Indeterminacy in-decisions – science, policy and politics in the BSE (Bovine Spongiform Encephalopathy) crisis

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Increasingly, non-human geographies have unfastened nature from its foundational moorings. In a parallel development, the benefits of adhering to precautionary and participatory forms of decision-making have become common place in environmental geography and in government policy. And yet, on closer inspection, there is a danger in these latter approaches that old certainties regarding non-human natures remain unquestioned. The result can be a tendency to gravitate towards bureaucratic and technical solutions to, or closures on, what are, first and foremost, political and open-ended problems. This paper uses an empirical engagement with BSE-related scientific and policy practices, along with insights from non-human geographies, science studies and poststructuralism to suggest that such certainties and resolutions are misplaced.

keywords: uncertainty, BSE, environmental geography, decision-making, precautionary principle, actor network theory

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Introduction

Increasingly, geographers are developing understandings of human-nonhuman relations which emphasize their mutability and their contestable status (see Anderson 1995, Braun and Castree 1998, Whatmore and Thorne 1999, Wilbert 2000). This work, which notably engages with literatures in feminist science studies (Haraway 1991 1997) and sociology (Latour 1987 1999), brings into focus the 'unnatural' quality of a strictly 'human' geography (see Murdoch 1997; Wolch and Emel 1998; Whatmore 1999a). The result has been a willingness to discuss 'nature' issues, not as matters that lie outside our societies and that need to be brought into the fold, nor as matters that are already thoroughly cultural, but as living relations with all their differences, discontinuities and entangled formations (Whatmore 1997; Hinchliffe 2000a). Meanwhile, as concern grows over a raft of possibly unprecedented environmental changes, over 'new' genetic technologies and over a range of environment-related food and health issues, there is new impetus to develop our understanding of bio-geographies (see also Whatmore 1999b; Castree 1999).

The implications of this work for environmental politics and policy have been and continue to be matters for debate and experimentation. And yet, despite the work of Wynne (1992, 1996, 1997), Darier *et al* (1999) and Burgess *et al* (2000), a tendency remains in government departments and in some policy-relevant academic writing to treat nature and environment as matters of fixed identity. The result, which is well illustrated in the BSE case, is that there is little or no consideration of the *extent to which* nature can be known. Instead, the task of government and of environmental policy-makers seems only to make sure that the best representation of nature is made available at the time of making a decision (with any failure to do so

ISSN 0020–2754

being a result of underdeveloped science or of political failings). Once represented, the immutable and incontestable character of a natural entity will form the basis for a consensual approach to decision-making (for a version of this approach to environmental politics, which is central to the politics of the Third Way, see Giddens 1998). In this paper I demonstrate that the failure to take up the lessons from environmental geographies has had and continues to have serious consequences. I will argue that the BSE crisis was in part the result of a failure to acknowledge the mutable and contested nature of the disease. Furthermore, this overstating of the degree to which the uncertainties surrounding human-nonhuman relations can be reduced in future remains a common feature of public policy. Indeed, the BSE Inquiry and its final report tended to reproduce this sense of determinate uncertainty. In short, the contested politics and geographies of human-nonhuman relations continue to be downplayed.

The background to this paper is a partial engagement with the British Government's BSE Inquiry (held in 1998 and 1999, and which reported to Ministers in October 2000). One immediately obvious impression of the inquiry was the almost total absence of what would normally be recognized as social scientists from the Inquiry team or indeed from the list of those who gave evidence.[1] This is not to say that social science issues were absent from the evidence or from the analyses of the Inquiry. But it is worth noting that the approach taken was somewhat different to current themes in contemporary geography and the social sciences more generally. It is a conviction in writing this paper that there are concepts and approaches current within geography and the social sciences that would have been *relevant* to the Inquiry (see Stengers 1997 for a useful definition of 'relevance'). I include in this respect current understandings of openness and indeterminacy (Wynne 1992; Mazis 1999; Deleuze and Guattari 1994; Massey 1999b), relationality and materiality (Latour 1999; Whatmore 1999a; Hetherington and Lee 2000) and dialogical and political conduct (Shotter 1993; Rydin 1999; Mouffe 2000; Hinchliffe 2000a; Thrift and Dewsbury 2000; Holloway and Kneale 2000). Each of these understandings can, I will argue, offer important insights into the production of BSE, and by extension other environmental, science and of course food-related, crises. In demonstrating the potential importance of these works to a major public policy issue, this paper also adds to debates in geography over the policy relevance of geographical work and methods (see Martin 1999 2001 and Amin and Thrift 2000). Contrary to Martin (2001), I aim to demonstrate that the geographical 'postmodern and cultural turns' offer significant potential for re-engagement with political and policy practices.

In focusing upon indeterminacy, materialities and conduct, the paper also engages with the variety of works that are subsumed under the labels 'actor-network theory' (ANT) and 'feminist studies of science' and the recent encounters between the former and post-structuralist philosophies (see Hetherington and Law (2000 for a review). The approach taken is both informed by these debates and modestly marks an attempt to take some issues forward through its empirical interest in the science and policy of BSE. In particular I adopt Rheinberger's (1997) notion of 'epistemic things' and Mazis' (1999) notion of a 'knowing of indeterminacy' to operationalize further the potentially fruitful relationship between ANT and post-structuralism.

Finally, there is a concern here with the nature of participation and democratic politics as it is constructed within geography and in particular within environmental debate. There has been a tendency to assume that wider forms of public participation and broader deliberation in science and environmental controversies will automatically generate better policy decisions. Whilst it is fair to say that in certain situations a broader constituency is evidently warranted, the elevation of deliberative and participative democracy to a general rule risks losing sight of the power relations, antagonisms and exclusions that make such forms of democracy possible (Mouffe 2000). In this paper I use the example of the conduct of BSE decisions to demonstrate the importance of considering the style of democracy as well as its constitutive elements.

After introducing, in the next section, debates over the proper conduct of environmental and science decision-making, I develop two case studies of BSE decisions in action. The first focuses upon the laboratory science that was drawn upon in policy debates to inform approaches to the disease. The second takes a particular set of events, as they were mediated by the BSE Inquiry, when policy responses to the recognized onset of BSE were being debated. Both accounts share a concern with *what is involved* in taking a decision, and what

such decisions produce in their wake. Finally, in placing these two decision-making situations next to one another, I hope to demonstrate that 'what went wrong' in the BSE case was not simply 'good science', which was let down by 'bad policy' (an increasingly prevalent reading of the official BSE Inquiry, or Phillips, report). Rather, there was a combined failure to translate a 'knowing of indeterminacy', or a geographical understanding of social and natural difference, into suitable actions.

1. Open science and policy?

In 1997, the British government's Chief Scientific Advisor, Robert May, drafted a paper entitled 'The use of scientific advice in policy making' (May 1997). The paper was in part a response to the perceived problems of a disjointed relationship between science and policy that had become evident during the Bovine Spongiform Encephalopathy (BSE) crisis in Britain in the 1980s and 1990s. May emphasized two issues. First, there was unlikely to be one science that could unequivocally inform policy. There was, at least in the medium term, bound to be scientific (or at least, epistemological) pluralism. Second, scientific knowledge claims would be accompanied by uncertainty.

May's response to this plural and uncertain world was to recommend that scientists should become more involved in the framing and continual re-assessment of policy. Scientists would best be able to handle a range of scientific opinion, understand the various forms of uncertainty, and develop the grounds for deliberative decision-making and/or consensus formation. In other words, a one-off provision of scientific advice should be replaced with an integrated decision-making forum, which included scientists at many more points in the policy process (from formulation, to processing to implementation). In addition, the aim would be to develop a more procedural, iterative approach to policy.

In a separate, though clearly related, set of developments, which many date to 1980s' debates on marine pollution in Europe (see O'Riordan and Cameron 1994), scientific pluralism and uncertainty are linked to prescriptions that are in part designed to avoid the risk of procrastination and the stalling of policy implementation through appeals to uncertainty. The prescriptions are often collectively termed 'the precautionary principle' (O'Riordan and Jordan 1995, O'Riordan 1995). The latter extends conventional policy thinking in at least two ways. First, the timing of policy action is brought to the fore. The recognition of long-term uncertainties has led to a problematization of not only *how* to act but also *when* to act. Second, the recognition of plurality is taken beyond the traditional scientific institutions in order to incorporate a wider range of expertise, including lay and practical knowledge communities (see also Irwin 1995). Thus, for O'Riordan (cited in ESRC/GEC 1999, 17), there is a set of decision-making rules which should be followed in order to act in a precautionary fashion (see Box 1).

Box 1: Elements of a precautionary approach to environmental and science policy – after O'Riordan (ESRC/GEC 1999, 17).

1. Where unambiguous scientific proof of cause and effect is not available, it is necessary to act with a duty of care
2. Where the benefits of early action are judged to be greater than the likely costs of delay, it is appropriate to take a lead and to inform society why such action is being taken.
3. Where there is the possibility of irreversible damage to natural life support functions, precautionary action should be taken irrespective of the foregone benefits
4. Always listen for calls for a change of course, incorporate representatives of such calls into deliberative forums, and maintain transparency throughout
5. Never shy away from publicity and never try to suppress information, however unpalatable. In the age of the internet, someone is bound to find out if information is being distorted or hidden
6. Where there is public unease, act decisively to respond to that unease by introducing extensive discussions and deliberative techniques

In essence, O'Riordan and May offer up a more complex version of policy making than was prevalent in the days when science was thought to operate as a an unequivocal 'answering machine' (Rheinberger 1997, 32) that could be called upon at

will by decision-makers. Accordingly, scientific knowledge should no longer be considered as something that can necessarily be produced to order. The result is a partial rejection of any linear model of science-policy practice, whereby theoretical questions are generated, knowledge is produced and policy, which is consistent with this knowledge, is generated. Scientists and other relevant parties ought, in these critical takes on policy, to be drawn into the decision-making process in more explicit and extended ways. Outcomes will only be beneficial to the extent that they reflect something of this expanded and prolonged participation and that they consider acting in the absence of definite understanding of all the consequences. It is worth noting at this point that the need to recognize scientific pluralism, and the requirement for 'in house' scientific advisors who could provide continuous monitoring of scientific developments, are prominent recommendations of the final report of the BSE Inquiry Team (the Phillips' Report, BSE Inquiry 2000). Similarly, calls for the application of a precautionary approach to public and environmental health issues are a prominent feature of the report and its media reception (ibid, see also Bradley 2000).

Persistent problems

With what are undoubtedly improved conceptions of knowing and policy-making processes, precautionary approaches introduce a set of ideas and prescriptions that highlight knowledge plurality and uncertainty, broadening participation and the requirement, in a world where consequences of inaction can be severe, for proactive policy. However, for all the talk of change and despite renewed interest during the BSE Inquiry, there are elements in this pluralist, deliberative and precautionary decision-making model that remain problematic and unresolved and that should be of concern to geography. They give rise to three persistent problems. I will translate each problem into a question that will be addressed in subsequent sections.

First, there seems little analysis of how the precautionary/deliberative model actually works *in practice*. It is a normative model, being a statement of what should happen, given the possibility for an 'ideal speech situation'. Looking at the model gives us very little sense of the *situated conduct* of policy. If we follow the action a little more closely, we might find that the model is unhelpful in taking us from current practice to more useful formulations of policy and decision-making processes. In particular, following those geographical practices and performances that are not only situated in the world, but also actively make worlds, or situate the world, seems crucial if an over generalized form of politics is to be avoided. So the first question is, *do specific policy and scientific practices give us a handle on the shortcomings of policy and of the limitations of current normative models?*

Second, whilst precautionary approaches to policy-making can be accompanied by models of the natural world which play down determinate cause and effect knowledge, and speak of irreversibility, indeterminacy and contingency, there remains a tendency to objectify nature and translate indeterminacy and contingency 'into problems of deterministic uncertainty' (Wynne 1997, 137). In other words, uncertainty is recognized only to the extent that it relates to the as yet unknown aspects of nature as substance. Alternative understandings of indeterminacy work with notions of probability that are not simply concerned with the risks of being 'right' or 'wrong'. In Mazis' terms;

> Probabilities function as explanatory principles, a way of representing the notion that entities are processes rather than substances, irregular in their unfolding, open to other events, fluctuating in their identity, a *knowing of indeterminacy*. Such entities are never really anywhere, as discrete, self-founding beings, and the probabilities represent the gaps in their substantiality and in the Cartesian world, rather than designate a paucity of knowledge about the system described (1999, 226 emphasis added).

Given the predominant reading of indeterminacy in environmental policy circles (an indeterminacy or uncertainty of knowing rather than a knowing of indeterminacy), we are left with a form of realism based upon *a* pre-existing natural world.[2] The participative, deliberative decision-making model can all too easily become a means to adjust and or reject various representations of that single world, in order to reach the best available outcome, which is itself finally arbitrated by the uncompromising external nature. This form of epistemological pluralism, combined, as it tends to be, with a substantive nature, or a natural *uni*versalism, produces two interrelated outcomes. First, the goal of rationalizing human choice through progressive understanding of a pre-existing natural world, remains. The result is that

the power of human cognition to *eventually* determine and predict the natural world is largely left unquestioned. Whilst there is no logical inevitability of following understandings of causality with prediction and control, the enlightenment assumption of human control over nature remains. Second, the notion of a singular natural object or physical risk around which there are multiple perceptions suggests that, in time, and through appropriate procedure, a consensus and or closure on the problem under consideration is possible. In holding up the possibility of an object-centred agreement, uncertainty is reduced to being largely a problem of making accurate representations. Even though, in the precautionary model, it is incumbent upon decision-makers to act in the absence of formal closure, the sense is that expert panels, consensus conferences, citizen juries, and so on will manage to *reduce*, rather than expand, uncertainty over time. A consequence of this is to continue to underestimate the significance of the various aspects of incertitude (as, for example, indeterminacy and contingency). It is also to downplay the significance of social and natural interrelations, and by doing so reduce the sensitivity towards social and natural diversity. Taken together, there is a reduction in the degree to which other legitimate concerns, values, materialities and experiences can count in the longer term (see Wynne (1996 and 1997) for arguments on uncertainty and on the failure of reflexive modernization to take up the challenge of social and natural diversity). This requirement to consider the plurality and relationality of natures as well as societies underpins the second question for this paper. *How can a reconfigured understanding of nature inform policy?*

The third persistent problem, which is present in precautionary and deliberative models, is the continuation of an essentially bureaucratic, rationalist decision-making framework that neglects to say very much at all about the politics of policy. If politics makes its way into these debates it tends to do so through particular constructions of the political. Interest group pluralism and deliberative democratic processes are both imagined in ways that tend to make political choices and actions sound like cosy negotiations and/or a benign means of reaching 'the best possible decision in terms of the public good, through argument and practical judgement' (Jacobs 1997, 225). O'Neill's rationalism is indicative of the latter approach:

> Through common deliberation, the citizens may show better sense than the best of individuals. Such common deliberation is a necessity in the modern world. Given that a variety of knowledge and practices inform many choices, it is only through such common deliberation that rational policy is possible. Moreover, good practical judgement, the product of local and concrete experience as well as training in theoretical abstract disciplines, are requirements of rational ecological policy (1993, 141).

Whilst proponents of deliberative democracy promise more than compromise, and are gratefully aware of the risks of moulding or presuming a consensus, there is an assumption that a procedure for reaching an inclusive agreement can be approximated. Agreement can be reached, protagonists argue, through an appeal to a rationalized logic, which may be a form of communicative rationality and which often, in the environmental sphere, implicitly relies upon a universalized, external, nature. In this sense, and unlike the forms of radical democracy that have been usefully elaborated upon by Mouffe (1993 2000), and spatialized by Massey (1995), there is very little consideration of the spurious bases for rationality in and exclusions of liberal democracies (for an exception, see Rydin (1999). Mouffe's (1999a, b) reading of Schmitt is particularly useful here. In brief, part of the argument runs that democratic forms are exclusive at the moment of inclusion. Therefore, there is an often unspoken tension in those models of deliberative democracy which have as their aim an inclusive, rationally achieved, morally informed, agreement. Such agreements, or decisions, can only proceed through an exclusion of an array of non-agreeable matters (humans, non-humans, experiences and so on).[3] In this, de*cisions* are, as their etymology suggests, also cuts, or incisions, and therefore perform distributions over what does and what does not matter (Serres 1995; Law and Mol 1996). Whilst these distributions are thought to be amenable to optimization by those who champion a participative and/or rationalist decision-making framework, for Mouffe, conflict cannot

> either be reduced to a conflict of interests, to be managed through negotiation – as in the model of interest group pluralism – or visualized as resolvable through rational deliberation thanks to the adoption of an impartial standpoint, as in the deliberative model. In both cases what is foreclosed is the properly antagonistic dimension, the dimension that would preclude the

possibility of a rational solution (Mouffe 1999a, 5).

Any consensus, solution, or closure, will be an expression of a hegemony and 'the crystallization of power relations' (Mouffe 1999b, 46). Therefore, the deliberative democratic aspects to current constructions of an ideal policy situation are in danger of reducing politics to a cosy pluralism or talking shop. In doing so, they will miss the multifaceted ways in which power relations, difference and antagonisms can and will be felt and expressed over and through any conflict and its so-called decisive solution. *A third aim or question is how can a political process that seeks to remain open to contest, to refuse deliberative or any other form of closure, inform the policy process?*

The geographical practices, the natural realism and the spatial politics of policy-making will all feature in the following two sections. In these, I engage with some of the science and policy-making that accompanied and framed the BSE crisis in Britain in the 1980s and 1990s. Section two follows the production of decisions over the scientific nature of BSE and related diseases. It raises the issue of the status of non-humans in knowledge production and thereby challenges the sense of a passive and universal nature waiting to be determined and drawn into the social world. Section three is devised as an attempt to clarify on matters of decision-making in policy, and similarly challenges this division of natural and social worlds. It highlights the exclusions that are collaboratively and dialogically performed by policy decisions and the ways in which those exclusions are, temporarily and disastrously, rendered invisible by appeals to technical rationality, material functionality and universal natural properties. The failure of those decisions to recognize the sociality of natural objects (that is their ability to make associations), and the spatiality and temporality of those associations, results in the intensification of risks rather than their amelioration. Together, the two sections develop into a commentary upon current understandings of the environmental policy process.

A note on methodology and sources

The case study material for this paper has to a large extent been drawn from the archives and files of the BSE Inquiry, which took place in London from March 1998 to December 1999 and which reported to Ministers in October 2000. The Inquiry involved 138 hearing days, 630 witnesses giving one or more written statements and 333 people giving oral evidence. The majority of this and other departmental information has been made publicly available, much of it in electronic format currently available at the BSE Inquiry web site (www.bse.org.uk). According to the Inquiry Chair, this collection constitutes a unique archive, allowing public access to the substance of advice to Government Ministers, and to the processes through which the advice is formulated (BSE Inquiry 1999). However, it should be noted that many of the documents are undoubtedly mediated through a judicial or quasi-judicial framework. That is, from minutes of meetings at government departments to oral evidence given at the Inquiry hearings, a degree of reflexivity as to the legal implications of any form of account helps to frame not only what is said but also what is recorded.[4] I am therefore dealing with texts which are not only strongly circumscribed but are also evidently cross-referenced to form paper trails where responsibility for statements can easily be dispersed through an organizational framework. The analytical difficulties in using this archive should not therefore be underestimated.

Meanwhile, and in addition to the complex, inter-textual nature of the record, the archive is vast. Therefore, for practical purposes, there is a focus on particular episodes and on particular issues. It is fair to say that these particular takes on events are not necessarily indicative of a broader picture, or necessarily more significant than other episodes. Yet, neither are they random selections. They have emerged from a long process of following controversies, arguments and debates through the empirical materials that were available. I was aided in this through the use of the Inquiry team's 'factual accounts' as initial familiarization devices. The factual accounts are arranged according to specific events and organizations that make up the crisis (including for example, the events leading up to the ruminant feed ban, the role of Central Veterinary Laboratories and so on). They contain specific reference to Inquiry evidence, written statements and oral evidence, which, in many cases, can then be accessed electronically. Using the accounts as the first port of call was therefore a means of ordering the Inquiry's documentation and of prioritising analysis. They enabled, for example, the identification of moments in the inquiry when things didn't add up or where there

were serious questions over a particular account of events or item of evidence. From this point, I would follow up the issue as far as possible through various paper trails and attend the Inquiry hearings on those days when key people were expected to be giving evidence.

In order to handle the large quantities of text and to build up cross-referenced accounts of policy events I used the navigation and theory building capabilities of *Atlas-ti*, a computer package designed to facilitate qualitative data analysis (for a review and critique see Crang *et al* (1997), and Hinchliffe *et al* (1997). Finally, in addition to material that was directly relevant to the Inquiry, other scientific writings were consulted and added, where possible, to the textual database.

2. Experimental systems and the materialities of decisions

The BSE debacle in the UK gathered momentum in the late 1980s, reached a peak in terms of press coverage in 1990 and 1996, and continued to produce media debate into the present (Miller 1999; Ratzan 1998; Woods 1998). One of the characteristic features of policy and media debate throughout has been the legitimacy and relevance of scientific advice on the disease (Miller 1999; Winter 1997). In terms of policy model, every effort was made to (be seen to) follow a linear model of decision-making (ideas making science, making policy). Committees were convened and staffed by eminent scientists (the Southwood and Tyrell committees and SEAC, Spongiform Encephalopathy Advisory Committee, are all examples of this reliance on science). Nevertheless, this was not an exercise in scientific pluralism. The scientific advisors were chosen not for their experimental awareness of the disease but for their 'non-territorial' expertise. For example, scientists with considerable experimental experience at the Neuropathogenisis Unit in Edinburgh were made peripheral to the initial policy debates on the disease, and there was a deliberate decision to exclude anyone who was involved in the controversies over disease agency (Lacey 1994; Pennington 2000). This geography of knowledge, which involves the systematic exclusion of those involved in (scientific and funding) controversies (and particularly those labelled as 'controversial') from policy debate, is something that has concerned May (1997) and the BSE Inquiry. But there is another aspect to this exclusion, an aspect that relates to a different geography of knowledge. Exclusion of experimental scientists from debate produced, I will argue, a tendency to downplay the practical, material and situated aspects of disease knowledge. And it was this kind of experimental, situated knowledge which could produce what I earlier referred to as a 'knowing of indeterminacy'.

Experiments and epistemic things

> We have begun to understand . . that the pair human-nonhuman does not involve a tug-of-war between two opposite forces. On the contrary, the more activity there is from one, the more activity there is from the other. The more ... [a laboratory scientist] works in [their] laboratory, the more autonomous [their] ferment becomes (Latour 1999, page 147).

As cranial dissections of clinically affected cattle proceeded, the symptomatic, the visual, the structural and then the molecular characteristics of the unsympathetically named 'mad cow disease' were recorded. Who knows how many diagnoses occurred before the disease was recognized as a singular entity – as *a* disease rather than an unexplained fatality here, or as a strange variety of something else there? There are procedures in place for vets to record and compare experiences and to seek advice. This was one function, at least, of the regional veterinary centres and the state veterinary service. In time, and to cut a complex and no doubt controversial story short, animal health experts moved from the recognition of an unclassified set of symptoms, to a new disease (based on similarity of symptoms) to a classification of the disease as a TSE (Transmissible Spongiform Encephalopathy). At and before this 'point', similarities were drawn in terms of clinical symptoms between the new disease and sheep scrapie. The latter had been endemic in British sheep for well over 200 years (it was first detected in the early eighteenth century). It was also reasonably well studied.

Scrapie had always been something of a mystery. Whilst its symptoms were well known, even to the extent that various clinical strains were recognised, its transmission was far from clear. Over the course of the twentieth century, attempts to understand disease transmission tended to make the identification of discrete disease agents the priority.

This normally involved the 'purification' and biological/ molecular/ biochemical characterization of a replicating 'life' form. In other words, the disease was expected to have a particular (micro-)spatiality. Discrete entities were expected, to which primary properties and agencies could be attributed. To this end, there was a tendency to equate the transmission of disease with the conveyance of nucleic acid in stable form from one individual host to another.

The 'central dogma' of molecular biology undoubtedly informed a good deal of the experimental work on scrapie and related diseases (Keyes 1999a). Crick's formulation of this dogma, stating only that sequential information could pass from nucleic acid to nucleic acid, or from nucleic acid to protein, but not from protein to nucleic acid or from protein to protein (see Crick 1958; Keyes 1999a), set the scene for James Watson's more prescriptive assertion that, as the slogan had it, 'DNA makes RNA makes protein'. Watson's linear and one-way flow of biological information prevailed throughout the 1960s, and into the 1970s – periods in which theories of replication and genetics brought molecular biology to the brink of disciplinary pre-dominance.[5] Nevertheless, and as I will detail, the framework of virology and the central dogma cannot be described as determining the scrapie experimental science. The virological stories about the agency of scrapie did not hold up in the laboratory. For some reason, the infectious agents were 'objecting' to the stories that were being told about them (see Latour (2000) for this definition of objectivity). To understand these objections, it is useful to consider what else is involved, other than stories, in experimentation.

For the biologist and historian of science, Rheinberger, experimental systems are the genuine working units of contemporary research in which the scientific objects and the technical conditions of their production are inextricably interconnected (1997, 2). Furthermore, it is the activity of experimentation, its embodiments and conduct that are the conditions of possibility for new knowledge. This active sense of the production of knowledge is quite different from the idea that science proceeds through the generation of abstract ideas, followed by experimentation. Experiments have for too long been regarded as constituting instances of verification, or corroboration, of refutation, or of the modification of theories . . . as mere empirical instances in the evolution of theoretical propositions (Rheinberger 1997, 15).[6] In an earlier paper, Rheinberger sets out his alternative understanding of experimental systems.

> The trajectory of an experimental system can be considered to consist of a progressive reproductive refinement, and of a series of bifurcation points. It is characteristic of such bifurcation points that at the very time when a decision has to be made as to which of the possible lines of inquiry to follow, the information necessary to anticipate all the possible consequences of the choice is usually not in place. This is an intrinsic, epistemic characteristic of the research process, and it cannot be reduced either to an individual mind's competence for judging a given research situation, or to the particular circumstances of a local research environment with its facilities or impediments. Therefore *the future of an experimental system is never the future of a past alone: it is a series of events that is underdetermined by that past* (Rheinberger 1996, 411, emphasis added).

In order to evoke some of the historial[7] materiality of these experimental events, Rheinberger adopts the term *epistemic thing*. These are scientific objects, 'whose unknown characteristics are the target for experimental inquiry' (238). For Rheinberger, following epistemic things, rather than following scientists (Latour 1987) or, before the sociology of science, following scientific theories, provides opportunities for reducing our reliance on subject- or object-centred versions of scientific trajectory (see Latour (1999) for a similar, if distinct project). Far from being pre-existing entities, epistemic things are produced in and co-productive of experimental systems. Epistemic things are not simply matters that can 'be brought to light through sophisticated manipulations' (Rheinberger 1997, 28). Paradoxically, epistemic things embody what one does not know. They are vague and they are absent in their experimental presence. They present themselves in a characteristic, irreducible vagueness. As I will show in a moment, in terms of TSE science, they are the strange textures and mixtures that didn't fit or perform in accordance with the central story of molecular science.

Rheinberger's interest in objects shares a good deal with others who have sought to take pragmatogony (or the genealogy of scientific objects) seriously. But what Rheinberger seems to offer is a more explicit sense of the creativity, or spatio-temporal multiplicity (see Massey 1999a), of experimentation. For Rheinberger, science in action involves more than the drawing together of texts, apparatus, money, ideas, humans and nonhumans.

This attention to 'drawing together' that becomes the focus of at least some forms of network analysis (especially those that tend to objectify the noun network rather than highlight the relationality of the verb net-work, see Law (1994) tends to presuppose a closed system. That is, in some (perhaps more epistemological) versions of Actor Network Theory (ANT), a truth claim can to all intents and purposes be explained with reference to the various associations that made it possible. But in this kind of exercise, there is no excess – and therefore there is no clear room for the radically new. Indeed, any 'thing' or process that looks new or innovative is re-described as an effect of a wider set of relations or network. Now, excess seems to be something that we need to associate with scientific activities in our times. As Rheinberger notes, 'we would not have the incommensurable plurality of the sciences as we experience – and fear – them today if their movement were not excessive, if they were not continuously producing a surplus that is beyond what we may have wanted, beyond what we may have been able to imagine' (1997, 23).

So for Rheinberger, experimentation is *'an embodied disclosing activity that transcends its technical conditions and creates an open reading frame for the emergence of unprecedented events'* (ibid 31, emphasis added). The experimental system, to be successful, is therefore a machine for generating the future (not the exposition of the past). In contrast to the traditional view from the philosophy of science, experimentation is not equivalent to 'an answering machine'. As Latour (forthcoming) and Stengers (1997) would concur, it is the question generating machine of science. It is a moment of risk taking, or of not being in total control, and of finding the conditions of possibility for being fundamentally affected by the world (see Latour forthcoming).[8] The case of TSEs helps to illustrate this facility of experimental systems.

TSEs in the science laboratory

> Since the scrapie agent multiplies in the host animal, it has been assumed that nucleic acid must be a part of its structure. However .. our data strongly support the conclusion of Pattison that this agent is likely to be of *an unusual nature* (Alper *et al* 1966, 283, cited in Keyes 1999a, 11, emphasis added).

As I stated earlier, TSE research was largely organized through the discursive practices of virology and molecular biology's central dogma (Keyes 1999a; Ridley and Baker 1998). In classic reductive mode, the search for an agent progressed through activity trials that were performed as a series of productions and reductions in scale. Over many years the most infective parts of animals with scrapies were identified through these experiments. Aided by filtration apparatus, centrifuges, sterilising equipment and a huge number of small mammals, including mice (later transgenic), guinea pigs, marmosets and chimpanzees, workers gradually produced filtrates, precipitates and supernatants with higher and higher activity rates (or ability to produce infection in a host animal – see Ridley and Baker (1998) and for a review of the animal stress caused in experiments on transgenic mice see Jenkins and Combes (1999). Despite this experimental 'success', the 'agent' was behaving badly in terms of the central dogma. The necessary features for a pathogen were seemingly missing. Tests on the infective scrapie material in the 1960s and 1970s suggested an absence of nucleic acid. The inferred result being that disease transmission was being caused by an unconventional agent with neither DNA or RNA in its make-up. This absence of a sequential, informational coding molecule in a disease agent was regarded as heresy by a majority of scientists.[9]

It is worth re-visiting Rheinberger's characterization of experimental systems at this point. The materialities of the experimental system, and of the epistemic object, along with the open reading frame adopted by these experimental scientists, created the conditions necessary for a bifurcation point (Rheinberger 1996, 411). This was a moment where conceptual and material undecidability produced the conditions of possibility for novelty. The experimental science was not, contra Doyle (1997), a functive series of operations, disciplining the epistemic object so that it became a matter of reference. Rather, an event was taking place, wherein the 'object' was 'objecting' to the normal utterances that were being made about it (see Latour 2000). Such a bifurcation required a practice that was at least as conceptual (belonging to an irregular and open reading frame) as it was functive (a disciplining mode of organising knowledge – see Deleuze and Guattari (1994). In other words, the experimental science associated with TSEs could only proceed with an irregular, if bounded, approach. The epistemic object mattered. The experimental object was not simply narrated into being, and scientists were not in control. The

decisions made in laboratories were complex productions – that is they folded together subjects and objects, concepts and materials (see Stengers 1997).

In short, the substance produced in the laboratory according to technical conventions was not entirely what was expected. But the bifurcation point was by no means a revolution. There were plenty of attempts to rescue Watson's linear model of information transfer in the face of these experimental problems. For example, preparations of the agent were dogged by so-called impurities that 'could well be masking the infectious agent's true properties' (Keyes 1999a, 16). And indeed, whether scientists looked for nucleic acid or not, the spatial logic of 'true properties' remained. The idea of a discrete agent continued to inform the epistemic objects produced in the laboratory.

Continuing the heretical story, in the 1980s, equipped with new experimental techniques (including gel electrophoresis), Prusiner and colleagues in San Francisco were able to produce an infective fraction that was predominantly if not solely proteinaceous. An acronym, Prion – small *pro*teinaceous *in*fectious particles (Prusiner 1982, 1995) was assigned to this unusual disease agent. In some senses, the experimental object was starting to stabilize and to take shape. Even so, the noun and the shape managed to raise a series of new questions in its experimental setting. Indeed, the prion hypothesis seemed to open up rather than close down scientific conflict. One of the key opponents to the prion hypothesis was Richard Kimberlin (see for example, Kimberlin 1982, 1986) – who, despite the official decision to exclude scientists involved in controversies over TSE, was to become one of the main scientific advisers and risk analysts in the BSE debacle. Kimberlin was wary of abandoning conventional thinking on disease and replication, and noted that the existence of transmissible strains of the disease suggested that nucleic acid needed to be present in the agent in order to convey this strain specific information.

Nevertheless, and despite strong arguments against the theoretical possibility of prions, Prusiner and colleagues continued to shuffle materialities, trace new results, and 'purify' different substances in their experimental system (although the infectious protein was so sticky and insoluble that purification has remained no more than a theoretical possibility). In 1982, a seemingly unique protein was isolated from scrapie infected hamster brains – a protein that was thought to be absent in non-infected hamster brain and which demonstrated activity rates in keeping with dose concentration. Prusiner and colleagues labelled this protein PrP – prion protein (Prusiner 1995). However, as the sequential character of amino acids for this protein was mapped out, it soon became apparent that prion protein was equally present in infected and non-infected brain material. The noun prion had not only shifted to being an adjective, it also started to lose much of its specificity. So not only did the agent confound the linear information transfer model which had become the central dogma of molecular biology by seemingly having no specific nucleic acid, it also seemed to confound another basic tenet of biology. If the sequential character of the molecule was not determining its pathogenic character, then another form of information transfer was needed. The differentiation of scrapie and normal prion protein on the basis of a secondary fold seemed to rescue the idea of an agent with primary properties. Nevertheless, debates rumbled on as to the exact means by which the scrapie prion protein managed to replicate in a host body (see Ridley and Baker (1998) and Pennington (2000) for reviews).

Indeed, scrapie and other TSEs remained contentious in their experimental system. The disease agent as epistemic object continued to generate questions. And the challenges multiplied in coming years as not only did TSEs threaten the central tenets of molecular biology, they also added to the growing sense of concern over the state of food production and supply in Britain and elsewhere (see Lang 1998; Macnaghten and Urry 1997; Hinchliffe 2000b). Furthermore, just as prions, like viruses before them, managed to challenge divisions between living and inert matter, they also started to unsettle species' boundaries and divisions. As evidence of transspecies transmission of BSE started to grow, the 'entrenched cartographies' of species being, or the 'spaces of species' identity, were disturbed (see Whatmore (1997) for the ethico-political significance of this type of disturbance, and Ansell-Pearson (1997) for a detailed development of the 'reversals' in narrative that viroid lives can perform). Before I go on to look at the attempts to stall this 'superconductive' event (Clark 1997), I will summarize what this account of the experimental system of TSEs has provided.

First of all, the science of TSEs most definitely did not conform to a linear model of knowledge production where ideas were tested through experimentation. Rather, experimentation and the production of ideas developed hand-in-hand. Experiments could not be conceptualized as answering a set of pre-ordered questions. Indeed, there is little sense from the accounts and reports that the experiments were predominantly set up to adjudicate upon clearly stated questions. Which is similar to saying that decisions in the laboratory were not made by a purely cognitive agent, in control of his or her experimental system. Indeed, the ability to listen to, respond to and even feel (albeit through a range of media and instrumentation) the vagueness of the epistemic thing was a condition of possibility for the production of new knowledge. In addition, and out of this open reading frame, the experiments generated as many uncertainties and as much ignorance as they did answers. So, as the TSE research progressed, there was no clear relationship between the quantity of experimental time and the level of uncertainty and ignorance. If anything, ignorance regarding the disease itself, and disease in general, became more rather than less apparent. The central dogma of molecular biology was threatened, as were notions of biological agency, replication, biological information, infection, protein synthesis and folding (see Keyes 1999a,b). Meanwhile, scientific disagreement became more, rather than less, apparent as the research progressed.

Second, there can be no appeal to obdurate objects-in-nature in order to explain the experimental trajectory. From what I have already said, it should be clear that the epistemic things of the TSE experimental system were far from being *any*thing. They were not shapeless or hapless materials waiting only to be reformed, and subject only to the vagaries of narrative or 'social' fashion. Indeed, they could be said to have had a hand in fashioning the experimental system and were instrumental in experimental decisions. But, neither were prions a-social beings. If sociality means an ability to associate (see Latour 2000), and if such ability to associate is far from being a fixed property (so that we don't make the mistake of distinguishing between primary and secondary qualities – ibid.), then prions start to lose some, but not all, of their singularity. As their stickiness and 'impurity' testify, prions don't stand still. Furthermore, they can be social within and amongst a range of animal species. Indeed, their mobility across tissue and species boundaries has been one of the more challenging aspects of TSE history. More than this, prions are not simply relational matters, they can also act in ways that are characteristic of what Hetherington and Lee (2000), following Serres, have termed blank figures. In other words, TSEs generally, and prions more specifically, form one of a number of conditions of possibility for this particular set of (diseased) inter-species associations. In being able to cross species barriers, prions are, to some extent at least, constitutionally indifferent to their placement in specific or species orders. This means that they are motile as well as mobile (ibid). So, knowing prions is not simply a matter of drawing molecular diagrams, and labelling fixed properties (although this is part of it). And it is not simply admitting that there is more to know about that structure or set of properties. It is also the realization that prion sociability, and their role as facilitators of sometimes detrimental sociability, both in the laboratory and in other set-ups (including of course the industrial-agricultural set up), can *exceed* their known properties. Knowing prions is therefore a knowing of indeterminacy. It is this form of knowing, and the status of prions as epistemic things, that we should refuse to actively forget as the history of prion discovery is written. Likewise, geographies of prion associations can assist in generating a greater sense of the mutability, and therefore uncertainty, of non-human identities.

To be sure, and as the continuing search for 'pure' prions testifies, a knowing of indeterminacy does not always follow from experimentation (see Strand (2000) for a discussion of the durability of natural realism in molecular life sciences). But, this form of knowing, which rejects primary properties, and diverges from a geography of self-identical objects, was ruled out of court when experimental scientists were excluded from early discussions of BSE policy. In the next section, I turn to another aspect of the BSE policy process, this time focusing on the implementation of an industry-wide ban of feed-stuffs that was conceived and first enacted (if not wholly implemented) in 1988. The purpose here will be not only to develop further the relevance of these heterogeneous materialities, or social natures. It is also to address the third question, introduced in part 1, concerning the political forms that are invoked in policy situations where decisions are made in situations of appreciable uncertainty.

3. The politics of decisions

> [In 1992, a farmer from near York, who had reported what he suspected was a BSE affected calf] was visited by the first Ministry vet from Leeds, who said 'Yes, that is BSE' clinically put a restriction order on it, and a few days later . . a second Ministry vet took a cursory look at the animal and said: 'It is not BSE because it is born after the feed ban', and said to the farmer: 'Have it slaughtered and send it into the food chain if you want to use it.' (Richard Lacey's oral evidence, BSE INQ: tr980317, 93–94).

The ban, referred to in this excerpt, specified that ruminant derived protein would no longer be allowed to be fed to other ruminants (an industrial-agricultural practice that had been common for most of the twentieth century). The feed ban, which was introduced in 1988, has been widely presented as a success (Tyrrell and Taylor 1996; Harpold *et al* 1998), a clear example of acting with a duty of care, going further than the current evidence would have sanctioned, and, by doing so, succeeding in slowing the spread of disease in cattle. But, as the calves born after the feed ban attest, the precautionary policy did not halt the disease. The case also suggests that the second vet's confidence in the ban was misplaced, and that false assurances might have led to prolonged human and animal exposure to health risks. To understand something of that confidence and of the other conditions which made disease transmission possible after the ban was implemented, I need to engage in some detail with the policy decisions. In doing so I will tease out the deficiencies of a-social understandings of nature and of a-political conceptions of policy procedure. The following is therefore a partial history of a policy, in three parts. The first introduces some of the agonistic processes that are involved in making a policy. I argue that precautionary policies are a struggle to align all manner of discourses, materialities, institutions and knowledges, making them an inherently political exercise. Moreover, this struggle does not result in a consensus or agreement without there being distributions and exclusions. So, the second part highlights the distributions that were performed by the ruminant feed ban policy. Far from being 'total' (as it has been described to the BSE Inquiry by the Government's Chief Veterinary Officer from 1988 to 1997, Keith Meldrum – see BSEINQ: tr991110: 101), the policy assumed and then performed a disease geography. In other words, the policy depended upon a multitude of decisions over what and where did and what and where didn't matter. The third part looks more closely at the denial of this distributive or geographical work. In its presentation as the outcome of technical and internally consistent deliberation, based on an unsubstantiated though widely held view that the agency and materiality of the disease was both straightforward and predictable (conforming to determinable and universally applicable properties), the ruminant feed ban embodied a reduced sensitivity to the inevitable indeterminacies that both pre-exist and are brought into existence in the implementation of policy (see Wynne 1992). The example suggests that in order to be something other than hackneyed slogans, phrases like precautionary principle and open or deliberative decision-making need to be firmly set in a politically contested and contestable setting.

Conducting policy

At a meeting on 8 January 1988, the animal health experts at MAFF (Ministry for Agriculture, Fisheries and Food), along with the chief vet (then Howard Rees) and an epidemiologist (John Wilesmith) all agreed that BSE was becoming a significant disease. There were now 30–40 new cases each month and the trend in the rate of new cases was rising alarmingly. The epidemiologist presented a case arguing that disease transmission had primarily occurred through contaminated meat and bone meal (MBM) and possibly tallow in animal feed. The most probable source of this contamination was the entry of scrapie infected sheep, and possibly the re-cycling of BSE infected cattle, into the feed system. The feeding of ruminant remains to domestic, zoological and farm animals was not new (it had been going on for the most of the century), so it was the failure to de-activate the disease agents in the processing of animal feed that was the focus for concern. Having noted the hypothetical status of this prognosis, and after raising animal health and welfare concerns and the possible implications of the disease for human health, a precautionary approach was agreed upon. Those present would send a submission to the Agriculture Minister, recommending that clinically affected cattle should be slaughtered, prevented from reaching the food chain and farmers should be compensated to encourage

compliance. They also recommended that further work needed to be commissioned on the MBM hypothesis, with a view to producing a precautionary measure of removing ruminant derived protein from animal feed.

The submission's journey from the animal health division at MAFF to the Minister for Agriculture took 1½ months. On route, the paper work passed through a number of hands, including those of Permanent and Under Secretaries, who had responsibilities for adding cost estimates, checking the submission's compatibility with other agricultural policy, raising issues of a legal nature and so on. Could farmers be compensated from the public purse if no danger to human health had been demonstrated? Wouldn't this contravene the 1981 Animal Health Act? Did compensation set a precedent for a raft of other crop and animal disease problems and so effectively sanction further subsidization of the agricultural sector?[10] Could a ban on ruminant-derived feed be enforced in lieu of a definitive statement on the origins of the disease? What would the effect be on the feed industry and on farming practices? These questions were all appended to the submission in verbal and written form as it moved (see BSEINQ, RFA07).

The questions spoke of concern regarding financial costs to the public (MAFF was under severe budgetary restraint), and costs to the farming industry (as MAFF's Parliamentary Under Secretary put it 'the slaughter policy . . would undoubtedly have immediate detrimental effect on exports' BSEINQ: RFA07: 18). But the questions also exhibited nervousness over the status of uncertainties. The eventual recipient of the submission, John MacGregor, Agriculture Minister at the time, told the Inquiry that, '[he was conscious of] taking actions and then finding that they were not justified by the evidence that was coming forward; you would then be subject to legal actions' (BSE INQ: tr981202, 33). This kind of reasoning may have contributed to the decision to take no statutory action in February when the Minister first saw the submission. In contrast, 3 months later, MacGregor decided to act swiftly, at least with respect to the proposed feed ban (the slaughter policy would take even longer, as its sanctioning required non-agricultural allies in the Department of Health, see BSEINQ RFA07). The time had been spent attempting to 'firm up' the meat and bone-meal hypothesis (with little real success), and in discussion with feed manufacturers and renderers, legal advisers and accounting experts. Crucially, these discussions provided enough indication that the uncertainties over the cause and transmission of the disease would not interfere with the logistics and legalities of introducing a ban. In effect, sufficient allies had been made to start the implementation or better, the translation to action, of the proposed feed ban. As the term translation suggests, this alignment produced some subtle and not so subtle shifts in the ban's meaning.

The crucial issue for my purposes is that the production of policy is a struggle to align all manner of people, utterances, departments and knowledges. The alliances furnished the precautionary policy with stability, reduced its potential costs and also allowed for the broader distribution of responsibility across a range of agencies and institutions. A precautionary policy does not, therefore, survive or fall on its own merits. It is a networked achievement – with the result that as it is inserted into the complex juridical and financial world, changes will occur. This is not necessarily a bad thing, it need not always be considered as a watering down of content. But, as I will suggest in the next two subsections, any active forgetting that such changes have been performed results in a denial of the politics of decisions, a denial that can have disastrous effects.

Distributions

From February to May 1988, the feed ban policy and the MBM hypothesis were being 'firmed up', to use MAFF officials' terms. Despite acknowledged uncertainties with respect to the evidence from a series of tests on feed processing plants designed to demonstrate that the disease remained active during feed manufacture (the testing procedures were heavily criticized by industry representatives), the Minister for Agriculture recommended that precautionary action was needed. 'There was a need to be seen to be doing something (BSE INQ, RFA07, 35). To this end, 'a *complete withdrawal* of the material in question from rations for ruminants was the only safe option . . . the evidence pointed to a *speedy and compulsory* ban of sheepmeat material in feed for ruminants (BSE INQ: tr991110, 10–11, emphasis added).[11] There was *a* decision to act. But this statement was not as straightforward as it might at first read. Indeed, its success as *a* decision was in part a product of the *multiplicity* of the statement.

As I have already indicated, a good deal of work had already gone into networking the ruminant feed ban so that it didn't fall at the first fence. The Minister's statement was already, in a sense, a robust statement. It was robust in terms of the alliances that had been made, and, as importantly, in terms of its ability to be translated into action. For, in the months following the initial submission, some fairly stark distinctions, marking distributions between matters of relative importance, had been made and incorporated into the ban. A geography of the disease, of where it might strike, had been drawn up. As I will demonstrate, that geography conformed to a particular cartography of states, species and industrial practices. As I will also show, the sociable disease agent was drawing, and was increasingly able to draw, some rather different maps.

First, as a result of the pre-decision discussions, the '*complete* withdrawal' was not so complete. It had already been qualified by the words 'from rations for ruminants'. So potentially infected feed could still be manufactured if it was fed to non-ruminants. Pigs and poultry could continue to be fed with the suspected material. Meanwhile, at that time, the ban did not extend across the English Channel or over the Irish Sea (the lack of reported incidences of BSE was interpreted as no risk). So cattle, sheep and goats, along with other farm animals, could continue to eat suspected feed in other European states. In short, the feed industry could continue to manufacture the same ruminant-derived feed products, so avoiding costly changes to industrial and supply processes. Markets were being protected, an activity that performed a particular distribution or geography of animal disease-susceptibility. British sheep and cattle, fed protein rich supplement, were the critical group. All others, including pigs and poultry, foreign ruminants, ruminants fed in field (even on land fertilized with MBM derived products), domestic pets and most zoological animals were 'spared' the feed ban.

Second, despite all of this pre-statement distributive work, the Minister's decision soon ran into trouble when it came to worrying further about implementation. As one MAFF official told the inquiry, 'the ban was completely unenforceable if it was not voluntarily obeyed (BSE INQ, t980623, 136). The assumption was that implementing an immediate, compulsory ban would meet huge resistance from the farming industry, with possible legal challenges and compensation claims. The lack of scientific evidence was such that the outcome of any legal challenge would be uncertain. So MAFF officials sought to promote a degree of voluntary self-interest within the industry and engender a sense of deferred responsibility.[12] In effect, the ban was not presented as *compulsory.*

Third, this inability to enact a compulsory ban demanded an even greater degree of agreement across government and the farming industry as to the importance of the ban. A series of meetings was held in late May and with increasing frequency in early June in order to discuss the ban with the feed industry, wholesalers and farmers groups. In the course of the discussions, several clarifications to the ban became apparent. As well as not being *complete*, and no longer being strictly *compulsory*, the ban would not involve the *withdrawal* (in the sense of 'recall') of ruminant derived feed. The financial, legal and logistical barriers to a recall of stock were regarded by MAFF officials to be insurmountable. This meant that manufacturers, compounders, wholesalers and farmers would be able to use up pipeline stocks. In turn, the ban was not *speedy*. Pipeline stocks could be delivered to farmers for use by 18 July, a period of 1½ months after the ban was announced. This 'period of grace' was arranged to allow the industry to adjust its practices. There is every indication from minutes of meetings that the period would have been longer had MAFF's information division not raised the matter of another important ally in making the ban work. They suggested that any further extension to the ban could threaten what had so far been an favourable press reception (BSE INQ: RFA07, 55).

In the pre-ban discussions, and in further discussions over implementation, alliances were being made that had geographical consequences. The time-spaces of the disease were being mapped across states, across species, and across the everyday workings of a farming industry with its complex supply rhythms and modes of ordering. In hindsight, it is easy to state that it was an 'unrealistic' mapping. Factories and farms continued to provide conditions for disease transfer. Feed for non-ruminants continued to be mixed with feed for ruminants. Pipeline feed stocks took a long time to be used up (partly as a result of summer pasture feeding). Meanwhile, the temporary and voluntary status of the ban translated into non-urgent action in some quarters. The list of potential boundary crossings, from states to farms to tissues and cells,

is a long one. The disease was sociable in ways that were not anticipated in the ban, but also in ways that the ban itself facilitated (in for example, the continued exports, and in the cross contamination of feed stocks that were nevertheless labelled as fit for ruminants).

In order to explain this cartographical failure it would be conventional to name and shame the usual (social or human) suspects (see Serres 1995). It might be argued that plural interests, represented in the meetings and memos, produced an agreement with which everyone could live. Indeed, there can be little doubt that some MAFF officials were concerned to protect the commercial interests of the various parts of the feed industry (especially exports), and that various elements of the feed industry lobbied MAFF officials publicly and privately. To be sure, there is no intention here to let these characters off the hook. But there may well be more to the ruminant feed ban than this, and possibly more to politics than the representation of and negotiation between established interests. To demonstrate this, I want to make two arguments. First, the policy owed as much to dialogics as it did to the representation of interests. Second, in order for a decision to be reached through a dialogue, certain assumptions about the material character and properties of the disease became established. Thus, to reach agreement, the discussions seemed to require a 'naturally' stable understanding of the disease. Any knowing of indeterminacy associated with the epistemic object of BSE was actively erased as consensus was developed. My argument will be that such an erasure might have been avoided. Furthermore, by keeping indeterminacy in the foreground, it might have been more difficult to discount the contested geography and politics of the food production business.

Dialogical decisions and doses

In the hubbub of deliberation, 'interests' look less clear than might be expected. Elements of the industry, for example, were both wary of the ban but were also only too aware of the dangers of refusing to comply. Dangers included shifts in public and consumer sympathy and the potential for liability should they refuse to fully implement the ban. Parts of government, meanwhile, were concerned that industry would not co-operate, at the same time as being afraid that they would implement the ban with too much vigour, so raising the public profile of the disease (see BSE INQ: stat24c, 5). Further, and to compound this difficulty of mapping interests on to interest groups, the latter were far from being uniform entities. Industry, for example, was made up of a complex set of elements and relations. Renderers, compounders, farmers, unions, breeders, meat producers, pet food manufacturers, multi-nationals, private abattoirs, and so on could not be relied upon to toe one particular line. Even discrete industry associations like UKRA (the UK Renderers Association) and UKASTA (the UK Agricultural Supply Trade Association) found it difficult to represent the full range of their members' interests, and could not, of course, represent those renderers and agricultural suppliers that lay outside their organizations. Similarly, MAFF included laboratory scientists, vets, accountants, permanent civil servants and government ministers who were organized into a fairly robust hierarchical structure, but one that did not necessarily coalesce into an interest group. What this suggests is that maintaining a steady set of interests and matching these to desirable outcomes was by no means a straightforward task. Rather, interests, means and outcomes shifted as uncertainties were discussed, deliberated and incorporated into further actions. And, as many witnesses told the inquiry, the speed of scientific and political developments was such that it was impossible to settle interests and to make clear representations during key moments in the crisis.

There can be little doubt that those involved in policy discussions negotiated what might be in their interests. In other words, interests were a relational achievement. Parties second-guessed their counterparts' reactions to matters that they might put forward, a form of 'double-voiced' dialogue (Holloway and Kneale 2000, 76). So, for example, by initially making the ban temporary, and by not insisting on complete withdrawal, officials were hoping to win over industry, thinking that anything more would jeopardize the whole operation. In turn, this temporary status of the ban along with the decision not to withdraw feed had effects on how industry saw their interests. It meant that when industry representatives asked for an extension to the period in which they could use pipeline feed, they did so, in part, because they assumed that officials considered the disease a non-urgent problem (BSE INQ: Stat024C: 2). The ban's geography was therefore not solely a matter

of pre-existing interests, but was also an outcome of a dialogue and of second-guessing the other's motives.

Most importantly for my purposes here, this geography was dependent on another set of dialogically performed translations. In the midst of all this uncertainty, the coalescence of interests was facilitated by the storying into existence of a stable, materially homogeneous, disease agent. As the ban was discussed, a shared understanding of disease transmission was produced. This was particularly evident at a series of meetings held just as the ban was to be introduced. At a meeting of MAFF officials and industry representatives on 1 June 1988, the latter raised questions concerning the assumed aetiology of BSE. Given that the disease had not, at that time, been reported in other countries, which nevertheless received exports of British produced feed, how could feed be the cause? The newly appointed Chief Vet, Keith Meldrum, argued that as Britain exported relatively small amounts of this material and that most of the exports were probably not fed to bovines, it would have been unlikely for cattle abroad to have received *sufficient material* to develop the disease (BSE INQ: RFA07: 41, emphasis added). Nine days later, at a separate meeting, the UKRA representatives repeated the question, this time referring in particular to the absence of BSE in Ireland where there were similarly high levels of scrapie and MBM was processed in the same way. The Chief Vet explained the apparent discrepancy by referring to the amount of material consumed by cattle, saying 'BSE was *dose related* and . . . Irish cattle had probably not had sufficient exposure for it to develop' (BSE INQ: RFA07, 43, emphasis added). Finally, in meetings with the Federation of Fresh Meat Wholesalers and the National Cattle Breeders Association, both of whom had expressed confusion that MBM could still be incorporated into fertilisers and ingested by grazing cattle, the CVO stated again that BSE was dose-related and very little would be consumed in this fashion (BSE INQ: RFA07, 45).

In short, there was an assumption that, in order for an animal to contract the disease, 'it had to ingest a massive dose of infected material' (MAFF animal health expert, BSE INQ: tr980612, 146). This account of the disease was sufficient to allow officials to explain various aspects of the ban to others and to themselves. The continued exports, the manufacture of ruminant-free animal feed and potentially infected feeds on the same production lines, the period of grace and the use of MBM in fertilisers – all of these made sense if the dose required for the transmission of the disease was 'massive'.

This shared understanding caused some bemusement at the Inquiry. In part this is because there are records of meetings, held prior to the formulation of the ban and with senior MAFF officials present, where uncertainties over dose characteristics were expressed, and even where the idea that the dose of infective material could be very small indeed was mentioned. For example, on the 4 March 1988, John Wilesmith, the epidemiologist, told a meeting of MAFF officials and industry representatives that a possible transmissible agent had been discovered, and that the effective dose of the disease was very small (BSE INQ tr981019: 64). Interestingly, though the meeting was chaired by Keith Meldrum, a senior vet and soon to be chief vet at MAFF, the government have no record of this presentation (the minutes were supplied by the rendering association). During the Inquiry Mr. Wilesmith has gone further to suggest that an undisclosed officialdom 'were perfectly aware of what the likely amount, that being a small amount, was necessary to infect animals' (BSE INQ, tr980731, 15). Partly in response, Keith Meldrum has denied that such knowledge was widespread. Citing discussions he was having at the time with Richard Kimberlin (a well known detractor from the prion protein -hypothesis, see earlier), Meldrum has suggested that 'we simply did not know what was the dose necessary to cause disease . . . or indeed the titre necessary in that dose . . . I was not in a position to make an informed judgement on the issue and I did not attempt to do so' (BSE INQ: tr981019 67). In a later hearing, Meldrum went further to suggest that the possibility of a small dose being sufficient to transmit the disease 'was not the view, the *corporate view* that we held at the time based on the advice that we received from the experts. We did not know. And I do not think John [Wilesmith] knew either, frankly (BSE INQ: tr991110, 107, emphasis added).

The 'corporate view', apart from being a reflexive expression used in an Inquiry setting to signal the dispersal of accountability, was one of ignorance. And yet, in following the policy discussions and deliberations, the idea of a massive dose requirement inhabits conversations and decisions. So how did a critical area where there was clear

scientific disagreement, and significant scientific uncertainty, develop to become a largely unquestioned view that the required dose for transmission was massive? Senior vets at the Inquiry could not explain why they had assumed such a view to be reasonable at the time. Alan Lawrence, from the animal health division at MAFF, told the inquiry that he was confused as to how such an assumption could have arisen. He added, in his defence, that his understanding had followed from a broad consultation with veterinary and administrative colleagues (BSE INQ: Rfa07up: para 161a, and tr980612, 145).

This translation of uncertainty concerning the transmission of a disease into an assumed material character is indeed difficult to explain. As the bemusement of Lawrence and others at the Inquiry attests, this was not a case of a simple scientific misunderstanding. Nor does it seem likely that this was an elaborate and preconceived obfuscation of the 'facts'. Nevertheless, as I have already suggested, it does seem likely that the translation of dose characteristics was politically expedient. It helped to anchor the coalescence of emerging interests. The easiest way for MAFF officials to sell the policy to industry and to generate a necessary degree of consensus was to appeal to an impartial arbiter, a knowable nature. This would save a lot of time compared to the degree of painful explanation that would be required to haul the policy back over the political orderings and distributions that had made the decision possible. Whether or not my interpretation is justified, or even reads as far too functional in itself, the *effect* of the dose clause was to close down debate, lending false legitimacy to preceding decisions and distributions. It is an example of the ability and possibly the will to shortcut due political process by referring the case to a non-social body (see Latour 2000). In turn, it should be remembered that the externally and impartially conferred authority of the policy contributed to the production of new conditions of possibility for disease mobility and motility. Perhaps most significantly, the continued assumption of dose dependency meant that cross contamination between feed lines was largely ignored. As ruminant derived feed, possibly containing scrapie and BSE infected MBM, could be fed to non-British, and/or non-ruminant animals for some time to come, there was potential for material from one product line to cross over into another. This particular danger was in fact considered in 1988, but it was not regarded as significant given that the disease was understood to require larger doses than this process could generate (see Meldrum on this mismatch between uncertainty and action, BSE INQ stat184e). It was not until 1994 that the significance of feed mill cross-contamination was addressed.

There are two implications of this work that I would like to highlight. First, the indeterminacies of natural–social relations can easily be sidelined in current constructions of deliberation and policy formulation. In seeking to form a consensus, the materiality of the disease was reduced to an external anchor or arbiter. This recourse to a predictable and determinate material world impeded an open discussion of the politics of decisions, their distributions and exclusions. In contrast, placing a knowing of indeterminacy in the foreground of policy considerations may well have helped to impede such a singular version of dosage, and in turn, helped to emphasize the political construction of decisions. It may have also led to a more thorough questioning of the disease mappings that relied upon a questionable cartography of species, states and agricultural-industrial practices.

Second, in order to understand the construction of policy, attention needs to be paid to the conduct, and more specifically, the dialogical character of policy-making. Interests and understandings were not simply brought to the discussion table. They were, at least in part, formed as discussions ensued. So, the final question I want to ask is this; would a wider constituency have produced a better policy? My answer would be that extending the discussion and broadening the debate may well have introduced different concerns. Matters that MAFF officials and industry representatives together would have found it difficult to consider may well have made it on to agenda (including moral questions concerning feeding practices, increased concern for animal welfare, lay understandings of risk and so on. See Macnaghten and Urry (1997) and Wynne (1996) for such arguments). However, this inclusion of other stakeholders, values and ideas would not, in and of itself, guarantee that the resulting policy would have succeeded. Indeed, if broader discussion, in the form of deliberative democracy, continued to have as its aim the production of rational argument and impartially sanctioned agreement, which was not mindful of the inevitable distributions and exclusions that such agreements perform, then there would be

every chance that similar failings would be reproduced. The case of the dose agreement is symptomatic of a process that requires impartial solutions to politically difficult problems. These are problems where conflict and disagreement may be best handled by keeping them alive, rather than burying them under the false hope of impartial arbitration. The contention here is simply that the forging of a policy decision through an appeal to an apparently uncontroversial material property is akin to resolving differences through 'rational deliberation thanks to the adoption of an impartial standpoint' (Mouffe 1999a, 5). We need to recognize that inclusion and representation are not the only ways of doing environmental politics. Some people and things will necessarily remain 'outside', and those 'inside' will not have their differences and antagonisms erased. To imagine otherwise is, as I have detailed in this example, to perpetuate what is effectively an anti-democratic notion of social and natural control.

Conclusions

Non-human geographies have recently unfastened nature from its foundational moorings and loosened its fixed identity, at the same time as demonstrating a 'wildness' that is not reducible to extant social orders. And yet, despite some important academic and political interventions, environmental policy making tends to work with a more staid natural order. Even the welcome developments in precautionary and participatory approaches to environmental decisions tend to underplay the possibility of natural indeterminacy. In short, there remains a tendency to assume that agreement or consensus on issues of policy can be produced by referring the case to an existing, or, once temporary uncertainties are banished, soon to be existing, natural object. I have demonstrated in this paper that such objects fail to materialize in this form. Even in experimental set-ups where singular versions of nature might be expected, controversies over the materiality of TSE diseases remained. In addition, I have made a case that the assumed existence of incontestable natural entities can contribute to environmental problems. Such objects are a means of short-cutting due political process (Latour 2000). In the dosage case, assumed properties legitimated a map or geography of the disease that was effectively drawn for economic and political purposes. Moreover, this geography relied upon cartographies of disease that bore little relation to the sociable mappings of BSE. Species and state boundaries, and the space-times of industrial-agricultural practices, were no match for the mobility and motility of the disease.

In attempting to explain something of these shortfalls in policy-making, and to evaluate policy recommendations made in the wake of the BSE crisis, I highlighted three questions regarding the geographical practices, the natural realism and the spatial politics of environmental decision-making. First, by engaging in detail with a portion of the science and policy-making that accompanied the BSE crisis I have provided an account which takes the situated conduct of science and of policy seriously. In following the science and policy in action, and in particular by following some of the socio-materialities that made up the disease, I have drawn attention to the *events* that mark science and policy practices. In emphasizing this potential creativity, I have also demonstrated how experimental and policy systems managed to generate a form of indeterminacy that is rarely considered in policy debates (and was excluded early on from the science-led BSE debates). Rather than a temporary affair, uncertainty seems an endemic aspect of the practices that I have followed. Indeed, by following the material relations that were co-produced during the pursuit of pure agent, the construction of primary properties and in the attempts to shortcut due political process (Latour 2000), I have suggested that indeterminacy was ironically produced at the same moment as its denial.

Second, I have demonstrated that living and non-living nature (and the boundaries between such are not easily drawn) that inhabits experimental and agricultural-industrial spaces, is not one of a kind. Nor is it social in the conventional, 'social science', appropriation of the term. But it is sociable. This may not be a recipe for an infinite multi-naturalism, but it does suggest the importance of 'things' that are able to 'object' to science and policy-making (Latour 2000), and object in ways that, by definition, are open to change. Again, the emphasis of such a conclusion lies in the knowing of indeterminacy of these epistemic things. Which is also to say, contrary to the conclusions of the BSE Inquiry, that our failure to get the right policies was not simply a matter of misrepresenting the science (although that was part of it). It

was also a matter of scientists and others failing to translate a knowing of indeterminacy into the political process.

Third, precautionary and deliberative models of decision-making can miss the multitude of distributions, inclusions and exclusions, which are endemic to experimental and policy situations. In their appeal to externally sanctioned agreement these political forms tend to deny the struggles to produce policy statements and the provisional though important 'crystallization of power relations' (Mouffe 1999a, 5) should such statements be accepted. To be sure, this is not to say that the non-agreeable matters (the human-nonhuman relations that are formed and excluded in the performance of a distribution, or a decision) can or should be included in future decisions or agreements. In any case, 'they' may not even pre-exist the distribution or decision, making their inclusion nonsensical. Rather, and given the inevitability of distributions, the argument pursued here would be that to continue the quest for impartially sanctioned decisions is to miss the significance of taking decisions. It is the pursuit of closure, rather than simply the exclusion of voices, that is antidemocratic. Further, it is to suggest, along with Wynne, that the decisionism that inhabits so much environmental policy thought and practice is antirelational and ahistorical. Whilst Wynne is referring to more explicitly technical decision-making forms than have been dealt with in this paper, a dose of participative democracy and deliberative decision-making does not, it seems to me, alter the statement that: decisionist approaches leave 'no room in principle for recognising that the assumed object of the decision may itself be a contingent human construct which excludes other legitimate concerns, values and experiences – even if these may not be easily articulable' (1997, 148). I would only add that the material-heterogeneity and therefore mutability of such an object may also be excluded to our cost.

Finally, in this paper I have endeavoured to make empirically informed theoretical contributions to geographical concerns over policy relevance, human–nonhuman relations and the pursuit of inclusive forms of environmental politics. In terms of policy debates, I have demonstrated that what may seem to be esoteric and theoretical pursuits in academic geography are vital to certain areas of policy-making. Indeed, it can be productive to engage political and policy problems with something other than modes of thought which are solely functional, and therefore limited to a particular machinery of activity – functives as Deleuze and Guattari (1994) term them – and that inhabit so much policy practice. The openness of epistemic things and of a knowing of indeterminacy can do some serious work in guiding policy practice (for a similar argument couched in a different language see Amin, Massey and Thrift 2000). Meanwhile, and in terms of nonhuman geographies, the paper has started to map an alternative to the social construction/natural reality contest. I have experimented with natures' (plural) sociability and hence mobility and motility to start to outline what Latour calls social realism (1999 2000). I have also used Latour's (2000) sense of objectivity and Rheinberger's (1997) epistemic things to underline the productiveness of other than human embodiments. Rheinberger's utilization of notions of excess has enabled me to emphasize the prospect of novelty and events in human–nonhuman relations. Finally, I have made a start in rejecting any simple spatial register that seeks to include the excluded in environmental politics. I have started to trace a politics and spatiality of decisions that speaks of distributions, divisions, associations, antagonisms, differences as well as exclusions and inclusions. The result is a sense of the production of a range of social and spatial relations that cannot easily be wished away through an appeal to a time- or space-less logic, a universal matter or nature. Imagining that decisions can be anything other than fraught and contested, or that they can be materially neutral, is to imagine a world without difference. It is also to refuse those moments when natural and social sciences produce a knowing of indeterminacy.

Acknowledgements

Thanks to Ron Martin, John Allen, Nick Bingham, Martin Gren, Dave Featherstone and Doreen Massey and three anonymous referees for their advice in re-drafting this paper. Also thanks to seminar audiences at the Open University (Science Education), University of Southern California, UCLA, Kings College, London and Hull and at the University of Wales for listening to sometimes much earlier versions of this and for asking demanding questions. Visits to the BSE Inquiry were made possible by a small research grant from the Geography Discipline at the Open University.

Notes

1 The lack of social scientific evidence is not necessarily a fault of the Inquiry team alone – there were several open invitations to contribute evidence to the Inquiry and I can only assume that most social scientists (with the exception of Time Lang) did not take up the opportunity. However, in the social scientists' (and my) defence, the Inquiry did have an air of absolute functionality about it, which would have made social science interventions difficult to handle. I return to this notion of functionality in later sections of the paper.

2 The term realism is being used in a particular way here. Preferring to reconfigure a realist approach to environmental issues and/or nature is not meant to invite accusations of idealism or voluntarism. Rather, it is meant to dispel those accounts of the natural that imagine natural reality as something that humans do not take part in. Likewise, reality should not be considered as something that humans can wholly determine. The co-production of reality, in ways that will rarely be characterized by an equitable share of the spoils, is closer to the process that I want to convey in this paper. See Latour (1999, 2000) for an extended discussion of reality in relation to the science studies literature.

3 I take it that Mouffe's arguments are fairly specifically referring to a modern, Western version of democracy and so I am bypassing the charge that this part of her argument treats particular forms of conflict as universal features of social conduct. See Massey (1995) for a critical reflection on this point.

4 Thanks to Sarah Whatmore for highlighting this procedural and methodological issue.

5 There is an interesting parallel between the narrative forms and metaphorical devices that inhyabit molecular biology and standard theories of knowledge and practice. The conventional understanding of 'ideas make science make policy' clearly inhabits a similar informational world.

6 Rheinberger's texts are also clear as to the role of such experimentation in the construction of historical and social knowledge forms. It is a deficit of my own paper that I haven't sufficiently reflected on the process that led to some of the arguments being developed. I would only add that the long periods of assembling relevant texts, following paper trails, attending Inquiry days, trying out and triangulating arguments and drafting papers amount to something akin to the materially heterogeneous process that Rheinberger and others associate with scientific experimentation.

7 The term is borrowed from Derrida, and is used to evoke 'the temporal characteristics of any signifiying activity as a process without definitely assignable origin or ground' (Rheinberger 1997, 239).

8 Experimental social science is similarly a machine for generating futures. There is something to be learned from experimental natural sciences in this respect. Adopting an open reding frame and allowing the 'objects' of research to object to the trajectory of the research is a vital lesson (see Latou 2000). As is the ability to write this research in ways that retain a degree of openness (a lesson that is even harder to learn – thanks to Doreen Massey for pointing out the question-answering style that still drives many parts of this paper).

9 A similar resistance to scientific work that had contravened Watson's linear information flow version of the central dogma is evident in the early reception of work on reverse transcriptase, whereby virus RNA can encode sequential information into host DNA before the normal sequence of replication proceeds. Such work was largely ignored according to the experimental scientists, Howard Temin – see Keyes 1999a, 7.

10 Of particular concern was rhizomania, a disease of sugar beet, which was considered to have no possible link to human ill health, and therefore farmers had not been able to claim compensation on destruction of the crop. If compensation was paid on BSE cases in the absence of a statement on the potential risks to human health, then this might provoke framers to claim for similar treatment over rhizomania.

11 The material is being quoted from a minute of a meeting held on 18 May, following a submission by Howard Rees, then CVO, to the Minister on 6 May 1988. The reference to sheepmeat may well have been a slip, as the intention seems to have been to ban bovine, ovine and caprine derived material from ruminant feed. Indeed, the ban stipulated all three at a later stage (BSEINQ 991110, 23).

12 MAFF officials told members of the industry that failure to implement the ban could lead to an intensification of the disease, would endanger livelihoods and livestock and could even result in civil prosecution in the future. Regarding the latter, there were recent well-known precedents where the industry's failure to act had led to civil actions that had cost the feed manufacturers dearly. (An example being the case of Newcastle Disease, see BSE INQ: tr980623 for a description of this approach from members of MAFF's animal health division).

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