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## MAY THE SHEEP SAFELY GRAZE? A REFLEXIVE VIEW OF THE EXPERT-LAY KNOWLEDGE DIVIDE

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It has been a frequently remarked feature of the environmental issue that it is so strongly characterised in scientific terms, even though the modern cultural reflex to do so may be seen as part of the deeper roots of the 'environmental' problem (Yearley, 1992; Redclift and Benton, 1994). Likewise, even environmental NGOs, originally the creative cutting edge of a new cultural sensibility, are seen as selling the pass when they trade so heavily in the scientific discourse of modern officially espoused environmentalism (Jamison, this volume). It is not clear how far and in what precise ways modern environmentalism as a social movement and a historical shift of societal awareness represents a crystallisation of more complex forces and realities than straightforward physical-biological environmental threats. However, that it does so has been persuasively argued, without falling into the sterile polarisation of 'real' environmental processes versus 'unreal' social constructions (Grove-White, 1991; Wynne, forthcoming).

It is interesting to note a parallel feature of the sociological debate over the 'risk society'. Here too, Beck's (1992 [1986]) original thesis has been criticised for an overly realist account of the generation – via the growth of real risks which are now universal and unmanageable – of a new cultural consciousness which introduces modernity and its institutions to pervasive public scepticism, or 'self-refutation'. Hence the epistemologically realist underpinnings of the political edge of 'reflexive modernity'. In this light it is perhaps unsurprising that a defining feature of the sociological treatments of modernity and postmodernity, and of the risk society thesis, has been their almost exclusive focus on expert knowledge. For example, in his exchange with two of the most productive sociological analysts of modernity's transformations, Giddens and Beck (Beck et al., 1994: 200), Lash noted that:

Beck's and Giddens's virtual neglect of the cultural/hermeneutic sources of the late modern self entails at the same time a neglect of this crucial dimension of politics and everyday life. It means further that their conceptions of sub-politics or life-politics focus on the experts with relative neglect of the grass roots. It

means for them a concentration on the formal and institutional at the expense of the increasing proportion of social, cultural, and political interaction in our increasingly disorganised capitalist world that is going on outside of institutions.

In attempting to clarify our understanding of the transformations of modernity, different authors have developed their own ideas of reflexive processes and their consequences (Giddens, 1990, 1991; Bauman, 1991; Beck 1992, and this volume). These bear different relationships to the concept of risk, and imply different characteristics of scientific knowledge or expert systems, which are agreed to be central to those transformation processes. As implied by Lash, a major dimension of these transformation processes is excluded, or at least dealt with only in unsatisfactory ways. This is the 'grass-roots' or lay public dimension.

However, I will argue that this is far more than merely an omission, because it implicitly reproduces just those fundamental dichotomies which are key parts of the problem of modernity: natural knowledge versus 'social' knowledge, nature versus society, expert versus lay knowledge. It also reflects – and reinforces – a more basic lack of recognition of the cultural/hermeneutic character of scientific knowledge itself, as well as of social interaction and cognitive construction generally. In consequence it also radically delimits our understanding of the sense of risk which may be seen as a central element of those transformation processes. In particular, I suggest that this neglect of the cultural/hermeneutic character of modern knowledge, specifically of modern scientific knowledge itself, seriously constrains the imagination of new forms of order and of how their social legitimation may be better founded. Adopting a more social constructivist epistemology than Beck and Giddens do in their conception of scientific knowledge and of the ecological wellsprings of late modernity's pervasive risks, I also thus problematise their uncritical conception of science and knowledge *per se*. It is important to distinguish here between their recognition of the (in recent years only) *contested* nature of scientific knowledge, and their uncritical reproduction of a 'realist' concept of scientific knowledge. This realist epistemology also, I argue, gives rise to an unduly one-dimensional understanding of the underlying dynamics of the nature of 'risk' in the risk society. A more constructivist perspective on scientific knowledge also problematises their conceptions of trust, and indeed of the nature of social relations generally. Here I align partly with Lash in his suggestion that their perspectives are unduly influenced by rational-choice models of the social. This seems to be another way of saying that they have understated the importance of the cultural nature of science, and especially of the implications of fundamental indeterminacies in knowledge which a cultural perspective should be able to capture.

Again, it is not that Giddens especially ignores the lay public dimension; but in his account it is exclusively concerned with the interpersonal and intimate, and even then I agree with Lash, in an overly rational-choice way. *There is never the slightest hint that there could in the public realm be the basis of alternative forms of public knowledge, and order, from those*

given in existing forms of instrumental expertise. However, I also argue that Lash neglects the hermeneutic dimensions of science when he – rightly in my view – takes Beck and Giddens to task for overlooking the cultural/hermeneutic dimensions of (reflexive) lay responses to expert intervention and disruption of everyday life.

In this chapter I therefore wish to address the reflexive processes amongst the ‘grass-roots’ or lay publics – those who can be thought of as outside of the expert systems which in the debate so far have been the almost exclusive focus of analysis. In doing so I will also throw a different, more thoroughly cultural, light on the prevailing conceptions of the scientific or expert, and on the concept of risk itself. Although prevailing treatments do recognise reflexivity amongst lay people, this is inadequately captured by being restricted to the intimate and interpersonal.<sup>1</sup> Thus alternative, more culturally rooted and legitimate forms of collective, public knowledge – and of corresponding public order – which could arise from the informal non-expert public domain are inadvertently but still systematically suppressed. There is a wide range of suggestive fieldwork on public responses to science and expertise which is a valuable resource for clarifying the issues and possibilities here (Wynne 1994), and I will use some of this to illustrate my arguments.

In their later work on reflexive processes (Beck et al., 1994) both Beck and Giddens have begun to recognise the importance of non-expert apprehensions and responses to expert systems (whether to the culture of these in general, or to specific ones such as genetic technologies). They have thus begun to redress what was previously a lacuna, or rather perhaps a taken-for-granted issue, in accounts of the complex relationships between lay publics and experts. Beck has perhaps the least deferential and subordinated model of public groups, ‘non-experts’, as is captured in his ideas of sub-politics. Yet both these authors tend to take the public and its knowledges for granted. This appears to be a function of the fact that to these authors, it seems, defining expert knowledge, expert, and expert system is unproblematic.

This lack of reflexive attention to the question of how the category of ‘expert’ is defined and bounded is somewhat surprising given the centrality of the concept to those debates and theories. One way of interpreting this state of affairs would be troubling to the whole debate, namely that the sociological categories of modernity, postmodernity and its variants can only themselves be entertained as unambiguous and ‘real’ by trading on a realist conception of expert knowledge. This would be an implication of Latour’s more radical questioning of the very terms of this debate in *We Have Never Been Modern* (Latour, 1992). The constructivist ethos of sociology of science which has problematised what scientific knowledge means, has also suggested that there has never been any such thing as modernity in the mythic sense described in the rhetoric of scientists and the prescriptive descriptions of philosophers and epistemologists. Scientific ‘modernity’ has always been imbued with tradition, a point

recognised and developed since Kuhn (1962) and Polanyi (1958). What is more, those elements of tradition, in the sense of received authority, dogmatic commitment and ‘mechanical solidarity’, have been recognised as *essential* to scientific culture, not temporary and localised *betrayals* thereof. If the so-called essence of science is the creative social tension between ‘modernity’ (openness) and tradition (closedness), this also opens up more complex and interesting ways of conceiving of the democratic possibilities of science, and thus of the reconstruction of politics. In this reconstruction out of the same transformations that Beck and Giddens are trying to illuminate, a central issue will still be the construction and authority of universals; but these will be universals whose human basis can also be apprehended, and negotiated.

### Illusions of Trust: Recognising Dependency and Alienation

Trust has long been recognised by sociologists as an essential and somewhat taken-for-granted element of social relations (Garfinkel, 1963). In his earlier treatments of modernity, Giddens (1990) also took for granted public trust in expert systems, in which lay people simply assumed expert competence and trustworthiness. Giddens understood reflexive transformations not as being due to public questioning of expert authority but as rooted in private interpersonal responses to processes of globalisation and disembedding. Although expert systems were central to these processes, in Giddens’s account public trust in experts was not an issue, and reflexive processes were driven by private responses to the interventions by expert systems in intimate microsocial worlds.

Beck’s (1992) concept of the risk society, on the other hand, involved the central idea that modernity’s institutions were ‘self-refuting’ because they could no longer live up to their claims of managing and controlling the escalating risks from modern science and technology – hence his useful notion of focusing not on knowledge and planning, but on ignorance and the unanticipated (Beck, 1994). Implicit in Beck’s model is the proposal that lay people are losing their sense of trust in science and expertise because they feel betrayed by them. Note also for later reference that this notion of public mistrust is instrumental-calculative, implying a rationalistic and contractual model of the wellsprings of social response.

In his more recent work, Giddens (1994) has moved towards Beck’s more political framing of the processes of transformation of modernity. Here Giddens recognises, if belatedly, that expertise is contested in public. However, the way in which this contestation is conducted and, if not resolved at least contained, is through reflexive processes *conceived by Giddens in terms of rational choice*. In the face of contestation of expert claims, publics invest active trust in expert systems – that is, trust is invested in particular experts via deliberate choice between recognised alternatives. Previously it seems, in ‘simple modernity’, they simply trusted

and believed, as a taken-for-granted. This development of Giddens's view (from Giddens, 1990 and 1991, to Giddens, 1994) has been conflated somewhat with a descriptive account of historical change, from earlier 'simple' modernity in which automatic trust supposedly prevailed, to 'reflexive modernity' in which, it is claimed, actively calculated and chosen trust now has to be invested.

These basic theoretical conceptions deserve to be more carefully and critically examined. If we are indeed dealing with a proposed account of social change rather than a change of view encouraged by productive interchange with other theorists, it is first of all worth examining what the relationships of lay publics to expert systems is or was under 'simple modernity' conditions. Contrary to Giddens I argue that the supposed earlier conditions of unqualified public trust have never prevailed, and that Giddens has reproduced what is a widespread confusion between unreflexive trust, and reflexive dependency and private ambivalence (Wynne, 1987, 1992). He thus makes two mistaken and mutually reinforcing assumptions – that the earlier, ostensibly publicly uncontested status of expertise equalled public trust; and that the reflexive processes of late modernity in which expertise is widely and openly contested are a result of the choices that have to be deliberately made by people exposed as they are (on this view) to a new dimension of insecurity, namely the problematisation of (supposedly) previously unproblematic expert authority.

An analogous assumption to those of Giddens outlined above has been made about changes in public perceptions of nuclear expertise and nuclear power. The conventional wisdom amongst political and academic commentators is that opposition to nuclear power and public scepticism towards nuclear expertise only began in the 1970s, encouraged by the rise of environmentalism as a political force, and by immediate plans for rapid growth of the industry.<sup>2</sup> Before that, it is assumed, the public unambiguously believed in and trusted the experts.

Yet Welsh (Welsh, 1993, 1995; McKechnie and Welsh, 1994) has shown for example that well before the 1970s and the emergence of organised national and international political opposition to nuclear power, there was by no means a condition of automatic lay public trust in such expert systems. Well before people were confronted by nuclear experts disagreeing amongst themselves in public, they were actively challenging the expertise that they were monolithically offered as authority, and questioning their enforced dependency upon it. In the earliest years of the UK programme, Welsh found ample evidence of public mistrust and opposition to the industry, and alienation from its perceived scientific hubris. Two elements of a dominant idea are combined in the conventional wisdom which Welsh challenges, and they also shape Giddens's perspective:

- that public mistrust only follows increasingly open expert dissent and contestation. This needs revision, perhaps even complete inversion. It

- may well be that expert dissent is often only encouraged and sustained by the existence of a public backcloth of scepticism or alienation.
- that an observed lack of overt public dissent or opposition means that public trust exists. As I discuss below, public alienation from and ambivalence towards expert institutions are not necessarily manifested in behaviour or overt commitments, so that observation of no dissent cannot be taken to mean that trust exists and alienation does not.

Indirect support for the more sceptical view can be drawn from other aspects of the nuclear risk debate which indicate how easily lay public authorship of knowledge is deleted from social recognition. In the 1970s local claims were made by ordinary people living near the Sellafield nuclear reprocessing complex, that excess childhood leukemias were occurring in that area. These observations persisted despite official denials not only by the operators British Nuclear Fuels but by the public health authorities. When environmental groups heard of these informal local observations and concerns they commissioned a researcher to gather proper statistics so as to test the claims, but they were refused access to the health authority data. Eventually, the issue came to the attention of TV researchers, and a national documentary programme was eventually broadcast in 1983, essentially supporting the lay public claims. This dramatic intervention brought an immediate official inquiry of blue-ribbon experts chaired by the Department of Health's Chief Medical Officer, Sir Douglas Black. The inquiry (Black, 1984; MacGill, 1987) confirmed a persistent cluster of excess childhood leukemias in the immediate vicinity of the plant, though it was unable to attribute it to any cause. A large investment of research was initiated into such cancers and their possible causes, and a new government standing committee of experts was established to examine and report on the medical aspects of radiation in the environment.

However, the significant point underneath all this new flurry of expert attention to this risk issue was that the excess cancers around the Sellafield plant were almost routinely referred to as having been *discovered* by the Black Committee (MacGill, 1987; McSorley, 1990). The prior authorship of the ordinary non-expert lay public, which even had to endure expert denial and refusal of access to data, was thus obliterated from social discourse. It is easy to see how non-institutional forms of experience and knowledge come to be systematically deleted from recognition, and alternative collective idioms of identity and order thus pre-empted.

Parallels can be taken from history of science too. Much of the more recent work in this field has concerned itself with the public role and authority of scientific knowledge, and the co-construction of epistemic and political order (Shapin and Schaffer, 1985; Golinski, 1992; Shapin, 1994). In this work a tension has always existed between on the one hand narrative commitments which stress the influence and sometimes imply the *automatic* power of elite discourses that entwine together particular constructs of natural and social order, and on the other those which stress

the autonomy of dominated social groups. In examining the role of the discourse of Darwinian evolution in the nineteenth century workplace, Desmond (1987) counters the 'cultural dupe' model of the lay public:

Pinpointing the scientific trade that [social elites] were plying in working class markets is different from revealing the sorts of intellectual commodities that the artisans themselves were prepared to buy – or make; for we might picture the artisan-craftsmen not as passive recipients of bourgeois wisdom, but as active makers of their own intellectual worlds, their own really useful knowledge.

I suggest that Giddens's concept of simple modernity is misconceived; and further, that the way in which it is wrong pervasively affects his concept of reflexive processes and of the relationships between expert and lay knowledge more generally. The key element of this misconception is something akin to a 'cultural dupe' model of the place of lay people in relation to expert systems in the condition of (simple) modernity. Lack of overt public dissent or opposition towards expert systems is taken too easily for public trust. Yet there is ample sociological evidence supporting a different theoretical conception of this relationship, one which recognises ambivalence and also the clustered problems of agency, identity and dependency. In other words, the reality of social dependency on expert systems should not be equated with positive trust, when it could be better characterised as 'virtual' trust, or 'as-if' trust. This has radically different implications from Giddens's concept, and it problematises his idea that public responses have changed from non-reflexive to reflexive-calculative. Indeed it suggests that public relationships with expertise and its institutions has always been reflexive, though in a more thoroughly hermeneutical sense than the rational-calculative model of Giddens. The sociological work which has identified the unrecognised sense of dependency and lack of agency which pervades public experience of and relations with expert institutions also identifies the unsuspected reflexive ways in which this is manifested as lack of overt public dissent or mistrust (Michael, 1992; Wynne, 1992, 1994; Irwin and Wynne, 1995). It shows how people informally but incessantly problematise their own relationships with expertise of all kinds, as part of their negotiation of their own identities. They are aware of their dependency, and of their lack of agency even if the boundaries of this are uncertain; and awareness of these conditions occasions anxiety, a sense of risk, and an active interest in evidence, for example about the basis of their unavoidable as-if 'trust' in those experts. These lay public processes are deeply imbued with reflexivity even though no public dissent or contestation is apparent.

Different parts of the sociological evidence for the view that public responses to scientific expertise are based in a realistic lay public appreciation of and accommodation to (even enculturation of) social dependency on expert institutions, are reviewed by Wynne (1987, 1994). Cases reviewed in the earlier work showed how the social construction of responsibility by expert institutions around accidents, risks and environmental problems worked to obscure the social structure of 'effective

causes' or responsibility, so that these often appeared as Acts of God which no one could have possibly anticipated or controlled. This is closely similar to Beck's analysis of modern science's systematic denial of responsibility for creating modern risks (Beck, 1988, 1994). The expert institutions are often thus concealing their own agency and responsibility; but in doing so they are in my analysis amplifying the diffuse but powerful sense of (social) risk on the part of publics who appreciate their own dependency on expert institutions to control the (physical) risks.<sup>3</sup> As is now well recognised, the issue of the trustworthiness of controlling expert institutions is a crucial factor in affecting not just public risk perceptions *qua* perceptions or symbolic fears, but also the objective scale of the material risks. Institutions which can be seen to be reconstructing history so as to confirm their own blamelessness whilst attempting to manufacture public trust and legitimation are *prima facie* likely to be undermining public trust rather than enhancing it.

Several pieces of work indicate just how ambivalent public relationships with expertise are, and how deeply dependency relationships are enculturated into social habits and identities. Erickson's (1976) study of the 1972 Buffalo Creek dam disaster in an Appalachian mining community indicated how the community was torn apart not just by the grief inflicted on it, but by the release of a sense of chronic disaster which pre-dated the actual dam-burst and flood. This *chronic* syndrome was attributable to an unspoken but deep sense of stigma caused by the community's recognition of its complete dependence on a coal company which, they also knew, held a ruthless disregard for them. It was not at all that they had trusted the company and then been betrayed by the accident – which would be the conventional analysis consistent with the approach of Giddens. Rather, their implicit and long-standing sense of self-denigration at 'allowing' their own dependency on such an untrustworthy owner and employer had been confirmed and rendered explicit.

In another set of studies of public perception of risk information amongst residents around major hazard chemical plants in the UK, Jupp (1989) found that there were deep ambivalences about the trustworthiness of the companies. The response was given that the companies were trusted 'so long as they are well policed'. This was combined with a realistic appreciation of the public dependency on the company experts. A majority of the public ranked the company as the first source of information to which they would refer in the event of a felt need – but this was not associated with trust in the company; indeed it ranked lowest in terms of public trust, on the list of possible sources of information. It was found that the reason the companies were ranked top as a sought-after information source was not at all a reflection of trust, but was to reinforce the *prescriptive* message that industry should be made to fulfil a responsibility for providing public information, and this was one way of registering and enforcing that point. The 'obvious' reading of the sociological data would have been utterly misleading.

It is notable in respect of Giddens's view of public trust and contestation that in neither of these two cases (the latter involved research at several sites) was there any contestation of the risks, either between different experts or between experts and the public. On the face of it there was nothing but public trust, and public assimilation of expert view of the risks; but more careful inspection revealed:

- extensive realism about the existence of risks nearby – the public was not living in an illusory world of belief in a risk-free environment (Wynne, 1990);
- unsuspected levels of informal mistrust and ambivalence about the expert institutions and their public claims;
- fundamentally different reasons from those conventionally assumed, for a given public disposition towards the expert institutions (in this case, industry);
- considerable unrecognised levels of resilience and adaptability to this situation of informally acknowledged dependency and ambivalence. The residents effectively had to behave *as if* they trusted the experts because it would have been socially and psychologically unviable to do anything else when they were so dependent on them.

Thus the assumption of lay public trust in expert systems under conditions of so-called simple modernity has to be replaced by a more complex notion of this relationship, in which ambivalence is central and trust is at least heavily qualified by the experience of dependency, possible alienation, and lack of agency, though there are of course many areas of experience where relationships between experts and lay publics are well integrated and non-alienated.

A key point of my argument is that this critical analysis is not only relevant to the model of 'simple modernity', but it implies a more basic challenge to the very categories of simple and reflexive modernity as advanced by Giddens. The change from simple to reflexive modernity as conceived by Giddens cannot be correct if it starts from such a false starting point. People are already more reflexive about their relationships with expert institutions than Giddens recognises. Even when people do align their identities with those of expert bodies, and do believe and trust in them (Michael, 1992), this 'trust' is much more conditional and indeed more fragile than the notion of 'simple modernity' reflects. Lay relationships with expertise are thus routinely (if informally) more sceptical, more ambivalent and more alienated from expert institutions than is recognised in Giddens's schema. It also follows from this that he is mistaken to treat the reflexive processes (of deliberately judged and allocated trust) as brought about only by expert contestation, which then generates critical distance on the part of the lay public. It seems that there has always been more reflexive public ambivalence than this. Therefore the basis of the supposedly categorical historical transformation from simple to reflexive modernity is thrown into question.

It might well be asked, how could lay publics so successfully conceal their alienation and ambivalence, and the ferment, to which I am drawing attention, of continual reflexive self-negotiation of their relationship to expert systems? Here it is important to acknowledge the subtlety and extent of the cultural processes whereby dependency and powerlessness are rationalised into ingenious social constructions of agency and responsibility.

Erickson (1976) in his Buffalo Creek study noted how the powerless always tend to rationalise and thus consolidate their own impotence and apathy because to do otherwise is to expose themselves to the greater pain of *explicit* recognition of their own neglect and marginality. Not only withdrawal occurs, but justification of that withdrawal in *cultural* narratives, as consistent with cosmic principles. Thus beliefs about cause and effect in the experiences they encounter become integrated with their established social relationships and identities. When those relationships and experiences are highly prescribed by others, yet follow logics that are obscure and apparently capricious, this can be encapsulated and 'naturalised' in fatalistic beliefs, identities, and senses of (non-) agency. Erickson's account of the Buffalo Creek survivors' beliefs reflected this alienated human reaction, 'a sense of cultural disorientation, a feeling of powerlessness, a dulled apathy, and a generalised fear about the state of the universe'. The effective causes of their impotence and arbitrary suffering were in this case socially relatively visible (the coal owners and bosses), but the sudden dam-burst disaster was tantamount to the condensation onto a single catastrophic event of years of identity-stripping and denigration by those significant others.

A coal company and its management of a dam might be seen as a relatively transparent set of 'effective causes' of risks, with immediately identifiable lines of control and responsibility. Yet even here the impetus to wrap these up in alien and fatalistic natural idiom was overwhelmingly strong, and deeply entrenched in history. If such familiar systems as a coal company and a dam can be seen as alien and impenetrable, how much more must this be true of the complex and interconnected global systems of the modern biotechnology and information revolutions? In these the controlling human agents and relationships are far more extensive, complex, esoteric, diffuse and socially remote. It is often impossible for anyone, let alone the ordinary public encountering them, to identify or to identify with the effective causes in such socio-technological systems. Yet the pervasive and increasingly close importance of these systems requires that people construct some working rationalisations of their troubling and confusing experiences of them, even when they do not unleash dramatic interventions into their lives.

Psychiatrists have examined clinical cases involving images of technology which people have constructed and lived by. These have apparently often taken the form of spectres; that is, condensed forms of agency which short-circuit the emotionally impossible complexity of experiences of powerful

but obscure forces such as those involved in modern technology and risk. Daly (1970: 420) has defined such spectres as potent, artificially created but invisible behavioural forces:

A sense of the operation of such forces arises when men [sic] find they cannot account for emotionally significant events by ascribing them to the conventional sources of power and efficacy (e.g. human, natural, divine) which are believed to make things happen in the world. When such inexplicable events persist and are experienced by numbers of people, agencies are created to account for these events. These agencies are given names, made into realities, and adapted to as powerful things.

The spectral view of technology arises from a sense of domination by mysterious forces or agencies which are, or were, linked to technological enterprises but which are now apprehended as being beyond the control of any particular [human agency] . . . [People] behave as if the spirit of meeting specifications in many discrete, limited and finite human ventures had taken flight from the hands of responsible agents and become an independent reality – a reality which has come to overhang the modern world and to enter into the dynamic processes of personality – as a spectral object.

There is in other words a kind of defence mechanism for coping with the overwhelming difficulty of living with inexplicable and uncontrollable, yet emotionally important forces, which is to convert them into identifiable agents, even superhuman ones. A central point of Daly's analysis is that these conditions are not confined to definable individuals – they are in his view mass cultural conditions. They may also be considered to be reasonable reactions of people to irrational situations in which they have been placed by expert systems. McDermott (1974) gives an example taken from the Vietnam war, in which GIs created such a technological spectre. The US GIs were operating in the jungle, constantly sniped at and ambushed by Vietcong guerrillas who could not be identified or pinned down, but who vanished into their own terrain. The GIs were regularly shelled, shot at, mined and rocketed, but were never sure whether it was the enemy or their own side; and they received orders but never explanations from their distant superiors who knew little of their situation. Their experience was terrifying, confusing, contradictory and utterly obscure as to its effective causes and dynamics. They could not find and engage a definite enemy, and they could not identify their friends. They received orders, and were attacked, in equally arbitrary fashion, and no one could tell them what was happening and why. Their very high risks were a combination of physical and social realities.

As part of their attempted rationalisation of this frightening and disorientating predicament the GIs had come to condense the potent but diffuse and invisible effective causes of their experience onto a single symbolic agent that they had discursively created – they lived a relationship with a 'huge-fucking' gun which hid in a hollowed-out mountain and which emerged unpredictably and at whim to unleash death and destruction onto them, in a manner which authentically described their actual daily experience. Thus they had obscured and 'naturalised' the social relations in which

they were enmeshed – with the Vietcong enemy, the US war machine and Vietnamese villagers – into a technological spectre, a metaphor for the social relations they could not begin to identify and explain, but which controlled their fate in acute and menacing fashion.

This kind of enculturation process *normalises and consolidates* whatever dependency and lack of agency is thought to exist. It obscures the alienation and ambivalence or worse which people may feel in relation to elites and expert institutions. Thus it may help to explain why it is that ambivalence is apparently routinely overlooked, even though it is a widespread if not universal part of lay public relationships with expert systems. This may in turn help explain why lack of *expressed* public ambivalence or alienation is mistaken for unambiguous trust.

Contradicting the received view as reproduced in the canonical sociological treatments of modernity and its transformations, is a growing body of work which highlights the reflexive processes of lay public responses to scientific expertise, and the ways in which people construct their social identities in relation to such potent external agents which intervene so often and so multifariously in their lives. This work (for example, Michael, 1992) underlines that a reflexive public stance does not automatically mean a critical one; but equally that a deferential relationship may be based on a sense of inevitable – and perhaps socially impenetrable – dependency rather than a considered and decisive investment of trust. This in turn may engender a sense of ambivalence that is harboured in relative privacy. Furthermore, the most fundamental dimension of risk expressed in such social interactions is that of the *risk to social identity* which is felt to be involved in this kind of dependency, upon expert institutions which disseminate and impose such questionable models of the human and the social, whilst pretending to deal only with objective facts. The empirical cases described later exemplify these points.

Aided by this more multivalent reading of trust, dependency and ambivalence in lay relationships with expert systems, we can begin to develop a more thoroughgoing culturalist conceptualisation of the character of risk than that given in the risk society thesis.

### Risk: The Cultural Dimensions

Both Beck and Giddens offer a similar model of the pervasive sense of risk which they argue now grips industrial society, and which is a new logic superseding that of class conflict. The features of this model are that modern science and technology now produce truly global risks from which not even the rich and powerful can escape. In addition, according to Beck, the principle of insurance wherein risk damage could be capped and controlled, allowing the further unimpeded growth of modern science, technology and their risks, has with these new global dangers ruptured itself. As identification with existing institutional structures erodes and

individuals are increasingly thrown upon their own resources and networks for the negotiation and sustenance of identity, so modernity's quintessential institutions of technology and science self-refute their own enlightenment promises and programmes. In particular, the failure of those institutions to control the risks they have created, seen most acutely in the ecological crisis of industrial society, has generated a more profound and pervasive sense of risk. As the contradictions grow more frequent and intense, so the sense of risk grows, and the legitimacy of those institutions which have designated themselves the saviours erodes correspondingly. Giddens's account is essentially consistent with this, though he emphasises more the disruptions effected by expert systems and globalisation processes in private lives, and the transformations brought about by this. A key part of the sense of risk is, as in Beck's theory, seen to be the existential insecurity associated with the expansion of 'individual choice' and the erosion of traditional forms of existence.

Much of this account is persuasive and original. Despite this Lash observes, correctly in my view, that their basic ethos is too rooted in rational-choice neo-classical economic models of human behaviour and response – that is, in modernist concepts of the human and the social. However, Lash's criticism itself invites critical development.

The basic model of reflexivity and risks in modernity is that lay people reflect critically upon the failure of modern scientific institutions to control risks such as ecological and nuclear risks adequately. Those institutions thus contradict their own self-legitimatory promises and induce further independent critique from alternative expertise and further erosion of the cultural authority of modernity and science and its formal political institutions. Included in this model of the erosion of modernity under its own intrinsic dynamics is the claim that science and technology also regularly disrupt the familiar patterns and identities of daily life and empty it of meaning, thus further encouraging 'retreat' into informal sociations and lifestyle- or sub-politics, outside the formal sphere. As Lash (1994a) has noted, this is based on a rationalistic model of social and cultural response to the experience of science, technology and modernity. In this conception, human response is rooted in an instrumental-calculative standpoint. The modern institutions and culture have failed to live up to their promise and to deeply rooted social expectations because the risks and side-effects are now unacceptably high; so the response is to disengage from and reconstruct the prevailing institutions and political culture. Behaviour and the ensuing redefinitions of identity are driven by instrumental concern about security from ecological risks, and the failure of modern expert institutions to provide that security whilst pretending to do so.

To argue that this model is too exclusively framed by the 'realist' assumption that public responses to expert institutions are based in responses to their handling of real risks, is not to imply that there are no ecological risks, nor that people do not think instrumentally and care about

those risks. However, my point is that the same basic social dynamics in the transformations of modernity could be occurring whether or not those risks objectively exist 'out there'. It is likely therefore that their explanatory role is not as large as presently assumed.

My proposal instead is that much of the dynamics of the self-refutations of modernity is instead explicable by a more thoroughly hermeneutical perspective. Through their rationalist discourses, modern expert institutions and their 'natural' cultural responses to risks in the idiom of scientific risk management, *tacitly and furtively impose prescriptive models of the human and the social upon lay people, and these are implicitly found wanting in human terms*. This analysis connects closely with a non-realist conception of the basis of public risk perceptions argued in the risk field since 1980 (Wynne, 1980), and itself connected with the argument in the previous section about the importance of tacit dependency in the social and cognitive dynamics of risk issues.

Against the dominant idea that public risk perceptions relate to perceptions or evaluations of what is thought to be an objectively existent physical risk as the object of experience, I have argued that public perceptions of and responses to risks are rationally based in judgements of the behaviour and trustworthiness of expert institutions, namely those that are supposed to control the risky processes involved.<sup>4</sup> That is, the most germane risks are (social) *relational*. There are several parts to this thesis which are worth taking one by one:

1. Most risks are actually intellectual constructs which artificially reduce larger uncertainties to ostensibly calculable probabilities of specific harm. The tacit social assumptions which create such 'natural' frames are rarely expressed or recognised. Such expert 'natural' knowledges thus typically embody implicit models of the social and human. Risky activities are often objects of much greater commitment on the part of promoters than the scale of particular projects or plans may reflect.
2. Given these kinds of uncertainty it is rational of people not to limit themselves to assessing the magnitudes of claimed risk that exist, because such estimates will always be subject to the larger uncertainties indicated above. It is instead logical for them to ask, how trustworthy are the institutions supposedly in charge? How likely is it when faced with evidence which overturns existing understandings, or with changes of circumstances which alter the terms of the commitments involved, that they will act in a way consistent with, rather than compromising public health and environmental protection, and democratic principles of open participation?
3. As I have noted before, such institutional dimensions (trustworthiness, competence, independence, etc.) actually influence the scale of material risks anyway. If for example a regulatory body does not rigorously inspect and guarantee standards of maintenance of technical plant, the risks of physical accident will materially increase, and not

just public perceptions of the social risks of dependency upon such incompetent or non-independent institutions.

4. Thus public risk perceptions rationally involve some element of judgement both of the quality of relevant social institutions, and of their relevance, in other words of the roles of different social agents including one's own relationship to them.
5. Involved in this judgement is an assessment of the extent and implications of dependency upon those institutions, for safety or for the protection of other valued aspects of life, including valued social relationships. This returns us to the complexities of dependency and its rationalisations as briefly outlined above.

As nearly all studies of public risk perceptions and responses show, ordinary people bring more to their definitions and evaluations of risks than recognised in the reductionist framing of experts (Otway, 1992; Slovic, 1992). There are two aspects to this expert reduction: first there is the neglect of socially valued dimensions such as whether the risk is imposed unnegotiated by distant authorities, whether it is voluntarily engaged in, or whether it is reversible or irreversible. Second, there are assumptions made by the experts about the actual risk situation and the issue in hand. For example, this may be framed very narrowly if the experts take for granted the competence and trustworthiness of the controlling bodies, whereas if this is questioned the risk issue to be addressed will be wider and more indeterminate. These framing questions about what the issue is are often pre-empted by the institutionalised authority of scientific discourses which tacitly impose such evaluative social commitments embedded in the scientific knowledge, without anyone even noticing or at least being able to protest effectively that this has happened. Frequently, such framing commitments embody models of the social world and relationships of lay people which are at least open to question, but which are innocently imposed on those people as prescriptive commitments. In the course of such processes from risk analysis to regulation or policy commitment, what may have begun life as hypothetical assumptions about those social worlds (for example, whether nuclear reactor maintenance and operating personnel will always follow the rules rigorously or not) become increasingly prescriptive 'demands' to be *ordered* into existence so as to confirm the expert analysis. In this process, and well hidden in the depths of their objectivist discourse, scientists are acting in effect as naive sociologists, except that they may have the power to bring into being the implicit social assumptions or commitments that tacitly shape their knowledge.

This kind of analysis of scientific knowledge as constructed and used in risk and environmental issues opens it up to recognisable indeterminacies as to whether the controlled and artificial conditions assumed in the analytical process (perhaps the laboratory) will actually prevail in practice, so it is implied, everywhere and at all times. These indeterminacies correspond to the inevitable gap between laboratory conditions and real-world

conditions in which risks are actually experienced. The resolution of the empirical question of whether the expert knowledge is correct depends partly and significantly upon the reciprocal question: can the social conditions be brought into being and maintained so as to correspond with and confirm the underlying assumptions of the expert knowledge? Hence truth depends partly upon commitment and the open-ended issue as to whether the appropriate conditions can be organised into existence. It is in this sense too that this perspective corresponds with Latour's (1987) ideas of the coproduction of intellectual and social orders, in which it is otiose to use the one to try deterministically to explain the other, in whichever direction the causal arrow is pointed.

Thus the risks from social dependency on institutions which may be supposed to be controlling the 'direct' physical risks are a dimension of the risk society which is inadequately recognised in Beck and Giddens. These risks are in essence threats to basic social identities – threats brought about by the alien and inadequate models of human nature and human relations tacitly embodied in the objectivist expert discourses. They are threats because they come not as mere assumptions or hypotheses to be tested – and perhaps revised – in practice, but as prescriptions or forms of social control. It is important also to note that many of these threatening and alienating models of the human, which general category constitutes a key dimension of the risk society not recognised by Beck, are articulated and imposed by modern institutions supposedly advancing *solutions* to modern environmental risk problems.

The vernacular, informal knowledge which lay people may well have about the validity of expert assumptions about real-world conditions – say, about the production, use or maintenance of a technology – is also an important general category of lay knowledge that is usually systematically under-recognised. This oversight is understandable, and indeed inevitable in the absence of a constructivist conception of scientific knowledge. It is therefore not surprising that both Beck and Giddens overlook this kind of lay knowledge, and misconceive its relationships with formally defined expert knowledge, since they do not understand scientific knowledge to be intrinsically cultural. I suggested above that a general reason for possible divergence between expert and public knowledges about risks is that expert knowledge embodies social assumptions and models framing its objectivist language, and that lay people have legitimate claim to debate those assumptions. I further suggested that these assumptions are much more than that – they are incipient social prescriptions, or vehicles of particular tacit forms of social order, relationships and identities. I therefore argued that a central part of the reflexive processes of lay discomfort, alienation and distance from expert knowledges and interventions is not the purely rational-calculative one which Beck and Giddens conceive as the driving force of reflexive modernity. It is the more thoroughly hermeneutic/cultural one in which alien and inadequate tacit models of the human are imposed on lay publics through the discourse of



'objective' science in such potent fields as environmental and risk management and regulation.

Here it is worth noting that Lash does recognise, *contra* Giddens and Beck, that the responses of ordinary people to expert interventions and disruptions of their lives are cultural/hermeneutic, not just calculative-rationalist. But those expert interventions and the resulting public alienation and delegitimation are understood by all three of these authors in terms of risks 'out-there', and responses to them. In my view they should be seen more as responses to the identity-risks arising from the fundamentally impoverished and morally-emotionally threatening models of the human which are silently embodied in the objectivist science of those modernist expert institutions, ironically intervening increasingly in the name of 'public protection' from risks. Thus whereas all three authors talk of the interventions of expert systems as 'emptying their lives of meaning' (that is, the public's) I would argue that far from *emptying* indigenous lives of meaning, the expert knowledges are typically *importing dense but inadequate* meanings. Thus, contrary to Giddens and Lash, science is not meaning-free or meaning-neutral, but dripping with impoverished and expropriated meanings, and ones in which there is no longer ordinary participation and access. It is just because people know these to be indeterminate that they also intuitively recognise the depth and importance of what has been expropriated by expertise here. Just because those meanings and identities are open and indeterminate, a whole lifetime of relations and negotiation is involved; yet the interventions of expert systems would often exchange this for a one-off response – say, to a consumer survey or a contingent valuation questionnaire.

Thus essentialist and deterministic concepts, including a non-constructivist concept of science, also delete the extent of cultural devastation and moral provocation wielded by modernistic decision approaches and expert systems. To claim that science is objective propositional truth as Giddens does is to sell the pass here by thinking of science as meaning-neutral, thus only emptying lives of meaning, rather than seeing how on the contrary it *fills* them with meaning – but of a problematic and even provocative kind. It is conflict and reactions at this unarticulated hermeneutic level which create the spin off into alienation and self-refutation of modernity's institutions, and hence the growth of the informal extra-institutional sector of 'cultural politics'. It is also worth emphasising that this largely negative hermeneutic dimension must have been strongly amplified by the enhanced role which social science has played in environmental and risk policy work, for example in the huge elaboration of rational-choice economic modelling of environmental choices, of contingent valuation surveys of risk acceptability, and of social-psychological work on public risk perceptions. All of these, and others, in various ways impose individualist, instrumental, essentialist and decisionistic models of the human, in the name of 'neutral' scientific method and observation. Thus in important respects the intensity of this provocation – and the

growth of the public sense of risk – with respect to its portrayal of the human, must have increased as the role of social science has expanded in these public domains.

Thus whereas Giddens proposes that science contains propositional truths only, my perspective on environment, modernity and risk involves the recognition that it disseminates not only propositional truth claims, but formulaic and hermeneutic ones too. Sociology of science has long since left behind the simplistic rationalistic idea that scientific discourse is one-dimensional and literalistic, and has recognised that scientific discourses contain both propositional and formulaic truth claims, where formulaic means performance-related, even if they are couched overtly only in propositional terms. However, I would go one step further and suggest that scientific discourses contain not only tacit claims to performance in the sense of credibility rituals (like citation practices: Gilbert and Mulkay, 1984), but also hermeneutic truth claims. Thus whilst Giddens can only conceive of possible critical interactions between lay publics and scientific expertise on propositional truth claim grounds (and even then with the public only vicariously involved via dissenting bodies of expertise) since this is all that science supposedly purveys, I propose that critical interactions occur on hermeneutic and formulaic grounds too. This is tantamount to saying that public critique of science can be, and is, based on more than its propositional contents alone (Wynne, 1994).<sup>5</sup>

An implication of this is that the basis of lay public responses to expert knowledge is always potentially an epistemological conflict with science about the underlying assumed purposes of knowledge, or at least the scope of that epistemic remit, which is wrongly assumed to be just given in nature. This raises questions not only about the basis of the relationships between 'objective' scientific knowledge and 'subjective' lay knowledge, but about the extent to which scientific knowledge is open to substantive criticism and improvement or correction by lay people. In other words, how far might lay people be involved in shaping scientific knowledge, and thus in providing the basis of alternative forms of public knowledge that reflect and sustain different dominant conceptions of the human, and of the social purposes of public knowledge? In the dominant approaches, the answer by default is – not at all. I next examine some cases which provide suggestive if embryonic alternative answers to these questions.

#### Lay Knowledges and Alternative Orders

The predominant perspectives on the risk society and the transformations of modernity which I have discussed so far, implicitly treat the non-expert world as epistemically vacuous. It may be reflexive, but such reflexivity is implied to have little or no intellectual content in the sense of having cognitive access to nature or society. It has no apparent instrumental value as measured against a scientific world-view, so is assumed to have no real

content or authority beyond the parochial, subjective and emotional world of its carriers. This is a diminished view of both science and lay knowledge. It recognises nothing of the fluidity, porosity and constructedness of the boundaries established between them; and, as well as misconceiving the conflicts between public and scientific knowledges, it recognises nothing of the constructive kinds of interaction and mutual inspiration or dependency which may exist between them.

In research on public reception of scientific expertise in a variety of contexts, a very different view has been substantiated, with important implications (Hobart, 1993; Irwin and Wynne, 1995). It is worth examining two such case-studies.

#### *Cumbrian Sheep Farmers and Environmental Radioactivity*

Wynne (1992) studied the responses of sheep farmers in Cumbria, a mountain area of north-west England, who had been subjected to administrative restrictions on sheep movements and sales due to radioactive contamination caused by the fallout from the 1986 Chernobyl nuclear accident. The farmers interacted over some years, but most intensely for a year or two, with scientists from a variety of agencies who were responsible for official views of the behaviour of the radioactivity, and of the likely duration of the restrictions. These interactions, between a relatively well defined lay public and a particular, but fairly typical, form of expert system, illuminate several more general points about the social basis of scientific knowledge and its public credibility, and about the nature of lay knowledge.

After initial denials of any problems in the UK from the radioactive fallout, in June 1986 the UK Ministry of Agriculture imposed a sudden blanket ban on the movement and sale of sheep from defined areas in hill regions such as the Lake District of northern England. This was potentially ruinous to what was a marginal and economically fragile sector of British farming, because these farmers depended for almost all of their annual income on being able to sell a large crop of surplus lambs from midsummer onwards. Not only would their income be devastated, but they would be unable to feed this lamb flock if it was forced to remain in their hands, because these hill areas comprise mainly meagre grazing on open mountains and only very limited valley grass, which would 'become a desert in days', as one farmer put it, if the lambs were not moved on. Thus wholesale slaughter of sheep flocks was on the agenda, with long-term ruin of the hill-farming economy and its distinct culture.

These dire threats were alleviated when the Ministry announced that the ban would be only for three weeks, a period which would return the situation to normal well before the annual sales would begin. This reassuring projection was based on prevailing scientific assumptions about the behaviour of radiocaesium in the environment. According to those beliefs the fallout would be washed off the vegetation which sheep grazed,

into the soil where it would be adsorbed and 'locked up' chemically, unavailable for any further mobility and possible return to vegetation and the sheep food chain. Thus a once-through model of contamination was involved, and this meant that there would be no further dose to the sheep after the first flush. Thus measured body burdens of radiocaesium in sheep were assumed to be peak levels which would decay according to the biological half-life.<sup>6</sup> Since the biological half-life for caesium in sheep was about 20 days, it was estimated that the contamination levels would fall below levels at which action was required, within three weeks. This view was expressed by the experts with utter confidence unqualified by any hint of uncertainty.

Given the crucial importance of avoiding anything more than such short-term restrictions, the shock announcement in July 1986 that the three-week ban would have to be extended for the indefinite future was a stunning contradiction of previous reassurances and even denials of any problem at all from scientists, officials and ministers. Contrary to scientific beliefs and pronouncements, measured levels had shown no decrease, and an urgent reappraisal of existing understanding was begun. In order to allay rising fears of the imminent collapse of hill farming, the restrictions were altered so as to allow sheep from the contaminated area to be sold, so long as they were marked with a stipulated dye which marked them as unfit for human consumption. Thus they could at least be sold and moved out of the grazing-poor and contaminated hill area, even though they could not be sold for slaughter until deemed clear. It was expected that once the sheep were on uncontaminated land, this would not be long.

It is a point of general importance to observe just how completely controlled by the exercise of scientific interpretation the farmers felt themselves to be. Thus if they sold their marked sheep and avoided their overpopulation and possible starvation, they lost money badly in the markets because these sheep were blighted – indeed so were those that were unmarked but still from the affected area. This social reality was not recognised by the experts until a great deal of upset and loss of credibility had been caused amongst the farmers. Yet if they held on to the sheep, the farmers could only survive by incurring large extra imported-feed costs as well as build-up of disease and other problems. Still believing in their short-term model of the high levels of radiocaesium, the scientists continued to advise the farmers to hold on just a little longer, expecting the restrictions to be removed soon, even if later than originally thought. Caught whichever way they turned, many farmers still followed this advice despite the evidence of expert mistakes; but their hopes were dashed as the early removal of restrictions promised by the experts never materialised.

It gradually became clear that the 'three weeks only' scientific judgement which had been translated into public policy commitments and predictions had been a mistake, but this became evident only over the following years, as more research and debate ensued. The predictions of only a three-week-long problem had been based on the assumed existence of alkaline clay

soils (on which much of the original observations had been made). In such soils the behavioural properties of radiocaesium envisaged by the scientists do indeed occur. The problem was that the scientists had overlooked the essentially localised nature of this knowledge, because clay soil was not a universal condition, and in other soils such as those in the hill areas, very different behaviour prevailed. In these areas acid peaty soils predominated, and in such soils radiocaesium remains chemically mobile, hence available for root uptake from the soil, back into the vegetation which the sheep grazed. Thus, because they had assumed that the knowledge drawn from particular conditions was universal knowledge, the scientists did not understand that in these conditions the sheep were exposed to continual recontamination and hence probably much longer-term restrictions.

In the heat of the crisis over the Chernobyl accident and the restrictions, it arose as an issue whether there had been an innocent scientific mistake or a deliberate attempt to cover up knowledge of a longer-term problem, so as to avoid public reaction. Even the admission of a mistake was never made clearly and unambiguously. But in addition a further issue took off from this. At the outset of the restrictions a large area the size of the county of Cumbria was included. Within three months this had been reduced to a small crescent-shaped area in the mountains near the coast, and just downwind from the huge international Sellafield nuclear reprocessing complex. When this area persisted with high levels of contamination, against the confident predictions of the scientists, given its position various people began to ask whether the measured contamination had really been from Sellafield rather than Chernobyl, and had actually existed unnoticed or concealed by the expert authorities. Given Sellafield's notoriety as a discharger of radioactive contamination into the environment (MacGill, 1987; McSorley, 1990) and the history of the world's worst civil nuclear accident in 1957 in a reactor on this site (Arnold, 1992), this was by no means a frivolous suggestion. The 1957 fire was known to have spewed radiocaesium and other radioactive materials over this same area, and had resulted in the banning of milk sales for some weeks afterwards. Some local farmers even argued that the government and the nuclear industry had known all along but had been waiting for a convenient alibi for this environmental contamination; Chernobyl provided it.

Scientists dismissed these suggestions as unfounded, and pointed to what they regarded as unambiguous scientific proof, in the radioactive 'fingerprint' of the radiocaesium samples collected from the environment. The radiocaesium emitted from nuclear fission processes is made up of two isotopes of caesium-134 and caesium-137. The latter has a half-life of about thirty years while the former's is about one year. In nuclear fuel of a given level of burn-up the ratio of caesium-137 to caesium-134 fission products will be the same, but as they age with passage of time, the ratio increases due to the different half-lives. In fresh Chernobyl deposits it was about 2:1, whereas for typical Sellafield emissions (from reprocessed fuel often stored for many years on site before processing) or old 1957 accident

emissions, the ratio would be about 12:1. The two isotopes emit gamma radiation of different specific frequencies, thus according to the scientists there was a clear means of distinguishing the two possible sources in the radioactive fingerprints of samples. The scientists asserted – again without any hint of uncertainty – that the origin of the contamination found in the environment was Chernobyl, and not Sellafield. This did not persuade the farmers, and it is worth examining the grounds of their scepticism.

First, they had just experienced the experts as having committed a huge mistake over the predictions of contamination, having expressed that mistaken view unqualified by any sense of uncertainties, and not having admitted any mistake. In this case also there was much more uncertainty in the technical process of discriminating between different sources than the experts' confident assertions implied. Actual soil samples contained mixed deposits, so that measured isotope ratios involved combined isotope intensities and assumptions about the precise ratio from any single source. It was later admitted that sampled deposits typically contained 50% Chernobyl radiocaesium, and 50% from 'other sources', which meant Sellafield and atmospheric weapons testing fallout (Wynne, 1989, 1992). This was quite a significant move in the direction of the sceptical farmers' beliefs, from the initial expert assertion of certainty about Sellafield's innocence.

In addition to direct observation of Sellafield's position and the otherwise unexplained and unanticipated persistent crescent of contamination around it, the lay public also noted various elements of institutional 'body-language' which placed the experts' claims to credibility in question. The exaggerated certainty of official science was one element, but so too was the way that questions about environmental data from the affected area before 1986, which were designed to test the claim that the levels had not been high before that, were deflected to either data from after 1986, or data from other areas. This suggested that either there were data which showed the suspected high levels but which were being covered up, or there were no data at all, in which case there had been gross negligence considering that this was an area that had been affected by the 1957 fire. The choice of judgement of the expert authorities seemed to be either corruption or complacent incompetence.

The historical experience of secrecy and misinformation by official institutions also acted as a direct evidentiary input to the existing risk issue. Effectively recognising that they had to trust the experts and could not independently generate knowledge of the environmental hazards, the public had good reason from past experience of social relationships with what were to them the same institutions, not to invest that trust. This reinforced the evidence from the current issue. Yet feeling mistrustful of the experts, they were nevertheless realistic enough to recognise pervasive dependency on them, and often spoke and behaved *as if* they trusted them. A typical farmer's assessment<sup>7</sup> was: 'The scientists tell us it's all from

Chernobyl. You just have to believe them – if a doctor gave you a jab up the backside for a cold, you wouldn't argue with him, would you?' In other words, we might look as if we trust them, but just because we have no choice but to 'believe' them doesn't mean we don't have our own beliefs.

A further factor involved in the public's evaluation of the scientific knowledge-claims was the way in which the official experts neglected elements of the local situation, including specialist farming knowledge, which were relevant to the understanding and social management of the crisis. Thus the scientists did not understand the implications of the restrictions on hill sheep farming, and appeared not to recognise the need to learn. For example, they assumed that farmers would be able to bring sheep down from the high fells where contamination was highest, to the relatively less contaminated valley grass, and thus reduce levels below the action thresholds. This and other expert misconceptions were scornfully dismissed by the farmers as utterly unrealistic. Outbursts of frustrations at the experts' ignorance occurred often, here in response to their assumption that straw would make up for the drastic shortage of grazing:

[The experts] don't understand our way of life. They think you stand at the fell bottom and wave a handkerchief and all the sheep come running. . . . I've never heard of a sheep that would even look at straw as a fodder. When you hear things like that it makes your hair stand on end. You just wonder, what the hell are these blokes talking about?

In addition, farmers' specialist knowledge of local environmental conditions and sheep behaviour was ignored by the experts, much to the provocation of the farmers. The scientific knowledge constructed out of field observations began life as highly uncertain and uneven – the farmers watched scientists decide in apparently arbitrary ways where to sample mountainsides or fields with huge variations of readings, and they helped scientists as they changed their recorded monitoring readings of sheep contamination by changing the background reading, or the way the monitor was held to the sheep. Yet these kinds of uncertainty and openness were obliterated by the time the knowledge returned to that same public as formal scientific knowledge in official statements.

Many of the conflicts between lay farmers and scientists centred on the standardisation built into routine structures of scientific knowledge. The quantitative units involved often encompassed several farms and even valleys with one measurement or value, when the farmers knew and could articulate various significant differences in environment, climate factors, management practices, etc., between neighbouring farms, indeed even on a single farm. These variations often reflected substantial elements of skill and specialist identity on the part of the farmers, yet they saw these wiped out in the scientific knowledge and the ignorant or insensitive ways it was deployed. A typical lament indicated this conflicting embryonic epistemological orientation, which was connected to a conflict between central administration and bureaucracy and a more informal, individualist and adaptive culture: 'This is what they can't understand. They think a farm is a

farm and a ewe is a ewe. They just think we stamp them off a production line or something.'

In other cases the scientists ignored the farmers' informal expertise when they devised and conducted field experiments which the farmers knew to be unrealistic. An example was an experiment intended to examine the effects of bentonite spread on affected vegetation in reducing sheep contamination. The experiment involved penning sheep in several adjacent pens on similarly contaminated grazing, and spreading different specified amounts of bentonite over each pen area, then measuring contamination levels in the sheep, before and at intervals after. The farmers immediately observed amongst themselves that these experiments would be useless because hill sheep were unused to being penned up, and would 'waste' in such unreal conditions – that is, they would lose condition and their metabolisms be deleteriously affected, thus confounding the experiment. This was a typical arena in which expert knowledge and lay knowledge interacted and directly conflicted over the appropriate design of scientific experiments.

After a few months the scientists' experiments were abandoned, though the farmers' criticisms were never explicitly acknowledged. In this and other cases, also for example over the levels of recognised uncertainty and standardisation, the lay public were involved in substantive judgement of the validity of scientific commitments. Much of this conflict between expert and lay epistemologies centred on the clash between the taken-for-granted scientific culture of prediction and control, and the farmers' culture in which lack of control was taken for granted over many environmental and surrounding social factors in farm management decisions. The farmers assumed predictability to be intrinsically unreliable as an assumption, and therefore valued adaptability and flexibility, as a key part of their cultural identity and practical knowledge. The scientific experts ignored or misunderstood the multidimensional complexity of this lay public's problem-domain, and thus made different assumptions about its controllability. In other words, the two knowledge-cultures expressed different assumptions about agency and control, and there were both empirical and normative dimensions to this.

This example corresponds with many others in which expert and lay knowledge cultures interact, for example Lave's critical examination of the assumptions made by cognitive psychologists and other scientists about the artificial nature of controlled testing of lay people's mathematical reasoning abilities (1988). Dickens (1992) found similar underlying factors involved in conflicts between weather forecasting scientists and lay publics over the prediction of extreme weather events such as hurricanes. Martin (1989) also identified essentially the same cultural dimensions of conflict between the knowledges of working class women and biological scientists about menstruation. These were not a matter of lay public 'cultural' responses to 'meaning-neutral' objective scientific knowledge, but of cultural responses, to a *cultural* form of intervention – that is, one

embodying particular normative models of human nature, purposes and relationships.

Many other studies of the interactions between scientific expertise and lay publics support this kind of analysis (for example, Davy et al., 1992; Long and Long, 1992; Hobart, 1993; Watson-Verran and Turnbull, 1994; Irwin and Wynne, 1995). These and other studies show the following common features:

- scientific expert knowledge embodies assumptions and commitments of a human kind, about social relationships, behaviour and values;
- it also embodies problematic 'structural' or epistemic commitments, for example about the proper extent of agency, control and prediction, or of standardisation;
- it neglects and thus denigrates specialist lay knowledges;
- at a secondary level it then defines lay resistances as based on ignorance or irrationality rather than on substantive if unarticulated objections to these inadequate constructions of lay social identity which the expert discourses unwittingly assume and impose;
- thus a further reinforcement takes place, of tacit public ambivalence about being dependent on social actors (experts) who engender such alienation and social control;
- hence the fundamental sense of risk in the 'risk society', is risk to identity engendered by dependency upon expert systems which typically operate with such unreflexive blindness to their own culturally problematic and inadequate models of the human.

It is worth examining the correspondences with a case taken from a very different situation, this time an agricultural development context in South America. The account is drawn from Van der Ploeg, 1993.

#### *Potatoes, Knowledge and Ignorance*

Van der Ploeg's (1993) study of Andean potato farmers, their knowledge and interactions with scientific expertise is instructive on a wider scale, illustrating more general issues in the relationships between indigenous knowledges and those which have achieved scientific status. He first notes that such local knowledges are interwoven with practices, in this case craft-based agriculture. These are highly dynamic systems of knowledge involving continuous negotiation between 'mental' and 'manual' labour, and continual interpretation of production experiences. This induces a highly complex and detailed knowledge system and a flexible, adaptive culture which does not lend itself to standardisation or planning. However, because it is so multidimensional and adaptive, experience is rarely expressed in a univocal, clear form. This is frequently mistaken for lack of theoretical content, akin to Bourdieu's (1980) questionable characterisation of this kind of knowledge as going from practice to practice without ever developing an overarching abstract or theoretical discourse.

Van der Ploeg's analysis of potato farmers' knowledge challenges this dismissive modernistic view of indigenous knowledges. He shows that there is indeed systematic theory, even though this is in a syntax linked to the local labour process and does not presuppose a universal and impersonal world. Seen from the epistemological standpoint of modern science it is a highly variable and non-universal knowledge. Seen from the farmers' vantage point this variability is a reflection of the conscious purpose of building diversity into practice, and of adaptively coping with multiple dimensions in the same complex arena. For example, the farmers deliberately seek to increase the variety of ecological conditions of their agricultural plots rather than to standardise them, and they use the variety of conditions and results dynamically to evaluate possible improvements; but they do not assume a singular optimality. Multidimensionality is taken for granted. Furthermore their use of concepts and measures such as hot/cold or high/low also shows this informal multidimensional sophistication. Van der Ploeg describes the outsider analyst's surprise at understanding that the apparent imprecision of terms such as 'higher' being used to describe fields that are mathematically lower than other fields, reflects the embodiment of further qualifying factors such as exposure to the wind or frost, and interpenetration with other terms such as 'heat', into an interrelated network of meaning.

Certain key properties of these indigenous knowledge systems are noteworthy (Van der Ploeg, 1993: 212):

These and other concepts are not unequivocal, nor do they lend themselves to precise qualification. They cannot be built into a nomological model of the kind used in applied science and in technology development . . . When one separates these concepts from the people who use them or from their context they indeed become 'inaccurate'. Of course this inaccurate character does not prevent farmers from establishing quite accurately the overall condition of specific plots. They are also quite able to communicate with each other on this topic. The inaccurate nature of the concepts used even seems favourable for such an exact interpretation of a plot's condition and the ensuing dialogue. For interpretation and communication can only be active processes; concepts must be weighed against each other every time a specific plot is being considered. Hence the conceptual overlap becomes strategic. In synthesis: *it is precisely the vagueness or 'imprecise' character that allows for this active process of interpretation and change.* [my emphasis]

A key point here is the observed imprecision in meaning of scientific terms when their use and interpretation amongst specialists in their esoteric, private and local scientific subcultures is examined (Star and Griesemer, 1989; Jordan and Lynch, 1992). This openness is not usually acknowledged when the same science is presented in the public domain, even if that 'imprecision' is functionally valuable within science. This suggests that the difference between science and indigenous knowledges may be more to do with the degree of felt need for social closure around what *appear to be* monovalent publicly agreed meanings than with any measure of intrinsic precision *per se*. In any case, Van der Ploeg's analysis

corresponds with that of the Cumbrian sheep farmers in showing the dynamic, complex and sophisticated nature of such local knowledges, and their built-in reflection and sustenance of important cultural and material values. Scientific knowledges are not neutral in this respect but also correspond with particular cultural and epistemic principles – instrumentalism, control and alienation.

Although the epistemology of control has been emphasised as a dominant feature of scientific knowledges, two observations are necessary. First, this epistemic commitment is more of a statement of aspiration or expectation – an orientating prescriptive commitment – than a statement of actual achievement, as all the discussion of the unanticipated nature of modern risks, dangers, accidents and environmental and other ‘side-effects’ underlines. As in the artificial conditions of the laboratory, science controls only to the extent that it manages to achieve the exclusion of all the factors it does not control, including those of which it is ignorant. It is the achievement of credibility for this implicit promise – or hope – of control, rather than the belief in actual control, which grants whatever authority they have to scientific ways of knowing.

Second, it should be noted that local or lay knowledges do not celebrate some romantic state of lack of control. They too seek control, and this does not exclude forms of social control (Douglas, 1966; Geertz, 1983; Scott, 1985); but it is of a kind which is radically different from that embodied in normal scientific epistemic commitments. This kind of knowledge is manifestly local and contextual rather than decontextual and ‘universal’ precisely because (Van der Ploeg, 1993: 212): ‘it presupposes an active, knowledgeable actor, who actually is the “agent” of the unity and constant interaction of mental and manual work. It can also be defined as local because it allows these actors to obtain a high degree of control and mastership over the highly diversified local situation.’

In other words, this idiom of knowledge allows control, but of a contextually dense and multidimensional reality in which adaptive flexibility towards the uncontrolled is still recognised as a necessary attribute, and where the reductive, decontextualised and alienated ‘control’ of other situations in the ‘universalistic’ manner of science is pre-empted. That is, this kind of adaptive ‘control’ is one which is exercised with personal agency and overt responsibility. It is just this property which, as Beck notes (1992), is missing from modernity’s particular discourse of control, namely science; and it is the reintegration of the deleted issues of human agency, responsibility and value which may lead to the democratisation, legitimisation and epistemic pluralisation of science.

Van der Ploeg describes the approach to potato farming taken by scientific culture. Whereas indigenous culture selected potato seeds – the genotype – according to the variable environmental conditions of the plots – the phenotypes – (and other changing criteria), and continually monitored and adapted such selection over a long and complex feedback cycle, science took exactly the opposite approach. It was founded on the

presumption of an ‘ideal genotype’ which contains the optimal combination of properties. Note that this assumes a standard and universal definition of the ideal type, even though this implicitly requires a standard specification of environmental growing conditions. These phenotype conditions which will render the ideal genotype effective are then derived, tested, defined and refined in experimental stations.

In other words, scientific culture starts with what is ‘standardisable’ as if an industrial mass-product, then attempts to reorganise the world to optimise the production of this standard universal ideal type. (Recall the ‘production line mentality’ lament of the Cumbrian sheep farmer about the scientific experts.) In the farmers’ case this meant enforcing standardised environmental conditions where before there had been diversity, complexity and indigenous learning. As Van der Ploeg puts it (1993: 217):

One of the consequences of this drastic change is that the new genotype will only prove to be an effective and rational innovation in so far as these required conditions can be effectively repeated in the fields . . . to ‘innovate’ is not just the simple adoption of a recommended object (‘a miracle seed’) but – as far as the farmer is concerned – a highly complex reorganisation of several farming routines.

And, he might have added, an externally prescribed reorganisation at that – one involving a fundamental transformation of agency. Thus begins the cycle of intensifying dependency, in which for example a specified level of soil nitrogen is demanded; to administer this without burning requires fertiliser spreading to a precise timetable, derived from the particular genotype properties. From this, water regulation requirements arise, ‘and so it continues. However the point to note is that these specified requirements must be repeated in the fields, as an integrated whole. Even if all the specified conditions are followed except their exact distribution over time, then the “innovation” fails’ (1993: 219).

Furthermore, these new requirements ‘initiate the creation of several new dependency patterns’ – new artefacts as specified in the scientific design; new procedures; new expertise and training; and new patrons in the banks and markets. This corresponds strikingly with Latour’s (1987) account of the development of scientific-technical networks through the reordering of society and nature together, and the achievement of action-at-a-distance in this realignment process by having knowledge and identities pass through ‘obligatory points of passage’ (here the ideal genotype potato and its necessary phenotype conditions) which impose standardisation on them. Science gains its image of intellectual universality by achieving social control over the standardisation of what are varied situations.

Van der Ploeg’s final point about this kind of interaction concerns the logic of destruction of indigenous knowledge and culture when faced with committed ‘modernisation’ programmes. A crucial feature of the flexible and informal farming knowledge is that it cannot be codified (1993: 220):

the outcome of such methods cannot be exactly predicted. Nor can the necessary methods for reaching pre-established levels be prescribed in detail. For farmers this is no problem whatsoever . . . But regardless of the advantages such methods can offer in a situation managed by local knowledge, in a scientific design they cannot be integrated, simply because they are insufficiently adaptable to the necessary standardisation. Local methods . . . fall outside the scope of scientific design. Consequently farmers as active and knowledgeable actors, capable of improving their own conditions, also fall outside the scope of scientifically managed rural development.

Thus an asymmetrical conflict seems to exist, between a system which is supposed to work in a superior way, so long as the demanding standardised conditions can be repeatedly fulfilled, and a 'local' system which demands too much skill and trust in a non-codified informal and variable craft expertise. The scientific culture brings with it the terms of its own validation, like any other culture. But the sting in the tail is that the scientific culture doesn't work even in its own terms, if one enlarges the scope and extends the timescale of evaluation. Thus the yields from the scientific varieties have degenerated rapidly – 'within three or four years they became incapable of generating even low levels of production' – leading to magical discourses about the scientific varieties 'having no power any more'. The scientific system only 'worked' by socially excluding large parts of the world from its purview – for example ignoring the quite respectable, and *sustainable* observed yields of the indigenous farming system. Thus, it seems (1993: 223), 'ignorance of the local knowledge systems, their dynamics and their scope, is a crucial precondition for the diffusion of the scientific knowledge system'.

Van der Ploeg's conclusion is interesting in respect of the relationships of scientific and lay idioms of knowledge. Reviewing the overall response of the potato farmers to the self-consciously modernising influences of scientific programmes he notes the powerful irony (1993: 222) that:

In more general terms this implies that the increasing influence of science in the world produces just the opposite effects, at least under the circumstances described: myths, vagueness, poly-interpretability and a certain subjectivity in the relation to nature are not superseded through heavy inputs of applied science, but rather reinforced and extended to farmers' relations to science itself!

If we make explicit the cultural, mythic character of science as well as the readily underlined cultural character of the local knowledges, then this reaction is understandable because the expert system can be seen as a system of myth which is alien and threatening to existing identities and effective knowledges. It is a more cultural or hermeneutic restatement of the self-refutations of modernity described by Beck. However, what Van der Ploeg unfortunately fails to emphasise in his account are the ways in which it points to potential transcendence of the sterile conceptualisation of dichotomy between on the one hand a monolithic culture of rationality and scientific modernity, and on the other a defensive and non-innovative, epistemically closed realm of indigenous 'traditional' cultures. The lay knowledge he describes is complex, reflexive, dynamic and innovative,

material and empirical, and yet also theoretical. It is experimental and flexible, not dogmatic and closed. Whatever its ultimate demerits or merits, it is epistemically alive and substantive. It also embodies implicit cultural models, of the human subject, agency and responsibility (just as, I have argued, does the scientific knowledge). It is difficult to see in this kind of non-expert knowledge culture only the defensive, private and epistemically vacuous implicit model of lay knowledge advanced by Giddens, or even the more generous but still expert-dependent version of Beck.

Thus transcending the misleading terms of 'The Great Divide' requires us to address the senses in which modern science is, like all other kinds of knowledge, thoroughly cultural, and the ways in which it conceals its own fundamental indeterminacies by subtly and tacitly building the cultural and institutional terms of its own validation. Latour's (1992) notions of hybridisation and purification offer one such avenue of insight. Science imbues natural categories with culture, and scientific-technical networks are built with a richly heterogeneous hybridisation of the natural, human and artefactual. But then this human achievement is purified of its human content and defined as only natural, to the disorientation of the social world and its concepts of responsibility and agency. These two processes occur side by side. It is difficult to give credence to the ideas of basic epochal transformation, whether from tradition to modernity, or from modernity to postmodernity. Instead, a less constrained examination is called for of the ways in which human responsibility for natural knowledge can be practically reclaimed and redistributed; and exploration of how natural discourses might be able to help stabilise human relationships without at the same time inducing extremes of alienation, exclusion and inflexibility. This will require re-examination of received ideas of the nature of scientific and lay knowledges.

### Conclusions

Whilst it shares some of the key elements of Beck's and Giddens's theories of the transformations of modernity, especially about the self-refutations of modern institutions and the rise of 'life-politics' outside of formal institutions, my perspective on expert systems changes quite fundamentally the conceptualisation of the driving forces in those cultural and political transformations, of the basic character of *risk* in the 'risk society', and thus of the possibility of constructing new forms of epistemic and social order which could be seen as enjoying more democratic public identification, legitimation or responsibility. In particular, the potentialities for new forms of political, moral and epistemic order – ones enjoying greater public identification, and reinvigorated democratic grounding – are significantly broadened by introducing the problematisation of 'expert knowledge' which my analysis has done. This approach is founded in the sociology of scientific knowledge, especially in three key tenets:

- its understanding of scientific knowledge as underdetermined by natural evidence and logical decision rules, and of scientific observation and experiment as underdetermined by prevailing theory (for example Latour and Woolgar, 1979; Cartwright, 1983; Collins, 1985);
- its understanding of scientific knowledge as rooted in local (such as specific laboratory) practices, whose claims to universality rest on successful discursive linkages, coordinations and correspondences being made between disparate local practices (for example, standardisation as described in Van der Ploeg's case-study; Latour, 1987; Star, 1989; Pickering, 1992) .
- partly as a function of these properties, the unavoidable embodiment in the constitution of scientific knowledge, of assumptions and commitments directly or indirectly about the human and cultural (Wynne, 1992; Watson-Verran and Turnbull, 1994).

Relating these more directly to the public domain, established concepts of 'good science' which lend politically privileged authority to particular scientific subcultures and exclude others, are not *naturally* given but culturally validated – and the reciprocal validation occurs too. A new domain of debate is therefore opened up, concerning the articulation of different public values and intellectual perspectives, with criteria of 'good science' that come to be institutionalised and exercised in many economic, technological, medical, health, educational and environmental domains. In practice, there is *de facto* reflection of the indeterminacy of such potent normative concepts, for example in the formulation and negotiation of regulatory and risk assessment science and policies (Jasanoff, 1990). But this open-endedness is concealed behind the public scientific rhetoric of objectivity and determinism. By falling into a resultant binary idiom, much of the sociological debate on modernity has crippled itself by an uncritical absorption and reflection of this rhetoric, even whilst affecting a critical orientation.

In identifying the indeterminacies and intrinsically local nature of scientific knowledge construction, my approach also implies recognition of the more substantive intellectual status of lay knowledges than is usually acknowledged (for example by Beck and Giddens). It is important not to misunderstand this as a claim for intellectual superiority or even equivalence for lay knowledges. This question is beside the point in the present context. However, it does imply much greater interdependence than is conventionally recognised between what come to be defined as lay and expert knowledges. Valuable though it is in several important ways, Van der Ploeg's (1993) case-study implies the bleak conclusion that there are categorically distinct epistemological systems, of modern science and indigenous ('cultural') tradition, even if the latter is more dynamic and practically effective than usually seen. He suggests that the two cultures are simply mutually incompatible and that it is either one, or the other. Dealing with identical issues – in their case between Aboriginal knowledge

systems and modern scientific ones – Watson-Verran and Turnbull (1994) manage an openness to these very basic categories which is more in basic sympathy with this chapter. In studying the often fraught and conflictual interactions as a matter of practical politics as well as epistemology, they observe:

What we are producing – practical criticism of past ways of understanding ourselves, and relations between two peoples, and reinterpretation of the political and social processes of those relations – is of course subject to standards of theoretical coherence and empirical adequacy. But its overall adequacy is not solely determined by such criteria. The constructions that we are generating [in these science-Aboriginal knowledge confrontations] are 'verified' also by participants engaging with the newly apparent sets of possibilities for action.

This is remarkably consistent with the sociological views of scientific knowledge validation as being based not in nature *per se*, but (which may of course give nature a large role) in the identification by participants of new possibilities for 'carrying on' the existing culture, not without new elements of practice, relationships and identity emerging.

This more diverse, open-ended and less dichotomised view of where legitimate knowledge and order might come from also of course carries important political implications in terms of potential redistributions of power and recognised authority to subcultures currently marginalised or outside formal institutional processes. It also corresponds with the view of the construction of knowledge as the construction of hybrid (Latour, 1992) or heterogeneous (Law, 1986) networks, necessarily paying no respect to putative boundaries between the natural, the social and the artificial (though constructing and reifying these for legitimation purposes). Thus arises also the challenge to the way in which modernity as such has been conceived, since in the otherwise rewarding perspectives of Beck and Giddens the fundamental divide between nature and culture which is a defining characteristic of Western modernist self-conceptions is ironically not challenged, but uncritically reproduced.

Once one introduces the idea that scientific expert knowledge itself embodies a particular culture – that is, it disseminates and imposes particular and problematic normative versions of the human and the social – then this fundamental divide is no longer tenable. An important strand of sociological and historical work on science has problematised the supposition of an objective boundary between science and the public domain, as if for example knowledge and cognitive influence only flow one way, and as if there were not cultural, epistemic and cognitive commitments that were in principle open, but held in common and mutually reinforcing across the boundary.

The problematisation of scientific knowledge as embodying hermeneutic (and formulaic) and not only propositional truths (as Giddens claims) of necessity also problematises the boundaries established as social constructions between the scientific and the non-scientific. Work to define boundaries between expert and lay as if these were objective categories given in nature becomes critical to the stabilisation of forms of authority. This



aspect of science and the public domain has been extensively treated, for example by Gieryn (1983, 1994), Jasanoff (1987), Star and Griesemer (1989) and Shapin and Schaffer (1985). Latour (1983, 1987) has also fundamentally problematised these boundaries. Despite significant differences in other respects, all these perspectives have in common the view that particular epistemic orders or cultures interconstruct and mutually reinforce with particular social or political orders, in a historically contingent way (which is very different from saying that they are subject to subjective whim). However, with few exceptions none of this work has yet been adequately connected with understanding of the nature of 'non-expert', public knowledge (Watson-Verran and Turnbull, 1994). In Giddens's view of reflexive modernity the reflexive processes of lay people seem to have no significant collective intellectual content; it is merely intimate behavioural reflexivity towards 'globalising' expert interventions in personal lives. Thus there is no problematising of a boundary between expert and lay domains of knowledge and epistemology – because the most lay people have to do (or are thought to be capable of) is to choose whom to trust, when experts disagree. If there is no expert contestation, there is apparently no problem anyway, since, it is supposed, only the domain of expert knowledge enjoys any intellectual substance. In Beck's only slightly different view, the problems of whom to trust arise because expert bodies have betrayed trust by not protecting society from the now-pervasive risks 'out-there'. However, in that these 'out-there' risks are identified by counterexperts the problems of trust and risk are only raised by expert contestation, and as in Giddens's account the public is only represented by different expert factions. The human dimensions of such natural knowledges, whether contested or not by other *experts*, is not recognised or problematised as a public issue.

The formal distinction which Giddens emphasises between propositional, formulaic and hermeneutic truth claims, and his assumption that science only deals in propositional claims and commitments, allows him to assert the absolute distinction between expert and lay knowledge. According to this widespread view, expert systems have unmediated access to nature hence peddle only natural knowledge, whilst lay publics are epistemically vacuous, and have only the emotional wellsprings of culture and ephemeral local knowledges. Yet once the picture is complicated in the way that this chapter proposes, science is articulating and imposing not only propositional claims but tacit formulaic and hermeneutic ones too – in other words, symbolic ones about the performance and legitimacy of social institutions, and about the 'naturalness' of particular models of human nature and relationships. Furthermore, once this crucial point is recognised, then lay public responses to scientific expertise can be seen as responses which combine human questioning or rejection of those implicit 'formulaic' and 'hermeneutic' expert system claims, with alternative propositional ones. Indeed it is exposed as problematic to use such categorical distinctions without further qualification, since the basis on

human and moral grounds of legitimate non-expert responses to scientific expertise shifts the epistemic framing of the social purposes of knowledge, and hence the criteria by which valid propositional claims would be established. On my perspective, therefore, it is impossible to accommodate the view that non-expert understandings are only represented in public debate and contestation by dissenting expert groups. Yet this is the view both of Beck,<sup>8</sup> as in his model of the sub-politics of reflexive modernity, and of Giddens, with his idea that publics now have to invest trust by deliberate decision and choice between competing experts, and that this reflexive lay awareness has only arisen because of expert conflict. Some sociologists of science have also committed themselves to this unhelpful dichotomous vision. For example, Collins (Collins and Pinch, 1994: 335) asserts that: 'It would be a strange world, and one that I would not welcome, if "the public [had] its own and legitimate interests in the very contents of science" '.

As already indicated, what count as the contents of science can be debated, but here Collins joins common cause with the dichotomous rationalism of Giddens and Beck on modernity and expertise. It is not surprising that his approach within sociology of science has been attacked for, in effect, closing down possible questions about the wider epistemic negotiability of reliable knowledge of nature. This perspective therefore forecloses the open issue of what is to count as 'good science' in public domains, and pre-empts fundamental questions about the indeterminacy of the human and natural orders (Callon and Latour, 1992; Collins and Yearley, 1992).

Because my perspective is vulnerable to such a common misunderstanding, let me utterly disown the reading which takes it as claiming that lay, or 'local' knowledge is to be championed as superior to scientific or universal knowledge. To conclude this from my analysis would be completely to miss the point. Collins again appears to fall foul of this dichotomous thinking when, in response to the suggestion that lay people may have a role to play in the substantive construction of scientific knowledge, he asserts (Collins and Pinch, 1994: 335): 'It would be a weird world in which our desire to avoid elitism stretched democracy to the level of the Chinese Red Guards. When anyone's opinion on a matter, irrespective of their depth of experience, is as good as anyone else's, then society has broken down.'

The public's proper role, according to Collins (Collins and Pinch, 1994: 335), lies only 'in saying how the boiling flux of expertise should be represented and applied to our society'. Thus an absolute boundary between expert knowledge and lay public knowledge is again reinforced, in which the latter only has purchase on local application. Like Giddens he appears to hold the view that when there is no expert conflict, there can be no problem.<sup>9</sup> There is no role for lay publics in evaluating and participating in the redefinition of what is to count as 'expert' knowledge, and no role recognised in renegotiating the proper constitution of scientific knowledge in terms of its normative embodiments of criteria of 'good science' – such

as its degree of standardisation and differentiation, its commitment to control and prediction, and the related issue of its treatment of indeterminacy and uncertainty. The separation between these essentially normative-cultural issues and the cognitive issues (which can be abstracted as purely cognitive only by taking for granted a particular cultural framework), is absolute in Collins's view, and in this he is at one with Giddens and Beck.

These are the exact epistemic issues about the proper constitution and scope of modern science as culture, which the modernity issue centralises. The environment and risk debates around which much of modern politics has been shaped<sup>10</sup> are quintessentially tied up with the larger crises of legitimacy of modern economic, scientific-technical and political institutions, and the search for new forms of legitimate order and authority. In this it seems that new forms of emergent political order, with new configurations of global vision and local rootedness, will emerge – are perhaps emerging – in which new imaginations of the relationships between universal knowledge and human values will be vital. In seeking the basis of more legitimate, less alienating forms of public knowledge, and stable authority out of present conditions of incoherence and disorientation, new constitutional norms of valid knowledge may be articulated. Necessary and legitimate involvement of lay publics in this process will also automatically involve them in negotiations, direct or indirect, of the intellectual contents of those new universals. Thus in reflection both of a moral and political impetus towards seeing ourselves as global citizens with corresponding responsibilities and relationships, and of the realities of global interconnections and dependencies, epistemic universals can likewise be expected to enjoy a legitimate and necessary long-term future. The romantic seductions of local knowledges and identities do not come as an *alternative* to modernity's ahuman and alienating universals, but as an inspiration to find the collective self-conceptions which can sustain universals that do not bury the traces of their own human commitment and responsibility.

To relegate the public to the role and identity given in the dichotomous conceptualisations of expert and public which I have criticised here, and to relegate scientific expertise to the associated condition of supposed cultural- and meaning-neutrality, is to commit society to further blind polarisation in the continuing transformations of modernity.<sup>11</sup>

## Notes

1. Reflexive processes are implicitly conceived as thoroughly calculative, under the autonomous authorship of an individual subject. This raises an interesting issue as to the extent to which culture is described in terms of its subjects' calculative faculties, implying a deliberative constitution. This deserves more extensive discussion, but in another place than in this chapter.

2. This view is so widespread it is difficult to pick out specific illustrations. See for example the pages of the monthly journal of the UK Atomic Energy Authority, *Atom* (London); and from pro-nuclear and sceptical stances respectively, see Pocock, 1977; and Roger Williams, 1980. See also S. Weart, 1988.

3. Those institutions are unlikely to escape from this constant erosion of the foundations of their own social legitimation if they are encouraged by sociologists into an erroneous view of the public as unreflexive.

4. This could be treated as an instrumental calculation of institutional trustworthiness of the same kind that I attribute to Beck and Giddens. However, I would argue that the process of apprehension and response is more immediate, engaged and less deliberative than this, more captured by describing it as partly an emotional process of identification or otherwise with those organisations and the kinds of human relations and identities which they appear to reflect, uphold or deny. It has to be acknowledged that the choice of descriptive terms here is at least in part a matter of the author's chosen moral engagement with the social world.

5. As the public understanding of science research field shows (Irwin and Wynne, 1995), lay public response to science is frequently – and legitimately – based upon understandings of science's institutional 'body-language' (for example whether it is reproducing private profits or public services) when scientific experts themselves imagine that they are or should be based only on its propositional contents.

6. This is the time taken for a given body burden to decrease to half the original level. It thus depends on the metabolic processes of excretion as well as on the physical radioactive decay rate of the radioisotope concerned. In this case the biological parameter is much the shorter, hence the dominant term.

7. This and the other farmers' quotes are taken from verbatim transcripts of structured interviews conducted during 1987–89 by the author, Peter Williams and Jean Williams as part of a project on public understanding of science funded by the UK Economic and Social Research Council. See Wynne, 1992.

8. It is worth noting that in his accounts of the ways modern expert institutions tacitly construct self-serving and socially disorientating versions of responsibility for the risks of modern science and technology, and in his focus upon the unanticipated, Beck comes closer to avoiding a rationalistic framework than Giddens. However, the mechanisms of self-dissolution or self-refutation, and thus the reflexive transformations of modernity, are still conceived by Beck solely in terms of uncontrolled 'out-there' aspects of the unanticipated – of risks, side-effects, etc. out there in nature or technology. I have explored and emphasised the nature of popular experience and response to science and modernity in the form of concrete interventions and discourses, which are not just about out-there risks, but also about modern expert institutions imposing non-negotiated 'in-here' moral and social identities and relationships. It is for these reasons that I would still characterise Beck's approach as fundamentally rationalist.

9. One of the main contributions of research on public understanding of science (Michael, 1992; Wynne, 1994), and before that on public risk perceptions (Wynne, 1983), has been to explode this simplistic view.

10. Whether or not Beck is correct to call this a historically new organising principle of society akin to the ownership and control of the means of production is a question which can be set aside for the moment. The globalisation of many risk problems does not seem to require the wholesale abandonment of analytical categories to do with capitalist accumulation; but the focus of the present analysis does not need a resolution of those questions.

11. However, to rejoin forces with those whose work I have critically examined here, the thorough lack of any essentialist claims or concepts defining the trajectory of modern social and cultural change is not only consistent with, but is obliged by, a fundamentally relational and cultural change is not only consistent with, but is obliged by, a fundamentally relational view of knowledge and ethics (Bernstein, 1983). If the grounding principles of knowledge are to sustain (ethically good) *relationships*, rather than fulfilment of abstract essentialist norms for which relationships are just the functional means, then the orientation of critical analysis such as this is bound to be contextual, as a matter of principle. It may appear to switch sides as one or another gains ascendancy and assumes those unreflexive properties associated with

domination; but this is only an appearance because actually this analysis is committed to no particular 'side', whilst being utterly engaged in their development and interaction.

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