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# DISCUSSION PAPER

**ABSTRACT** Science studies has shown us why science and technology cannot always solve technical problems in the public domain. In particular, the speed of political decision-making is faster than the speed of scientific consensus formation. A predominant motif over recent years has been the need to extend the domain of technical decision-making beyond the technically qualified élite, so as to enhance political legitimacy. We argue, however, that the 'Problem of Legitimacy' has been replaced by the 'Problem of Extension' – that is, by a tendency to dissolve the boundary between experts and the public so that there are no longer any grounds for limiting the indefinite extension of technical decision-making rights. We argue that a Third Wave of Science Studies – Studies of Expertise and Experience (SEE) – is needed to solve the Problem of Extension. SEE will include a normative theory of expertise, and will disentangle expertise from political rights in technical decision-making. The theory builds categories of expertise, starting with the key distinction between interactive expertise and contributory expertise. A new categorization of types of science is also needed. We illustrate the potential of the approach by re-examining existing case studies, including Brian Wynne's study of Cumbrian sheep farmers. Sometimes the new theory argues for more public involvement, sometimes for less. An Appendix describes existing contributions to the problem of technical decision-making in the public domain.

**Keywords** contributory expertise, interactive expertise, legitimacy, public, technical decision-making, sheep farmers

# The Third Wave of Science Studies:

## Studies of Expertise and Experience

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> **lay′man** one of the laity; a non-professional person; someone who is not an expert.[1]

## The Problem of Legitimacy and the Problem of Extension

Technical decision-making in the public domain is where the pigeons of much recent social science are coming home to roost. The problem can be stated quite simply: Should the political legitimacy of technical decisions in the public domain be maximized by referring them to the widest democratic processes, or should such decisions be based on the best expert

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advice? The first choice risks technological paralysis: the second invites popular opposition.

By 'technical decision-making' we mean decision-making at those points where science and technology intersect with the political domain because the issues are of visible relevance to the public: should you eat British beef, prefer nuclear power to coal-fired power stations, want a quarry in your village, accept the safety of anti-misting kerosene as an airplane fuel, vote for politicians who believe in human cloning, support the Kyoto agreement, and so forth. These are areas where both the public and the scientific and technical community have contributions to make to what might once have been thought to be purely technical issues.

Like many others, what we want to do is consider how to make good decisions in the right way. But our particular concern is to find a rationale which is not inconsistent with the last three decades of work in science studies. Our initial claim is that though many others working within the science studies tradition have studied the problem, and contributed valuably to the debate about technical decision-making, they have not solved it in a way that is completely intellectually satisfying. For us to claim to have solved anything would be to give a hostage to fortune, but we think we can indicate, firstly, the reasons why there may be grounds for both academic and political discomfort and, secondly, a direction in which the work might go.

This paper is not about social relations between scientists and society. For example, it is not about whether scientists are trustworthy, or whether they behave in a way that inspires trust in the public, or whether the institutions through which their advice and influence are mediated inspire trust. At least, in so far as the paper is about these things, it is only indirectly about them. What it is about is the reason for using the advice of scientists and technologists in virtue of the things they do *as* scientists and technologists, rather than as individuals or as members of certain institutions. In other words, it is about the value of scientists' and technologists' knowledge and experience as compared with others' knowledge and experience. The dominant and fruitful trend of science studies research in the last decades has been to replace epistemological questions with social questions, but we return to a rather old-fashioned approach, asking about the grounds of knowledge. What is different here, as compared with the debates about the the grounds of knowledge that took place before the 'sociological turn' in science studies, is that we try to shift the focus of the epistemology-like discussion from *truth* to *expertise and experience*. We think we need to start pursuing 'SEE' – Studies of Expertise and Experience.

One of the most important contributions of the sociology of scientific knowledge (SSK) has been to make it much harder to make the claim: 'Trust scientists because they have special access to the truth'. Our question is: 'If it is no longer clear that scientists and technologists have special access to the truth, why should their advice be specially valued?' This, we think, is the pressing intellectual problem of the age.[2] Since our answer turns on expertise instead of truth, we will have to treat expertise in the same way as truth was once treated – as something more than the judgement of history, or the outcome of the play of competing attributions. We will have to treat expertise as 'real', and develop a 'normative theory of expertise'.[3]

To those who share our feelings of political and academic unease with the existing situation in science studies, we want to suggest that the problem lies with the tension described in our first paragraph: the tension between what we will call 'the Problem of Legitimacy' and 'the Problem of Extension'. Though science studies has resolved the Problem of Legitimacy by showing that the basis of technical decision-making can and should be widened beyond the core of certified experts, it has failed to solve the Problem of Extension: 'How far should participation in technical decision-making extend?' In other words, science studies has shown that there is more to scientific and technical expertise than is encompassed in the work of formally accredited scientists and technologists, but it has not told us how much more.

To save misunderstanding, let us admit immediately that the practical politics of technical decision-making still most often turn on the Problem of Legitimacy; the most pressing work is usually to try to curtail the tendency for experts with formal qualifications to make *ex-cathedra* judgements curtained with secrecy. Nevertheless, our problem is not this one. Our problem is academic: it is to find a clear rationale for the expansion of expertise. But a satisfying justification for expansion has to show, in a natural way, where the limits are. Perhaps this is not today's practical problem, but with no clear limits to the widening of the base of decision-making it might be tomorrow's. It is just possible, of course, that setting a limit on the extension of expertise will soothe the fears of those who resist any widening of participation, on the grounds that it will open the floodgates of unreason. It is just possible, then, that this exercise will help with today's practical problems, even though we approach the matter with a different aim in view.

## *Painting Waves with a Broad Brush*

This is in some ways a polemical paper, and we proceed in a direct manner. We start by sketching idealized models of what we call 'three waves' of science studies. Violence is often done when one compresses the work of many authors and thinkers into a few simple formulae, as the ludicrous accounts of SSK associated with the 'science wars' show us. 'Ask not for the meaning but the use', Wittgenstein tells us; but here we are setting out meanings with a somewhat cavalier attitude to use. So we apologize to all the contributors to these movements whose work we caricature, and hope the violence is not too great; fortunately, the project depends not on historical or scholarly accuracy, but on sketching the broad sweep of ideas.[4]

If what we paint with a broad brush is not totally unreasonable, then it shows that the First Wave of Science Studies had no Problem of Extension, and was unaware of the Problem of Legitimacy. It shows why the Second Wave of Science Studies was good for solving the Problem of Legitimacy that it inherited from Wave One, but replaced it with the Problem of Extension. We propose that the Third Wave of Science Studies (and we might only be labelling a movement that already exists in embryonic form) should accept the Second Wave's solution to the Problem of Legitimacy, but still draw a boundary around the body of 'technically-qualified-by-experience' contributors to technical decision-making.

To show that our argument is more than a programmatic gesture, we will indicate one way to start to build a normative theory of expertise, and what it would mean for technical decision-making. There are, no doubt, many other ways to go about such an exercise, but to focus attention on the goal by providing an example of one approach to it is at least a start.

### *Language and Presentation*

Though we are going to talk about widening participation in technical decision-making, we will abandon the oxymoron 'lay expertise'.[5] As we see at the head of this paper, the dictionary definition of 'layman' includes the sentiment 'someone who is not an expert', and this makes it all too easy to over-interpret the term 'lay expertise'. If those who are not experts can have expertise, what special reference does expertise have? It might seem that anyone can be an expert. We say that those referred to by some other analysts as 'lay experts' are just plain 'experts' – albeit their expertise has not been recognized by certification; crucially, they are not spread thoughout the population, but are found in small specialist groups. Instead of using the oxymoron, we will refer to members of the public who have special technical expertise in virtue of experience that is not recognized by degrees or other certificates as 'experience-based experts'.

Since all humans have enormous expertise in language speaking and every other accomplishment that requires an understanding of social context, the term 'experience-based expertise', if it is to do any work in this context, has to be used to refer to specialist abilities. To use the term to mean something more general would strip it of its power to solve the Problem of Extension.[6]

The nature of the exercise means that we need to move swiftly into our arguments and therefore, in the main, we discuss earlier work which bears upon them in an Appendix (pp. 272–83, below). The main body of the paper still contains some references, acknowledgements, and discussions of previous work (mostly in the Notes), but the Appendix deals more fully, if not exhaustively, with the existing literature. It shows where we agree and disagree, in a substantive way, with certain others who have looked at the same problems, and where our approach differs markedly from work which may, at first glance, look similar. And the Appendix shows, if it needed showing, that the idea of a *normative theory of decision-making* has been discussed by a long line of distinguished scholars. It shows, then, that in so far as we are trying to do something new, our aim is modest – to try to discover a systematic rationale for a *normative theory of expertise* that is compatible with SSK, and that contributes to the normative theory of decision-making that others have essayed. The Appendix follows the major section headings of the main text, in so far as that is possible.

## Three Waves of Social Studies of Science

### *The First Wave of Science Studies*

To simplify outrageously, let us say that there was once what seemed to many to be a golden age before 'the expertise problem' raised its head. In the 1950s and 1960s, social analysts generally aimed at understanding, explaining and effectively reinforcing the success of the sciences, rather than questioning their basis.[7] In those days, for social scientists and public alike, a good scientific training was seen to put a person in a position to speak with authority and decisiveness in their own field, and often in other fields too. Because the sciences were thought of as esoteric as well as authoritative, it was inconceivable that decision-making in matters that involved science and technology could travel in any other direction than from the top down. This wave of 'positivism' began to run into shallow academic waters in the late 1960s with Thomas Kuhn's book and all that followed. By the end of the 1970s, *as an academic movement*, it had crashed on to the shore.[8]

### *The Second Wave of Science Studies*

The following wave of science studies, which has run from the early 1970s, and continues to run today, is often referred to as 'social constructivism', although it has many labels and many variants. One important variant is the sociology of scientific knowledge (SSK). What has been shown under Wave Two is that it is necessary to draw on 'extra-scientific factors' to bring about the closure of scientific and technical debates – scientific method, experiments, observations, and theories are not enough. With science reconceptualized as a social activity, science studies has directed attention to the uses of scientific knowledge in social institutions such as courts of law, schools, and policy processes such as public inquiries. The emphasis on the 'social construction' of science has meant, however, that when expertise is discussed, the focus is often on the attribution of the label 'expert', and on the way the locus of legitimated expertise is made to move between institutions.

By emphasizing the ways in which scientific knowledge is like other forms of knowledge, sociologists have become uncertain about how to speak about what makes it different; in much the same way, they have become unable to distinguish between experts and non-experts. Sociologists have become so successful at dissolving dichotomies and classes that they no longer dare to construct them. We believe, however, that sociologists of knowledge should not be afraid of *their* expertise, and must be ready to claim their place as experts in the field of knowledge itself.

Sociologists of knowledge must be ready to *build* categories having to do with knowledge; we must be ready, then, to develop a 'knowledge science' using knowledge and expertise as *analysts' categories*. SEE, the Third Wave of Science Studies, is one approach.

*Downstream to Upstream*

An important strand in our argument is to indicate the compatibility of a normative theory of expertise with what has been achieved in Wave Two. The relationship between Wave One and Wave Two is not the same as the relationship between Wave Two and Wave Three. Wave Two replaced Wave One with much richer descriptions of science, based on careful observation and a relativist methodology (or even philosophy). Wave Two showed that Wave One was intellectually bankrupt. Wave Three, however, does not show that Wave Two is intellectually bankrupt. In this strange sea, Wave Two continues to roll on, even as Wave Three builds up.[9] Wave Three is one of the ways in which Wave Two can be applied to a set of problems that Wave Two alone cannot handle in an intellectually coherent way. Wave Three involves finding a special rationale for science and technology even while we accept the findings of Wave Two – that science and technology are much more ordinary than we once thought. The aim of this paper, one might say, is to hammer a piton into the ice wall of relativism with enough delicacy not to shatter the whole edifice (the destruction that so many critics believe is the only solution).

To be willing to find a rationale for a special place for science and technology, now that so much has been deconstructed under the Second Wave of Science Studies, means *reconstructing* knowledge. As we have said, the Third Wave of Science Studies must emphasize the rôle of expertise as an *analyst's category* as well as an *actor's category*, and this will allow *prescriptive*, rather than merely *descriptive*, statements about the rôle of expertise in the public sphere.

The shift to a prescriptive theory of expertise, as commentators have pointed out, seems incompatible with much that the authors of this paper have previously argued. Commentators have said that it follows from Wave Two analyses, with which the authors have been, and still are, closely associated, that the 'experts' who play a rôle in a debate can be distinguished 'only after the dust has settled, after it becomes clear whose claims became convincing in the ongoing course of things'; and that 'it does not matter who defines the expert, actor or analyst; judgements of who the experts are always lay downstream'.[10] We might label this the 'expert's regress', by analogy with the 'experimenter's regress'. Because of the experimenter's regress, the class of successful replications of an experiment can be identified only with hindsight; because of the expert's regress, the class of experts can be identified only with hindsight. The trouble is that the expert's regress gives no more positive help with the problem of technical decision-making in the public domain than the experimenter's regress gives positive help with settling scientific controversies. But decisions of public concern have to be made according to a timetable established within the political sphere, not the scientific or technical sphere; the decisions have to be made *before* the scientific dust has settled, because the pace of politics is faster than the pace of scientific consensus formation. Political decision-makers are, therefore, continually forced to define classes of expert *before* the dust has settled – before the judgements of history have been made. In defining classes of expert actors in the political sphere, they are making history rather than reflecting on it. What we are arguing is that sociologists of scientific knowledge, *per se*, might also have a duty to make history as well as reflect on it; they have a rôle to play in making history in virtue of *their* area of expertise – 'knowledge'.[11]

The dilemma is not a new one, and has been present within Wave Two all along, though largely unremarked. One of the current authors (HMC) has discussed it in the course of his work on artificial intelligence (AI). Here, rather than reflecting upon the way the controversy about AI unfolded, he found himself taking an active part in the controversy, using his *knowledge about knowledge* to contribute to the debate. He referred to this activity, in contrast with more reflective science studies, as 'knowledge science'.[12] We might say that in knowledge science one works to affect the flow of the river of history, rather than examining its turns and eddies. In the same way, what we are doing here is 'upstream work' rather than 'downstream work'.[13]

Doing upstream work without abandoning the insights of Wave Two may involve a degree of compartmentalization of activity, but compartmentalization can often be avoided only on pain of paralysis.[14] It is also worth noting that, for better or worse, in Wave-Two work involving tacit knowledge and its consequences for replication of experiments and so forth, experimental and other skills have always been used as an 'upstream' category – something real and fixed that can be transferred from one person to another, or can fail to be transferred. Indeed that idea *has already* been used upstream in an attempt to smooth and steer the course of science.[15]

Let us try one more way of putting the matter. Wave Two deals with the problem: 'How is scientific consensus formed?' Some form of relativism in respect of the outcome of that consensus is vital if the answer is not to risk circularity. Wave Three deals with the problem: 'How do you make decisions based on scientific knowledge before there is an absolute scientific consensus?' Wave Three does not replace Wave Two because the problems are different. For Wave Three, something in addition to relativism is needed. One way to approach the problem of Wave Three is to look at the way science is granted legitimacy in the political, legal, or other spheres, and much existing writing in science studies which deals with science in the public domain has approached the problem in this way. But what we are trying to understand is why science *should* be granted legitimacy because of the kind of knowledge it is. In the case of this question, referring back to the way legitimacy is granted is what carries the risk of circularity.

We are asking the same kind of question about what makes science special as the sociologist Robert Merton (and any number of philosophers and political theorists) asked in the aftermath of World War II. But we are trying to answer it in the aftermath of the demise of Wave One.

We understand, of course, that any arguments we put forward will merely enter the stream as another ripple rather than divert it wholesale, and we realize that any ideas that are found useful in the paper will themselves be used as devices within continuing debates about the boundaries of expertise and the like. Realizing this, however, is not a reason to give up – the course of the stream might be at least slightly changed by such an intervention.[16]

## Core-Sets, Core-Groups, and their Settings

We now build up our approach to Wave Three in yet another way, using diagrams to enrich the exposition. Though our problem is about sciences and techologies in the public domain, we will start with the esoteric sciences. No doubt other approaches are possible, for example treating 'public-domain sciences and technologies' (those which directly affect, rather than merely being of interest to, the public), as essentially different to the esoteric sciences; but we have chosen a different analytic strategy. Starting with the esoteric sciences has the advantage that they are familiar to the authors, and that they are the traditional 'hard case' starting point for more general studies of science. We find that it is possible to work outward from our esoteric starting point in a coherent manner, ending up with the public-domain sciences which are our target.

A core-*set* has been defined as being made up of those scientists deeply involved in experimentation or theorization which is directly relevant to a scientific controversy or debate. A core-set is often quite small – perhaps a dozen scientists, or half-a-dozen groups. A core-*group* is the much more solidaristic group of scientists which emerges after a controversy has been settled for all practical purposes.[17] If the science is esoteric, then only the members of the core-set or core-group (hereafter 'core-scientists') can legitimately contribute to the formation of the consensus, or develop the science thereafter.[18] It is not always easy to define the boundaries of a core-set, because disputes within core-sets often involve the 'boundary-work' of trying to define people in or out – that is, defining them as legitimate or illegitimate commentators.[19] Nevertheless, if one takes a really esoteric scientific controversy such as that over the detection of gravitational waves, or the detection of solar neutrinos, or the likelihood that binary neutron stars will collapse into black holes just before they spiral into each other, or if one takes the sciences that follow after them, then members of Western society know, without having to agonize, that anyone who is not a recognized physicist with a great deal of equipment or special theoretical knowledge will not be, and *should not be*, counted as a member of the set of decision-makers in respect of the *scientific knowledge* itself. Were members to take a different view of this matter, they would no longer participate in

Western scientific society as the term is used here. There are those who would not accept that scientists have any special rights even in these esoteric matters, but here we must simply state our starting point that, as members of the scientific community broadly conceived, and contributors to Western scientific society, 'we think they do'. This is a reference to our culture, not a reference to the way political legitimacy is granted in our society. Should any politicians ever want to dismantle the right of the scientific community to settle esoteric issues within science, we would want to fight them.

As was pointed out to us, under Wave Two much intellectual leverage was gained precisely by denying the kind of sentiment that is taken to be self-evident in the last paragraph and those that follow.[20] We were asked whether we would be so happy to restrict judgement to such a small circle were the issue to be the importance of Tracy Emin's unmade bed, notoriously displayed by the Tate Gallery in London as an *avante-garde* work of art. It was said:

> No-one without the training and exposure to appropriate gallery-going is . . . 'competent' [to make a judgement]. So, can one derive the conclusion that only they *should* judge art?'

And the implication is certainly correct that we all feel we have something to say on the matter of 'the bed'. It was suggested that in respect of 'the bed', one could reach the conclusion that only a narrow range of people are competent to judge only if one makes realist-type assumptions about the quality of works of art. It was suggested that the same must go for the argument when applied to science.

These comments are correct in that it is necessary to make an assumption of a kind that is untypical of Wave Two if we want to restrict the sphere of judgement in science. But this seems to be inevitable if we want to do upstream work. That is why we have set out our assumptions as clearly as possible here, and we do not think we can do better than say 'this is the kind of society we like – one where we do consider that scientists with experience of an esoteric specialism are the best people to make judgements about what should count as truth within that specialism'. When placed alongside the terrible experiences of humans and humanity at the hands of others, what follows from the 'post-modernist' approach to knowledge is not that it is impossible to make judgements between plural realities, but that sometimes one just has to make judgements without timeless intellectual justification.[21]

### *Science and Art*

Later on, we will consider kinds of expertise that are different to those found in the esoteric sciences. At this point, however, it is worth noting something interesting in the comments made to us. It was suggested that restricting the circle of judges, in the case of esoteric science, to the core-set, is equivalent to restricting the circle of judges, in the case of 'the bed', to those with training and exposure to appropriate gallery-going. The appropriate group of judges, it was said, is not artists in general, nor even artists of the type who display their unmade beds (and the like), but art critics. In language we will explain more fully later in the paper, this is a class of experts with 'interactional expertise' rather than 'contributory expertise'. It may be that this is one of the ways in which science and art are different. The end-point of art, after all, is to be experienced, and that is why it is reasonable to suppose that those with special viewing, or experiencing, expertise – critics – rather than those with special creative expertise – artists – would be the best judges.[22] Science, on the other hand, is less obviously directed at consumers, and it is less clear that the audience has so much in the way of interpretative rights where science is concerned.[23] This might indicate that in the case of science, those who actually do it (who have contributory expertise) might have more relative merit as judges of scientific value than critics (those with interactional expertise), as compared with the case of art. So though we will cleave to our claim about the self-evidence of the nature of judgement in esoteric science, we would not want to be drawn to generalize the claim in respect of art or other cultural endeavours, and this analysis shows us one reason why one might not be a good analogy for the other.[24]

### *Politics is Intrinsic to Esoteric Science, not Extrinsic*

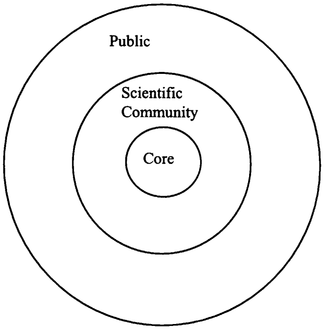
In setting out our view about who has the best claim to judge truth in the esoteric sciences, we have made a prescriptive statement about expertise! Since, as we have intimated, it is hard to get an 'ought' from an 'is', our prescriptive statement is based on a clearly stated preference about a certain form-of-life and what follows from it. We have argued that this preference and its corollaries do not necessarily hold in the case of art. We will also argue that they do not hold in the case of public-domain science. Thus, given common agreement on the self-evident nature of the case for esoteric sciences, we have already established that to understand the importance of contributions to technical decision-making by different elements of society, it is necessary to know what kind of science we are dealing with. Our analytic strategy is to proceed by drawing out the systematic differences between esoteric sciences and other sciences, starting with the core-set as found in the esoteric sphere.

One can represent a core-set as the bull's-eye of a 'target diagram' with two or more rings surrounding it.

In Figure 1, the 'bull's eye' is the core-set, the first ring out represents the scientific community who have no special knowledge or experience of the esoterica which concern the core-set, while the third ring represents the general public. Other rings might be used to represent the media and/or scientific funders and policy-makers, but we do not need them for the purposes of this analysis.

It might be thought that the stress on the scientific pre-eminence and exclusiveness of core-scientists in esoteric sciences flies in the face of the

**FIGURE 1**
**Core Scientists in the Wider Setting**




whole Wave Two analysis. According to the sociology of scientific knowledge, politics is never absent from the centre. Sometimes this will be the politics of the scientific community, but sometimes 'big-P' politics will play a significant rôle. This claim remains valid even for us, self-proclaimed, members of Western scientific society; indeed, one of the authors of this very paper was one of the first to describe the issue in print. How is this position compatible with the prescriptive statement we have just made, that in the case of esoteric sciences: 'anyone who is not a recognized physicist with a great deal of equipment or special theoretical knowledge will not be, and *should not be*, counted as a member of the decision-making group?'

The answer is that in these cases it is the esoteric decision-making group alone that disposes of the political influences that bear upon it. Thus, in what is probably the clearest example of this genre which involved 'big-P' politics, Steven Shapin showed that in 19th-century Edinburgh, scientists studying the brain observed features that were homologous with their position in local Edinburgh politics, and that the core-set were influenced by such considerations in reaching their conclusions.[25] And yet it would be quite wrong to say that because the phrenology debate was influenced by Edinburgh politics it would have been right for the brain scientists, and the public they served, to have consulted local Edinburgh politicians in order to form their opinions on brain structure. Such a view would, quite properly, be counted as encouraging 'bias', and would be incompatible with the 'form-of-life' of Western science. Anyone who held such a view would, by that fact, prefer to inhabit a different social and conceptual space to the authors of this paper. What Shapin's and similar studies show is that politics of this sort may influence science, but not that it is a *legitimate input* to scientific decision-making. Setting aside Lysenkoism and the like – still seen as pathologies by members of Western scientific society – one would never set out to design scientific or political institutions to enhance the influence of 'big-P' politics on the content of such an esoteric science: one would do quite the opposite.[26] We might say that the SSK studies show that politics is 'intrinsic' to science, but they do not license 'extrinsic' political influence.[27]

Thus, while SSK-type studies reveal various influences on the formation of views within science, they do not legitimate it any more than the revelation of similar influences in the justice system would legitimate their enhancement. In justice, as in esoteric science, one always tries to minimize external influence. Later, we will re-examine the question for less esoteric sciences and find it more complicated, but at least we have shown how to *break into* the Problem of Extension, in spite of what has been learned under Wave Two. More exactly, we have discovered that those of us who think of ourselves as living in Western scientific society have always lived with a partial solution to the Problem of Extension, even while we were emphasizing the non-expert influences on expert decisions. The 'circle was squared' because under Wave Two our analyses were descriptive not prescriptive: the hidden preferences were preserved because we never discussed them. We might say, in respect of our search for a boundary between legitimate and illegitimate inputs into esoteric scientific knowledge-making, that even those of us who have been practising SSK without compromise, have been 'speaking prose all along' – it has been the prose of the form-of-life of Western science. This is to reiterate the point already made about the degree of largely unremarked compartmentalization already found in Wave Two.[28]

*Beyond the Core*

According to the sociology of scientific knowledge, 'distance lends enchantment'.[29] Core-scientists are continually exposed, in case of dispute, to the counter-arguments of their fellows and, as a result, are slow to reach complete certainty about any conclusion. In general, it is those in the next ring out in Figure 1 – the non-specialists in the scientific community – who, in the short term, reach the greatest certainty about matters scientific.

These outsiders reach certainty more easily than core-scientists because they learn of goings-on in the core of the science only through digested sources, such as conversations with their colleagues, scientific journals, the scientific media, and the broadcast media. Inevitably, such sources condense and simplify – that is their job. Only exposure to the lived history of the core-set can reveal the richness of a dispute and its potential for being re-opened. For those at the heart of matters, scientific disputes are seen to linger on long after the wider community takes matters to be settled.[30]

There is a second reason why debate closes down in the wider community before it closes in the core-set. The consumers, as opposed to the producers, of scientific knowledge have no use for small uncertainties. Decisions about action generally involve binary choices – 'we will fund cold-fusion research, or we won't'; 'we will impose a carbon tax, or we won't'. When a decision is made to act, it can 'read back' on the scientific debate at its core and make any remaining doubts harder to sustain.

**FIGURE 2**
**Core Elongated in Time**

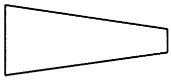
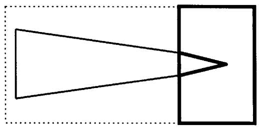


**FIGURE 3**
**Apex of Certainty Constructed by Wider Scientific Community**



(Though there are circumstances in which exposure to the public opens controversy, rather than closes it down.)

We can represent some of these processes by modifying the target diagram. The diagram can be stretched horizontally, and the left-to-right dimension used to represent the passage of time. The vertical dimension will be used to represent uncertainty. Thus, in Figure 2, the processes that take place in a core-set are represented by a narrowing triangle; as time goes on uncertainty decreases, though never quite reaching an apex of certainty.

The next figure, Figure 3, reintroduces the wider scientific community – the rectangle. Now we see that an apex of certainty has been added to the core-set, but it has been added, not by the core-set's deliberations, but by those in the next area out – the wider scientific community. The apex of certainty is shown, therefore, as belonging to this group. The line representing the wider scientific community changes from dashes to solid only as it begins to play its part in the perception of the outcome of the science. This is the process that has been labelled 'distance lends enchantment'.

Figure 4 introduces the general public. In esoteric sciences which are controversial, the public merely watches as disputes play out, but when the science becomes popularized, the apex of certainty becomes public property. The next generation of scientists are also introduced to these certainties by textbook writers who collapse the time dimension of the science they write about. It is only the apex of certainty that is visible to these new generations, and all the preceding years of experimentation and argument disappear into it.[31]

The processes represented in Figures 1 to 4 show how the nature and the history of science are turned into a mythology as a result of exposure beyond the core-set. Exposing and exploring the details of the process has been a major part of Wave Two. The potential for damage to science occurs when sciences find themselves exposed to the public early on, before consensus has developed within the core-set. Under these circumstances, expectations developed from exposure to the view represented in Figure 4 are applied to sciences at a time when core-set debates are too unsettled

**FIGURE 4**
**Apex of Certainty Visible to Public**

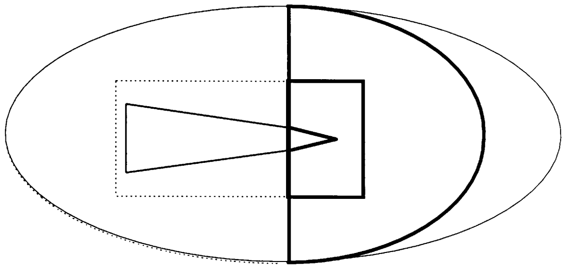
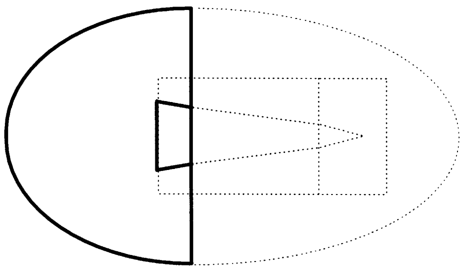


**FIGURE 5**
**Science Becomes Visible before it Becomes Certain**



(too wide in the vertical dimension), to give rise to a robust apex of certainty. This situation is represented in Figure 5: the dotted areas to the right of the solid vertical line are still in the future; the public sees the left-hand end of the core-set and expects, or at least is generally believed by policy-makers to expect, the same kind of outcomes as they have previously seen at the right-hand end.[32] But now they find that the scientists, who previously revealed a relatively united and robust front, argue with each other with different sides having rough parity; they change their minds, and are no longer a source of confidence. It is easy to understand why scientists prefer to keep their work private until they have reached something closer to unity.

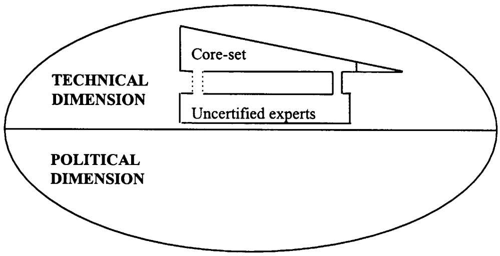
We might look at this situation in the following way: in the 1970s, sociologists began to study scientific controversies as 'breaching experiments' which opened up the hard, formal, though mythical, shell of science, exposing the soft social inside filled with seeds of everyday thought.[33] When the left-hand end of a core-set is publicly exposed, it too is a kind of breaching experiment, but one visible to all; everyone gets to see the soft flesh of the scientific fruit and the familiar passions and arguments that constitute it. Both kinds of breaching experiment show that scientists rely on ordinary reasoning to bring their technical arguments to a conclusion, and this closes the gap between science and the rest of us. Suddenly, the conclusions formally wrought by science alone are the property of everyone, and each has a right to contribute their opinion along with that of the no-longer-so-special scientists. This is where Wave Two, just like the approaches of Ulrich Beck and Anthony Giddens (see Appendix), struggles with the question of how to weight the opinions of the myriad potential contributors.

*The Third Wave of Science Studies?*

The Third Wave of Science Studies, SEE, turns, as we have said, on a normative theory of expertise. The aim is to approach the question of who should and who should not be contributing to decision-making in virtue of their expertise. Crucially, rights based on expertise must be understood one way, while rights accruing to other 'stakeholders', who do not have any special technical expertise, must be understood another way. Stakeholder rights are not denied, but they play a different rôle to the rights emerging from expertise. In a rather old-fashioned way, reminiscent of Wave One, Wave Three separates the scientific and technical input to decision-making from the political input. This is not an attempt to go back to Wave One, because Wave Three takes into account all that has been learned during Wave Two and, as we stress, Wave Two runs on as strongly as before; we are trying, under Wave Three, to reconstruct knowledge, not rediscover it. Thus, under Wave One, political rights made almost no contribution to technical decision-making, being almost entirely overwhelmed by top-down expertise; under Wave Three, expert and political rights can be seen to be much more balanced because of the new understanding of contested science that emerged from Wave Two. To represent this feature of Wave Three, we cut the diagram in half horizontally, reserving the bottom half for political and stakeholder rights, and the top half for scientific and technical debate. Scientists and technologists appear twice in this diagram. They appear in the bottom section of the diagram because they have rights as citizens and stakeholders; they appear in the top half in virtue of the rights that grow out of their specialist expertise.

In Figure 6, the top half of the oval contains the core-set. We have simplified the Figure in one important respect compared to the previous diagrams. *The wider scientific community no longer plays any special part in the decision-making process.* Henceforward, in our treatment, the wider scientific community is indistinguishable from the citizenry in general.[34] This,

**FIGURE 6**
**Uncertified Experts and the Core**

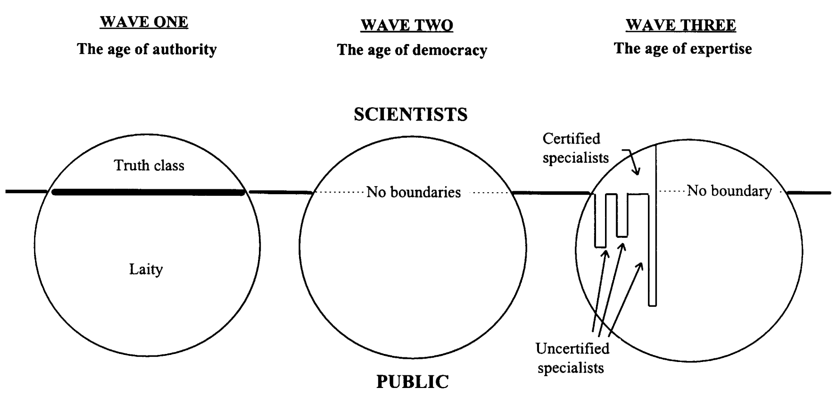


we would argue, is more than an analytic convenience: the wider scientific community *should* be seen as indistinguishable from the citizenry as a whole; the idea that scientists have special authority purely in virtue of their scientific qualifications and training has often been misleading and damaging. Scientists, as scientists, have nothing special to offer toward technical decision-making in the public domain where the specialisms are not their own; therefore scientists as a group are found in the bottom half of the diagram. In making this clear, Wave Three differs markedly from Wave One.

Within the top half of the oval, Figure 6 shows a small rectangle representing experience-based experts. The rectangle of experience-based experts feeds into the core-set. This is another way in which Wave Three departs from Wave Two. Wave Three, as we have said, distinguishes between two kinds of citizens' rights in technical decision-making. There are those from the bottom half of the oval, which we have already mentioned. And there are those in the top half, which accrue in virtue of the existence of *pockets of expertise* among the citizenry, and which are properly described as being within the technical rather than the political domain. Under Wave Two, it has been easy to confuse these types of expertise with rights accruing within the political sphere. Wave One located all expertise within the scientific community; Wave Two, reacting to this incorrect picture, made it hard to distinguish between scientific expertise and political rights; Wave Three is intended to re-establish the distinction, but with the dividing line set in a different place within the population. This difference in approach is summed up in Figure 7.

Figure 7 shows the location of expertise as conceptualized under the three idealized waves of science studies. Crucially, under Wave One, the dividing line was horizontal, separating the certified scientific community

**FIGURE 7**
**Three Waves of Science Studies**




**Wave 3 and Wave 1 differ epistemologically and politically.**
Knowledge and truth are grounded in scientific procedures; expertise is most often grounded in experience.
Expertise extends into the public sphere whereas access to knowledge and truth is strictly bounded.

from the laity; under Wave Three it is vertical, separating specialist experts, whether certified or not, from non-specialists, whether certified or not.

## The Nature of Expertise

We now begin to develop a classification of expertise which will help us understand what is in the rectangular box that lies within the technical sphere (Figure 6), and the odd-shaped pockets of expertise found among the lay public (Figure 7). The classification will show how the rights that accrue from expertise differ from more diffuse political rights. The object is to develop a discourse of expertise which will help to put citizens' expertise in proper perspective alongside scientists' expertise.

To carry out this task, it is necessary to recognize and categorize, not only different types of expertise, but also different types of science. Much excellent work has been done under Wave Two by 'deconstructing dichotomies', dissolving boundaries, and the like, but like it or not, the world is made of distinctions and boundaries. One of the styles of Wave Two argument is to concentrate on boundary problems. It is shown, and it can often be easily shown, that the boundary between entity 'A' and entity 'B' is unclear, and it is often argued that this means that A and B are not really separate things at all. Interesting studies of the way actors create and patrol boundaries can then be carried out.[35] Some writers, however, have gone further, and taken the fuzziness of many boundaries as the empirical counterpart of a philosophical prescription: 'Dissolve all dichotomies'. Here we approach from the other direction. We intend to point to differences by starting at the extreme points of our continuum – we will take 'ideal types' of this kind or that kind of expertise as our initial examples, and worry about the boundary problems later. In this way it is possible to begin to think about how different kinds of expertise combine in social life, and how they should combine in technical decision-making. There will be no clean and easy solution, because the boundary problems present themselves, not only to the analyst, but also to the actors as any potential new institution enters the arena of political discourse. Nevertheless, the first step must be to develop the appropriate terms for the discourse; we must learn a language which facilitates talk about the kinds of expertise that are relevant to the dilemma with which the discussion began. It follows that the types of expertise we discuss must be treated as 'real'.[36]

### *Experience and Expertise*

What kinds of expertise are candidates for reification?[37] The very term 'experience-based experts' that we adopt to describe those whose expertise has not been recognized in the granting of certificates, shows how important experience is to our exercise in demarcation. Experience, however, cannot be the defining criterion of expertise. It may be necessary to have experience in order to have experience-based expertise, but it is not sufficient. One might, for example, have huge experience of lying in bed in the morning, but this does not make one an expert at it (except in an amusing ironic sense). Why not? Because it is taken for granted that anyone could master it immediately without practice, so nothing in the way of 'skill' has been gained through the experience.

More difficult, one might have huge experience at drawing up astrological charts, but one would not want to say that this gives one the kind of expertise that enables one to contribute to technical decision-making in the public domain. Why not? Here, unlike lying in bed, an esoteric skill has been mastered which could not be mimicked by just anyone – at least not to the extent that it could pass among skilled practitioners of astrological charts. Astrology is, rather, disqualified by its content. It is hard to say much about which kinds of expertise are excluded in this way, but we can say something.[38]

Stephen Turner divides expertise up into five kinds, according to the way they obtain legitimacy from their consumers.[39] For Turner, the first kind of expertise (Type I) is like that of physics, which has gained a kind of universal authority across society in virtue of what everyone believes to be its efficacy. Type II expertise has been granted legitimacy only among a restricted group or sect of adherents; Turner gives theology as his example, and we might put astrological expertise in the same category. Type III experts, such as new kinds of health or psychological 'therapist', create their own adherents, or groups of followers. Type IV and Type V experts have their adherents created for them by professional agencies which set themselves up to promote a new kind of expert, or, like government departments, become specialist consumers of new kinds of expertise.

Our concern, in this paper, is very largely with Type I expertise. But what *argument* might we provide to justify stopping at Type I expertise? This remains an unresolved problem for upstream work in SSK. The best we can do is note that the adherents of all the kinds of expertise we value positively, were they to have what we will call 'contributory expertise' (see below), could make a reasonable claim to be members of the core-set relevant to any particular technical decision. That is to say, their expertise would be continuous with the core-set's expertise, rather than discontinuous with it; astrology and theology are discontinuous with those of radiation ecology, whereas the expertise of sheep farmers is not.[40]

In drawing a boundary around legitimate contributors to decision-making, then, two kinds of judgement are made in logical sequence. The first judgement is about what fields of experience are relevant. We might decide, for example, that astronomy is relevant to some question and that astrology is not, in spite of the claims of its adherents. But we have almost nothing to say here about this choice except the groping remarks just made about continuity and discontinuity, and a reference back to the form-of-life of Western science. Our views on which fields are legitimate and which are not are certainly not fixed for all time, and they may change as the flux of history brings one field out from the cold and pushes another into it.[41] Nevertheless, this choice has to be made ahead of the choice of who is an expert within a field. The point is clarified in Figure 8.

**FIGURE 8**
**Expertise in Consensual and Fringe Sciences**

| | | NATURE OF FIELD | |
|---|---|---|---|
| | | *CONSENSUAL* e.g. astronomy | *FRINGE* e.g. astrology |
| EXPERT PRACTITIONER | *YES* | 1 | 2 |
| | *NO* | 3 | 4 |

In box 4 of Figure 8 are inexpert and inexperienced practitioners of fringe fields, whereas box 2 contains expert astrologers, remote-viewers, and forth. In box 3 are found inexpert practitioners of fields which are acceptable to the broad consensus of Western science – people who are just bad at science. In box 1 are the experts in consensual fields. We have, as indicated, very little to say about the horizontal dimension in this table except that it is there – our concern is with the vertical dimension of the left-hand side of the table. The burden of this paper is that there are people whom qualified and credentialled scientists would want to consign to box 3, who ought to belong in box 1. Our licence for saying this is the expertise, drawn from SSK, in the matter of the nature of science and knowledge. In particular, Wave Two studies of the essential craft content of science have shown that it is more difficult to separate the credentialled scientist from the experienced practitioner than was once thought: when we move toward experience as a criterion of expertise the boundary around science softens, and the set of activities known as 'science' merges into expertise in general. But box 3, as we have stressed, still contains only small subsets of the population at large.[42]

Choice between fields and expertise within a field is, then, orthogonal. Disagreement is on yet another orthogonal dimension (in Figure 8, one can visualize it coming up out of the page). Thus, as the core-set studies show over and over again, experts, all of whom belong in box 1, disagree. It merely complicates matters a little that they sometimes express their disdain for other experts with whom they disagree by saying that they do not belong in box 1 at all, but should be relegated to box 2. It is, of course, no surprise that officially appointed radiation scientists might disagree with the views of Cumbrian sheep farmers, even though their knowledge of sheep is continuous with the sheep farmers' knowledge, and it is no surprise that they might express their disagreement by saying that these sheep farmers are not experts. We (that is, sociologists of scientific knowledge) claim the right to disagree about this last judgement.

To sum up once more, what we are dealing with is types of expertise that are actually or potentially continuous with what Turner calls Type I expertise. The classification that we need lies, then, *within* the envelopes of the categories of expertise discussed by Turner, and mostly within the envelope of his Type I. Our classification is, perforce, of a kind which is quite different to his.[43]

*Three Types of Expertise*

There are dozens of ways of classifying competence and expertise.[44] Classifying competence is the basis of much educational theory, psychology of intelligence, sociology of employment, and so forth. It also forms the foundation of the study of artificial intelligence and expert systems. One of the authors of this paper has himself classified expertise in several different ways.[45] Here we choose our starting point for our classification on the basis of familiarity. We thought it might be persuasive to begin with something that many potential readers know about from first-hand experience. We start, then, with ourselves and our practices as sociologists of scientific knowledge.

One way in which the group of analysts who practise SSK have to confront the concept of expertise is in the problem they themselves face in trying to gain a cultural foothold in the areas of those sciences they want to analyse. Typically, SSK fieldworkers enter scientific fields which they do not know, and try to learn enough about them to do sociological analyses. Rarely, however, do they reach the level of expertise of a full-blown participant. In the case of the esoteric sciences, the fieldworker hardly ever participates in the science itself. Thus, to begin with, by reflecting on certain sociologists' fieldwork experiences, we can distinguish three levels of expertise:

1) *No Expertise*: That is the degree of expertise with which the fieldworker sets out; it is insufficient to conduct a sociological analysis or do quasi-participatory fieldwork.
2) *Interactional Expertise*: This means enough expertise to interact interestingly with participants and carry out a sociological analysis.
3) *Contributory Expertise*: This means enough expertise to contribute to the science of the field being analysed.

Since reflecting on the practice of sociologists tells us that there is a difference between these three states, we have made another important step: we have begun to understand expertise as an analyst's category, as well as an actor's category. In this case, expertise is an analyst's category in a very direct way: it is a category which analysts use to think about themselves, and this is why we think it is a persuasive starting point. Since we already use this language to describe ourselves (speaking prose all along!), there should be less obstacle to using it to describe other actors.[46]

Of course, these three categories are ideal types and, as with most such classifications, there will be boundary problems. For example, the attainment of category 2 is hotly disputed by 'science warriors', who frequently claim that sociologists do not have enough scientific expertise to carry out their sociological analyses, and have failed to escape category 1. We could, if we wanted, give an attributory account of our own experience. What does count as having enough expertise to do fieldwork, or even to contribute to a science? The trick, however, is not to become paralysed by these problems, but to proceed with an imperfect set of classifications, just as other experts proceed. Instead of worrying about the imperfections of our science, we should note that the very fact that we, as sociologists of science, work hard to acquire a level of expertise that enables us to defend ourselves against the charge of being insufficiently expert shows that *we do not* act as though it is all a matter of attribution. As empirical researchers in the sociology of knowledge, we act as though there is something in the nature of expertise that can be acquired if we work at it. Furthermore, at least some of us have found that we have been unable to acquire sufficient expertise to analyse certain scientific fields to our satisfaction, and this too is a salutary experience.[47] Again, just occasionally, we do manage to acquire enough expertise in some field we study to begin to contribute.[48] Thus, though there are boundary and definitional problems, they do not have to be fatal.

Having accepted that to categorize expertise makes sense in spite of the boundary problems, the task is to begin to work out what these types of expertise mean and how they fit together. For example, having interactional expertise does not give one contributory expertise, but one might think the former was a necessary condition for the latter. But it may not be! We will work out some of these differences by referring to what is fast becoming the paradigm study of so-called 'lay expertise' – Brian Wynne's study of the relationship between scientists and sheep farmers after the radioactive fallout from the Chernobyl disaster contaminated the Cumbrian fells.[49]

Wynne found that the sheep farmers knew a great deal about the ecology of sheep, and about their behaviour (and that of rainwater) on the fells, that was relevant to the discussion of how the sheep (and the fells) should be treated so as to minimize the impact of the contamination. Since the Windscale-Sellafield plant was built soon after World War II, the farmers in the locality had long experience of the ecology of sheep exposed to (radioactive) waste. The farmers have all the characteristics of core-group experts in terms of experience in the ecology of hill sheep on (mildly radioactive) grassland, even though they had no formal qualifications. In our terms, the farmers had contributory expertise which in some respects exceeded that of scientists working for the relevant government department.

The scientists, however, were reluctant to take any advice from the farmers. Now, for the farmers to have contributed to the science they would *not* have had to engage in a symmetrical conversation with the scientific experts – all that would have been necessary was for the scientific experts to try to learn from the farmers. This seemingly trivial point helps us to understand what expertise is, but also points out where the social location of change needs to be. The normative point that follows is that the body of expertise that should have emerged in respect of Cumbrian sheep was a combination of the separate contributory expertise possessed by the scientists and the farmers. The scientists' expertise was not at risk of being displaced by that of the farmers; it was, or should have been, added to by that of the farmers. Should the situation have been symmetrical, it might have been an arbitrary matter whether the farmers' expertise was absorbed by the scientists or the scientists' expertise was absorbed by the farmers, but it was not symmetrical. To produce the optimum outcome, the scientists needed to have the *interactional expertise* to absorb the expertise of the farmers. Unfortunately, they seemed reluctant either to develop or to use such expertise.[50] Here we begin to see how our theory of the interrelationship of types of expertise might gain in richness and practical relevance. We have two theses:

> Thesis 1: *Only one set of experts need have interactional competence in the expertise of another set of experts for a combination of contributory expertises to take place.*
>
> Thesis 2: *In such a case, only the party with the interactional expertise can take responsibility for combining the expertises.*

Thesis 2 says something about the social responsibility of different parties, but there could also be a 'Thesis 3' that says something more prescriptive still about how these different parties should interact:

> Thesis 3: *In such circumstances the party without the interactional expertise in respect of the other party should be represented by someone with enough interactional expertise to make sure the combination is done with integrity.*

In effect, Thesis 3 is suggesting that the Cumbrian farmers might well have had more success in their dealings with the scientists from the UK Ministry of Agriculture, Fisheries and Food (MAFF) and from British Nuclear Fuels Ltd (BNFL), if their concerns were mediated by a Greenpeace scientist, a Brian Wynne, or the like. Clearly such an individual would need to be briefed by the farmers about what the certified scientists were doing wrong, but such a person may have been able to phrase the problem in ways more familiar to the scientists, making it more credible (or less resistible). This problem was recognized by AIDS treatment activists in the USA, who found that they had to learn the language of science if they were to represent the interests of the wider community within the clinical trials process.[51]

### *Referred Expertise*

Sometimes expertise in one field can be applied in another. A third category of expertise which seems useful is 'Referred Expertise'; it is, as it were, expertise 'at one remove'. Consider the managers and leaders of large scientific projects. In general they will not possess contributory expertise in respect of the many fields of science they must coordinate. In the field study of one of the authors, this became a bone of contention. As one expert scientist put it:

> . . . What I found disappointing was that after two years the project manager still didn't really know what it meant to do interferometric detection of gravitational waves.

Whereas a manager saw it this way:

> Once you professionalize, the guys who are very good in the lab where you control everything, no longer have their arms around it all. Other people can work very well in that environment. They interface with the experts who built the electronics and understand what they need to of that; they interface with the computer people and do very well at that; and some people can work in this broader environment technically. Some people make the mistake of saying that as soon as you are in this broader environment it's a management problem; it's not a management problem! The technical part is actually more technical and more sophisticated.

If we stay with the terminology we have developed so far, we would have to say that in respect of the science they are managing, the managers have only interactional expertise (and, one would hope, discrimination, and also the ability to translate: see below). But in so far as they have contributory expertise, it is expertise in management rather than in science. Does this mean that their scientific expertise is no greater than that of, say, the visiting sociologist? The answer has to be 'no', or our theory would be reduced to the absurd (as well as disagreeing head-on with the last sentiment expressed in the above quotation).[52]

The resolution seems to be that to manage a scientific project at a technical level requires, not contributory expertise in the sciences in question, but *experience of* contributory expertise in some related science.[53] In other words, the managers must know, from their own experience, what it is to have contributory expertise; this puts them in a position to understand what is involved in making a contribution to the fields of the scientists they are leading at one remove. As one might have 'referred pain' in a leg as a result of back injury, this is 'referred expertise'. It would be quite reasonable to expect that managers of scientific projects with referred contributory expertise would manage much better (and with much more authority and legitimacy) than those without it.[54]

### *Translation*

There are at least two other kinds of ability that go into the making of technical judgements of the kind we are discussing: the first is the ability to translate. For groups of experts to talk to each other, translation may be necessary. Some people have a special ability to take on the position of the 'other', and to alternate between different social worlds and translate between them.[55] The translator will have to have at least interactional expertise in both areas.

> Thesis 4: *A necessary but not sufficient condition of translation is the achievement of interactional expertise in each of the fields between which translation is to be accomplished.*

If the translator has one or more bodies of contributory expertise, so much the better, but contributory expertise is not a necessary condition for translation. Returning to Thesis 3, it seems important that those who represent one group to another group must be able to translate.

If the ability to translate consists of more than having multiple interactional expertises, what is the extra bit? Presumably it has to do with the skills of the journalist, the teacher, the novelist, the playwright, and so forth, skills notoriously hard to explain – as qualitative sociologists know to their cost.[56]

*Discrimination*

The second requisite is the ability to discriminate. Social actors can sometimes make judgements between knowledge-claims based on something other than their scientific knowledge. Judgements of this sort can be made on the basis of actors' social knowledge: does the author of a view come from within the right social networks, and has he or she the appropriate experience to make their claim credible? Such things as the personal demeanour of the expert might be the crucial inputs to these judgements, rather as one might judge a politician. The questions are: Does the author of a claim seem to have integrity? Is the author of a claim known to have made unreliable claims in the past?[57] There are also secondary features of a claim itself that can be judged with only minimal scientific understanding: Is a claim internally consistent or inconsistent, or consistent with other claims made by the same person? Does the claim seem so self-serving as to give concern?

To make the notion of discrimination do any work, it is once more necessary to distinguish between specialists and the population as a whole. Most members of a society, just by being members of that society, are able to discriminate between what counts as science and what counts as non-science. This is the ubiquitous judgement on which we rely when we dismiss astrology and the like as potential contributors to the *scientific* element in technical decision-making. Most members of our society have sufficient judgement to know that the social and cognitive networks of, say, astrologers do not overlap with the social and cognitive networks of scientists with (Turner's) 'Type I' expertise.[58] This kind of discriminatory ability comes with participatory expertise in the matter of living in society!

To see how this works, consider cold fusion. Most reasonably literate members of this society 'know' that cold fusion has been tried and found wanting; though there was a time when cold fusion was contiguous with science as we know it, its cognitive and social networks no longer overlap. This knowledge has nothing to do with scientific competence. On the contrary, it is vital to ignore scientific credentials. Thus Martin Fleischman, the co-founder of the cold fusion field, is immensely well-qualified and has both interactional and contributory expertise in cold fusion, yet still believes in it. What people in Western populations have in common is what they have heard about cold fusion in the broadcast media. Their consensual view emerges from the making of social judgements about *who* ought to be agreed with, not *scientific* judgements about *what* ought to be believed. The crucial judgement is to 'know' when the mainstream community of scientists has reached a level of social consensus that, *for all practical purposes*, cannot be gainsaid, in spite of the determined opposition of a group of experienced scientists who know far more about the science than the person making the judgement. This ability is gained through membership of what the *Guardian* newspaper calls the 'chattering classes'. Note that this is not the sort of judgement we would expect even an immaculately qualified scientist from 'another planet' where the *Guardian* does not circulate, to be able to make.[59] What the members of the chattering classes have is what we might call 'discrimination'.

But contrast this with the more locally informed kind of discrimination of the Cumbrian sheep farmers. The farmers had contributory expertise about the ecology of their farmland, but they could also do a special kind of discrimination. The dispute was between the local community, MAFF and the Sellafield authorities (BNFL), but extensive dealings had already taken place between the parties over the years; as a result, the interaction was tense. Through experience, the farmers had developed discrimination in respect of the pronouncements of (in particular) the Sellafield authorities: they found the authorities more questionable than they otherwise would, and more questionable than they would seem to an outsider with less experience of this particular social and geographical location. (In the Appendix we will discuss another case described by Brian Wynne where there is ambivalence between *local* and *ubiquitous* discrimination.)

### *The Lack of Expertise of the Wider Scientific Community*

As we have mentioned, one big mistake that has been made in the past is to exaggerate the importance of the referred expertise of the wider community of scientists. At the very outset, when we discussed Wave One, we noted that in the 1950s scientists were often attributed with authority to speak on subjects outside their narrow area of specialization. The Second Wave has shown how dangerous it is to take this kind of referred expertise at face value, since the pronouncements of the wider scientific community are nearly always based on simplified and retrospectively constructed accounts of the scientific process. Quite simply, scientists' supposed referred expertise about fields of science distant from their own is nearly always based on mythologies about science, rather than on science itself. That is why we have stressed the continuity between the wider scientific community and the public in all but specialist areas, and represented this point in the Wave Three diagram within Figure 7. Organizations such as the UK Committee on the Public Understanding of Science (COPUS), in its first incarnation, and many self-appointed scientific spokespersons, by making science as a whole the focus of their campaigns, have oversold it; it is the work of *specialists*, not generalists – not the whole scientific community – that should be the focus of campaigns to raise the status of science. Of course, the former type of campaign treats science as a world view – competing with religion and the like – and therefore is accompanied with the thrill of zealotry, or what might be called 'scientific fundamentalism'; the latter type of campaign would be a comparatively mundane enterprise, stressing experience and professionalism rather than priestly virtues.

Examining Wave Three more closely, as it is represented in Figure 7, the same point can be seen from a different angle, as it were. What differentiates core-scientists from their fellow scientists on the other side of the vertical line in the top part of the diagram? It is not credentials! The core comprises those who have actually done relevant experiments, or who have developed or worked with theories relevant to the issue in question.

To express this in more general terms, the core-scientists' special position, apart from their possession of specialist equipment, arises from their long experience and integration into the specialist social group of which such expertise is the collective property. Core-set members do not possess extra formal qualifications, and they have not undergone special periods of formal training over and above what they needed to qualify as certified scientists in the first place. It is not more certification that qualifies them for membership of the core. In terms of formal criteria they are indistinguishable from the rest of the scientific community; the difference between the core-set and the others is informal.[60] This informality – the fact that membership of the most esoteric groups is based on experience – gives us licence to dissolve the boundary between the certified experts and experience-based experts; in sum, the demarcation lines run vertically in Wave Three (Figure 7) because they demarcate the set of all experts, certified or uncertified, with relevant experience, from those without it; in Wave One (Figure 7) they run horizontally, demarcating all of those with scientific qualifications from those without them.[61]

To sum up, in the vertically partitioned world of Wave Three, the right to contribute technically to a technical decision is to be assessed by examining expertise. The appropriate balance of contributory expertise, interactional expertise and referred expertise has still to be worked out, and so has the rôle of discrimination and translation. In this world, certification as a scientist has little or no importance. The role of expertise and the rôle of democratic rights are separate.

## Case Studies

### *Increasing Interaction: Cumbrian Sheep and AIDS Treatment in San Francisco*

Let us now see how this categorization of expertise helps us understand some of the well-known case studies. We start by discussing the Cumbrian sheep farmers once more. The sheep farmers had contributory expertise that was complementary to that of the MAFF scientists. They developed it through their long collective experience in the ecology of the fells and the sheep that live on them. They failed to make much impact with this expertise because they lacked interactional expertise; they needed help, either from generous-minded ministry scientists, or from intermediaries with interactional expertise and the ability to translate. The sheep farmers also had a special level of local discrimination in respect of the BNFL scientists; this had developed out of long experience.

Thus, what Brian Wynne's study shows is not what it has often been taken to show – that scientific expertise is to be found among the public – but that, in this particular case, there were not one but two sets of specialists, each with something to contribute. The sheep farmers were a small group in possession of a body of knowledge as esoteric as that of any group of qualified scientists. The sheep farmers were not 'lay' anything – they were not people who were not experts – they were experts who were not certified as such. To repeat, in Wynne's study can be seen the working out of the interactions, not of experts and the public, but of two groups of experts.

Now, it is also true that the sheep farmers had some rights in the matter in virtue of their ownership of the sheep – but this can be distinguished from the matter of their technical expertise with a thought experiment. Imagine that just prior to the Chernobyl explosion a group of London financiers had got together to buy the Cumbrian farms as their private weekend resort, employing the farmers as managers so as to preserve the existing ecology: the financiers, not the farmers, would then be the owners of the sheep, yet all the expertise would remain with the farmers. 'Overnight', much in the way of stakeholder rights would have been transferred from the farmers to the financiers, yet this would not make them members of the core-set; it remains the case, however, that the farmers should have been included in the core-set. Thus it is easy to see the difference between political rights and expertise.

To clarify the point further, Figure 9 sets out some indicators of the difference between the political and the technical contributions to technical decision-making; the first two rows in the Figure have already been discussed, the second two rows are newly introduced.

The first row in Figure 9 tells us that the way politics enters the two 'phases' of the decision-making process is different.[62] In the political phase, the politics is readily visible and is treated as an extrinsic feature of the scientific decision. But, as in the Edinburgh phrenology case, politics

**FIGURE 9**
**Political and Technical Contributions**

| | | PHASE | |
|---|---|---|---|
| | | Political | Technical |
| | Politics | Extrinsic | Intrinsic |
| NATURE | Rights | Stakeholder | Meritocratic |
| OF | Representation | By Survey | By Action |
| | Delegation | By Proxy | Impossible |

enters the technical phase intrinsically – it is amalgamated into the science in such a way that its effect is usually hidden unless picked out in studies such as Steven Shapin's.

The second row repeats what we have just said: contributions to technical decision-making are made by right by stakeholders in the political phase (the landowners), but by merit in the technical phase (the experienced farmers and the scientists).

The third row points out that the contribution of stakeholders could be represented by something like an opinion survey or a vote, whereas technical contributions have to respond continually to unanticipated developments in the live science and technology, so that the expertise has to be carried in the person of the contributor.

The fourth row is a corollary of the last point, that stakeholders could appoint a proxy (for example, a solicitor) to represent their interests, whereas no proxy can exercise skills on someone else's behalf.

Returning to events post-Chernobyl, it is not clear whether the Cumbrian sheep farmers' advice ever actually entered the core of the post-Chernobyl discussion, but we can be unabashedly prescriptive and say that it *should* have become part of it.

We have argued that one of the reasons that the sheep farmers made less impact than they might have done was their lack of interactional expertise. The AIDS-treatment controversy in the San Francisco gay community is an example where the non-certified experts succeeded in gaining an entrée to the scientific core.[63] But they did not manage this until they gained interactional expertise – that is, until after they learned the language of the relevant science. Their case is represented in Figure 6. The dashed-line nexus between the pocket of experience-based expertise and the core is where the AIDS activists might have made their contribution, but they did not make it until later – as represented by the solid-line nexus.[64] One outcome of our analysis, we hope, will be to encourage the movement of such a nexus to the left – that is, to encourage the involvement of experience-based experts earlier in the game – possibly by encouraging such groups to look for spokespersons with interactional expertise in the science in question, or to encourage the growth of intermediary groups to speak for the scientific knowledge of the uncertified, not as campaigners, nor as experts themselves, but as translators. There is, of course, a certain *naïveté* about this suggestion, but unless all hope of unbiased action is to be abandoned (and why, then, are we academics?), it is our duty to be naïve from time to time.[65]

*Decreasing Interaction: Crashing Fuel Flasks and Aircraft*

One of the characteristics of the analyses of the relationship between experts and the public under Wave Two is that they all push in the same direction: increased participation by the public to solve the Problem of Legitimacy. One cannot but feel a little uncomfortable when every treatment has the same political recipe, because it makes it all too easy to imagine that the prime motivation is political rather than analytical. A reassuring feature of the Wave Three approach, which puts expertise at the centre of the analysis, is that there are cases which push in the opposite direction – cases where, according to our analysis, participation by the public should have been *decreased*, because their expertise was insufficient to make a contribution.

On 17 July 1984, in Leicestershire, England, the British Central Electricity Generating Board (CEGB) decided to demonstrate the safety of their method of transporting, by train, spent nuclear fuel around the country. They crashed a train travelling at high speed into a nuclear fuel flask. On 1 December 1984, at Edwards Air Force Base, in California, NASA and the FAA deliberately crashed a remotely controlled Boeing 720, carrying 75 dummy passengers and a full load of 'anti-misting kerosene' (AMK) into the ground; AMK, as opposed to ordinary jet-fuel, was supposed to reduce the likelihood of the catastrophic life-taking fires that usually follow otherwise survivable aircraft crashes.

In both of these cases, the public was brought into the heart of the scientific process by being given grandstand seats at the demonstrations – some directly, by being at the scene, and many more indirectly, through what they saw on television.[66] What the public saw was that the flask did survive the spectacular crash with its integrity unscathed, whereas the aircraft was almost completely destroyed by fire. In the case of the flask, the audience was invited by the late Sir Walter Marshall, then Chairman of the CEGB, to draw the conclusion that the flasks were a safe means of transport. He said on television: 'If they're not convinced by this they won't be convinced by anything'. The conclusion of the audience watching the aircraft crash, bolstered by headlines in all the newspapers, was that AMK was a failure. And yet these TV audiences were in no position to make such judgements – they did not have the necessary expertise. In these cases, giving the impression that the public could judge the meaning of the tests was misleading and seemed designed to serve political ends.[67]

Thus, according to other experts, the train crash could not be taken to imply the safety of the method of transport because of certain special features of the test whose significance was evident only to the expert eye. These included the absence of the railway lines beyond the point of impact, and the removal of the wheels of the wagon on which the flask was placed: the lines could have penetrated the flask had they been there, and the wheels could have dug into the ground, enhancing the impact.[68] Likewise, the plane crash could not be taken to imply the non-safety of AMK, according to the experts, because the crash was more severe than was intended (a steel pylon entered one of the engines), the fire was in any case much less severe than it appeared and some passengers could have escaped it, and there was much unburned fuel left in and around the aircraft which helped to cool the flames in the first instance.

Including inexpert members of the public within the groups judging the meaning of these two crashes meant that debate was cut off prematurely before the appropriate expert analysis, of the kind we have sketched in the last paragraph, had time to make a mark; the public who witnessed the events simply did not have the contributory, or even interactional, expertise to make sensible judgements (though they seemed to have enough 'discrimination' to find Sir Walter Marshall's account unconvincing). In these cases then, the irony is strong: the environmental lobby, who are usually in favour of widening public participation in decision-making, would have preferred the interpretation to be the opposite of the immediate one. In these cases a better interpretation would have been accomplished by narrowing the group of decision-makers to certified experts alone.[69] This group, of course, would not have been limited to the 'official' experts and would have included representatives of environmental and safety-conscious lobbies, but they would have had to be *expert* representatives.[70]

### *Understanding Interaction: The Magicians and Benveniste*

It might be thought that the prospect of bringing non-certified experts into scientists' core-sets and core-groups is near to zero, even if the idea makes sense. It might be argued that the professional pride of the scientific community would always prevent a change in this direction, just as it did in the case of the Cumbrian fells. It might be said that the San Francisco gay community, even though they were allowed to enter the core discussions, were allowed to do so only after they had adopted the personae, and perhaps the persuasions, of the scientists.[71]

Fortunately, another kind of case shows us that when the circumstances are appropriate, professionalism is not a barrier to the inclusion of experience-based experts into the very heart of scientific decision-making. Thus, in the case of cold fusion, there was a veritable feeding-frenzy of rejection by members of the scientific community who were not core-set members – suddenly there were experts everywhere. In the case of still more heterodox ideas, such as parapsychology, or Jacques Benveniste's claims about the power of zero-solutions (homeopathy works through the molecular 'memory' of water), stage magicians were brought in to pronounce on the propriety of the science, and their work was admired to such an extent that one of them was even given a prestigious McArthur 'genius' award.

There are two ways of looking at the stage-magician phenomenon. One would be to account for it as an aberration from proper science that needs to be explained in terms of political expediency. Thus, one might see it as a quick and dirty way for scientists to accomplish rejection of 'fringe phenomena', with the maximum publicity, and without having to do the messy, difficult, and immensely time-consuming work of trying to prove a 'null' (there are no paranormal effects; plain water never has special biological properties). In other words, it is a way for core scientists to get their rejections straight into the public arena without going through the ordinary core-set process. If that is what is happening it is hard to remain neutral in the face of the process; we find ourselves wanting to be prescriptive and say that this is 'wrong' – it is a dereliction of scientific duty.[72] After all, among other things, scientists are there to help us know whether there are paranormal effects or homeopathic effects, but their input should be based on their best scientific efforts; ex-cathedra statements, or dirty tricks, are of no special value, nor should scientists pass their responsibility to outside groups. We noted above that scientific expertise cannot be transferred to a proxy, and the business of electing stage-magicians as science's representatives has to be questioned in this light.

Represented in the language of Figure 6, this case would reveal a strong and wide nexus from core-set to specialist pocket. A too-ready passing over of responsibility could be represented by a nexus so wide that the whole core-set would flow down it, like water down a drain, leaving the entire decision to the uncertified specialists. Revealing too ready a willingness to abandon responsibility as scientists – the moral guardians of a certain way of understanding the world – is, for obvious reasons, a dangerous game for scientists to play.

However, another way of analysing the stage-magician phenomenon – and this is how scientists tend to explain it – is precisely in terms of pockets of specialist expertise. Under this interpretation, in employing stage magicians, scientists are reaching toward specialist but experience-based expertise that has particular application in cases where fraud is suspected. Looked at this way, it seems less like an abandonment of scientific responsibility and more like a very reasonable extension of the core-set into a social group who may be formally unqualified and 'uncertificated', but who still have many of the qualities of core-set members in terms of long and relevant experience.[73]

## Different Types of Science and Technology

So far we have only differentiated between types of expertise; we have not differentiated between types of science. Wave Three, however, needs a categorization of sciences as well as a categorization of expertise. This is because the appropriate way to integrate the public into policy processes depends on the nature of the science and technology. In some cases, the public seems to be an integral part of the knowledge-base that is needed to make policy decisions; in other cases, their potential to contribute is much less clear. Let us start with the most well-worked-out case, that of technologies for wide-scale or mass public use.

### *Integral Public Expertise in Public-Use Technologies: Cars, Bicycles, Personal Computers*

Consider technologies, such as cars, bicycles, computers and computer programs, where end users comprise a large proportion of the public, whose preferences are taken into account in the very design process. In these cases, specialist uncertified expertise is integral to the development of the technology (and thus of the related sciences).

There are at least two kinds of experience-based expertise relevant to such cases. First there is the narrow specialist expertise of computer 'buffs' and the like. Indeed, companies now take advantage of this kind of expertise by nurturing 'lead users' among their customers. In effect, these users acquire contributory expertise, and this is then recognized by the companies, who then consult them as experts.[74] Social groups such as computer hackers are similarly expert, although their intervention is not so welcome, at least, not by the computer companies.

Secondly, there is the much broader category of those whose legitimate contribution to the 'closure' of a technological design grows out of the very fact that they, being the users (or active non-users) of the object, are integral to the establishment of its meaning and success. In effect, these groups have some form of contributory expertise that shapes the future design, form and function of the artefact. This kind of argument has been most forcefully put within what has become known as 'the new sociology of technology'.[75] Were this case to be included in a version of Figure 6, it would show that the 'pocket' of specialists in the top, technical, half of the diagram, would exhaust nearly all the space outside the core.

Even in the case of a technology like this, however, there is still a political dimension. For example, in the case of cars, both drivers and non-drivers have a political say in the design of cars, based on their political preferences – often preferring designs which minimize fuel consumption and tax regimes which discourage pollution. These preferences, pressures and rights are better represented as belonging in the bottom half of the diagram, though in this case there is a serious borderline problem – the rights of the public as public, and the public as car-drivers, are very hard to untangle. In spite of the severe borderline problem in this kind of case, it is important, here as elsewhere, not to generalize from such extremes to science and technology as a whole.

### *Integral Public Expertise in Local-Interest Technologies: Planning*

In planning processes, 'local' knowledge often seems to confer special expertise on certain social groups. Like the car users discussed in the previous paragraph, the users of a locality seem to merit special involvement with the technical experts in the planning process. Local people can be seen as a large pocket of experience-based expertise when the issue within the core is local planning. However, thinking critically about expertise helps to distentangle the force of this localness.

In planning, local knowledge is a kind of expertise because local people can be said to have long experience of the local environment. But this expertise has to be used carefully, because local experience, when it is not combined with other kinds of experience, is partial, and this will frame contributions in a particular way. Thus, in the case of mineral extraction or waste disposal, the local population will tend to have a disproportionate understanding of the *disadvantages* of any development: they will know exactly how such developments will harm the local environment. But they may not have any special knowledge, or even any knowledge at all, of how developments will *advantage* the population of the larger regions within which the locality is embedded – the county, the nation, and so forth – and the users of the product. It is likely to be planning specialists who understand these things.

So far we have said nothing about local political interests, only local expertise. And one can see that local expertise is likely to favour the locality, even before the politics enters the equation. It is tempting to say that any attempt to separate the expertise and the politics is doomed to failure. In practice, this may be so, but the two phases are still easy to separate analytically. Thus, in the case of mineral extraction and waste disposal, local expertise will almost certainly militate against location of new plants in the local area, as such plants are almost certain to damage the local environment and increase public health risks. The political interests are more ambivalent, however, and likely to split along class lines. Thus, the building of a new quarry is likely to have an adverse affect on property prices in the locality, but a positive effect on employment, wages, and the profits of small shops. So the expertise and the political interests of the higher social classes are likely to be congruent, while the expertise and political interests of those who work for a weekly wage are likely to pull in opposite directions. Thus, even in local decision-making, it is still possible and useful to separate the political considerations from the technical considerations.[76]

### *Esoteric and Controversial Sciences*

In public-use technologies and planning, the involvement of the public as experts is 'integral' to the science itself. Now let us return to sciences where this is not so. At a first approach, four kinds of science of this type can be distinguished. To these we will apply the labels 'normal science', 'Golem science', 'historical science' and 'reflexive historical science'.

In *normal science* there are no major disputes, and the science is as settled as it ever can be. In these cases, scientists can fill the rôle of consultants without problem unless matters are opened up again by exposure to a controversy, such as in a courtroom or larger dispute. In courtrooms and the like, even the most routinized procedures with the longest historical entrenchment can be the subject of heated and detailed analysis. But this ground has been thoroughly studied by others, so we will not cover it again here.[77]

*Golem science* is science which has the potential to become normal science, but has not yet reached closure to the satisfaction of the core-set. The exposure of the public to Golem science is represented in our Figure 5. For example, in the debate over genetically modified organisms (GMOs), the argument about whether rats' stomach linings are affected by certain kinds of genetically modified potatoes is science of this kind; in the BSE ('mad cow') debate, the question of the strength of the causal link between BSE and Creutzfeld-Jacob disease is science of this kind. In neither of these cases is there any reason to think that the core-set will not reach a consensus eventually, nor is there any reason to want to say that the decision they reach should be influenced by anyone who does not work in a specialist scientific laboratory or medical school. It seems wrong to argue that the outcomes of these decisions should be the prerogative of the political sphere, indeed much of the complaint from the public is that the science has been prematurely passed to politicians who tried to impose a closure to the debates that would reassure the public about the safety of the new technologies when no closure had been reached by the scientists.[78]

This, of course, is *not* to say that the decision about what should be done now about GMOs and BSE can or should be left to certified specialists alone. There are two reasons: firstly, they do not have the answers; and, secondly, they may not have been given questions that correspond with public concerns. For example, their view of what is acceptable in terms of ethics, or risk, may not match the view of the public. Thus, in the case of Golem sciences, it can be seen that the balance of the two spheres of decision-making separated by the horizontal line – the technical and public – is bound to favour the public, as compared to normal science. It should be expected, however, that as time – and it may be many decades – passes, the balance would slowly shift back again as a core consensus is reached.

*Historical sciences*, on the other hand, are those in which it is not to be expected that there will be any closure in the core-set debate in the foreseeable future. Such sciences have also been understood for a long time, even though new developments in science and technology have brought them much more to the fore in recent decades. Historical sciences deal with unique historical trends rather than repeatable laboratory tests.[79] The question of global warming is a historical question; long-term weather forecasting is a historical science; the ecological effects, as opposed to the effects on single organisms, of GMOs is a historical problem. The reason closure on these matters should not be expected in the foreseeable future is that the whole system in which they are embedded is too complicated to model accurately, and may even be impossible to predict accurately because of the working of chaotic processes.

In *reflexive historical sciences*, the potential for uncertainty becomes even greater, as the long-term outcomes are affected by the actions of humans themselves. For example, the science of global warming, as well as being historical (as just explained), is also reflexive. This means that the input variables will include the outcome of political and ethical debates among humans.[80]

When an environmental decision has to be made, Golem and historical science are in some ways similar and in some ways different. They are similar in as far as the scientific input is equally uncertain; but they are different in that the certainty which Golem science can eventually reach through normal scientific processes, cannot be attained in historical sciences. In reflexive historical sciences it cannot be approached without social or cultural regulation. Thus, in the case of all historical sciences, society needs certified and experience-based expertise in the scientific fields belonging to the problem, as well as political input; while in reflexive historical sciences, politics, policy, regulation and sociology enter in the top half of the diagram – expertises in the sciences of politics, policy, and so forth, are needed, as well as political input in the more ordinary sense.[81]

In the case of historical science, the rôle of political and social interests is, perhaps, especially prominent, as there is no hope of any major increase in scientific input, so the institutions that are designed to meld the expert and the inexpert would have more permanence than they would in the case of Golem science. In the case of reflexive historical science, futures must be based not just on permanent social institutions for the regulation of science, but on the development and maintenance of new social institutions for the regulation of social life. In this way, these historical policy sciences are more like the public technologies discussed earlier, as they rely on the participation of the lay public (or at least a large portion of it) for their success.[82] It can be seen, then, that, even when the science is esoteric and controversial, thinking critically about the nature of expertise makes it possible to understand how and when different types of decision-making processes are needed.

## Conclusion

We have argued that, although science studies has made an enormous contribution to our understanding of the relationship between science and society, there is more to do. Wave Two of Science Studies has shown us the many ways in which science cannot solve technical problems in the public domain. In particular, the speed of political decision-making is faster than the speed of scientific consensus formation. As a result of this emphasis, Wave Two's predominant motif has been the need to legitimate technical decisions – to solve the *Problem of Legitimacy.* Decisions will have no legitimacy if they continue to follow the intellectually unsupportable, top-down, authoritarian model of Wave One. Nevertheless, it would be disastrous to solve the Problem of Legitimacy by dissolving the distinction between expertise and democracy. To do this would be to create a new *Problem of Extension*. We argue that expertise *should* feed into the decision-making process, but not in the old Wave One way; solving the Problem of Extension without re-erecting the Problem of Legitimacy depends on recognizing and using new kinds of expertise emerging from non-professional sources. We argue that to do this we need a Third Wave of Science Studies, with the ability to develop a normative theory of expertise. Wave Two has been enormously successful, and continues to be enormously successful, in deconstructing knowledge; without abandoning Wave Two, we now need to reconstruct knowledge and develop Studies of Expertise and Experience – SEE.

We use a series of diagrams to explicate the way science studies has contributed to our understanding of the science/society relationship, and how it might do so in the future. The diagrams indicate that decision-making rights that emerge from the political sphere, and those that grow out of expertise, should not be confounded. We resurrect the old distinction between the political sphere and the sphere of expertise, but in our model the boundary is found in a new place. This boundary is no longer between the class of professional accredited experts and the rest; it is between groups of specialists and the rest. This follows from distinctions that scientists make themselves: in any specialism it is easy to distinguish between a core group of experts and scientists in general, yet the core holds no special professional qualifications. We find that to make these classifications work well we have to distinguish between esoteric sciences, on the one hand, and public technologies, such as cars and computers, on the other.

We go on to indicate, first, that it is possible to have a normative theory of expertise without abandoning the insights or the programme of Wave Two. We begin to show what the components of such a theory might include. We show that we can classify scientific expertise into *interactive expertise* and *contributory expertise*. We show that these ideas emerge from sociologists' own practice, and this offers one persuasive way into a normative theory. We develop some thesis-like propositions using this classification of expertise. We also introduce the ideas of *referred expertise*, *translation* and *discrimination*. In discussing discrimination, we distinguish between ubiquitous and specialist knowledge that has been gathered as a result of local experience. Using these ideas, we argue that scientists as a class have no special contribution to make to technical decision-making in the public domain, and that if there are to be public defences of science, they should concentrate on scientists as specialists, rather than as generalists.

We briefly re-analyse a series of case studies to show how our new categories work. In particular, we show that Brian Wynne's well-known study of the Cumbrian sheep farmers should not be understood as a defence of 'lay expertise', but as the interaction of two communities of experts, one without certificates. We argue that institutions are needed that can translate the knowledge of such pockets of experience-based expertise so as to make it less easy for certified scientists to resist their advice. Such bodies of experts already exist, but tend to be associated with campaigning organizations.

We re-describe the success of the AIDS activists studied by Steven Epstein, using our new term, 'interactional expertise'. We redescribe Harry Collins's study of crash demonstrations, showing that our theory sometimes leads to the conclusion that there should be less public involvement in technical decision-making. That our theory sometimes indicates more and sometimes less public participation seems to us a strength, as compared with the monotony (in the mathematical sense) of other theories that look at the same area of concern. We show that scientists' use of stage magicians to settle certain disputes reveals that in some circumstances professional scientists are happy to absorb pockets of uncertified expertise.

Finally, we argue that this kind of analysis has a dimension that relates to types of science. We distinguish *normal science*, *Golem science*, *historical science*, and *reflexive historical science*, each of which has different implications for our futures. What we have tried to do is to provide a language and some concepts for debating these issues. Each different case of public-domain science will need its own combination of expertise. The sheep farmers were a particularly clear case of the failure to utilize a pocket of experience-based expertise, but the same analysis will not always apply.

The romantic and reckless extension of expertise has many well-known dangers – the public can be wrong.[83] Let us give some examples. When scientific advisers concluded that the battery additive AD-X2, launched in the mid-1940s, had no significant effect, there was an intense lobbying campaign, supported by both industrial and individual users. This campaign eventually led to the Director of the US National Bureau of Standards, Dr Allen Austin, being fired. He was subsequently reinstated following protests from the scientific community, and the battery additive was finally withdrawn from sale in the mid-1960s.[84] More recently, Greenpeace, probably feeding on public acclaim for its actions, blocked the disposal of the 'Brent Spar' oil platform, only to have to admit later that its scientific assessment was incorrect.[85] Similarly, citizen groups, who campaigned in support of Laetrile, a purported cure for cancer that was labelled a hoax by the FDA, seem to have been fooled.[86] More controversially, citizen groups in the USA continue to lobby for creationist science to be taught in schools, while, in the UK, at the time of writing, vaccination levels for measles are falling as a result of an alleged link between the triple measles, mumps and rubella (MMR) vaccine and childhood autism which seems to find virtually no support among the scientific community. These observations merely indicate the kind of work and analysis that has to be done before 'the public' as a whole is attributed with expertise.

The job, as we have indicated, is to start to think about how different kinds of expertise should be combined to make decisions in different kinds of science and in different kinds of cultural enterprise. The job is to contribute to the debate by deploying the science studies community's specialist *contributory expertise* in the matter of the nature of knowledge and expertise. To do this is to embark on SEE, and to act as knowledge scientists. One obvious next step is to find ways to think about how to weight contributory expertise, interactive expertise and referred expertise, along with translation and discrimination, when judgements about a variety of public-domain technologies are made. This has the feel of a classic problem, and we would guess that better scholars than ourselves will discover that the distinction and its consequences have already been discussed in the Greek city state,[87] in the post-1945 debate about the relationship between politicians, civil servants, industrialists, managers, scientists and other producers of culture,[88] and where critics and artists have confronted each other.

Though this is in many ways a programmatic paper, it is meant to do real work in changing the way we look upon the enterprise of science studies and the way it handles questions to do with the relationship between science and society. We argue a case, but also show how the work of building a corresponding structure, theoretical, empirical and institutional, could be carried forward. This is a pressing problem if we are to navigate our way between the Scylla of public disillusion and the Charybdis of technical paralysis.

## Appendix

This paper draws on a range of existing empirical and theoretical work. This Appendix discusses some of this background, but makes no claim to be exhaustive. Instead, the aim of the Appendix is to show how our analysis shares certain concerns already present in STS, though we reformulate the old problems and approach from a different starting point. As noted in the main text, the structure of the Appendix mirrors that of the main paper and the main subject headings in the paper re-appear here. Items of literature are often relevant to more than one heading, so the arrangement under headings is to some extent arbitrary.

## The Problem of Legitimacy and the Problem of Extension

However, we are not the first to have noticed that there is a problem with the way in which science interacts with the wider society. For example, in 1977, Edward Lawless discussed 45 cases of controversial science in the USA that occurred between 1948 and 1973, and listed many more.[89] In the UK, the BSE crisis, documented in the Phillips Report, and public opposition to GM ingredients in food, are two well-known examples.[90] Recent policy documents, such as the 'Science and Society' report produced by the House of Lords Science and Technology Committee, the European Union White Paper on Governance and the Loka Institute's citizen panels set up to consider 'telecommunications and democracy' and 'genetically engineered foods', all show that it is widely recognized that there is a problem to which some response is needed.[91]

Almost invariably, the call has been for greater dialogue between science and the public, and for increased participation in decision-making about science and technology. For example, the Report of the House of Lords Science and Technology Committee recommended:

> That direct dialogue with the public should move from being an optional add-on to science-based policy-making and to the activities of research organisations and learned institutions, and should become a normal and integral part of the process.[92]

Studies have shown that suspicion within the wider society does not manifest itself in respect of every area of science and technology. For example, mobile phones, replacement hip joints and microwave ovens are not perceived as problematic by the public. In 2000, a review of science communication and public attitudes to science in Britain showed that:

> 84% of people think that scientists and engineers make a valuable contribution to society, and three quarters think that science and engineering are good careers, and that science, engineering and technology will provide more opportunities for the next generation.[93]

In the USA, surveys that address the topic of science and technology in general, as opposed to their specific applications in various fields, also show a broad support for science and technology. For example, the most recent edition of the National Science Foundation's *Science and Engineering Indicators* reports that: 'In general, Americans express highly favorable attitudes toward science and technology'.[94] Perhaps surprisingly, a recent *Eurobarometer* survey dealing with European attitudes to biotechnology found that, even in cases where scepticism might be expected to be very strong, respondents were still more likely to agree than disagree with the statement that technologies such as telecommunications, information technology, space exploration and biotechnology, will improve life over the next 20 years.[95] This support was not uniform, however, suggesting that where the public do have concerns about science and technology they seem to be about specific aspects or applications. Thus genetic engineering scores lower than communications and information technology – but nuclear energy alone was more distrusted than trusted.

In the same way, suspicion does not extend to all scientists. Although not enjoying the same level of public support as some professions, opinion poll evidence for the UK routinely shows that scientists are amongst the most trusted sources of information in the public domain. For example, a Mori poll, 'Trust in Scientists', conducted in March 2001 for the British Medical Association, found that 65% of the sample would 'generally trust scientists to tell the truth'.[96] The result has to be put into perspective in that 89% of doctors were trusted in spite of the well publicized cases in which doctors have been seen to be less than honest in their dealings with patients.[97] Teachers were trusted by 86% of the population, professors, judges and the clergy by 78%, and television news readers by 75%.[98] All these, then, were trusted more than scientists. On the other hand, scientists were trusted more than civil servants (43%), trade union officials (39%), business leaders (27%), government ministers (20%), journalists (18%) and politicians generally (17%).[99]

Similar sentiments can also be found in the Advisory Report on the Regulation of Biotechnology, in which the section summarizing the findings of the consultation with the People's Panel notes that:

> Government advisory groups are a trusted mechanism for decision making, but membership should be broadly based. Scientists and healthcare professionals are seen as particularly important contributors to decision-making. There is a widespread demand for as much information as possible and again Government Advisory Groups, scientists, healthcare professionals and consumer and environmental groups would be trusted to provide this information. But the media, retailers and industry were not trusted.[100]

In formulating our definition of the problem, therefore, it is specific episodes of science in context that we use to illustrate our arguments.

One indication of where things might be going wrong can also be found in the same survey data. It turns out that the trust in scientists expressed by these members of the public is sensitive to the wording of the questionnaire, and falls substantially when the scientist is associated with government or industry. What seems to happen is that the distrust of the scientist's organization outweighs the more positive evaluation of science in general, leading to an outcome that is not dissimilar to that for government or industry in general.[101] As a result, a scientist working for the government or industry is seen as much less trustworthy than one without this affiliation. This is what Brian Wynne has referred to as 'scientific body language'.[102]

These survey results are backed up by a range of more qualitative studies. For example, in 1998, Anne Kerr, Sarah Cunningham-Burley and Amanda Amos published the results of a series of focus group discussions on the public perception of, and reaction to, the new genetics. They found that genetic science was frequently interpreted in the context of a wider understanding of the nature of scientific work, noting that discussion in the focus groups included such topics as:

> . . . competition and cooperation among scientists; sources for funding, especially the relationship between the new genetics, pharmaceutical companies, and government; and the relationship between geneticists and the media.[103]

Thus participants in the focus groups did not evaluate science in general, but science in practice, and seemed well aware of the scientists' need for publicity, publications and grant income.

Similar results have been obtained in a wide range of other studies, such as those done at Lancaster University, where the importance of the public perception of scientific institutions has been repeatedly highlighted. Perhaps the most remarkable of these, and certainly the most prescient, is the 1996 study that predicted the controversy over GM foods in the UK a year or two before it actually happened. In the follow-up study (published as *Wising Up*),[104] the same concerns continued to dominate the discussion of GM foods, but the more positive evaluation of mobile phones highlights the fact that the concerns being expressed were not simply general anti-science attitudes, but responses to specific characteristics of the GM food industry. Similar themes have also been identified in a recent paper published in this journal by Steven Yearley, while three overview collections of public understanding of science research, one under the auspices of COPUS, and the others edited by Alan Irwin and Brian Wynne, and by Ian Hargreaves and Galit Ferguson, also contain references to related findings.[105] Together these highlight several of the more robust findings from the public understanding of science research literature. People are not typically concerned with science in general but with particular concrete instances and applications. What is more, in responding to this application of science, they are often sensitive to the uncertainties surrounding the science itself, and do not distinguish between the science and its sponsor. Indeed, one of the most frequent and striking observations of this research (for example, by Alan Irwin, Alison Dale and Denis Smith) is the way the science effectively 'disappears' from the dispute at a relatively early stage.[106]

Finally, surveys conducted in Britain, such as that by Geoffrey Evans and John Durant, and case studies such as that by Ian Welsh, have found that, amongst those most critical of specific applications of science, there can be groups that have considerable scientific knowledge.[107] Likewise, a recent survey of American public opinion, focusing on life sciences and stem cell research, found a similar picture, with 30% of college graduates and 24% of those who considered themselves 'very informed about science' being somewhat or strongly opposed to research using embryonic stem cells.[108] In other words, more knowledge does not necessarily lead to more support. In some cases, the expertise of these stakeholder groups or their representatives will be substantive, and will constitute a direct challenge to the science.

In other cases, stakeholders will question the ability of institutions and regulations to deliver the standards of performance needed for the scientific advice to be implemented safely, challenging, not the science, but the assumptions on which it is based. In the BSE crisis in the UK, regulations concerning the slaughtering and disposal of animals seem not to have been applied, and this seems to be the cause of concern. Other examples from the STS literature include Brian Wynne's discussion of the ways in which people living around the Sellafield (BNFL) nuclear plant interpret statements about safety in the context of previous, largely negative, experiences. Similar themes can also be found in the focus group discussions concerning air quality monitoring in Sheffield that are analysed by Steven Yearley.[109]

These studies inform much contemporary thinking about the way in which the public responds to science. In particular, the studies have been successful in counteracting the 'deficit' model – the argument that public opposition to science followed from public ignorance of science, and can be 'cured' by removing the 'deficit' in the public's knowledge and understanding.[110] It is now acknowledged that what we refer to as 'The Problem of Legitimacy' is much more complex than the deficit model would imply.

Another way to approach the Problem of Legitimacy is from the direction of theoretical developments in social science.[111] Ulrich Beck, for example, argues that modernity is undermining its own institutions and that science, in particular, is increasingly the cause of, and not the solution to, societal problems.[112] At the centre of this development is the recognition that 'invisible' risks, such as those created by radiation, pollution and environmental change, are making concern about the uncertainty and contingency that accompany scientific and technical innovation a central feature of contemporary society. Policy-makers have become pre-occupied with avoiding technologies that may ultimately create more problems than they solve. The emergence of ideas such as 'sustainable development' and the 'precautionary principle', which are central to new policy discourses, shows that the problems exemplified by 'the environment' have crossed traditional boundaries to become, simultaneously, social, cultural, economic, ethical and scientific problems.

A second theme, associated with Anthony Giddens, that runs through the reflexive modernization literature, is the problem of identity that results from the undermining of traditional institutions.[113] The critical element of the risk society literature is that the decoupling of individuals from traditional institutions and rôles has politicized identity and lifestyle. In effect, social life becomes an ever-increasing series of individual choices and responsibilities. In the case of science, this change is manifested in the increasing proliferation of expertise and counter-expertise, and hence the need to make choices about who or what is be trusted in this new context. More generally, the change is reflected in the importance of alternative social movements that provide the social spaces within which the 'sub-politics' of individual life are played out and given meaning. One solution is to find ways of incorporating these new social movements and political alliances within the institutions of governance. We emphasize, however, that this approach begs important questions: How should this be accomplished? How much more inclusive should these new institutions be? Who should be included and who excluded? In our terms, this is the 'Problem of Extension'. The attempt to resolve the Problem of Extension takes us well beyond the reflexive modernization literature.

## Three Waves of Science Studies

As work by authors such as Ian Welsh demonstrates,[114] there was opposition to the power of science and technology even during the high days of Wave One. This shows how broad is the brush with which we are painting. Nevertheless, thinking about Wave One as a coherent body of thought can be legitimated by referring to writers such as Karl Mannheim, who insisted that sociological analysis should draw back when it encountered natural science. Michael Mulkay summarized the key points in Mannheim's sociology of knowledge as follows:

> In the first place, the phenomena of the material world and the relationships between them are seen as being invariant (Mannheim 1936: 116). Mannheim regularly refers to the natural world, and to the concepts appropriate to its study, as being 'timeless and static'. Valid knowledge about such objective phenomena he maintains can be obtained only by detached, impartial observation, by reliance on sense data and by accurate measurement (Mannheim 1952: 4–16; 1936: 168–9). Because the empirical relationships of the natural world are unchanging and universal, the criteria of truth by which knowledge claims are to be judged are also permanent and uniform (1936: 168). It follows that natural science develops in a relatively straght line, as errors are eliminated and a growing number of truths discerned. In short, scientific knowledge evolves through the gradual accumulation of permanently valid conclusions about a stable physical world.[115]

Robert Merton's sociology of science, with its identification of the norms of scientific activity, also contributes to the understanding of science as different from other kinds of knowledge-generating culture.[116]

We do not mean to imply that the intellectual arguments that underpinned the First Wave of Science Studies have disappeared completely. The old arguments tend to be promoted by philosophers and, more recently, by scientists concerned to resist what they see as an attack on science. The latter tendency is part of what has become known as the 'Science Wars'.[117]

We feel it unnecessary in this paper to provide extensive references to Wave Two of Science Studies, except where those studies could be seen to overlap on the project described in the main paper, in particular in describing the basis of expertise.

## The Nature of Expertise

Sheila Jasanoff's studies are an example of Wave Two work which does overlap with our concerns. Her researches into regulatory legal proceedings in the USA are classic examples of the critical insight that the sociological perspective can give.[118] Jasanoff argues that the adversarial nature of the legal proceedings through which US regulatory policies are tested performs a range of useful functions:

> At their most effective, legal proceedings have the capacity not only to bring to light the divergent technical understandings of experts but also to disclose their underlying normative and social commitments in ways that permit intelligent evaluation by lay persons. . . . Controversies about risk are perhaps the domain in which courts have made the most impressive contribution to the civic culture of American science and technology. . . . By insisting on their prerogatives in this regard, courts have repeatedly affirmed that the ultimate power to guide technology policy is vested not in experts but in the citzenry.[119]

In this context, the selection of the witnesses and experts who testify, and their ability to demonstrate their expertise under cross-examination, is crucial. Much effort has been put into developing criteria for the selection of expert witnesses and to instituting various forms of quality control within the courts. Recent examples of this process in action are the three Supreme Court rulings issued in the 1990s that effectively encouraged judges to take a more active rôle in sifting 'expert' testimony, so that juries were only presented with relevant and reliable testimony. This represented a change from previous practice, when such evaluations were typically left to the jury.

These and many other aspects of the American system are described in detail in Jasanoff's publications, and we will not attempt to summarize them here. Instead, we concentrate on the epistemological status of the outcome of this process. As Jasanoff argues, the decision to prohibit or permit something combines scientific content with regulatory power. For example, in order to reach a decision about whether or not a particular chemical or process is hazardous, the court may have to decide whether the 'LD50 test' is appropriate. (This test measures the toxicity of something by establishing the level of exposure at which 50% of the test animals are killed.) Likewise, the courts can argue about whether results from laboratory animals can be generalized to humans. In other words, the regulatory decision cannot be made without attributing credibility to one set of experts and denying it to the other. This implies making a judgement that has traditionally been the preserve of the core-set scientists alone. In the main paper, we separate out the different dimensions of this process. We say that these decisions fall under the 'political phase' of the decision-making process, which deals with the societal response to scientific uncertainty. Decisions about the content of science fall into the 'technical phase'. Unlike some of the more recent attempts to achieve a scientific consensus, or at least minimize controversy in the political phase, by restricting participation to 'approved experts', in our model the scientific decision invariably gets made after the political one.

The legal examples are important because they are one way in which non-scientists become involved in making scientific decisions. Again, this focuses a concentration, as in our Problem of Extension, on the boundary of the decision-making group. During the 'recombinant DNA' debate of the mid-1970s, the Cambridge Experimental Review Board (CERB) pushed these boundaries out further than the courts. The deliberations of the CERB have been analysed from a rhetorical perspective by Craig Waddell.[120] The CERB gave decision-making powers not to judges but to a citizen panel that, in 1976, was asked by the city authorities in Cambridge, Massachusetts, to make recommendations about whether, and under what conditions, research using recombinant DNA techniques would be permitted in the city's universities.[121] The Board comprised 12 members selected from the city's population, and their job was to weigh the evidence presented by both proponents and opponents of the research and make appropriate recommendations. The outcome of the Panel's deliberations was that research was to be permitted, under conditions that broadly mirrored national guidelines, and the whole process was widely seen as a success by all who participated in it.[122] In the context of our paper, the important aspect of the CERB study is that, like the studies of courtrooms, it shows that non-scientists can lend credibility to decisions concerning science and technology.

These are positive arguments for increasing participation, and there are also negative arguments stemming from the failure of more restricted practices. One set is brought out by a case study of the regulation of the chemical 2,4,5,T.[123] The negative arguments focus on the neglect of the assumptions that underpin and frame scientific knowledge claims, and highlight the problems that arise when scientific knowledge is generalized uncritically. One of the successes of Wave Two was to draw attention to the contingency and uncertainty of scientific knowledge whilst also highlighting alternative knowledge(s) that can (or even ought) to complement or replace it.

The case of the regulation of 2,4,5,T (an organophosphate pesticide used by farm workers) is a well-known example of this argument. A Scientific Advisory Committee in the UK concluded that 2,4,5,T was safe to use, subject to the caveat that appropriate precautions were taken. Farm workers, on the other hand, argued that, because the appropriate precautions could not be taken in the day-to-day settings in which the chemical was actually used, then it was not safe. In this case, the embodied experience of the farm workers is advanced as an alternative, contextual knowledge resource that could (and should) have been a legitimate input to the decision-making process, and to which scientists were largely blind.[124] Other settings in which similar ideas receive empirical support include the gendered and culturally specific experiences of science and technology, such as have been discussed by Evelyn Fox Keller, Helen Longino, Sandra Harding and Donna Haraway;[125] the participation of AIDS treatment activists in clinical trials as described by Steven Epstein;[126] the capacity of people with other illnesses or injuries, such as 'miners' lung', CFS and RSI, to contribute to the medical understanding and treatment of their condition, as researched by Hilary Arksey and Michael Bloor;[127] the contribution of community groups to public inquiries and planning processes, as discussed by Arie Rip, Thomas Misa and Johan Schot;[128] and the development of multi-discipinary teams and end-user groups in industry and research, as analysed by Michael Gibbons and his colleagues.[129]

One could say that the tendency to dissolve the boundary between those inside and those outside the community reaches its apogee in 'Actor Network Theory', as first adumbrated by Bruno Latour and Michel Callon. Here even the boundary between human experts and non-human contributors to the resolution of conflict is taken away.[130]

Another contribution to thought about the meaning of 'expert' is provided in another study by Brian Wynne. He describes the experience of apprentices working in the radioactive materials industry. He suggests that the apprentices felt they had no need to contribute to their own safety by trying to understand the science of radioactivity, because they were 'intuitively competent sociologists' and 'vigilant and active seekers of knowledge ... tacitly and intuitively, positioning themselves, using their knowledge of their social relationships and institutions'.[131] Wynne argues that the apprentices used their social understanding as a basis of trust in their employers. In a later paper, referring to the same group, he says that these apprentices' 'technical ignorance was a function of social intelligence'.[132]

There are two ways of looking at Wynne's contribution. It could be an example of what we have called 'local discrimination'. In this case, the apprentices would be seen as using their hard-won specialist competence in understanding the trustworthiness of their particular employers and their own place within the social networks of trust operating in that particular workplace, to assess the safety of the procedures to which they were exposed. There is, however, the danger that this analysis is vulnerable to the same kind of ambiguous interpretation that we find in the case of the sheep farmers – namely, that specialist expertise that is not recognized with a certificate is confounded with the capabilities of humans in general, in virtue of their 'socialness'.[133] The danger is, in our terminology, that local discrimination and more ubiquitous discrimination are being confused.

Thus, let us assume that the apprentices also hold bank accounts. Would Wynne want to say that they felt no need to understand economics because they were, to use his phrases, 'intuitively competent sociologists' and 'vigilant and active seekers of knowledge . . . tacitly and intuitively, positioning themselves, using their knowledge of their social relationships and institutions . . .' when they paid their cheques into the bank? Would he argue that the apprentices would be using their social understanding as a basis of trust in their bankers, and that their economic ignorance was a function of social intelligence? The answer is that if he did say this, he would be right, but he would be talking of the relations of trust in general that pertain throughout any smoothly functioning human society, rather than a specific locally acquired discriminatory ability. Once more, for discrimination to be a useful concept, we must solve the Problem of Extension in respect of those who can discriminate.

We have discussed Stephen Turner's paper at length in the text, and note here that though he makes a useful classification of expertise, he does not discuss the levels of competence within an expertise that license contribution to a technical decision. This difference in emphasis may arise from difference in concerns between the UK and the USA. The STS literature shows that public participation and opposition often start with the 'neighbours' of a technical problem, and in particular those who are directly affected by it or unable to avoid its (potentially) negative consequences. As a reading of James Petersen makes clear, in the USA these concerns chime with a wider tendency to be sceptical of government and to place a high value on public involvement as a mechanism for ensuring accountablity.[134] For example, the Economic Opportunity Act of 1964 actually makes the participation of socially and economically disadvantaged groups in community development plans into a political right. In the 1970s, this idea was extended to include a wider range of science and technology policy areas.[135] Some consequences of these developments can be seen in the referenda that are a growing feature of the US political landscape, especially in relation to science and technology issues such as airport expansion, nuclear power, and the like, the increasing use of public opinion research by administrative agencies, and in experiments with deliberative forums. The claim is that:

> . . . substantial public input ensures a more thorough and open debate on questions of science and technology policy. This is especially important in that the public has so frequently been excluded from decisions on technical questions. In this context, extraordinary measures may be required to facilitate effective citizen participation to counterbalance the current elite domination of technical policy making.[136]

This quotation encapsulates the tension that motivates our paper. Although two reasons are given for participation, only one of them is supported by the STS literature. The first claim, and the one that is supported by STS research, is that public participation ensures fuller debate, which has the effect of ensuring that more of the available options and assumptions are questioned and tested, and perhaps more importantly, seen to be tested. Thus, as Ian Welsh argues, one important rôle played by protest groups is to keep doubts alive in the wider community and maintain this questioning of expert advice.[137] The importance attached to this scepticism has, no doubt, been reinforced by a series of technically based controversies,[138] and the observed failure of past expert advice (for example, with regard to nuclear power as a source of safe and abundant source of energy).

The second reason given, which is not supported by STS, is that public participation redresses élite domination of technical decision-making. This is presumed to be a good thing. But is it? We think the answer begs a solution to the Problem of Extension. The issue can be clarified by asking whether or not the same urge for participation is found in other policy areas and, if it is not, would it make sense to advocate it in these contexts. One of the most striking contrary examples is the case of economic policy, where the tendency in most of the major economies has been to move towards independent central banks, effectively giving the power to make key monetary policy decisions to an élite group. In other words, once the targets have been set, and monitoring mechanisms set up, the responsibility for meeting these targets resides solely with the central bank and its advisers. There is not, however, any direct requirement for more public input to these decisions. In the UK, interest rate decisions are made by the Monetary Policy Committee (MPC), whose members are all economists working mainly in the financial or banking sectors, although industry voices are also represented.[139] In the USA, there is little obvious pressure to make the US Federal Reserve replace Alan Greenspan with a more participatory process.[140]

From a European perspective, the equation between public participation and better decisions is less persuasive, and the 'élitist' starting point to debates about the extension of expertise seems unremarkable. This means that the debate about appropriate sources of advice has a different tone. Although participation is encouraged, this is seen as a problem of efficiency rather than democracy. Thus individuals or groups are said to be able to contribute to a consultation process because they have some relevant experience, rather than in the context of a discourse of rights and accountability. The most formal implementation of the European perspective is to be found in the Constructive Technology Assessment (CTA) approach, described by Arie Rip and his colleagues, which seeks to maximize the benefits from the more informal assessments that are triggered by scientific and technical controversies.[141]

The CTA approach is explicitly sociological, and is closely related to the Second Wave of Science Studies. Within this perspective, the emphasis is on the networked nature of knowledge, with the robustness of knowledge claims being related to the amount of work that one has to do in order to challenge them. Controversies are useful, therefore, because they can destabilize existing networks and expose the work that goes into creating new ones. Social learning occurs as different frames, knowledges and sources of expertise are articulated, and the network made more or less robust. This notion of articulation is important. It implies that only certain types of contributions will promote social learning and that, whilst participation should not be restricted to established institutions and actors, only certain types of contribution are to be welcomed. Thus Rip argues:

> The effectiveness [of extraparliamentary dissent] lies in the attention given to knowledge claims in addition to negotiations between interests, and in the broadening of the agenda that occurs by including more parties in the debate. These advantages are relevant for public participation in general, but have to be set against the disadvantage that rules for interaction and the emergence of consolidation require some boundary. Introducing a new party in the debate may offset the balance of forces; this should only be done when a gain in articulation is to be expected. Concretely, this implies that participation is not a citizen's right *per se*, but has to be earned on the basis of specific claims about the issues in the controversy.[142]

Characterizing the process as one of 'social learning' also has implications for the nature of the outcomes. Consensus is not necessarily the goal, as the process will have worked if all it does is raise awareness of questions and uncertainties. This is particularly important for many of the scientific controversies that occur in policy debates precisely because the existence of controversy itself signifies a lack of consensus. According to the CTA approach, in such circumstances what is needed is a process that will enable the new network of knowledge to be developed in a context in which it is unclear who knows what and what, if anything, needs to be learned. For this process to develop, there needs to be a means of identifying potential participants, processes for orchestrating the interaction between different parties, and a purpose to motivate their interaction. Our paper is aimed mainly at the first of these problems, but we do not deny that the other stages also pose significant difficulties.

For example, one obvious problem faced by any institution dealing with such controversies is that the arguments are not just about science and facts, but about interests and resources. As a result, the forum created for resolving the conflict can become just another resource within it, and is thus used strategically by the participants. Thus, rather than facilitating a Habermas-type discourse between equals, the participatory forum becomes another part of the already contested decision-making process. In this context, Rip cites the case of the 2,4,5,T debate in the USA, where opponents of the use of the herbicide refused to participate in a second 'dispute resolving conference' because they felt the first such conference had co-opted them, and they did not want to be associated with a similar outcome. Similarly, conferences about power in Holland and Austria became so dominated by pro-nuclear groups that the public they wished to persuade actually stayed away![143]

Despite these problems, the need for social learning remains, and the acknowledgement that controversy exists, even if it is badly organized, is still a more useful response than the repression or denial of its legitimacy. These ideas are reflected in the discussions at the recent EU Workshop, 'Democratising Expertise', in which it was accepted that there was a need to involve experts and stakeholders at the earliest possible stage in technical decision-making processes, and to retain their involvement as decisions need to be revisited and re-evaluated in the light of new evidence.[144] The workshop participants also acknowledged that the definition of expertise needs to be broad, and to include theoretical and practical knowledge from across the range of sciences and stakeholder groups, including the public at large, whilst also emphasizing that 'democratising expertise' is not about majority voting in science.[145] Instead, there is a need to elaborate principles on the way expertise is developed, used and communicated, and to develop mechanisms to make expert advice more widely available so that representatives can take more informed decisions.[146] In other words, decisions need to be taken by accountable decision-makers, but the quality and legitimacy of those decisions are enhanced if they are seen to take the full range of views into account.[147]

The idea of extending decision-making rights outward from the generally recognized core-set of certified experts has a resonance with the idea of 'maximum objectivity', which Sandra Harding defines as follows:

> A maximally objective science, natural or social, will be one that includes a self-conscious and critical examination of the relationship between the social experience of its creators and the kinds of cognitive structures favored in its inquiry.[148]

There is, however, a difference between our view and that of the advocates of standpoint science. In our case, participation is predicated on experience-based expertise. In the case of standpoint science, however, political position in society is itself taken to legitimate an input to science; there would be a feminist science, a black science, and so forth.[149] These sciences would be discontinuous with each other. In our model, the contributions of women or members of ethnic groups to science would be continuous with it. Women, blacks, and other groups, would contribute specific experience-based expertise which could be gained no other way, except through participation as members of those groups. They would contribute their special kinds of expertise wherever such expertise was relevant. We would make no claim to legislate in advance for where such expertise was relevant – that would be a matter to be settled in each particular case. But such expertises would almost certainly not be universally relevant – there would be no female or ethnic physics, just distinctive contributions to areas of science by women and certain ethnic groups wherever this was appropriate.

To us it seems strange that academics, in particular, should want to adhere to the opposite view to the one expressed above. The ready acceptance of the idea that science is politicized through and through rules out the possibility of complaint when we find that certain scientific and technical arguments are hopelessly biased by their sources. For example, do we never want to say that the tobacco industry has for years falsified the implications of epidemiological studies out of a concern for selling more cigarettes? Do we want to say, rather, that this was just the tobacco industry's point of view and the only fight there is to be had with them is a political fight, not a scientific fight? Do we want to say that the estimates for the success of Patriot in shooting down Scuds during the Gulf War were not 'illegitimately affected' by the interests of the parties, only 'affected'?[150] Accepting the arguments for standpoint sciences would imply that such concerns are a category mistake because science is indistinguishable from politics. Oddly, the one group of people who would be most affected in terms of loss of power is academics, because their only source of power is the legitimacy of their arguments, critical or otherwise.

It seems, then, important to retain a notion, even it is an idealized one, of a core-set community in which expertise is used to adjudicate between competing knowledge-claims and to determine the content of knowledge. The wider society still has a rôle to play in forming a view about the socially acceptable use of such knowledge and what to do while such knowledge remains contested, but this contribution lies in the political sphere. Lay people as lay people, however, have nothing to contribute to the scientific and technical content of debate. Even specific sets of lay people, as demarcated by gender or colour, have a special contribution to make to science and technology only where it can first be shown that their special experience has a bearing on the scientific and technical matters in dispute.

## Case Studies

Cases of common illness also give rise to analyses which confuse expertise among the general public with the experience-based expertise of a specific group. For example, although Hilary Arksey makes much of the expertise of the public in general, the 'Repetitive Strain Injury' (RSI) sufferers whom she studied were actually a specialist group. As one sufferer said, and with some justification in our view:

> We're the experts: not the doctors, or the consultants, or the physios. We're the ones who have to live with it [RSI] day in day out. It's us they ought to be asking if they want to find out about RSI.[151]

It may not be the case that the RSI victims hold all the keys to their illness, but they surely hold some of them. Arksey seems to think that this discovery licenses a much more general positive evaluation of skills among the public. She uses this to critique what she sees as the élitism of the suggestion that the general public were not able to evaluate the crash tests seen on television. And she is not alone in making such inferences. In a similar vein, Simon Locke argues that books such as *The Golem* series underestimate the extent to which the public are able to understand the limitations of technology.[152] Jon Turney, echoing Locke's position, agrees that 'it is . . . possible to doubt that lay publics are quite as sociologically naive about scientists and scientific knowledge as Collins and Pinch's approach suggests'.[153] Similarly, Brian Wynne says that the public are not 'imprisoned by the experts' control of the technical dimension'.[154]

Our response, as has already been indicated, is to examine the specific expertise involved, rather than to make general claims about the developed expertise of the public. We find that the two are very often confused. Thus Locke considers that the public have little to learn about science from treatments such as are found in *The Golem* series, because they are already chock-full of sociological knowledge about science. What a strange argument! There may indeed be pockets of the public that have such knowledge as a result of their experience, but why think that this is true of the public at large in this one area of (highly disputed) academic study? Once more the discovery of pockets of expertise seems to be being romantically extended to the public as a whole.

Perhaps these authors are mistaking the spreading cynicism about science among the public for sociological expertise. Distrust is easy; sophisticated evaluation is difficult. The hard problem is to make the evaluations sophisticated enough to be able to do more than just criticize; the public has also to struggle with the very, very difficult problem of making positive evaluations if the Problem of Extension is to be resolved for them as well as us.

## Types of Science and Technology

Our discussion resonates with Silvio Funtowicz and Jerry Ravetz's term 'post-normal science'. In their work, post-normal science is the uncertain and controversial science, such as the sciences of the environment, in which the stakes for decision-makers are very high but the uncertainties in the knowledge are enormous. As a result, in these sciences it is impossible to separate facts from the value commitments, themselves often controversial, that underpinned their production. Such a science has only limited epistemological authority and, therefore, only a weak claim to compel action, so that managing risks and uncertainty, which is a political rather than a scientific matter, becomes increasingly important. This means that there is a need for new, more inclusive, decision-making processes:

> Only a dialogue between all sides, in which scientific expertise takes its place at the table with local and environmental concerns, can achieve creative solutions to such problems, which can then be implemented and enforced. Otherwise, either crude commercial pressures, inept bureaucratic regulations, or counterproductive protests will dominate, to the eventual detriment of all concerned.[155]

Although agreeing that there are cases where science alone clearly cannot provide the answers, we believe that the idea of a 'post-normal science' does not help with the Problem of Extension. The trouble is that the concept of 'post-normal' science conflates different themes from the public understanding of science literature by treating different types of expertise and knowledge as if they were interchangeable. These problems are discussed and investigated empirically by Steven Yearley,[156] who also compares the approach of Funtowicz and Ravetz with that of Wynne.

Yearley starts from a similar point to the one we have argued – that there are certain robust findings that have come out of the public understanding of science tradition, but that more general criteria for applying these findings to new contexts remain elusive. According to Funtowicz and Ravetz, we need to focus on the 'quality control' procedures that warrant knowledge claims. In the case of post-normal science, the proposed resolution is for a process of extended peer review, where non-scientist groups bring in 'extended' facts that may be relevant to the matter. As Yearley points out, there are some problems with this, even as a conceptual system. For example, when only one of 'decision stakes' or 'system uncertainty' is high, the need for extended peer review is less clear (for example, in cosmology and major industrial disasters), so that the theory does not always work in practice. Secondly, where any particular issue should be positioned – that is, where the boundaries between normal science, consultancy and post-normal science lie – is itself potentially subject to controversy, so that the identification of post-normal science itself is part of the problem.[157]

Assuming that post-normal science can be identified, the solution proposed by Funtowicz and Ravetz is that controversy be resolved through reducing either system or stake uncertainty through further research such that professional consultancy becomes appropriate. The problem with such an approach is that the research itself is potentially contestable, and it is not obvious how the membership of this extended peer community would be established and inclusion/exclusion criteria maintained. We would argue that the problem thus lies in the conflation of the technical and political phases of the decision-making processes. More research that might ultimately reduce uncertainty could be an appropriate response, and we would argue that its conduct will be enhanced if it draws on a wide range of expertise in its conduct, but it is unlikely to resolve the political problems in a realistic time-frame. Hence in these cases a separate process, based on different criteria, is needed to resolve the political need for action in the short term. In other words, the technical and political processes need to be conducted in parallel, with priority in the first instance going to the political phase.

In arguing this view, we are thus much closer to the position of Wynne, who typically talks about expertise rather than facts (or extended facts). Wynne's work is based on a distinction between different types of uncertainty: risk (where odds are known), uncertainty (where parameters but not odds are known), ignorance (where not even parameters are known), and indeterminacy (where the way in which systems will be used by others cannot be guaranteed). From Wynne's perspective, which is shared amongst much of the CTA literature, there is the sense that although scientific expertise is partial (in the sense that it rests on cultural assumptions and norms, and so on) its 'gaps' can be 'filled' by others with complementary expertise in the relevant areas. These areas might include: local knowledge about the system (natural or social) in which science is to be applied (sheep farmers, farmworkers, slaughterhouse employees, and the like) and knowledge about the past behaviours of the institutions involved, so as to enable them to make (better) informed judgements about whom to trust and whom not to trust (Sellafield inhabitants, people living around chemical factories, and so on). In this way criteria for inclusion emerge based on participation in particular social/cultural settings.

The regulatory problem for Wynne is to increase the attention given to what we don't know (ignorance and indeterminacy), and to find ways of bringing these inside regulatory programmes (that is, to build commitment to precautionary or anticipatory regulation), without reducing them (as typically happens) to the category of 'risk'. It is this translation, and imposition of a particular model of regulation, that is the problem in Wynne's view, which is sceptical both of public acceptance of experts in the past and of the rôle played by expert disagreement now. Once the translation of scientist's knowledge of the 'social' into pseudo-science is recognized, the power of lay knowledge as a critique of science is much more powerful than reflexive modernization as promoted by Beck and Giddens suggests.

Our view is not dissimilar from Wynne's, in terms of how we would understand the nature of knowledge and expertise. Where we do differ is in our willingness to be prescriptive about what should follow from this. In particular, Wynne's categorization of uncertainty and knowledge is typically very effective in structuring empirical data. The problem is how to turn these observations into an institutional response. Wynne repeatedly emphasizes the case-by-case and local nature of knowledge, suggesting no easy algorithm to its identification and incorporation in regulatory processes. Yearley, on other hand, is more optimistic, suggesting that focus groups can perform something like the peer review function suggested by Funtowicz and Ravetz, and provide some of the expertise needed to inform the 'broader debate' that Wynne says is needed. Our aim has been to go one step further and to articulate some of the criteria that could be used to institutionalize these responses more effectively.

We have divided what Funtowicz and Ravetz call post-normal science into three phases. Once these kinds of science are separated they do not seem to involve any deep abandonment of the expertise/value distinction, even though they argue for a stronger input from the bottom half of our diagrams. Thus, in the case of Golem science, the argument would be that the public has no particular rôle to play in developing the scientific consensus, although it may have a legitimate input to policy processes that decide what to do in the absence of scientific consensus. Crucially, however, it may still be the case that among the public there are pockets of expertise that do have a legitimate claim to enter into the core-set, and that these specialist groups should therefore contribute to the developing scientific consensus in a special way.

## Notes

The provenance of this paper is the theoretical work done at Cardiff University in putting together an application for an ESRC Research Centre, the 'Centre for the Study of Expertise and Environmental Policy' (SEEP). This initial work was done in the autumn of 1999, and the bid was submitted on 20 January 2000. Here is the opening paragraph of the submission:

> We face a crisis over the way we make decisions about the environment. We find ourselves caught on the horns of a dilemma: do we maximise the political legitimacy of our decisions by referring them to the widest democratic processes, and risk technical paralysis, or do we base our decisions on the best expert advice and invite popular opposition? This is the crisis that SEEP will address.

A little way below we find:

> Thus, on the academic side we want to create a new way of talking and thinking about *expertise* and *experience* to replace the old discourse about science and truth.

It can be seen that the framework of the argument presented in this paper was already in place at this point. We are grateful to various members of three Cardiff departments – the Schools of City and Regional Planning, Journalism and Media Studies, and Social Sciences – for providing an environment in which the theory could be beaten out. The paper also benefitted from critical comments by members of audiences at Gothenburg University (where a version was presented by Collins in September 1999) and at Cornell University (where a nearly finished draft was presented in November 2001). We are also grateful to Ingemar Bohlin, Martin Kusch, Arie Rip, Steve Yearley, Anne Murcott and members of the

Cardiff KES group for comments on earlier versions. We also thank the referees of the first submitted draft for providing us with the opportunity to improve the paper markedly.

1. *The Chambers Dictionary* (Edinburgh, UK: Chambers Harrap Publishers, 1993), 951.
2. For an approach which grows out of political philosophy, see Stephen Turner, 'What is the Problem With Experts?', *Social Studies of Science*, Vol. 31, No. 1 (February 2001), 123–49. Turner argues that it initially seems hard to square the notion of liberal democracy with the idea of élite groups of experts whose knowledge takes them beyond the reach of normal political judgements. He concludes, however, that a rationale exists for expertise to function in modern democratic societies.
3. Many other contemporary social analysts of science and technology have normative commitments but, so far as we know, none has developed a normative theory of expertise.
4. Among the variations to be found are those expressed by one of our referees, who insisted that the discovery of the negotiability of the boundaries of expertise was in no way connected to the idea that anyone should have a say in expert decisions. It seems to us that if there is no defining criterion for expertise, it follows that there is no way of defining people out of the category, and this invites unlimited extension. It also seems to us that many have read precisely this conclusion into 'Wave Two'. That the referee did not agree merely shows how difficult it is to describe a broad sweep in a way that will take everyone's interpretation into account.
5. The term is used liberally in, for example, Hilary Arksey, *RSI and the Experts: The Construction of Medical Knowledge* (London: UCL Press, 1998).
6. The wider use of the notion of expertise does, of course, do immense work in the debate about artificial intelligence, but precisely because it shows that so much expertise is *restricted* to humans, not machines – that is to say, it *extends* only to the boundary of social beings, and no further: see H.M. Collins, *Artificial Experts: Social Knowledge and Intelligent Machines* (Cambridge, MA: MIT Press, 1990); H.M. Collins and Martin Kusch, *The Shape of Actions: What Humans and Machines Can Do* (Cambridge, MA: MIT Press, 1998). If the term 'lay expertise' has an application, a better one might be to the kind of expertise that is distributed throughout the human race.
7. There were questions concerning the 'social responsibility' of science, but these very problems arose out of science's power; to raise questions of social responsibility is to ignore questions of the foundation of knowledge. That things were not as uniformly simple as our broad brush suggests can be seen in publications such as Anthony Standen, *Science is a Sacred Cow* (London: Sheed & Ward, 1952), and Ian Welsh, *Mobilizing Modernity: The Nuclear Moment* (London: Routledge, 2001).
8. Let us bear in mind that being philosophically 'high and dry' does not mean that positivism does not remain immensely strong in terms of political and economic power, as well as being the predominant driving idea in the tremendously successful natural sciences.
9. We resisted the pun. Both authors continue, unabashed, with their Wave Two-type studies, and so do their colleagues and students.
10. The quotation marks here indicate where we are quoting the words of the referees of an earlier draft of the paper.
11. We understand, of course, that any such contribution is not going to settle the problem 'once and for all' (to quote a critical referee).
12. See the works cited in note 6.
13. The distinction made here is not to be confused with the similar methodological difference between investigating the flow of the river of history while standing in the stream – by studying a contemporaneous science – and by studying it as a historian, after the river has reached its outflow. This is a distinction of methodology, not aims.
14. As has been argued in H.M. Collins and Steven Yearley, 'Epistemological Chicken', in Andrew Pickering (ed.), *Science as Practice and Culture* (Chicago, IL: The University of Chicago Press, 1992), 301–26.

15. As well as the work cited in note 6, see Trevor Pinch, H.M. Collins and Larry Carbone, 'Inside Knowledge: Second Order Measures of Skill', *Sociological Review*, Vol. 44, No. 2 (May 1996), 163–86; H.M. Collins, 'Tacit Knowledge, Trust and the Q of Sapphire', *Social Studies of Science*, Vol. 31, No. 1 (February 2001), 71–85.
16. Collins & Yearley, op. cit. note 14. Let us hasten to add that many Wave Two authors have made valuable 'upstream' contributions, and many of these are discussed in the Appendix and in the main body of the paper. We are simply trying to describe, systematize and set on a firmer foundation, the contribution of the sociology of scientific knowledge to what happens upstream. To give one example, Evelleen Richards has argued that it was part of the duty of science studies to give positive advice on the conduct of science, and Trevor Pinch , in reviewing her work, referred to it as 'third generation' SSK (though his 'first' and 'second' generations did not coincide with our First and Second Waves): see, for example, Brian Martin, Evelleen Richards and Pam Scott, 'Who's a Captive? Who's a Victim? Response to Collins's Method Talk', *Science, Technology, & Human Values*, Vol. 16, No. 2 (Spring 1991), 252–55; Trevor Pinch, 'Generations of SSK' (Review of Richards, *Vitamin C and Cancer*, & Sapp, *Where the Truth Lies*), *Social Studies of Science*, Vol. 23, No. 2 (May 1993), 363–73. Richards and Collins disagreed about whether her work was SSK, and Collins would still say it was not – it was knowledge science. This is the kind of distinction that we are trying to resolve here.
17. H.M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Beverly Hills, CA & London: Sage, 1st edn, 1985; Chicago, IL: The University of Chicago Press, rev. 2nd edn, 1992), passim; H.M. Collins, 'The Meaning of Data: Open and Closed Evidential Cultures in the Search for Gravitational Waves', *American Journal of Sociology*, Vol. 104, No. 2 (September 1998), 293–337.
18. The size of the core-set can be influenced by the 'size' of the claim made – for example, the extent to which it seeks to overturn a small or large part of the conventional theories: see Trevor Pinch, *Confronting Nature: The Sociology of Solar-Neutrino Detection* (Dordrecht: Reidel, 1986) – and the availability of resources which limit the ability of scientists to participate in the debate at all: see Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes, Bucks., UK: Open University Press; Cambridge, MA: Harvard University Press, 1987).
19. Thomas F. Gieryn, 'Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists', *American Sociological Review*, Vol. 48 (1983), 781–95; T.F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (Chicago, IL & London: The University of Chicago Press, 1999).
20. These remarks were made by Steven Yearley, who kindly allowed us to identify him as one referee of an earlier draft of this paper.
21. And this has been known for a long time by those convinced by the arguments supporting moral relativism; moral relativism does not lead to moral anarchy, but to the sad acceptance that, beyond a certain point, moral judgements cannot be justified, but are nevertheless right – one just has to take responsibility for them.
22. Or it might be that the appropriate circle of judgement for a work of art is still wider than the trained critics, and hence the claim that 'I may not know much about art, but I know what I like', is not entirely frivolous. Indeed, some art is intended to make a fool of circles of specialist critics, or to cause us to reflect on the nature of the establishment. But, setting all that aside, should we feel happy with: 'I may not know much about science, but I know what I like'?
23. This is not to say that once upon a time the public, or at least those who witnessed experiments, were not more important to the process of science. And it is not to say that such rights are not being increasingly demanded. It is this latter process in which we are interested.
24. In an unpublished paper to the conference on 'Democratisation Socialised', held at Cardiff University (25–28 August 2000), Harry Collins argued that a demarcation

criterion between science and art could be found in the relationship between the intentions of the author of a paper/work and the interpretation of the consumer – and that in scientific paper-writing, the author's intention must always be to limit interpretative licence, whereas in some forms of art or poetry, it might well be to provoke an unanticipated response or interpretation. Though our main three-fold classification – no special expertise, interactional expertise, and contributory expertise – was initially chosen because it is already present in the discourse and practice of social scientists, it has begun to feel less arbitrary as the argument has developed. The distinction seems to 'pop up all over the place' once one starts to think about these matters. In this case, pressed upon us by our referee, it seems the obvious way to think about the relationship between artists and critics.

25. Steven Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', in Roy Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge, Sociological Review Monograph* No. 27 (Keele, Staffs., UK: Keele University Press; London: Routledge & Kegan Paul, 1979), 139–78.
26. By 'Lysenkoism and the like' we mean cases where state power is used to over-rule scientific conclusions that are subject to broad consensus within the international scientific community. We note that all but the most and least radical of scientific commentators decry, for example, the involvement of the tobacco companies in supporting scientific research aimed at certain conclusions.
27. The degree of 'visibility' of the politics is not, by itself, a good criterion of 'intrinsicness' or 'extrinsicness' of the politics, since degree of visibility is contingent on historical events and contexts (we thank Charles Thorpe for this point). The *criterion* of intrinsicness has to be the extent to which scientists, or other commentators, would willingly endorse the input of politics into the science. To play the Western science 'language game' (and this whole paper stands and falls on an agreement to play it) means being unwilling to endorse, publicly, an input of political influence into science. However irreducible the political input, the politics must remain intrinsic if it is Western science that is being done. As we explain in the Appendix, this means that there are two ways to look at modern 'standpoint theories'. One way is to see the input of new classes of expert, such as women, as experts on women, as a way of reducing already existing political biasses so as to increase the integrity of the science. The other way is to see them as insisting that science is a product of its political milieu, that there are different sciences based on different political viewpoints, and that the influence of the 'standpoint' should be explicit and extrinsic. As we indicate, to argue in the second way is to abandon the language of Western science, something which we stand against.
28. But the compartmentalization is analytically vital. The difference between SSK's descriptions and its prescriptions seem to be at the root of certain earlier heated debates. The prescriptions, as in the case of the justice system, are a matter of knowing how to act appropriately within a set of institutions or 'language games'. Misunderstanding the difference between the analyst's 'is' and the analyst's 'ought' has led to some ghastly confusions: see, for example, Pam Scott, Evelleen Richards and Brian Martin, 'Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies', *Science, Technology, & Human Values*, Vol. 15, No. 4 (Autumn 1990), 33–57; Martin, Richards & Scott, op. cit. note 16; H.M. Collins, 'In Praise of Futile Gestures: How Scientific is the Sociology of Scientific Knowledge?', *Social Studies of Science*, Vol. 26, No. 2 (May 1996), 229–44. One might say that Scott, Richards and Martin, having noticed that SSK has shown that politics is intrinsic to science, believe it should be made extrinsic also. We disagree. When one moves upstream into the area of prescription, one must be aware that one no longer has the analytic privileges and advantages accorded to those who remain downstream. Likewise, staying downstream is incompatible with overt prescription, because symmetry is central to downstream analysis.

29. This phrase is due to Collins, *Changing Order*, op. cit. note 17, 145. The idea has been modified and extended by Donald MacKenzie, who points out that uncertainty and opposition can increase as science enters the policy-making sphere: D. MacKenzie, 'The Certainty Trough', in Robin Williams, Wendy Faulkner and James Fleck (eds), *Exploring Expertise: Issues and Perspectives* (Basingstoke, UK: Macmillan, 1998), 325–29.
30. We talk here of the cognitive debate. As Latour (op. cit. note 18) has argued, there are many factors that make scientific disputes more or less settled in practice.
31. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, IL: The University of Chicago Press, 1962; rev. 2nd edn, 1970); Harry Collins and Trevor Pinch, *The Golem: What You Should Know About Science* (Cambridge: Cambridge University Press, 1st edn, 1993; Cambridge & New York: Canto, 2nd edn, with new afterword, 1998). In the afterword to the second edition of *The Golem*, evidence is used to show the relationship between textbook accounts and other accounts of the foundations of relativity.
32. For example, the UK government's response to the possibility of a link between BSE in cattle and CJD in humans was orchestrated around these ideas, and government statements invariably took the line that there was no risk, or that beef was completely safe: see Barbara Adam, *Timescapes of Modernity: The Environment and Invisible Hazards* (London & New York: Routledge, 1998); B. Adam, 'The Media Timescapes of BSE News', in Stuart Allan, Barbara Adam and Cynthia Carter (eds), *Environmental Risks and the Media* (London & New York: Routledge, 2000), 117–29. The same concern for certainty was also reported in Brian Wynne's study of the sheep-farmers: B. Wynne, 'May the Sheep Safely Graze? A Reflexive View of the Expert–Lay Knowledge Divide', in Scott Lash, Bronislaw Szerszynski and Brian Wynne (eds), *Risk, Environment & Modernity: Towards a New Ecology* (London: Sage, 1996), 44–83; and, more recently, it can be seen in the response to concerns about the safety of the measles-mumps-rubella (MMR) vaccine given to young children and in the possible dangers posed to service men and women, and presumably civilians in war zones, by the use of depleted uranium (DU) ammunition.
33. What is meant is that, like Harold Garfinkel's famous breaching experiments, scientific controversies highlighted the rules of scientific behaviour and their ambivalances: see Harold Garfinkel, 'A Conception of, and Experiments With, "Trust" as a Condition of Stable Concerted Actions', in O.J. Harvey (ed.), *Motivation and Social Interaction* (New York: Ronald Press, 1963), 187–238.
34. See also Jean-Marc Lévy-Leblond, 'About Misunderstandings About Misunderstandings', *Public Understanding of Science*, Vol. 1, No. 1 (January 1992), 17–21.
35. See, for example, Gieryn, opera cit. note 19.
36. In other words, as indicated above, expertise is being treated in the way it would be treated under 'knowledge science'.
37. We are led to ask this question after, on the advice of a referee, re-reading Turner, op. cit. note 2.
38. Our claim in respect of astrology is not that it has never been used to contribute to decision-making at a variety of levels, but that very few of its proponents confuse it with science, any more than they would confuse the sayings of an oracle with science.
39. Turner, op. cit. note 2.
40. Part of our job could be described as helping to realize such continuities in expertise as continuities in social and cognitive networks.
41. Wave Two studies show that many of the arguments used by scientists to exclude some whole field or other from scientific consideration are based on risible or disingenuous oversimplifications of the way their own sciences work, but this is not to make the other fields valid: H.M. Collins and Trevor J. Pinch, 'The Construction of the Paranormal: Nothing Unscientific is Happening', in Wallis (ed.), op. cit. note 25, 237–70. The stress on the orthogonal nature of decisions about fields and decisions

about expertise within fields, and the subsequent setting out of Figure 8, emerged from the discussion at Cornell University in November 2001, mentioned above.

42. For a discussion of expertise and experience, see Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago, IL & London: The University of Chicago Press, 1995).
43. To anticipate a potential question, what we will call 'interactional competence' in an expertise can be a Type I interactional competence, even though it is not itself a full-blown Type I expertise.
44. There is a terminological difficulty here. Turner classifies expertise, rather than competence within an expertise. We want to talk about competence within an expertise. Unfortunately, the possession of certain expertise is also seen as a sign of competence, as when we say that certain humans are 'more competent at sports' than others if they possess more sports expertise. We don't think the terminological untidiness causes any great problems, however, as the meaning should always be clear from the context.
45. For example, into tacit and explicit knowledge, with two different more detailed classifications within these broad categories: see, for example, the work of Collins cited in notes 6 and 15. With another author, he has also divided human abilities into 'polimorphic' and 'mimeomorphic': Collins & Kusch, op. cit. note 6, passim. Probably the most currently well-known classification of expertise is that due to Hubert and Stuart Dreyfus (H.L. Dreyfus and S.E. Dreyfus, *Mind Over Machine: The Power of Human Intuition and Expertise in the Era of the Computer* [New York: Free Press, 1986]), but the Dreyfus model is not appropriate for answering the kinds of question we pose here.
46. Researchers in the sociology of scientific knowledge have long understood how difficult it is to employ research assistants, precisely because the skills needed to do the research are not the generic skills of the broadly trained social scientist, but must include interactional skills in the substantive topic of the field study. Here our starting point in the esoteric sciences is felicitous. Sociologists who do not study the esoteric sciences may not be so familiar with these distinctions, and may find them less immediately persuasive, but these distinctions are useful ones nevertheless. As was pointed out at the discussions at Cornell University in November 2001, this classification is very broad, and it may be that more refined classifications are needed. Nevertheless, this classification is all that is necessary to 'hammer in a piton'.
47. Collins experienced complete failure in his attempts to acquire interactional competence in the field of amorphous semiconductors.
48. Collins acquired enough competence to make significant published contributions to the field of the investigation of paranormal metal bending: B.R. Pamplin and H.M. Collins, 'Spoon Bending: An Experimental Approach', *Nature*, Vol. 257 (4 September 1975), 8. Of course, an identical defence could be made of the nature of science, and is made in the *tu quoque* argument. That is to say, in our work, we act as though there is such a thing as science. But this presents no problem, so long as our relativism is of the methodological kind. Likewise, there is nothing in this argument to prevent analyses based on methodological relativism in respect of expertise. We are just demonstrating another way to go about things.
49. Brian Wynne, 'Sheep Farming after Chernobyl: A Case Study in Communicating Scientific Information', *Environment*, Vol. 31, No. 2 (1989), 10–15, 33–39; Wynne, op. cit. note 32; B. Wynne, 'Misunderstood Misunderstandings: Social Identities and Public Uptake of Science', in Alan Irwin and Brian Wynne (eds), *Misunderstanding Science? The Public Reconstruction of Science and Technology* (Cambridge, New York & Melbourne: Cambridge University Press, 1996), 19–46.
50. Here we do not discuss the power relationships and protection of vested interests. Through our discussions, we merely want to use academic argument to lessen the impact of these interests in future incidents of this sort, by lessening their legitimacy.
51. None of this is to claim that making established scientists listen will be easy. For the AIDS case, see Steven Epstein, 'The Construction of Lay Expertise: AIDS Activism

and the Forging of Credibility in the Reform of Clinical Trials', *Science, Technology, & Human Values*, Vol. 20, No. 4 (Autumn 1995), 408–37. There is a wider question about the extent to which the treatment activists represented the whole gay community, let alone the still more heterogeneous group of people suffering from AIDS.

52. We are ignoring, for the purposes of our argument, the very obvious fact that the managers are also likely to be much better scientists than any visiting sociologist.
53. As well as the technical abilities remarked on in the quotation and the previous note.
54. Though in the case in question, some scientists thought that the referral was from too distant a site. They thought that high-energy physics, from where the managers came, gave them a misleading picture of the skills required to do interferometry. General Groves, who managed the Manhattan Project, was an interesting case who would seem to contradict this argument: see Charles Thorpe and Steven Shapin, 'Who Was J. Robert Oppenheimer? Charisma and Complex Organization', *Social Studies of Science*, Vol. 30, No. 4 (August 2000), 545–90.
55. Peter L. Berger, *Invitation to Sociology* (Garden City, NY: Anchor Books, 1963), passim; Collins & Yearley, op. cit. note 14.
56. The problem of translating between self-contained cultures, 'paradigms' or 'forms-of-life', is an old one: see, for example, H.M. Collins and Trevor J. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (Henley-on-Thames, UK: Routledge & Kegan Paul, 1982). In the history of science, it has been alluded to under the heading of 'trading zones': see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, IL: The University of Chicago Press, 1997), passim. Skills of journalists are compared with those of sociologists in Phillip M. Strong, 'The Rivals: An Essay on the Sociological Trades', in Robert Dingwall and Philip Lewis (eds), *The Sociology of the Professions: Lawyers, Doctors and Others* (London: Macmillan, 1983), 59–77.
57. These judgements are not dissimilar to those made by scientists within the scientific community. Thus Lewis Wolpert has said that 'scientists must make an assessment of the reliability of experiments. One of the reasons for going to meetings is to meet the scientists in one's field so that one can form an opinion of them and judge their work': L. Wolpert, 'Review of *The Golem*', *Public Understanding of Science*, Vol. 3, No. 3 (July 1994), 328–29, at 329. We will go on to discuss the relationship between our concept and similar issues discussed by Brian Wynne in 1992 and 1993: B. Wynne, 'Public Understanding of Science Research: New Horizon or Hall of Mirrors?', *ibid.*, Vol. 1, No. 1 (January 1992), 37–43; B. Wynne, 'Public Uptake of Science: A Case for Institutional Reflexivity', *ibid.*, Vol. 2, No. 4 (October 1993), 321–37. We, however, distinguish between specialist and ubiquitous expertises.
58. Poor social judgements are the problem with those who believe in, say, newspaper astrology *as a scientific theory*. They are making a social mistake: they do not know the locations in our society in which trustworthy expertise in respect of the influence of the stars and planets on our lives is to be found.
59. For a similar argument in respect of the rejection of claims about the existence of gravitational waves, see H.M. Collins, 'Tantalus and the Aliens: Publications, Audiences and the Search for Gravitational Waves', *Social Studies of Science*, Vol. 29, No. 2 (April 1999), 163–97.
60. Increasing the potential for debates about who is in and who is out – a typical boundary problem.
61. To make the point from the opposite side, so-called 'junk scientists', such as many of those who are called as expert witnesses in court rooms, often have paper credentials, but are not counted as experts by their peers.
62. The term 'phase' is used here in the materials-science sense – as in a 'phase diagram' for a material – rather than in the time-sequence sense.
63. Epstein, op. cit. note 51; Steven Epstein, *Impure Science: AIDS, Activism, and the Politics of Knowledge* (Berkeley, Los Angeles & London: University of California Press, 1996).

64. In the case of the sheep farmers, there was probably never a nexus.
65. If Figure 6 is taken to represent the Cumbrian case, there would be no solid-line nexus at all between the core-set and the 'pocket'. The dotted-line nexus would stay where it is, however – the sheep farmers should have been in the core-set from early in the game.
66. For a full account, see H.M. Collins, 'Public Experiments and Displays of Virtuosity: The Core-Set Revisited', *Social Studies of Science*, Vol. 18, No. 4 (November 1988), 725–48.
67. This sentence is not as naïve as it appears. Compare what has been said about the Edinburgh phrenology case. We are not trying to suggest any hard and fast distinction between science and politics, nor are we suggesting that these tests and their interpretations could have been carried out completely 'objectively'. What we are suggesting is that the way in which the political sphere encroached on the technical sphere in these cases was clearly illegitimate under almost any analysis of science. There is no difficulty in making prescriptive statements about it.
68. Collins was able to demonstrate the incompetence of audiences of university personnel by showing them a film of the crash, and asking them to criticize it without prompting. They always failed to notice the visible features that had been pointed out by Greenpeace's experts.
69. This is not to say that there are not groups of experience-based experts in different aspects of the safety of the transport of nuclear fuel in the population as a whole. For example, there are pockets of experience-based expertise concerning the degree of radioactivity on sections of rail (and sidings) used for railway transport. But these people are experts and, by that fact alone, not ordinary.
70. In the case of the train crash, the experts who pointed out the deficiencies of the test came from Greenpeace; in the case of the aircraft crash, the experts (who were represented on a subsequent TV programme) were from ICI – the manufacturers of AMK.
71. As Michael Bloor argues: see M. Bloor, 'The South Wales Miners Federation, Miners' Lung and the Instrumental Use of Expertise, 1900–50', *Social Studies of Science*, Vol. 30, No. 1 (February 2000), 125–40, at 126.
72. There is also the danger that this form of account takes us back to the sociology of error, in which deviant science is explained in a different way to 'proper' science.
73. The correct analysis varies from case to case, but we suspect that the motivation is most often of the first kind, as the stage magicians do not (and are not expected to) adopt the norms of the scientific community, such as honesty and openness. In either case, the welcoming of magicians into the heartland of science makes the point about the permeability of professional boundaries.
74. See, for example, Eric Von Hippel, *The Sources of Innovation* (New York: Oxford University Press, 1988). The rôle of these lead users is not entirely unproblematic, however, and, as Phil Agre has argued, can lead to the neglect of novice users in the design of technology: consult Agre's website: <*http://dlis.gseis.ucla.edu/pagre/*>. The result of this is that inefficient designs, particularly of IT interfaces, become embedded social practices, as manufacturers and lead users overlook the increasingly complex training and restructuring that is needed to make the machines work: see <*http://commons.somewhere.com/rre/2000/RRE.notes.and.recommenda19.html*>.
75. Wiebe E. Bijker, Thomas P. Hughes and Trevor Pinch (eds), *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology* (Cambridge, MA: MIT Press, 1987); W.E. Bijker, *Of Bicycles, Bakelites, and Bulbs: Toward a Theory of Sociotechnical Change* (Cambridge, MA: MIT Press, 1995).
76. Thinking about planning debates brings out two other kinds of ability that belong below the line in the diagram. There is the ability of the middle-class protestors and professional lobbyists, who know how to present an argument and how to penetrate the appropriate networks; and there is the skill of the activists who know how to cause the authorities the maximum inconvenience and expense by climbing trees, burrowing

into tunnels in the ground, and so forth. We could thus add these types of ability to the discriminatory and translation skills we identified earlier.

77. See, for example, Michael Lynch and David Bogen, *The Spectacle of History: Speech, Text, and Memory at the Iran-Contra Hearings* (Durham, NC: Duke University Press, 1996); Sheila Jasanoff, *The Fifth Branch: Science Advisers as Policymakers* (London & Cambridge, MA: Harvard University Press, 1990); S. Jasanoff, *Science at the Bar: Law, Science, and Technology in America* (Cambridge, MA & London: Twentieth Century Fund & Harvard University Press, 1995); Brian Wynne, *Rationality and Ritual: The Windscale Inquiry and Nuclear Decisions in Britain*, BSHS Monograph No. 3 (Chalfont St Giles, Bucks., UK: British Society for the History of Science, 1982); Roger Smith and Brian Wynne (eds), *Expert Evidence: Interpreting Science in the Law* (London: Routledge, 1989).
78. As we will explain in the Appendix, Silvio Funtowicz and Jerome Ravetz misleadingly refer to this kind of situation as 'post-normal', whereas it is simply 'pre-normal': see S.O. Funtowicz and J.R. Ravetz, 'Science in the Post-Normal Age', *Futures*, Vol. 25, No. 7 (September 1993), 739–55.
79. Karl Popper, in his *The Poverty of Historicism* (London: Routledge & Kegan Paul, 1957), used the term 'historicist' to refer to teleological theories which assume a progressive historical trend. We do not discuss progressiveness, only sciences that deal with long-term unique changes.
80. This kind of science has been examined by Barry Barnes in the context of economic decision-making: B. Barnes, *The Nature of Power* (Cambridge/Oxford, UK: Polity Press/Basil Blackwell, 1988), passim.
81. There is a certain symmetry here: just as the scientific community is the appropriate location for disposing of political influence as it impinges on the construction of knowledge, so the polity is the appropriate locus for decisions about the societal response to uncertain knowledge.
82. For example, do household conservation policies increase or decrease the output of greenhouse gases when one takes into account the environmental cost of collection and processing of recyclable waste? For a discussion of the role of SSK in urban energy policies, see Robert J. Evans, Simon Marvin and Simon Guy, 'Making a Difference: Sociology of Scientific Knowledge and Urban Energy Policies', *Science, Technology, & Human Values*, Vol. 24, No. 1 (Winter 1999), 105–31; for economic policy as social technology, see Robert Evans, *Macroeconomic Forecasting: A Sociological Appraisal* (London: Routledge, 1999).
83. Apologies to Malcolm Ashmore for this un-ironic use of the word 'wrong': M. Ashmore, 'Ending Up On the Wrong Side: Must the Two Forms of Radicalism Always Be at War?', *Social Studies of Science*, Vol. 26, No. 2 (May 1996), 305–22.
84. See Edward W. Lawless, *Technology and Social Shock* (New Brunswick, NJ: Rutgers University Press, 1977, at 418–25.
85. The Greenpeace version of this story is available on their website at: <*http://www.greenpace.org/~comms/toxics/dumping/jun20.html*>.
86. See James C. Petersen and Gerald E. Markle, 'Politics and Science in the Laetrile Controversy', *Social Studies of Science*, Vol. 9, No. 2 (May 1979), 139–66.
87. Bent Flyvbjerg discusses the Aristotelian concept of 'phronesis', which is a form of practical wisdom in a moral setting; prudence and wisdom capture some of its flavour: see B. Flyvbjerg, *Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again* (Cambridge: Cambridge University Press, 2001), passim. Unfortunately, the concept is somewhat slippery and includes components both of the political and of experience. To use the concept with confidence in this discussion, one would first need to redescribe natural science, using the term in the light of what we have learned about science over the last decades. Our paradigm case – the post-Chernobyl Cumbrian sheep farmers as discussed by Wynne – would not seem to benefit from the introduction of the term 'phronesis'. The point is that the sheep farmers had technical knowledge of sheep ecology, not prudent understanding of how

to act in a situation requiring ethical judgement, which is an essential element in Flyvbjerg's usage.

88. On artists in the media, see Frank Muir, *A Kentish Lad* (Reading, Berks., UK: Corgi Books, 1997). Muir explains the mass defection of programme-makers from London Weekend Television when the Board of Directors sacked their talented boss. He says (324–25):

> There was no contact at all between the board and the creative side of the company . . . Lord Campbell told us that in his experience all management was the same. 'You unit heads may think that managing talented producers and performers raises special problems but I have been in sugar all my life and I can assure you that the management of people in television is precisely the same as the management of sugar workers'.

On scientists, see Turner, op. cit. note 2, and David H. Guston, 'Evaluating the First US Consensus Conference: The Impact of the Citizens' Panel on Telecommunications and the Future of Democracy', *Science, Technology, & Human Values*, Vol. 24, No. 4 (Autumn 1999), 451–82; and for scientists and government, D.H. Guston, *Between Politics and Science: Assuring the Integrity and Productivity of Research* (Cambridge: Cambridge University Press, 2000).

89. Lawless, op. cit. note 84.

90. The Phillips Report was critical of the way in which scientific advice is solicited, interpreted and used. In particular, caveats inserted in the original advice were not given sufficient weight, contradictory evidence was discounted, and the initial recommendations were not reviewed often enough. The full report is available on the internet at <*www.bse.gov.uk*>. See also Anne Murcott, 'Not Science but PR: GM Food and the Makings of a Considered Sociology', *Sociological Research Online*, Vol. 4, No. 3 (September 1999); A. Murcott, 'Public Beliefs about GM Foods: More on the Makings of a Considered Sociology', *Medical Anthropology Quarterly*, Vol. 15, No. 1 (March 2001), 1–11.

91. House of Lords Science and Technology Committee, Science and Society (London: HMSO, 2000); European Union White Paper on Governance: Broadening and Enriching Public Debate on European Matters, *Report of the Working Group on Democratising Expertise and Establishing Scientific Reference Systems*, available on the internet at <*www.cordis.lu/rtd2002/science-society/governance.htm*>; Loka Institute <*www.loka.org*>, 'telecommunications and democracy' (April 1997); 'genetically engineered foods' (February 2002). The Loka Institute website provides links to reports on over 40 consensus conferences held in over a dozen countries.

92. House of Lords, op. cit. note 91, paragraph 5.48. Guidance on how government departments should put these principles into practice are given in the Office of Science and Technology (OST) publication, *Guidelines 2000: Scientific Advice and Policy Making*, available on the internet at <*http://www.dti.gov.uk/ost/aboutost/guidelines.htm*>, and in the *Code of Conduct for Written Consultations* produced by the Cabinet Office: <*http://www.cabinet-office.gov.uk/servicefirst/2000/consult/code/ConsultationCode.htm*>.

93. Wellcome Trust and the Office of Science and Technology, *Science and the Public: A Review of Science Communication and Public Attitudes to Science in Britain* (London: Wellcome Trust & OST, 2000), 8.

94. *NSF Science and Engineering Indicators, 2000*, quote at page 8–13 of on-line PDF version, available at <*http://www.nsf.go/sbe/srs/seind00/start.htm*>.

95. *Eurobarometer 52.1: The Europeans and Biotechnology* (Brussels: EU, 2000), available via the internet from <*http://europa.e.int/comm/dg10/ep/eb.html*>.

96. Perhaps surprisingly, the support for scientists was higher amongst younger people, defined as those aged between 15 and 24; it was 79%, higher than that for the sample as a whole.

97. Recent examples in Britain include: Dr Harold Shipman, a former GP in Manchester who is currently in prison after being found guilty of murdering over a dozen of his patients, and being implicated in the deaths of many more; the scandals at the Bristol Children's Hospital, where doctors continued to operate, despite much higher death

rates and the concerns of their colleagues; and the retention of children's organs by pathology labs without their parents' knowledge.

98. There are clearly some inconsistencies here. For example, professors receive a different rating to scientists, though it is not clear what the distinction is, as it is possible to be both. Similarly, news readers score significantly more highly than journalists, despite the fact that what they read is the product of journalistic endeavour (and many of them are trained and experienced journalists).
99. Michelle Corrado, 'Trust in Scientists', paper presented at the Annual Meeting of the British Association for the Advancement of Science (Glasgow, 3–7 September, 2001); also available at the Mori website: <*www.mori.com*>.
100. *The Advisory and Regulatory Framework for Biotechnology: Report from the Government's Review* (London: HMSO, 1999), quote at paragraph 36; available via the internet from: <*http://www.dti.go.uk/ost/rmay99/Bioreport_1.htm*>.
101. The survey data supporting this observation is summarized in Mori, *The Role of Scientists in Public Debate: Full Report* (London: Wellcome Trust & Mori, 2000), available via the internet from: <*http://www.wellcom.ac.uk/en/1/mismscnesos.html*>.
102. Interview by Brian Wynne given to River Path Associates, who were conducting research on science communication for the British Association: The River Path Report, 'Now for the Science Bit – Concentrate!', was published in 1997, and is available via the internet from: <*http://www.riverpth.com/library/*>.
103. Anne Kerr, Sarah Cunningham-Burley and Amanda Amos, 'The New Genetics and Health: Mobilizing Lay Expertise', *Public Understanding of Science*, Vol. 7, No. 1 (January 1998), 41–60, at 48.
104. Robin Grove-White, Phillip Macnaghten and Brian Wynne, *Wising Up: The Public and New Technologies* (Lancaster, UK: Centre for the Study of Environmental Change, Lancaster University, 2000).
105. Steven Yearley, 'Computer Models and the Public's Understanding of Science', *Social Studies of Science*, Vol. 29, No. 6 (December 1999), 845–66; COPUS, *To Know Science is to Love It? Observations from Public Understanding of Science Research* (London: COPUS & the Royal Society, no date); Irwin & Wynne (eds), op. cit. note 49; Ian Hargreaves and Galit Ferguson (eds), *Who's Misunderstandng Whom? Bridging the Gap Between the Public, the Media and Science* (Swindon, UK: Economic and Social Research Council [ESRC], 2000).
106. Alan Irwin, Alison Dale and Denis Smith, 'Science and Hell's Kitchen: The Local Understanding of Hazard Issues', in Irwin & Wynne (eds), op. cit. note 49, 47–64.
107. Geoffrey Evans and John Durant, 'The Relationship Between Knowledge and Attitudes in the Public Understanding of Science in Britain', *Public Understanding of Science*, Vol. 4, No. 1 (January 1995), 57–74; Welsh, op. cit. note 7.
108. A full report based on this survey is available from: <*http://www.vcu.ed/lifesciencessurvy/*>.
109. Wynne, op. cit. note 32; Steven Yearley, 'Making Systematic Sense of Public Discontents with Expert Knowledge: Two Analytical Approaches and a Case Study', *Public Understanding of Science*, Vol. 9, No. 2 (April 2000), 105–22.
110. For a brief statement of the 'deficit model', and a discussion of its weaknesses, see Alan Irwin, 'Science and its Publics: Continuity and Change in the Risk Society', *Social Studies of Science*, Vol. 24, No. 1 (February 1994), 168–184, at 170–72. See also Simon Locke, 'Golem Science and the Public Understanding of Science: From Deficit to Dilemma', *Public Understanding of Science*, Vol. 8, No. 2 (April 1999), 75–92, esp. the references listed at 90, note 14.
111. As we explain in the main text, Turner (op. cit. note 2) takes the Problem of Legitimacy to be one based in political philosophy.
112. Ulrich Beck, *The Risk Society: Towards a New Modernity* (London: Sage, 1992), 166 & passim.
113. Anthony Giddens, *The Consequences of Modernity* (Cambridge, UK: Polity Press, 1990), esp. 124–34.
114. Welsh's research on the nuclear industry clearly demonstrates that there was organized opposition to nuclear power from the moment it was first proposed: Welsh, op. cit. note 7.

115. Michael Mulkay, *Science and the Sociology of Knowledge* (London: George Allen & Unwin, 1979), 11. Cited works are: Karl Mannheim, *Ideology and Utopia* (New York: Harcourt, Brace & World, 1936); K. Mannheim, *Essays on the Sociology of Knowledge* (London: Routledge & Kegan Paul, 1952).
116. See, for example, Robert K. Merton, *The Sociology of Science* (Chicago, IL & London: The University of Chicago Press, 1973). See also Michael Mulkay, 'Norms and Ideology in Science', *Social Science Information*, Vol. 15 (1976), 637–56. Here Mulkay questions the adequacy of the norms as we move into the period of Wave Two.
117. Lewis Wolpert, *The Unnatural Nature of Science: Why Science does not make (Common) Sense* (London: Faber & Faber, 1992); Paul R. Gross and Norman Levitt, *Higher Superstition: The Academic Left and its Quarrels with Science* (Baltimore, MD & London: Johns Hopkins University Press, 1994); P.R. Gross, N. Levitt and Martin W. Lewis (eds), *The Flight From Science and Reason, Annals of the New York Academy of Sciences*, Vol. 775 (24 June 1996), i–xi, 1–593; Richard Dawkins, *Unweaving the Rainbow: Science, Delusion and the Appetite for Wonder* (London: Penguin, 1999); Noretta Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths About Science* (Oxford: Oxford University Press, 2000). A more recent attempt to develop a reasoned dialogue about the nature of science can be found in Jay Labinger and Harry Collins (eds), *The One Culture? A Conversation About Science* (Chicago, IL: The University of Chicago Press, 2001).
118. Jasanoff, opera cit. note 77.
119. Jasanoff (1995), op. cit. note 77, 215.
120. Craig Waddell, 'The Role of Pathos in the Decision-Making Process: A Study in the Rhetoric of Science Policy', *Quarterly Journal of Speech*, Vol. 76 (1991), 381–400; C. Waddell, 'Reasonableness Versus Rationality in the Construction and Justification of Science Policy Decisions: The Case of the Cambridge Experimentation Review Board', *Science, Technology, & Human Values*, Vol. 14, No. 1 (Winter 1989), 7–25. For a more critical account of the CERB's deliberations, see Rae S. Goodell, 'Public Involvement in the DNA Controversy: The Case of Cambridge, Massachusetts', *ibid.*, Vol. 4 (Spring 1979), 36–43.
121. Of course, it is possible to question the sense in which the members of the CERB 'represented' the population of Cambridge. Clearly they were unusual in that they were selected for, and then chose to be involved in, a complex scientific and technical controversy. For an account by a CERB member, see Sheldon Krimsky, *Genetic Alchemy* (Cambridge, MA: MIT Press, 1982); for a comprehensive account of the whole debate, see Susan Wright, *Molecular Politics: Developing American and British Regulatory for Genetic Engineering, 1972–1982* (Chicago, IL & London: The University of Chicago Press, 1994).
122. Waddell argues that whilst the rhetoric of science may be of facts and not emotion, the transcripts of the proceedings clearly reveal that advocates of both sides combined rational arguments (*logos*) with the other two elements of rhetoric (*ethos* or integrity, and *pathos* or emotional argument). In the end it was the combination of all three, with concrete examples of sick children being cured by a committed physician as a result of the research, that seems to have won the day.
123. Alan Irwin, *Citizen Science: A Study of People, Expertise and Sustainable Development* (London & New York: Routledge, 1995), 17–21 & passim.
124. How this participation is to be accomplished is, of course, another matter. The next section discusses the difficult choices farm workers and their representatives faced when they were, eventually, invited to participate in a US conference on 2,4,5,T.
125. Examples include: Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, CT: Yale University Press, 1985); E.F. Keller and Helen E. Longino (eds), *Feminism and Science* (Oxford: Oxford University Press, 1990); Sandra Harding, *Is Science Multicultural?: Postcolonialisms, Feminisms and Epistemologies* (Bloomington: Indiana University Press, 1998); Donna Haraway, *Modest_Witness@Second_Millennium.FemaleMan@Meets_OncoMouse™: Feminism and Technoscience* (New York: Routledge, 1997).
126. Described in Epstein, op. cit. note 63.
127. Arksey, op. cit. note 5; Bloor, op. cit. note 71.

128. Arie Rip, Thomas J. Misa and Johan Schot (eds), *Managing Technology in Society: The Approach of Constructive Technology Assessment* (London & New York: Pinter, 1995).
129. Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott and Martin Trow, *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies* (London, Thousand Oaks, CA & New Delhi: Sage Publications, 1994).
130. For an exchange of views on this issue, see the papers contained in Pickering (ed.), op. cit. note 14. The burden of our paper, of course, is to resurrect boundaries between expert and non-expert in order to resolve the Problem of Extension.
131. Wynne (1992), op. cit. note 57, 39.
132. Wynne (1993), op. cit. note 57, 328.
133. Collins, op. cit. note 6; Collins & Kusch, op. cit. note 6; H.M. Collins, 'Socialness and the Undersocialized Conception of Society', *Science, Technology, & Human Values*, Vol. 23, No. 4 (Autumn 1998), 494–516.
134. James C. Petersen, 'Citizen Participation in Science Policy', in J.C. Petersen (ed.), *Citizen Participation in Science Policy* (Amherst: University of Massachusetts Press, 1984), 1–17.
135. This is not to deny that the relevant agencies may not have embraced these changes as enthusiastically as they might. In practice, therefore, although procedural changes have enabled public representatives to participate in the development of policies, most of the reforms have focussed on making data and information available. As a result, they do not fundamentally challenge the existing definitions of who/what is an expert. In other words, they remain based on, and do little to challenge, a deficit model in which expertise is a resource denied to the socially and economically disadvantaged and abundantly available to the powerful. Participation therefore depends on redistributing expertise, but does not assume that it lies outside the scientific community. (See Petersen, op. cit. note 134, 26.)
136. Petersen, ibid., 6–7.
137. See, for example, Welsh, op. cit. note 7, passim. It is worth noting, however, that the importance attached to this is perhaps peculiar to the USA, and that culture's longstanding distrust in its own institutions. The contrast between US systems and those employed in France is clearly brought out in Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1995), esp. Chapters 6 and 7.
138. See Lawless, op. cit. note 84.
139. Information about the MPC's membership, past and present, can be found on the Bank of England website: <*www.bankofengland.co.uk*>.
140. Certainly, we would expect Mr Greenspan, and other central bankers, to consult widely, but there is no suggestion that anyone other than this group should weigh up the evidence and make the decisions.
141. Rip, Misa & Schot (eds), op. cit. note 128; Arie Rip, 'Controversies as Informal Technology Assessment', *Knowledge: Creation, Diffusion, Utilization*, Vol. 8, No. 2 (December 1986), 349–71.
142. Rip, ibid., 363–34.
143. Ibid.
144. EU Workshop on 'Democratising Expertise' and Establishing a European Scientific Reference System (Brussels, 30 March 2001); report available from: <*http://europa.eu.int/comm/governance/areas/group2/index_en.htm*>. This is also one of the recommendations of the Phillips Report into the BSE crisis: see note 90.
145. In addition, the participants at the EU workshop were concerned about the point at which science goes public. EU scientists were uncomfortable with following a US model, in which all meetings and debates are public, and would prefer private discussions to prepare their positions beforehand. Concerns were also expressed about the resources consultation might consume, how the 'cacophony of voices' would be dealt with and how 'consultation fatigue' could be avoided. Dorothy Nelkin's work, however, suggests that these latter problems are not usually as significant as scientists expect: see, for example, D. Nelkin, 'Science and Technology Policy and the Democratic Process', in J.C. Petersen (ed.), op. cit. note 134, 18–39.

146. The parliamentary debate on embryonic stem cell research in Britain may be an example implicitly referred to here, where scientific advisers gave technical information to both sides of the debate.
147. Cf. Evans, op. cit. note 82; see also Guston (1999), op. cit. note 88.
148. Sandra Harding, *The Science Question in Feminism* (Ithaca, NY: Cornell University Press, 1986), 250; cited in Isobelle Stengers, *The Invention of Modern Science* (Minneapolis & London: University of Minnesota Press, 2000), at 20–21.
149. For example, Harding, op. cit. note 125.
150. Harry Collins and Trevor Pinch, *The Golem at Large: What You Should Know About Technology* (Cambridge: Cambridge University Press, 1998), at 7–29.
151. Arksey, op. cit. note 5, 174.
152. Locke, op. cit. note 110; Collins & Pinch, opera cit. notes 31, 56 & 150.
153. Jon Turney, 'Review of *The Golem at Large*', *Public Understanding of Science*, Vol. 8, No. 2 (April 1999), 139–40, at 140.
154. Wynne (1993), op. cit. note 57, 333.
155. Funtowicz & Ravetz, op. cit. note 78, 751.
156. Yearley, op. cit. note 105; Yearley, op. cit. note 109.
157. For example, Yearley notes that the risks due to GM crops could be classed as 'high' or 'very high'. It may be, however, that the NUSAP notation developed by Funtowicz and Ravetz in other publications goes some way to addressing these concerns. See: Silvio O. Funtowicz and Jerome R. Ravetz, *Uncertainty and Quality in Science for Policy* (Dordrecht: Kluwer, 1990).

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We invite readers to contribute to the discussion initiated by this Discussion Paper. Candidate draft Responses should, in the first instance, be sent electronically to the Editor at *mel27@cornell.edu*, together with a covering note to confirm that all contributing authors have agreed to the submission, and that it does not contain material currently being considered for publication by any other journal.