# Uncertainty and environmental learning

## Reconceiving science and policy in the preventive paradigm

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**The author considers the implications for current assumptions about scientific knowledge and environmental policy raised by the preventive approach and the associated Precautionary Principle. He offers a critical examination of approaches to characterizing different kinds of uncertainty in policy knowledge, especially in relation to decision making upstream from environmental effects. Via the key dimension of unrecognized indeterminacy in scientific knowledge, the author argues that shifting the normative principles applied to policy use of science is not merely an external shift in relation to the same body of 'natural' knowledge, but also involves the possible reshaping of the 'natural' knowledge itself.**

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This paper was originally prepared for a conference in April 1991 on 'The Principles of Clean Production', organized for the Stockholm Environment Institute by Lancaster University's Centre for Science Studies and Science Policy. The paper has benefited from criticisms and encouragements from Robin Grove-White, Jane Hunt, Tim Jackson, Les Levidov, and an anonymous referee. The work is supported by the UK Economic and Social Research Council and the Stockholm Environment Institute. No-one but the author bears responsibility for the product.

One of the most important new goals of environmental and technology policies in the last decade has been the shift towards prevention. This change implies acceptance of the inherent limitations of the anticipatory knowledge on which decisions about environmental discharges are based. We can often find out only when it is too late, or at the very least, awesomely expensive, to clean up.

However, while the preventive paradigm is acknowledged in principle, its practice is extremely tenuous, not least because we cannot know definitively what is an adequate level of investment in technological or social change to prevent environmental harm. The preventive approach requires attention to be shifted, from 'end-of-pipe' to 'upstream' decisions about industrial processes, product-design, and R&D strategies. Inevitably, this means finding criteria to determine decisions affecting environmental loads, at a point much further removed than conventional pollution control is from the point of immediate environmental discharge, thus from the point(s) of identification of environmental effects.

The usual technical approach to clean production poses the general question, how can we improve the efficiency of industrial processes in terms of resource use and waste outputs? A more difficult broader question is whether environmentally sustainable futures are feasible even if we assume the most efficient systems of production to be universally in place tomorrow. Might not growing consumption and production simply swallow up the advances provided by those imagined technical utopias? It is striking how effectively environmental policy discourses manage to insulate the technical focus on clean production from the equally material social dimensions of ever-increasing resource-use and waste (including discarded product) output.

How do we provide authoritative knowledge for defining how far we need to enforce greater process efficiency and product-redesign (in both resource-use and waste-outputs), let alone control the cultural processes

0959-3780/92/020111-17 

of production and consumption? The uncertainties which pervade attribution of environmental effects to specific environmental discharges are often large enough to sustain chronic conflict and indecision. So how can we face the even greater uncertainties which are exposed by moving attention upstream? The first need is to recognize their existence, and then to understand their complex social character, even within the domain of scientific knowledge.

The scientific burden of proof in environmental regulation has become a matter of intensifying conflict in recent years.[1] This has embodied two linked issues. First, where should that burden be located on the spectrum from complete environmental protection to waiting for obvious damage? Second, what burden can the scientific knowledge actually sustain, or be expected to sustain anyway?[2]

Clearly, shifting the locus of environmental responsibility further upstream in the industrial commitment process exposes more of the uncertainty about eventual downstream environmental effects: the uncertainty was already there, but concealed or 'black-boxed'[3] as if all the upstream system were simply a given.

This enlargement of acknowledged uncertainty is not only in scale. There are at least two fundamentally new *kinds* of uncertainty which are introduced, suggesting that established concepts of risk and uncertainty are no longer adequate.[4] These qualitative changes relate to the ways in which we think of decision making about environmental discharges and damage, and the way we think of the role of scientific authority in relation to such decisions.

In this paper I attempt to illustrate and characterize these fundamentally different kinds of uncertainty which the shift to the preventive paradigm allows us to recognize. In particular, I emphasize the key distinction between *indeterminacy* and uncertainty as conventionally described; departing from the idea that indeterminacy (when recognized at all) is simply a larger-scale uncertainty. I argue that indeterminacy underlies the construction of scientific knowledge, as well as the wider social world in which we create environmental effects. The implications of this point are developed.

One particular regulatory principle which is associated with the preventive philosophy, and which gives it practical effect, is the Precautionary Principle.[5] This was first developed in Germany as a means of justifying regulatory intervention to restrict marine pollution discharges in the absence of agreed proof of environmental harm. Despite being difficult to define in precise terms, it has been taken up in other environmental policy arenas, including even global climate change.[6] The scope of the Precautionary Principle in terms of shifting the burden of proof onto the polluter is still not clearly defined in relation to the nature of scientific proof, and to the preventive philosophy. I will argue that the precautionary approach involves much more than simply shifting the threshold of proof to a different place in the same available body of knowledge. The different social premises which that shift implies also open up the possible reshaping of the natural categories and classifications on which that scientific knowledge is constructed.

Before discussing these kinds of issue, however, it is useful to set the scene by reviewing in outline the evolution of environmental risk assessment as a framework for generating knowledge and authority for environmental decision making problems.

[1] J.R. Ravetz and S. Funtowicz, *Global Environmental Issues and the Emergence of Second-Order Science*, Council for Science and Society, London, 1990; J. Brown, ed, *Environmental Threats: Analysis, Perception, Management*, Belhaven, London, 1989.
[2] S. Jasanoff, *The Fifth Branch: Science Advisers as Policy Makers*, Harvard University Press, Cambridge, MA, 1990; R. Smith and B. Wynne, eds, *Expert Evidence: Interpreting Science in the Law*, Routledge, London and New York, 1989.
[3] M. Callon and B. Latour, 'Unscrewing the big Leviathan', in K. Knorr-Cetina and A. Cicourel, eds, *Advances in Social Theory and Methodology*, Routledge and Kegan Paul, London, 1983, pp 277–302; B. Latour, *Science in Action*, Open University Press, London, 1986.
[4] S. Funtowicz and J.R. Ravetz, *Uncertainty and Quality in Knowledge for Policy*, Kluwer, Dordrecht, 1990; Ravetz and Funtowicz, *op cit*, Ref 1.
[5] K. von Moltke, *The Vorsorgeprinzip in West German Environmental Policy*, Institute for European Environmental Policy, Bonn, London and Brussels, 1987; D. Bodansky, 'Scientific uncertainty and the Precautionary Principle', *Environment*, Vol 33, No 7, September 1991, pp 4–5, 43–44.
[6] Ministerial Declaration of the Second World Climate Conference, 7 November 1990, *Environmental Policy and Law*, Vol 20, No 6, 1990, p 220.

## Risk and reductionism

Risk assessment as a scientifically disciplined way of analysing risk and safety problems was originally developed for relatively very well structured mechanical problems, such as chemical or nuclear plants, aircraft and aerospace technologies.[7] In such systems, the technical processes and parameters are well defined, and the reliability of separate components is testable or amenable to actuarial in-service analysis. Indeed, so controlled are the parameters of such systems that risk analysis did not develop *after* design and manufacture, to try to understand the built-in risks; it was an *integral part* of design, influencing criteria and choices in normative fashion, right through the whole process. In should be noted that these systems have often shown themselves to be less well defined than analysts and designers thought, exhibiting surprising properties – such as exploding – which indicate that the system was less determined by controlling forces than the analysts recognized.[8] Nevertheless, the point remains that, relatively speaking, this original cradle of risk analysis allowed its authors to build in assumptions of well defined and deterministic processes.

These intellectual and methodological origins of risk assessment are important to recall because its role has now grown far beyond these well defined *intensive* risk systems, to badly structured *extensive* problems, such as toxic waste or pesticides, and thence to environmental systems on a global scale. For these last mentioned kinds of problem the limitations of available knowledge are potentially more serious because the system in question, not being a technological artefact, cannot be designed, manipulated and reduced to within the boundaries of existing analytical knowledge. In constructing analytic models of environmental systems, externally defined significant end-points, or pragmatic considerations, such as what can actually be measured, frequently dictate the structure of the resulting knowledge. Many important parameters have to be charted at one or more removes, via observation of *surrogate variables*. In addition, variables are often used which combine more than one parameter in complex form. Even something so apparently simple and precise as a single pH measure for a lake is, strictly speaking, such a *composite variable*, because we have to extrapolate and weight sample measurements which are always limited, into the mean value for that variable.

These practices artificially reduce uncertainties and variations, for example by the ways in which averaging, standardization, and aggregation are performed. The fact that this is necessary and justified by the need to generate knowledge does not alter the point that it imposes man-made intellectual closure around entities which are more open-ended than the resulting models suggest. Yet these intellectual routines become so familiar to practitioners that their indirect and more provisional relationship to the ultimate parameters of interest is forgotten.

The very considerable amount of scientific work which has gone into the modelling of environmental risk systems over the past few decades cannot, therefore, be taken as reassurance that even the main dimensions of environmental harm from human activities have been comprehended. To understand this requires not only intense and open examination of the scientific evidence and competing interpretations in an area of interest; it also requires reflexive learning at a deeper level, about the nature and inherent limitations in principle of that knowledge, however competently produced.

[7] H. Otway, 'Introduction', in H. Otway and M. Peltu, eds, *Risk and Regulation*, Butterworths, London, 1985.

[8] B. Wynne, 'Unruly technology: Practical rules, impractical discourses, and public understanding', *Social Studies of Science*, Vol 18, 1988, pp 147–167.

- RISK – Know the odds.
- UNCERTAINTY – Don't know the odds: may know the main parameters. May reduce uncertainty but increase ignorance.
- IGNORANCE – Don't know what we don't know. Ignorance increases with increased commitments based on given knowledge.
- INDETERMINACY – Causal chains or networks open.

**Figure 1.** Different kinds of uncertainty.

Key distinctions for this task can be seen by reference to Figure 1.

In the first place, we can talk authentically about *risk* when the system behaviour is basically well known, and chances of different outcomes can be defined and quantified by structured analysis of mechanisms and probabilities.

Second, if we know the important system parameters but not the probability distributions, we can talk in terms of *uncertainties*. There are several sophisticated methods for estimating them and their effects on outcomes. These uncertainties are recognized, and explicitly included in analysis.

Third, a far more difficult problem is *ignorance*,[9] which by definition escapes recognition. This is not so much a characteristic of knowledge itself as of the linkages between knowledge and commitments based on it – in effect, bets (technological, social, economic) on the completeness and validity of that knowledge.

Since this third distinction is conceptually more elusive, an example is justified. In the aftermath of the Chernobyl nuclear acccident, in May 1986 a radioactive cloud passed over the UK. Heavy thunderstorms rained out radiocaesium deposits over upland areas, and, despite reassurances that there would be no lasting effects of the radioactive cloud, six weeks after the accident a sudden ban on hill sheep sales and slaughter was announced. Although this ban was expected to last only three weeks, because the radiocaesium was thought to be chemically immobilized in the soil once washed off vegetation, some hill farms in these areas of Cumbria and North Wales in particular, are still restricted six years later.[10] The scientists made a spectacular mistake in predicting the behaviour of radiocaesium in the environment of interest. It was gradually learned that the reason for the mistake was that the original prediction had been based on the observed behaviour of caesium in alkaline clay soils, whereas those of the areas in question were acid peaty soils. It was assumed by the scientists – wrongly as it turned out – that the previously observed behaviour also prevailed in the conditions which existed in the hill areas. Thus, contrary to the confident expectations of the scientists, the elevated levels of radiocaesium in the sheep from these upland areas did not fall, and restrictions had to be extended indefinitely, severely damaging the credibility of the scientists and institutions concerned. Eventually it was realized that the chemical immobilization which had been assumed took place only in aluminosilicate clays, and that in the upland peaty acid soils caesium remains chemically mobile, hence available for root uptake and recycle via edible vegetation back into the food chain.

[9] Ravetz and Funtowicz, *op cit*, Refs 1 and 4.

[10] B. Wynne, 'Sheepfarming after Chernobyl: a case study in communicating scientific information', *Environment*, Vol 31, No 2, March 1989, pp 10–15, 33–39.

It is important to recognize that this highly public scientific mistake actually followed normal scientific practice. Scientists attempted to predict the behaviour of an agent (here radiocaesium) by extrapolating from its observed behaviour under certain conditions, making some inadvertent assumptions about the new conditions. When the new observations did not fit with expected behaviour, the models underlying the predictions were (eventually) re-examined. Through this, certain previously unnoticed but significant differences were identified, and the models were elaborated accordingly.

Had this whole process taken place in the seclusion of the professional community of research scientists, it would have been wholly unremarkable (unless some scientist or another had been too committed to a particular model, in which case a dispute might have erupted, or a reputation could have been tarnished). The point is that scientific knowledge proceeds by *exogenizing* some significant uncertainties, which thus become invisible to it: as Kuhn noted, this is not a pathology of science but a necessary feature of structured investigation.[11] The built-in ignorance of science towards its own limiting commitments and assumptions is a problem only when external commitments are built on it as if such intrinsic limitations did not exist,[12] as happened when scientists and government officials pronounced in June 1986, on the basis of then-sovereign models, that radiocaesium levels would come down within a few weeks.

The above example underlines an important general point about scientific knowledge in public, and one not usually understood. The conventional view is that scientific knowledge and method enthusiastically *embrace* uncertainties and exhaustively pursue them. This is seriously misleading. It is more accurate to say that scientific knowledge gives prominence to a *restricted agenda of defined uncertainties* – ones that are tractable – leaving invisible a range of other uncertainties, especially about the boundary conditions of applicability of the existing framework of knowledge to new situations.

Thus ignorance is endemic to scientific knowledge, which has to reduce the framework of the known to that which is amenable to its own parochial methods and models. This only becomes a problem when (as is usual) scientific knowledge is misunderstood and is institutionalized in policy making as if this condition did not pervade all competent scientific knowledge.[13] This institutionalized exaggeration of the scope and power of scientific knowledge creates a vacuum in which should exist a vital social discourse about the conditions and boundaries of scientific knowledge in relation to moral and social knowledge.

As the later example demonstrates, social commitments are necessary to define the boundaries of, and to give coherence to, scientific knowledge – not only in the large but in quite specific ways. Whenever events expose the ignorance which always underlies scientific models used in public policy, the dominant response is invariably to focus on improving the scientific model. However, although this is important, it is not enough. A response of at least equal importance ought to examine critically the (often inflated) social commitments built over the existing knowledge, because it is here that ignorance and its corresponding risks are created.[14] Indeterminacy exists in the open-ended question of whether knowledge is adapted to fit the mismatched realities of application situations, or whether those (technical and social) situations are reshaped to 'validate' the knowledge.[15]

[11]T.S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago, IL, 1962.
[12]B. Wynne, 'Scientific uncertainty and environmental policy: Towards a new paradigm', paper for UN World Commission on Environment and Development, Geneva, May 1985.
[13]Y. Ezrahi, *The Descent of Icarus*, Harvard University Press, Cambridge, MA, 1990.
[14]W. Krohn and J. Weyer, 'Die Gesellschaft als Labor: Die Erzeugung Sociales Risiken durch Experimentale Forschung', *Soziale Welt*, Vol 3, 1989, pp 349–373.
[15]D. McKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*, MIT Press, Cambridge, MA, and London, 1990.

A fourth distinction, the important one between uncertainty and *indeterminacy*, will be illustrated later. Here, it is relevant simply to note that conventional risk assessment methods tend to treat all uncertainties as if they were due to incomplete definition of an essentially determinate cause–effect system. In other words, they suggest that the route to better control of risks is more intense scientific knowledge of that system, to narrow the supposed uncertainties and gain more precise definition of it.

I will show that many risk sytems embody geniune indeterminacies which are misrepresented by this approach; but I will develop the further argument that the scientific knowledge which we construct of risk and environmental systems is also pervaded by tacit social judgement which cover indeterminacies in that knowledge itself. Lack of recognition of this distorts public debate and understanding of the proper relationship between expert knowledge and public value-choices in constructing regulatory policies for sustainable environmental technologies. In particular, it limits the scope of conceivable change, including change in social identities and relationships, in response to what are called global environmental 'threats'.

Ravetz and Funtowicz, in their concept of 'second-order science', or 'post-normal science', distinguish between three types of risk science, according to two independent dimensions – the size of the decision stakes, and the scale of the system uncertainties involved in defining the risks.[16] When both are low, applied science is in order and risks are the problem. When both are middle-range, then, they say, technical consultancy is the corresponding form of knowledge and the dominant problem is uncertainty. When both are large, as they see it, uncertainty expands into ignorance and indeterminacy, requiring a new, post-normal or 'second-order' science. Rayner and O'Riordan use this classification with no significant adaptation.[17] The perspective offered here is fundamentally different from their approach; whereas that framework suggests that indeterminacy is simply a larger form of uncertainty, existing beyond the limits of 'normal' uncertainty, my perspective draws from social analysis of scientific knowledge in recognizing that there is indeterminacy underlying scientific knowledge even when 'uncertainty' is small. It is kept at bay by the interlocking social commitments and conventions which constitute scientific paradigms or technological systems.

Thus Ravetz *et al* imply that uncertainty exists on an objective scale from small (risk) to large (ignorance), whereas I would see risk, uncertainty, ignorance and indeterminacy as overlaid one on the other, being expressed depending on the scale of the social commitments ('decision stakes') which are bet on the knowledge being correct. Science can define a risk, or uncertainties, only by artificially 'freezing' a surrounding context which may or may not be this way in real-life situations. The resultant knowledge is therefore *conditional* knowledge, depending on whether these pre-analytical assumptions might turn out to be valid. But this question is indeterminate – for example, will the high quality of maintenance, inspection, operation, etc, of a risky technology be sustained in future, multiplied over replications, possibly many all over the world?

Hence the indeterminacy is embedded *within* the risk or uncertainty definition, not an extension in scale on the same dimension. As I will show, it pervades even apparently purely technical questions. It is the

[16] Ravetz and Funtowicz, *op cit*, Ref 4.
[17] T. O'Riordan and S. Rayner, 'Risk management for global environmental change', *Global Environmental Change*, Vol 1, No 2, March 1991, pp 91–108.

*unconditional* character that is artificially lent to knowledge which obscures its indeterminacy when applied to new situations. Risk, uncertainty, and indeterminacy are therefore not on the same dimension in the way that the characterization of Ravetz and Funtowicz suggests they are. Nor, in my perspective, can the 'decision stakes' and the 'uncertainties' be independent of one-another. Indeed, just what the 'decision stakes' are in any case is also indeterminate, and conditional.

To appreciate the full extent of our human responsibilities as they shape the basis of policy options requires us to examine more thoroughly the nature of indeterminacy in the systems we are engaged in changing through our human commitments and activities. This, in turn, requires us to re-exhume and explore the more subtle indeterminacies buried (sometimes as forms of self-confirmation) in our natural knowledge of those systems.

## Upstream decisions about environmental effects

The shift of attention upstream has at least two regular implications for the way we think about regulatory policies and processes.

First, explicit responsibility shifts more to the internal processes of industrial R&D, design and production, which introduce a range of complex organizational factors to do with how this behaviour is influenced. It is currently unclear to what extent it should be conceived as a self-contained process subject to external regulatory signals, or as an open-learning system within and between organizations, and in which new understandings and practical environmental criteria may become 'organically' embedded. Most of the research literature and policy thinking about regulation and environmental policy is framed in the former terms.[18] This conventional thinking tends to 'black-box' industrial decision processes and technology generally. To treat upstream challenges, new conceptual approaches are needed which are rooted in the guts of industrial–organizational processes of negotiation and commitment, with a fuller sense of both their constraints and flexibilities.[19]

Second, as the centre of gravity for analysis and decision moves further upstream and more distant from environmental effects, greater levels of uncertainty are obviously exposed in the investigation of possible causal links between decisions and environmental consequences. Less obvious, however, is that new *types* of uncertainty are exposed. This is most easily seen by referring to Figure 2, in which various stages of decision from upstream to diverse eventual environmental discharges are schematically portrayed.

The key point is that in trying to draw causal connections between an upstream decision option and downstream consequences of that option, the intervening uncertainties are better characterized as *indeterminacies*. They are not merely lack of definition in a determinate cause–effect system; the relationship between upstream commitments and downstream outcomes is a combination of genuine constraints which are laid down in determinate fashion, and real open-endedness in the sense that outcomes depend on how intermediate actors will behave. These intermediate actors include managers and workers such as plant operators, waste-traders and other commercial agents, consumers, inspectors and other local regulatory actors; in any given case, the significant actors and relationships, and hence the variables affecting environmental outcomes, will be different depending on the system of production,

[18] J. Schot, 'Constructive technology assessment: The case of Clean Technology', *Science, Technology and Human Values*, Vol 20, 1992, forthcoming.
[19] L.G. Zucker, 'Production of trust: institutional sources of economic structure, 1840–1920', *Research in Organisational Behaviour*, Vol 8, 1986, pp 53–111.

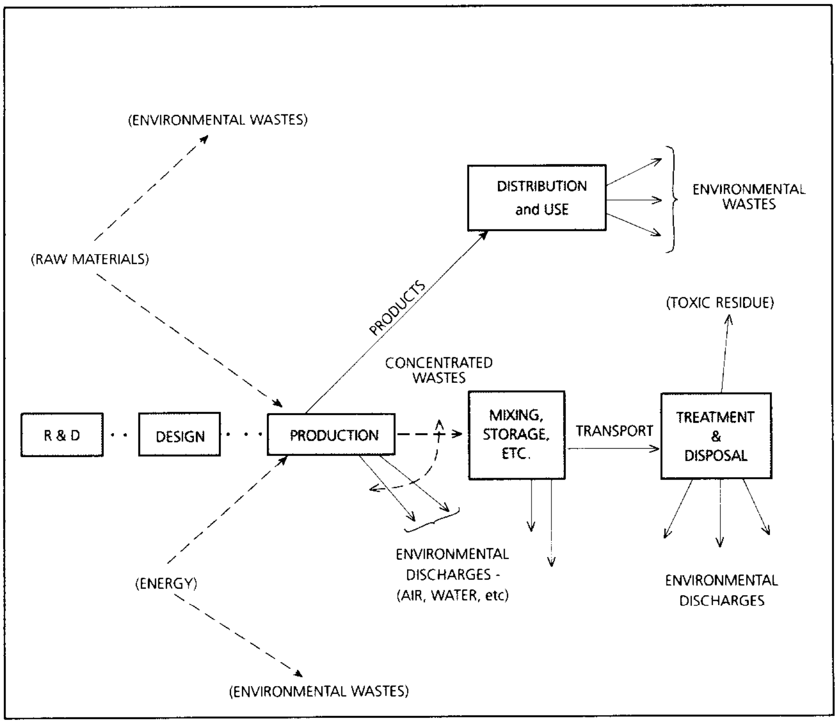


**Figure 2.** Production – waste–environment system (schematic).

waste generation and disposal, and the regulatory system in question.

By way of illustration, an industrial process generating more or less the same waste streams may present markedly different downstream environmental risks in the USA and the UK, because of significant differences in the regulatory cultures of these two countries. Even within the confines of a single system this is also true.[20] Different levels of stringency of allowed discharges from point sources of air or water mean that different waste streams are produced in concentrated form for removal, treatment and disposal. A liquid toxic waste stream, such as an inorganic acid, may under the UK regulations be legally landfill-discharged by co-disposal with municipal garbage. The US regulations rule this out. Even in the same country the same industrial process will vary in the environmental disposition of its wastes depending on many contingent factors, such as where it is produced, which company is involved, which waste disposal company (if any) it deals with, how prices for competing options are changing, and what opportunities exist for maximizing profits by exploiting recovery and recycle possibilities, or alternatively finding cheap disposal outlets in other countries with weak controls.

The distinction between uncertainty and indeterminacy is important because the former enshrines the notion that inadequate control of environmental risks is due only to *inadequate scientific knowledge*, and exclusive attention is focused on intensifying that knowledge, to render it more precise. Very often this extra *technical* precision is a surrogate for more 'precise' control of *social* actors and the indeterminacies they bear. As an aside here, existing interpretations of the potentially revolutionary Precautionary Principle do not seem basically to change this situation, since they imply placing the decision threshold further into the uncertainties, but on the assumption that this is an early-warning stance, which further scientific knowledge (less imprecision or uncertainty) would later prove correct.

[20]B. Wynne, *Risk Management and Hazardous Wastes*, Springer, London, Berlin and New York, 1987.

The extra concept of indeterminacy, therefore, introduces the idea that *contingent social behaviour* also has to be explicitly included in the analytical and prescriptive framework. (Of course, behavioural regulation is already implied in technical standards, but the full extent of contingency and indeterminacy, and the implications of this, are not recognized.) This corresponds with the distinction drawn in the risk field between *intrinsic* and *situational* risks from a given toxic waste.[21] The actual risks are a combination of the inherent properties of the chemicals composing the waste, and of the ways various people actually treat it. This contingent 'treatment' also includes how relevant commercial actors *define* the material, since they have some freedom (which varies between regulatory regimes) to define it as 'goods' not 'wastes' (for example, as raw materials for a recycling or energy plant), thus exempting it from regulation.

The type of indeterminacy so far discussed is the open-endedness in the processes of environmental damage due to human interventions. Risk frameworks have found these difficult to treat even for 'the human factor' in well defined mechanical systems. Almost by definition, analytical knowledge of risks involves the standardization of risk-situations, which implies the elaborate control and reorganization of social behaviour so as to conform with the implicit models of social behaviour embedded in the standardized analytical models. Thus an inherent contradiction exists between such standardizing tendencies and the realistic appreciation of the diverse and more open-ended situational forces and factors which defy such reductionist and deterministic treatment. The knowledge used to define risks and justify ensuing regulations is confirmed only if the social world can indeed be reorganized and controlled to reflect the assumption built into that knowledge in the first place. But if the social world does not fit, and wishes greater flexibility, it is an open question whether it should be controlled by determinate discourses from 'nature' or 'technology', or whether socially flexible technologies should be encouraged.

Thus, for example, the UK government's assertion that co-disposal of toxic wastes with municipal garbage is safe is based on studies of several landfills in the 1960s and 1970s. Embedded in that body of risk knowledge is the fact that during those studies great care was taken with respect to the management of the sites and what went into them. The risk knowledge may be valid only if that condition (*inter alia*) is fulfilled. Whether or not it is fulfilled in future cases depends on its being recognized as an indeterminacy in the system, and in the corresponding risk knowledge.[22]

## Scientific and social indeterminacy

When discussing the burden of proof for environmental decisions it is mistaken to assume that there is an objective level of uncertainty intrinsic to any piece of scientific knowledge at its current state of refinement. The level of recognized uncertainty is itself a function of the perhaps subconscious perceptions of the role(s) of that knowledge.[23]

An illustration is appropriate here. The scientific uncertainties about what happens chemically, physically and biologically in a landfill site are huge, and the opportunities for examining and reducing them extremely limited. Thus the effects of putting a given waste into a site can only be approximately known; and these effects are not in any case determinate,

[21] *Ibid.*

[22] *Ibid*; B. Wynne, 'Frameworks of rationality in risk management', in J. Brown, ed, *Environmental Threats: Analysis, Perception, Management*, Belhaven, London, 1989.

[23] H. Collins, *Changing Order*, Sage, London and Beverley Hills, CA, 1985; B. Latour and S. Woolgar, *Laboratory Life*, Sage, London and Beverley Hills, CA, 1979.

but depend (*inter alia*) on how the site is operated and managed. At which site a waste ends up, and in what condition, also depends on many social unknowns and contingencies. In the US political culture, the scientific uncertainties about what happens to a waste in landfills would be a hostage to fortune for regulators, who would have opponents exposing those uncertainties to insist that landfill was hazardous, that it was irresponsible to sanction it when its safety was so uncertain, and that it should be banned. Thus the *social threat* which exists in the extremely conflictual, mistrustful and adversary US regulatory culture, causes a scientific uncertainty to be *accentuated*. The social threat was avoided by the US decision to phase out landfill of toxic wastes. Uncertainty underlying decisions is a social risk because of the *institutionalized mistrust* which pervades the US system. Social discretion is not regarded as an asset, unless of course one can monopolize it.

In the UK political culture, on the other hand, the official attitude towards the same scientific uncertainties has been far more relaxed. The response has been that if things are uncertain they could therefore turn out better – there is no reason to assume the worst. For example, natural bacterial processes in the landfill may detoxify some chemicals so reducing the environmental risks; and if the risks depend upon sound operation and diligent waste handling, optimistic assumptions may be made unless strong evidence to the contrary exists. The point is that this official position has (at least until recently) been possible because the more consensual – and some would say complacent – UK political culture of environmental regulation has not experienced any social threats from opponents exploiting the technical uncertainties which underlie such environmental policy decisions.

Thus uncertainties in the scientific knowledge for environmental protection decisions cannot be properly described as objective shortfalls of knowledge, as most treatments suppose.[24] The extent of uncertainty seen in the scientific knowledge base is itself a subjective function of complex social and cultural factors. Scientific uncertainty can be enlarged by social uncertainties in the context of its practical interpretation, and it can be reduced by opposite social forces.[25]

I would like to give a more detailed example of the deep ways in which indeterminacies pervade the technical structure of scientific knowledge, before attempting to discuss the implications of this for the definition of environmental criteria. It is again drawn from the post-Chernobyl radioactive contamination issue in the UK, but from another aspect of the whole episode.

When monitoring was carried out after the Chernobyl radioactive fallout over the UK, high levels of radiocaesium were discovered. Against scientific predictions, these were found to persist in the fells and hill sheep of Cumbria, downwind and near to the Sellafield nuclear reprocessing plant. People soon began to question whether the government and its scientists had not secretly known all along that there was radioactive contamination in this area dating from well before the Chernobyl accident, either from Sellafield's routine emissions, from the 1957 Windscale reactor accident on the same site, or from atmospheric nuclear weapons testing, or from some combination of these.

Thus the question 'When did they know?' about the long duration of contamination of these hill soils and vegetation with radiocaesium became a highly charged one. Environmental groups critical of government secrecy argued that its scientists had known since the early 1960s

[24] UN Conference on Environment and Development, Preparatory Scientific Meeting, Ministerial Declaration of Action for a Common Future, UN Doc A/CONF 151/PC/10, 6 August 1990, Bergen, Norway.

[25] N. Gilbert and M. Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*, Cambridge University Press, Cambridge, 1985.

that in acid peaty soils radiocaesium persisted and remained available, unlike its behaviour in alkaline clay soils.[26] They pointed to a paper published in *Nature* in 1964 by a team from the Harwell nuclear research establishment as evidence that the scientists had known all along that the radiocaesium was mobile, and would recycle into vegetation from these acid soils.[27]

The *Nature* paper reported measurements of the depth profiles of given surface deposits of radiocaesium after yearly intervals from deposition up to 4.8 years, in six different soil types, including alkaline clays and acid organic peats. Contrary to the assertions of environmentalist critics, it did not conclude that the behaviour of radiocaesium in terms of its depth distribution with time was any different between these soils. Thus it was arguable that the false scientific prediction that high levels of radiocaesium would soon disappear in sheep was based on an innocent, if mistaken, extrapolation from observed behaviour in lowland clay soils to the (peaty) Cumbrian fells. However, further insights can be gained if we look more closely at the research and its relationship to the situation confronted in the post-Chernobyl emergency in the hill-farming areas.

The Harwell measurements of radiocaesium in the different soils were physical depth measurements. The authors observed that the mean depth from several measurements at each interval in each soil type showed no significant differences among the different soils. The only difference was that the peaty soils showed a wider range of variance, but the mean was assumed to be the significant value, and this was the same. Thus in terms of mean physical depth of radiocaesium as the key parameter, these soils were *the same*. On this basis, the mistaken extrapolation on which the false predictions were founded could be said to have been reasonable, and the conclusion reached that the scientists had been wrong, but not conspiratorial – cognitively deficient but at least not morally so.

However, this approach, reasonable as far as it goes, omits a further interesting dimension. The 1964 *Nature* paper was clearly premised on the assumption that the physical depth distribution with time was the main, indeed the only, parameter of interest. This corresponded with the assumption that the significant risks from such deposits of radiocaesium were from an external gamma radiation dose to a person standing on the surface. This kind of dose would be affected mainly by the physical depth-distribution of the radiocaesium. Yet in the post-Chernobyl crisis a completely different exposure pathway became the focus of concern; namely, the contamination of grazing sheep and subsequently of humans who ate them. On this different model of the risk-situation the central factor was the root uptake of caesium from soil into vegetation, and this depended on its chemical mobility as well as its physical disposition.

In terms of the chemical mobility parameter, the acid peaty soils and alkaline clay soils turned out to be very different, since in the former caesium remains chemically free and mobile, whereas in the latter it adsorbs onto the aluminosilicate molecules of the clays and is thus immobilized except for the relatively much slower processes of physical leaching of host particles. These chemical differences could indeed explain the wider range of variance (observed but not explored in the *Nature* paper) among the measurements in the peaty soil samples.

The example is outlined schematically in Figure 3. On the basis of a

[26]For example, Jean Emergy (McSorley) of Cumbrians opposed to a Radioactive Environment (CORE), personal communications, during late 1986 and 1987.

[27]H.J. Gale, D.L. Humphreys and E.M. Fisher, 'Weathering of Caesium–137 in soils', *Nature*, No 4916, 18 January 1964, pp 257–261.

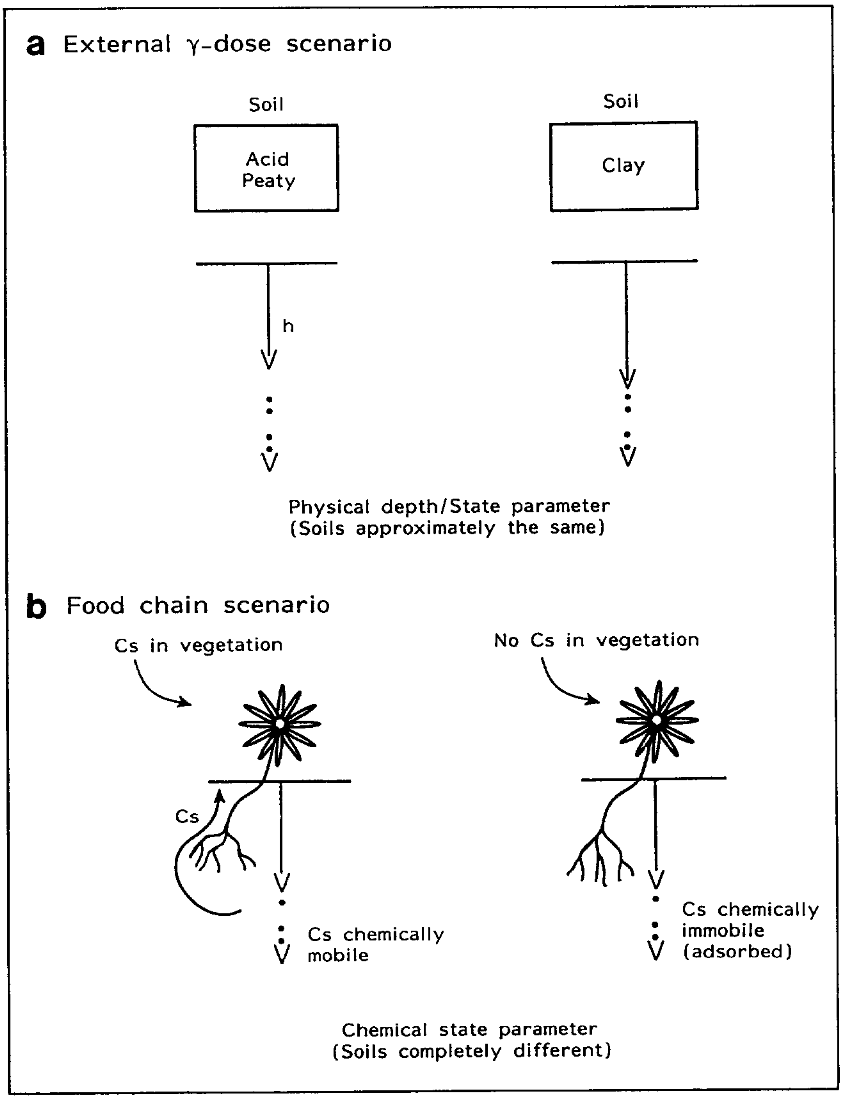


**Figure 3.** Exposure-scenario dependency of 'natural' scientific categories: (a) external physical γ-dose; (b) food-chain chemical–biological pathway.

taken-for-granted social scenario of external gamma exposure as the controlling set of behavioural factors, the scientific knowledge about soils and radiocaesium was constructed on the basis of physical depth measurements, and chemical parameters were not considered. On this taken-for-granted basis, the soils were found to be the same. Yet this was not the only scientific way of defining the question. On the basis of the exposure scenario which unfolded after Chernobyl, the chemical availability of radiocaesium for vegetation uptake became central, without any scientist apparently realizing it at the time. As gradually became clear, on these grounds the soils behaved differently. Sameness had switched to difference, within the same set of scientific observations. The very logic of science had been transformed, not by any new data, but by seeing from a different external perspective, namely a different scenario of human exposure.

This example illustrates how the detailed technical construction of scientific logics about environmental risks is not completely determined by the evidence from nature alone, but is partly open-ended depending on what parameters are treated as the most significant. As several authors have shown in the sociology of scientific knowledge, the construction of 'natural' classes of sameness and difference relations, is never completely determined only by nature, but is open to social commitment.[28] Usually, as with the *Nature* paper, such commitments are made without their authors realizing they have effectively made such choices: they are simply part of the culture of the scientific research specialty. Yet the scientific knowledge is not fully determined by 'the facts' – what 'the facts' are has to be actively read into nature to some extent. In other words, social mechanisms of closure around particular logical constructions have to occur in order to complete the otherwise incomplete logical construction. This is a further, more subtle and pervasive sense in which indeterminacies exist in the basis of authoritative natural knowledge about environmental risks.

The implication is that shifting an external policy-driven criterion, such as burden of proof of 'damage', may involve reconstructing the 'natural' architecture of environmental knowledge in reflection of those new moral commitments and identities, not leaving it immune from those currents, as if independently determined.

## Retrieving indeterminacy

I have aimed in this paper to identify less obvious issues for risk assessment and regulatory knowledge exposed by the policy shift towards preventive, or upstream strategies for integrating environmental criteria into decision making.

My main argument, that moving attention upstream exposes not just more uncertainty, but fundamentally different *kinds* of uncertainty, especially social indeterminacies even within scientific knowledge, could be used as an argument against upstream regulatory strategies on the grounds of their non-feasibility. My argument seems to imply that we cannot ever expect to find criteria for reasonable decision making of this kind. However, this misses the main point, which is to treat ignorance and indeterminacy more seriously as potential sources of risk in themselves, and to embrace them in a broader debate about the implications of societal commitment to such production processes.

The policy language of risk, as Donald Schon has noted,[29] falsely reduces the full range of uncertainties to the more comforting illusion of controllable, probabilistic but determinisitic processes. This conceals the dimension of ignorance behind practical policy and technological commitments based on a given body of scientific knowledge. It thus obscures further important questions about the decreasing margins for error, and the social control and manipulation involved in such commitments (in the form of technologies and environmental interventions) become more intensive and extensive in several dimensions at the same time. Thus, in another sense also, scientific uncertainty can be seen to be important not in itself, supposedly measurable on some objective scale, but as a function of (in relation to) the extent of technological or policy commitment riding on the body of knowledge concerned. As such commitments grow larger, we can *tolerate less uncertainty*, ironically as we discover more; the error costs rise alarmingly. Yet conventional risk science is unable to help illuminate these, what we might call 'second-order risks', which incorporate institutional demeanour and forms of social control, among other things. They need to be explicitly included in social and policy debate, but this requires a basic reconceptualization

[28]For example, J. Law and P. Lodge, *Science for Social Scientists*, MacMillan, London, 1984; T. Pinch, *Confronting Nature: the Sociology of Solar Neutrino Detection*, Reidel, Dordrecht, 1986; D. Bloor, *Knowledge and Social Imagery*, Routlege and Kegan Paul, London, 1976; S.L. Star, 'Scientific work and uncertainty', *Social Studies of Science*, Vol 15, 1985, pp 391–427.
[29]D.A. Schon, 'Risk and uncertainty', reprinted in S.B. Barnes and D.O. Edge, eds, *Science in Context*, Open University Press, London, 1982.

of the relationships between social commitments, moral identities and 'natural' knowledge.[30]

The above could be seen as a philosophical argument for the more radical version of the Precautionary Principle. However, even precaution involves uncertainties and risks.[31] Thus, as defenders of environmental discharges are fond of saying, if we ban production which cannot meet the zero-discharge standards of strict Precautionary Principle advocates, what happens if we cannot feed people as a result? Long before that, it would seem, consumers would be marching for pollution.

Bodansky, for example,[32] argues that the Precautionary Principle would not have captured CFCs, nor DDT, since the existing uncertainty along which the question of scientific proof for regulation was stretched was in each case the wrong question altogether, as we now know. For DDT, uncertainties were recognized only over acute toxicity; chronic toxicity was not even conceived of. For CFCs, the very property thought to bring low risk to biological species, long-term stability, meant it could reach the stratospheric ozone layer – but this was not even considered at that time. However, to conclude against precaution on this basis is to assume only a limited version of uncertainty.

It we take the indeterminacy point seriously, we do not know how far new technologies and social practices can be developed in order to meet new constraints of sustainability, and new opportunities for conviviality. The most important need is surely to develop 'regulatory' cultures which successfully encourage greater public debate on the social benefits, costs and indeterminacies of different products and processes, as well as on conventional environmental strategy questions. This will also mean exposure of and debate on the conditional social assumptions framing, and embedded in, 'natural' knowledges of environmental risks. Only this more rounded approach to the environmental assessment problem can offer the possibility of overcoming what is otherwise a fundamental limitation of the risk-science paradigm, which is its intrinsic inability to recognize ignorance and, thus, second-order risk, underlying present technological commitments and trajectories.

Indeed the dominant risk-science approach is more than a method; it is a misbegotten *culture* which inadvertently but actively conceals that ignorance. It thus blinds us to these more substantial kinds of ignorance and associated risk until they are upon us, and we are forced into remedial modes of operation yet again. Thus we cannot sustain a preventive approach without the reconceptualization which places scientific knowledge within the explicitly social, moral and cultural perspective I have outlined.

All this is relevant to the central issue in criteria for clean production decisions, because it should influence how we treat the issue of scientific burden of proof. With the advent of the Precautionary Principle, the burden of proof appears to have shifted, but some more basic expectations still remain. Thus, for example, most formulations of the Precautionary Principle (certainly in the UK) accept the need to stop a discharge in the absence of full scientific proof of harm, if it is *reasonably anticipated* to be irreversibly harmful.[33] However, this still suggests that scientific proof is expected soon for such decisions, which is a limited and mistaken way to view the problem. This conventional view appears to hold that the body of scientific knowledge remains qualitatively the same, while the threshold of acceptable risk is simply moved across the body of knowledge to a different position within that

[30] R. Grove-White, 'The emerging shape of environmental conflict in the 1990s', *RSA Journal*, June 1991, pp 43–51; B. Wynne, 'Risk and social learning: Reification to engagement', in S. Krimsky and D. Golding, eds, *Theories of Risk*, Praeger, New York, forthcoming 1992.
[31] Bodansky, *op cit*, Ref 5.
[32] *Ibid.*
[33] T. Jackson, 'Who needs principles?', background paper to Conference on 'Clean Production: Scientific and Policy Principles', Stockholm Environment Institute, Stockholm, 17–18 April 1991; see also the UK Government White Paper, *This Common Inheritance: Britain's Environmental Strategy*, HMSO, London, 1990.

knowledge – as it were, nearer to technology and further from nature (hence further into the uncertainties which exist, *pace* Figure 2).

However, the point of the example drawn from radiocaesium-soil research knowledge was to indicate 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 change. The 'external' normative choices also influence the 'internal' choices of 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 assimilative capacity approaches to marine pollution regulation, and, on the other, that underpinning the Precautionary Principle.

Dethlefsen 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 Möller from Germany, living on the other side of the North Sea'.[34] 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 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*, the normative choices about how strictly to regulate polluting activities. Thus normative responsibilities and commitments are concealed in the 'natural' discourse of the science, indicating the *fundamentally* negotiable definition of the boundary between science and policy.[35] The full range of moral and social issues at stake is *not* adequately described by leaving the 'factual' scientific realm as if it is a separate black box from the normative. It already reflects and reinforces tacit normative boundaries and constraints.

The precautionary scientific idiom from east of the North Sea is much more ready to accept:

- That *composite* variables, such as 'immunocompetence', 'disease' and 'stress', are legitimate components of scientific reasoning. Sindermann identified eighteen different factors, some natural, some anthropogenic, which might singly or combined result in stress;[36] and 'disease therefore has to be understood to be an unspecified response towards all kinds of stress'.[37] 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.
- The scientific legitimacy of *indirect* cause–effect inferences. For example, Dethlefsen reports a study of the possible correlation between diseases and marine contamination.[38] Although no direct correlation was found, bacterial levels in the blood of eels from a contaminated area of the North Sea averaged 80%, compared to 4% in eels from a relatively uncontaminated reference area. This

[34] V. Dethlefsen, 'Assessment of data on fish diseases', in P. Newman and A. Agg, eds, *Environmental Protection of the North Sea*, Heinemann, London, pp 276–285.
[35] S. Jasanoff, 'Contested boundaries in policy-relevant science', *Social Studies of Science*, Vol 16, 1986, pp 273–296.
[36] C.J. Sindermann, 'Fish and environmental impacts', *Archiven die Fische Wissenschaft*, Vol 35, No 1, 1984, pp 125–160.
[37] Dethlefsen, *op cit*, Ref 34, p 276.
[38] *Ibid*.

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 natural factor. Focusing on single-variable direct-cause explanation would in such a case completely miss the damaging role of pollution.

- 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 illustrates. The conventional assimilative capacity scientific idiom of marine pollution[39] appears on the face of it to avoid circumstantial reasoning by its reductionist epistemology; but, for example, in the putative connection between fish disease and contamination it defined the observation of high levels of disease away from inshore waters as an anomaly sufficient to 'disprove' the connection, on the general assumption that such offshore water were less contaminated. This conclusion was drawn as a sound scientific fact before measurements which showed the 'offshore is cleaner' assumption to be wrong, at least in this case. The application by assimilative capacity scientists of the general assumption to the specific case was just as much *circumstantial* reasoning as the explicitly accepted circumstantial reasoning sometimes adopted by the 'precautionary' idiom.

## Conclusions

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 past twenty years or so, that many of the intellectual commitments which constitute that knowledge are not completely validated, not fully determined by empirical nature.[40] 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 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 judgments of a relatively private research community which create closure and 'natural validation' around particular constructions of specialty scientific knowledge, need to be reopened

[39] T. Jackson and P. Taylor, 'The Precautionary Principle and the prevention of marine pollution', *Chemistry and Ecology*, special issue on the 1st International Ocean Pollution Symposium, Vol 21, 1991; J. Campbell, 'Assimilative Capacity', paper to 'Conference on Clean Production: Scientific and Policy Principles', Stockholm Environment Institute, Stockholm, 17–18 April 1991.
[40] Collins, *op cit*, Ref 23; Latour and Woolgar, *op cit*, Ref 23; Law and Lodge, *op cit*, Ref 28.

(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. Unless this element of openness in scientific knowledge can be recognized, we will not be able to see the extent to which existing scientific knowledge 'naturalizes' and limits our moral, cultural and policy horizons.

In this paper, I have developed the argument that the relationship of knowledge to the world of policy is fundamentally different from dominant notions. Scientific knowledge (in this respect, like any other knowledge) is generated in relation to social worlds, and its validity or invalidity depends not only on its degree of fit with nature (which is negotiable), but also on its correspondence with the social world. To achieve validation, for example in environmental policy, the institutions involved therefore need to control the social world to correspond with that knowledge, as the symbolic currency of their authority. This means *restricting* and controlling the indeterminacies emphasized in this paper, and which are concealed in scientific discourses.

The proper scope and basis of this attempted restriction should be debated and negotiated in public, as part of environmental policy discourse. This would include our complicity in one of the main fields of such restriction, namely the taking-for-granted of production–consumption technological and cultural systems as determinate and closed, with only marginal room for adjustment. Such a debate cannot be fostered while these dimensions are obscured by the dominant regulatory discourses of scientific knowledge and policy.

The subtle but deep indeterminacies which pervade the constitution of scientific knowledge have a large, but ill defined domain for which society has responsibility to exercise human values and negotiate moral identities, but which has instead been unconditionally abandoned to the implicit (reductionist and instrumentalist) epistemic commitments of science.

We cannot, therefore, expect to leave the responsibility for defining the criteria of clean technology to environmental science and risk assessment, nor to any such technical disciplines alone. Nor can we even expect them objectively to discover the different risks and benefits, for policy institutions then to exercise societal values and choices. The natural knowledge which those disciplines generate is already partly a reflection of tacit dominant cultural values and identities, ones which may be part of the problem. But this reflection is distorted by the discourse of objective natural determination in which knowledge and persuasion are couched. To confront fully the issues of values and policies will therefore require willingness to wrest open the scientific black boxes and consider their internal reconstruction. The preventive paradigm for environmentally sustainable technology is opening up a more radical shift in our relationship with scientific knowledge, and a correspondingly more radical challenge to society, than has yet been recognized.