policy ought to be defined (Jasanoff 1990), and whether the unreflective adoption of the dominant criteria in scientific institutions is an adequate basis for public debate. In other words, do the competing styles of science reflect different epistemological, institutional and cultural correlates? And do these contain different implicit prescribed boundaries, not only to the science-policy interface but also to the extent of problematization of the human subject or to cultural identities embedded within the processes of global change?

WHY SOCIOLOGY OF SCIENTIFIC KNOWLEDGE? RETRIEVING INDETERMINACY

Although the reflex reaction is still to understate and, where possible, conceal scientific uncertainties in public policy issues, it is now commonplace to find the inevitable limitations of scientific knowledge recognized as a fact of life which policy-makers and publics should learn to accept (Ravetz 1990, Smithson 1989, Jasanoff 1990, Nelkin 1979). Thus scientific uncertainty is widely discussed as the cross which policy-makers have to bear, and the main obstacle to better and more consensual or authoritative policies. Yet much of this debate still assumes that if only scientific knowledge could develop enough to reduce the technical uncertainty, then basic social consensus would follow, assuming that people could be educated into the truth as revealed by science.

There are two main sociological strands of criticism of this dominant conventional perspective. The interests-oriented strand would note that even within the constraints of an accepted natural knowledge consensus, legitimate social interests – and hence favoured policies – can be in conflict. A perspective from the sociology of knowledge would go further, to argue that dominant interests control expertise and hence shape the available knowledge to reinforce their interests.

A more radical strand would suggest that beneath the level of conflicting explicit preferences or interests lies a deeper sense in which scientific knowledge tacitly reflects and reproduces normative models of social relations, cultural and moral identities, as if these are natural. Thus, for example, the level of intellectual aggregation of environmental data and variables such as radiocaesium in the environment, when used to establish and justify restrictions on farmers operating in that environment, is effectively prescribing that degree of social or administrative standardization of the farmers.

In other words, at a deeper level than explicit interests the form in which scientific knowledge is practically articulated prescribes important aspects of their social relations and identities. In research on the interactions of scientists and farmers after Chernobyl, this point came out as the farmers' detailed and differentiated local knowledge of the environment and what it meant for optimal farming methods, even in the same valley, were denied by scientific knowledge whose 'natural' form aggregated and deleted them into single, uniform data categories combining and homogenizing several different valleys and many farmers. As one farmer caught by the Chernobyl restrictions lamented in this respect: 'this is what they can't understand; they think a farm is a farm and a ewe is a ewe. They think we just stamp them off a production line or something.'

This brief glimpse indicates that the scientific knowledge is not naturally determined; it could have been organized differently and still have respected the evidence from nature. Yet social commitments to such organizing epistemic principles as the levels of aggregation of entities into uniform conceptual classes and categories are so deeply enculturated into the scientific canons of given specialities or fields that they are mistaken as being completely determined by nature.

The indeterminacy which this kind of example exposes is fundamentally different from the uncertainty normally bandied about in discussions of science and policy. In trying to make sense of scientific consensus building in the context of the IPCC and global environment politics for example, Lunde (1991) tries to distinguish between 'standard' epistemic factors in consensus building, such as empirical observation, measurement and monitoring, and validation of models; non-epistemic factors, such as political interests and lobbying; and 'non-standard' epistemic factors, such as saliency and the perceived social role of knowledge. The problem with these distinctions is that they are exposed as vacuous as soon as they are applied to concrete instances. Sociology of scientific knowledge has shown repeatedly, and often in great detail, how a sacred canon of scientific method such as the replication of empirical observations - another 'standard epistemic factor' - is a fundamentally underdetermined normative principle 'controlling' scientific knowledge building. The same is true of inference rules and logical commitments which define entities as *belonging to the same* class or different collective categories depending upon which properties are taken as salient. The actual meaning of these 'natural' terms and rules have to be negotiated as research goes along. This is a fundamentally more open-ended process of knowledge construction than is recognized in conventional perspectives, which treat scientific knowledge as fully determined by nature alone, and which correspondingly treat scientific uncertainty as a kind of temporary pathology awaiting more rigour or precision which will supposedly reveal the 'true' determinism underlying things.

Radiocaesium behaviour in the environment

An example of the deep indeterminacies concealed in scientific knowledge can be drawn again from the post-Chernobyl emergency in 1986. This involved scientific knowledge of the environmental behaviour of radiocaesium, which is still important in trying to manage the fall-out from Chernobyl in hill sheep farming areas like the Lake District. It is now recognized that the scientific understanding on which policy was based at the outset of the crisis in summer 1986 was mistaken; the prevailing belief was that the radiocaesium deposited from rainfall on vegetation would be washed off by further rain into the soil and, chemically immobilized, would thus no longer be available for uptake into the sheep. The sheep would thus suffer only a 'one-pass' exposure and their contamination would therefore decrease rapidly.

However, this confident belief was based upon behaviour in clay soils where absorption onto aluminosilicate clay molecules does indeed take place. No allowance was made for the conditionality of this scientific belief upon the particular soil-type, so that the very different organic, acid peaty soil conditions of the fells was ignored in the implicit view that the prevailing scientific model was universal. On this basis scientists advised that the restrictions would not be necessary at all, but if they were established against this reassuring prediction, they would only be needed for three weeks or so. Nearly 150 Lake District farms were still restricted by radiocaesium levels from free sale of lambs in 1992, six and a half years later.

Following the unexpectedly long duration of the contamination - much longer than was originally predicted by scientists - and the costly and disruptive restrictions upon farmers, allegations were made by environmental groups and some farmers that the scientists had in fact known all along that the radiocaesium would remain mobile in the upland soils. They were thus alleged to have known that the restrictions would have to last much longer than three weeks, but to have entered into a conspiracy with government officials to conceal this from the farmers. These allegations were based upon the claim that the scientists had done research on radiocaesium behaviour in different soils, and had found the different behaviour as long ago as 1964. A paper published in *Nature* in 1964 was referred to in evidence (see Gale *et al.* 1964). This paper does indeed report the results of mobility tests with radiocaesium in six different soil types, including the two in question, but its reported results are ambiguous in a way which it is instructive to examine more carefully. The tests performed were of the physical mobility of radiocaesium by downward migration of a standard surface deposition after yearly time intervals of up to five years. Several samples of each soil type were set up, as is normal for this kind of experiment. The range of depth measurements at each time interval was wider for the acid peaty soils samples than for the clay (and the others), but the mean depths were the same. Thus if one took a measure of variance the soils behaved differently, but if one took mean depths the soils apparently allowed radiocaesium the same mobility. The latter would be grounds for saying that prevailing understanding was that all soil types behaved the same with respect to caesium mobility, hence in 1986 it was reasonable to extrapolate from clay soils to upland peat soils in making the post-Chernobyl predictions and policies.

Whatever the pros and cons of this political argument however, it is beside the present point. When one examines the 1964 *Nature* paper it is evident that the whole context of the reported research at that time was the fall-out from atmospheric weapons testing. The assumption then was that the critical exposure pathway to humans was not root uptake from the soils and into vegetation, thence into sheep meat from grazing, but that it was an external physical gamma radiation dose to a person on the surface direct from the radiocaesium in the ground, with shielding (reduction of the exposure) dependent upon the depth of the radiocaesium in the soil. This assumed exposure model, a social 'choice', based only on physical processes and parameters, was what determined the 'natural' scientific interest in physical depth profiles.

Yet in the post-Chernobyl sheep crisis a completely different exposure pathway came into dramatic focus, in which the key parameter was not physical migration by erosion and leaching downward in the soil, but chemical mobility and chemical availability of the radiocaesium for uptake by the roots of the ambient vegetation. Thus the scientific knowledge created by the 1964 paper, that the soils behaved in the same way, depended upon observation of *physical* parameters, a commitment dictated by the exogenous model of the critical exposure pathway assumed to exist (and probably also not unconnected with the fact that research and scientific advice was dominated by physicists, a syndrome still recognizable, though less acutely so, today). An exposure pathway of the kind identified after Chernobyl would have encouraged interest in *chemical* mobility, and would have then looked at chemical parameters instead. In this case the soils would have been different.

Thus, depending on different exogenous and contingent *social* models of exposure route, the scientific knowledge of the 'natural' properties of soils and radiocaesium could change diametrically from being 'the same' to being 'different'. These contradictory knowledges were not identified or confronted, and neither were the different conditions of validity of each stance clarified; the ambiguity and open-endedness was just left there in the wake of 'scientific progress', unresolved. The external, essentially social issue of which exposure pathway should structure research was subsumed into the 'natural' assumptions of the scientific domain, as if it were merely a scientific question. It was 'answered' by default, without anyone realizing that an arena of *social* responsibility and choice existed *prior* to and within the science. Lack of reflexive capacity on this dimension led science into the kind of trouble it encountered when its confident public predictions went wrong after Chernobyl.

This is not meant as an exercise in bashing science with the wisdom of hindsight. The aim is to show the intrinsically open-ended character of 'natural' choices and intellectual commitments embedded in, and shaping, the 'objective' knowledge outputs of science. It shows not only that what scientists knew at any given time is fundamentally problematic but, more important in this context, that even a taken-for-granted procedural principle within the research practice, such as the 'natural' significance of physical depth parameters, is a function of an externally defined commitment to a particular social scenario of exposure. Whether this is invisibly subsumed by default in the realm of science, decided upon by some science policy experts, or more widely debated and defined in society or political institutions, is itself a contingent matter which affects (and is affected by) where the boundary of science and policy is thought to be.

The precautionary principle

The development and interpretation of the precautionary principle in environmental science and policy also poses similar questions about dominant views of the relationship of scientific knowledge to policy values. The precautionary principle has been advanced as part of the general strategy of prevention, to avoid the problems associated with placing the burden of proof on the environment, and in that waiting for the evidence of harm, when remedial action is very expensive if not actually impossible, committing to policy intervention.

The standard way of defining the precautionary principle is to say that, because evidence of harm is uncertain but the error costs are very large, in some circumstances it is justifiable to intervene to protect the environment before all the evidence for a more confirmed cause-effect relationship can be gathered if it is 'reasonably anticipated' that an environmental discharge will be irreversibly harmful.

The UK government's stated acceptance of precaution closely reflects this perspective. There are two points to note. First, it retains a deterministic version of scientific uncertainty - namely, that this is a temporary matter of imprecision which will be eradicated when enough research has been devoted to the questions. Second, as a corollary, it assumes that the precautionary principle simply means moving the regulatory threshhold for intervention up the body of existing scientific knowledge - away from nature, as it were, and towards the potential polluter. *However, in the process of shifting the external policy values applying to the knowledge, that knowledge itself does not change its supposedly naturally determined internal shape.*

However, the example drawn from the radiocaesium soil-research knowledge indicated that 'when scientific knowledge knows what?' is more fundamentally open-ended, soft and thus more deeply problematic than this model recognizes, even when it expressly adopts a more precautionary standard. As we shift the normative rule through the body of scientific knowledge in this way, that body of knowledge itself may (or may need to) change as a function of the change in social or cultural values. The external normative 'choices' also tacitly influence the 'internal' choices of salient parameters, inference options, sameness and difference relations in theoretical models, and what is defined scientifically as problematic or not. On this point we can relate the radiocaesium-soils example to the production of competing kinds of environmental scientific knowledge - on the one hand, conventional research recognized under environmental assimilative capacity approaches to marine pollution regulation and, on the other, that underpinning the precautionary principle.

Dethlevsen (1988: 281) has alluded to deeper cultural differences pervading the two competing scientific approaches in his comment that 'workers who cannot see the correlation between pollution and diseases in their studies are with the exception of Moller from Germany, living on the other side of the North Sea'. But the point is that the scientists involved are not merely looking at the same body of data with different evaluative spectacles as it were, and then advising policy-makers of their policy-related judgements. Their epistemic, theoretical and methodological commitments (which may or may not include an explicit view about precaution) build up different bodies of 'natural' data or facts, impregnated with incompatible 'natural' logics, well before the policy actors come even to see, let alone exercise, normative choices about how strictly to regulate polluting activities. Thus some normative choices will have already been obscured.

Normative responsibilities and commitments are concealed in the 'natural' discourse of the science, indicating the fundamentally social, negotiable definition of the boundary between science and policy (Jasanoff 1990). The full range of moral and social issues at stake is *not* adequately described by treating the 'factual' scientific realm as if it is a separate objective 'black box' from the normative. It already reflects and expresses tacit normative boundaries and constraints.

The traditional and still dominant approach to marine pollution is a 'high-science' of the sea, akin to the reductionist idiom dominating climate research. It defines specific end-points such as fish toxicity, and focuses attention on single-variable cause–effect questions, defining environmental assimilative capacity as the maximum pollutant load the environment can carry before observable harm (defined by the choice, end-point variable) is done.

The precautionary scientific idiom is much more ready than assimilative capacity to accept:

1 that the composite variables, such as 'immunocompetence' 'disease' and 'stress', are legitimate components of scientific reasoning. Sindermann (1984) identified eighteen different factors, some natural, some anthropogenic, which might singly or combined result in stress; and 'disease therefore has to be understood to be an unspecific response towards all kinds of stress' (Dethlevsen 1988: 276). This idiom thus uses composite variables flexibly, recognizing the possible constituent factors but not discounting the larger picture just because the precise constituent variables in a composite such as 'stress' may not be defined);
2 the scientific legitimacy of indirect and multiple cause–effect inferences. For example, Dethlevsen (1988) reports a study of the possible correlation between diseases and marine contamination. Although no *direct* correlation was found, bacterial levels in the blood of eels from a contaminated area of the North Sea averaged 80 per cent compared to 4 per cent in eels from a relatively uncontaminated reference area. This was taken to indicate an *indirect* effect of pollution, causing reduced immune-system

strength in the eels and thus higher vulnerability to other disorders even if these had not shown at the time of sampling and even if they might be finally induced by a separate natural factor. Focusing on a single-variable direct-cause explanation would in such a case completely miss the damaging role of pollution, and put the effects down to 'natural causes';

3 therefore that, with due caution, circumstantial evidence for cause–effect mechanisms is legitimate.

Indeed, on closer inspection all scientific reasoning is unavoidably circumstantial as the radiocaesium soil example also illustrated. Returning to the competing idioms of physical supercomputer modelling methodology, and broader-based, historical climatology, the charge that the latter employed only surrogate variables and hence could not gain intellectual control of its inferences and causal constructs can also be seen to be misleading. The mathematical models also have to employ surrogate parameters and variables for the real values and behaviours of interest; it is just that these are more culturally esoteric, and more socially and intellectually inaccessible, so that this characteristic is not so evident as it was for the geographers and others in their research. The models may also be more buttressed by cross-cutting and interlocking scientific constructs in a 'thicker web' of consolidation than the less reductionist scientific idioms.

The conventional assimilative capacity scientific idiom of marine pollution (Campbell and Chadwick 1993) appears on the face of it to avoid circumstantial reasoning; but for example, assimilative capacity scientists claimed to have disproven the alleged connection between fish disease and contamination when the observation of high levels of disease away from inshore waters was reported. This was an anomaly sufficient to 'disprove' the connection, on the prevailing assumption that such offshore waters are less contaminated. This conclusion was drawn before measurements which showed the 'offshore is cleaner' assumption to be wrong – at least in this case. The convinced application of the general assumption (that offshore waters are cleaner) to the specific case was just as circumstantial as was the reasoning sometimes adopted by the 'precautionary' idiom.

There is always an ineradicable element of indeterminacy in deciding whether a new empirical situation is an instance of a class of entities under one theory or model, or another. (Is the soil in the upland sheep areas the same or different from the soil(s) on which the conceptual model of caesium behaviour is constructed? – it depends on whether we are concerned about sheep meat contamination or direct external gamma-ray exposures.) The traces of this endemic indeterminacy are usually already well-concealed (even from the scientists involved) by the time it comes to exercising policy responsibilities, even though the way the choices are made at such scientific points may have important policy implications.

We have learnt from the detailed analysis of the creation of scientific knowledge over the last twenty years or so that many of the intellectual commitments which constitute that knowledge are not completely validated, not fully determined by empirical nature (Collins 1985, Latour and Woolgar 1979, Barnes and Edge 1982). Always central to the process are not just uncertainties in the form of imprecision (which, it is assumed, will be narrowed down by more research), but fundamental indeterminacies – for example, as to whether things are classified as the same or different, and on what specific properties or criteria. The purely technical aspects of such intellectual commitment merge with epistemic questions as to why we are constructing such knowledge anyway; this is always open to social evaluation and negotiation, though that is very far from saying that scientific truth can be subject to social choice.

However, we can see that when scientific knowledge is deployed in the public domain, the social judgements of a relatively private research community which create closure and 'natural validation' around particular constructions of speciality scientific knowledge, need to be re-opened (deconstructed) and renegotiated in a wider social circle: possibly one involving different epistemological commitments and expectations, and correspondingly different definitions of the boundaries between nature and culture, or (objective) determinism and (human) responsibility. These too will have to be recognized and renegotiated in some way, as new, more-broadly legitimated principles on which scientific knowledge generation can be founded.

CONCLUSIONS

This chapter has attempted to outline the basis of a fundamentally different framework for thinking about the relationship between scientific knowledge and public policy, such as exists around global environmental issues. In particular it focuses upon the complex question of authority and credibility. It suggests that these should be conceptualized more in terms of social and cultural identification (or alienation) than in terms of the intensification of the natural warrant for policies, and then in their successful communication. These problems of cultural identification are more fundamentally difficult when the ambitions of scientific policy enterprises are inflated to global level; but, even within the cultural confines of the 'scientific' industrialized world, they are already more deeply problematic than is usually imagined.

The analysis shows that the construction of scientific knowledge is less completely determined by nature than conventional approaches assume, and that as a result the construction processes are more open-ended and contingent than is usually recognized. *Sociological analysis* of scientific knowlege has shown that the successful 'closure', and stabilization of these open-ended, incomplete constructs or knowledges, is achieved by mutual overlap and the reinforcement of provisional or incompletely warranted commitments by adjacent social-intellectual networks. An incompletely warranted intellectual commitment in social group A is 'confirmed' by its consistency with an apparently solid, but just as incompletely warranted, commitment in social group B; and vice versa. Thus both are consolidated and 'closed' as if naturally determined by processes of mutual affirmation.

This kind of network interdependency and mutual 'bootstrapping' of credibility is well-recognized in the sociology of scientific research, via interaction amongst different research specialities and groups. However it also occurs between 'scientific' and 'policy' networks and actors, in the myriad intermediate roles and institutions such a advisory committees, review working parties and the related institutional flux. Representations of the boundaries of science with policy are flexibly - and often inconsistently - articulated as an integral part of these processes.

As the earlier case-studies showed, one result is that social commitments, whether consciously or inadvertently, are built into the 'natural' knowledge so constructed and deployed as policy interventions. One might say that this shows science responding to changing dominant social values, in something akin to a respectable model of democracy. However, especially in the light of science's historical role as an ideological resource for dominant interests, this would be an extravagantly optimistic interpretation of the capacity of such processes as presently institutionalized, not only to reflect democratic values, whatever they are, but to encourage their mature and more effective articulation in relation to science and environmental challenges.

At this point it is important to distinguish between two typical approaches from within the sociology of scientific knowledge, because they carry very different implications in this context.

An interests-based approach would argue that scientific knowledge is shaped by the outcome of struggles and negotiations between different social interests, including the social-cognitive interests which scientists committed to a particular paradigm in reproducing and extending their familiar forms of technical practice and identity. Thus 'internal' and 'external' social interests may combine to produce particular bodies of 'natural' knowledge in which the essential indeterminacies referred to above are closed by those interests.

This approach would argue that dominant scientific knowledge, as constructed in the supercomputer climate models used by the IPCC, reflects developed countries' interests in obscuring the social and political inequities which lie at the heart of global environmental degradation. Buttel and Taylor (Chapter 11, this volume) appear to express this perspective in advocating a sociology of science for global environment. A similar 'interests' analysis was offered in the early 1970s about the systems computer modelling of global environmental disaster in *The Limits to Growth* (Golub and Townsend 1977).

However, Buttel and Taylor's persuasive general argument for a sociology of scientific knowledge approach to global environment does not make clear the distinction between their assumed analytical perspective and a more 'cultural' approach. In Chapter II they rightly note that

> when sociologists have attended to global change issues, they have tended to do so by uncritically accepting and appropriating the global 'constructions' of modern environmental problems that have emerged within both the environmental sciences and the environmental movement (p. 228).

They then observe that

> This is especially problematic since, within both science and politics, the 'globalization' of the environment has served to steer attention to common human interests in environmental conservation, and away from analysing the difficult politics that result from different social groups and nations having highly variegated - if not conflicting - interests in contributing to and alleviating environmental problems (pp. 228–9).

They then point out that such sociologists, by uncritically swallowing global environmental science constructs, have been caught unawares by the force of the radical critique of 'global environmentalism' and its rich world agenda, now emanating from the developing world. In other words, sociological deconstruction of global environmental science is necessary to expose the hidden interests of the rich countries in concealing the more demanding political challenges of exploitation, domination and inequity underlying global environmental change.

This kind of analysis is easy to accept. However, in the light of the cases described before, and indeed of much other work in sociology, the question is whether it goes far enough. It leaves largely untouched the possible connections and contingent reinforcements between scientific knowledge, constructions of the human subject, and the cultural milieu of late-modern society.

A different emphasis in sociology of global environmental scientific knowledge would also consider the ways in which scientific knowledge of environmental situations naturalizes and reinforces particular cultural and moral values or identities beyond the reach of what can be called 'interests'. Thus the inadvertent structural features of scientific knowledge which, for example,

- classify natural and social 'actors' in one way and not another;
- homogenize and 'fix' them at a particular scale of aggregation;
- define ambivalence as antipathetic to rationality because it undermines the assumed epistemological principle of prediction and control;
- rule circumstantial reasoning in one direction to be unsound, whilst covertly if innocently using it to construct alternative 'natural' conclusions; or
- define human actors as interested only in maximizing or satisficing utilities, and conceive the social science of global environment in such terms;

can be seen to reflect and to naturalize particular cultural norms, social identities, and even models of human nature which perhaps need to be problematized as part and parcel of the global environment predicament.

To give an example, unreflexively adopted implicit models of human agency buried within, and acting as preanalytical framing commitments that shape global environment scientific knowledge, may produce unpleasant surprises for policy-makers. The assumption is that increasing public awareness of global warming scientific scenarios will increase their readiness to make sacrifices to achieve remedial goals. Yet an equally plausible suggestion is that the more that people are convinced that global warming poses a global threat, the more paralysed they may become as the scenarios take on the mythic role of a new 'end of the world' cultural narrative. Which way this turns out may depend upon the tacit senses of agency which people have of themselves in society. The more global this context the less this may become. Thus the cultural and social models shaping and buried within our sciences, natural and social, need to be explicated and critically debated.

The unreflexive nature of science, including a substantial part of social science in this arena, serves to avoid the problematization, for example, of social relations where instrumental epistemologies derived unwittingly from the natural sciences assume essentialist or rational choice models of social behaviour. This imposes the prescriptive assumption that social interaction is not an activity with moral meaning and struggle against indeterminacy, to create value and identity, not an end in itself, but merely a means to the supposedly ultimate ends of maximizing utilities or preferences.

Likewise, a more culturally oriented sociology of scientific knowledge would recognize more of the reflexive questions to be asked about the intrinsically alienating effects of knowledge which positions and constructs people in environmental processes as if they are merely reproducing and extending consumer-based capitalism. Leslie Sklair, in Chapter 10, comments that

> For too long the social study of global environmental change has been focused on supply-side issues, (predominantly production systems), and has paid too little attention to demand-side issues (particularly consumption patterns) (pp. 221–2).

This comment resonates with the simple observation that for global sustainability rich-world, consumption-oriented behaviour is going to have to change more radically than is implied in the language of the economic and technical adjustments which monopolize mainstream policy thought. The changes are likely to bite much deeper, into the very social relations and identities from which we can gain sustenance (which is arguably vicariously sought from consuming material resources and generating physical environmental impacts).

However much or little this is true, the practical large-scale social exploration of these dimensions is already underway according to the sociological analysis of new social movements. The sociology of scientific knowledge is essential as an intellectual resource to retrieve the domains of human struggle, cultural reconstruction and social responsibility blindly sequestered by science, including its social science counterparts.

A final comment relates to the familiar criticism that a 'relativist' sociology of science undermines the basis of political critique, of the hidden interests shaping science as an ideological tool which needs debunking. The simple interests approach identified above stops short of that reflexivity, and in so doing retains the realist stance from which the radical critique of dominant versions of global environmental science can be mounted. It is notable that much of that critique (see, for example, Shiva 1989) displays a markedly realist tone.

So does the more reflexive version of the sociology of scientific knowledge – that which problematizes the epistemological commitments, cultural resonances and inadvertent social models implicit within scientific constructions – necessarily undermine critique? I would conclude that it does not; indeed that the reflexive critical examination which it helps to provide of late-modern society's deeper identifications with modern science as (instrumental and standardizing) culture is an essential component of an authentic and constructive response to global change in its widest sense. As such, and even though the relationship remains to be worked out, it ought to be a complementary perspective to the less reflexive, more directly political critique of the global environment problematique as defined by science, and of which IPCC may be considered as a modernist delusion - not so much in terms of its relationship to physical realities, whatever they may be, but in terms of its apparent expectations of cultural authority and global social purchase.

The sociology of global environmental science indicated here would imply the reopening of explicit and diverse negotiations of pluralist epistemological commitments within a more open-textured, culturally differentiated and socially permeable 'scientific' realm. This would inevitably mean that policy cohesion, presently vainly pursued through the ever more anxious and manifest manipulation of scientific consensus and 'natural authority', would have to be more openly based upon human commitments in the face of recognized indeterminacies. This reopening of such a human domain is not without its own risks. But it would reinforce the stance taken by many in the field that the policies most likely to help in combating global warming are worth doing anyway, on social, political, moral and even economic grounds, regardless of what they may or may not do to the environment.

This focus on the implicit cultural framing of scientific knowledge does not mean that such knowledge would be debunked or denied authority. Rather the conditions of validity would be critically explored, and the tacit social and moral commitments of knowledge exposed for debate and negotiation. This would demand the negotiation of plural forms of science with negotiable social and cultural boundaries, and correspondingly more social struggle to articulate emergent values and fluid identities. In this kind of process scientific uncertainties would not be an embarrassment, but - seen more properly as authentic human indeterminacies - the meat and drink of a more mature social learning process.

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