

## Misunderstood misunderstanding: social identities and public uptake of science

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This paper draws general insights into the public reception of scientific knowledge from a case study of Cumbrian sheep farmers' responses to scientific advice about the restrictions introduced after the Chernobyl radioactive fallout. The analysis identifies several substantive factors which influence the credibility of scientific communication. Starting from the now-accepted point that public uptake of science depends primarily upon the trust and credibility public groups are prepared to invest in scientific institutions and representatives, the paper observes that these are contingent upon the social relationships and identities which people feel to be affected by scientific knowledge, which never comes free of social interests or implications. The case study shows laypeople capable of extensive informal reflection upon their social relationships towards scientific experts, and on the epistemological status of their own 'local' knowledge in relation to 'outside' knowledge. Public uptake of science might be improved if scientific institutions expressed an equivalent reflexive discourse in the public domain.

### Introduction

The Chernobyl experience represents a major example of the general point that public responses to risks and risk information are rationally based upon their experience and judgment of the credibility and trustworthiness of the institutions which claim to be in charge.<sup>1</sup> Chernobyl provided a richly endowed laboratory for identifying the factors affecting the public credibility of science, and for examining the relationship between that and the 'public understanding of science' issue.

As the 1985 London Royal Society report on public understanding of science made abundantly clear, much of the impetus for the current interest in this subject stems from a broad anxiety among scientists and policy makers about what they see as the public's inability or unwillingness to understand 'correct' messages about risks as given to them by the experts.<sup>2</sup> This self-defeatingly scientific conception of the public understanding of science problem has been criticized before,<sup>3</sup> and attributed to the neurosis of scientific institutions about their public credibility. I have noted the irony that this formulation of the problem only encourages more public alienation, hence justifying and consolidating the neurosis.<sup>4</sup>

A more interpretive framing of the public understanding of science problem, and of the closely associated risk perception issue, starts from the observation that public experiences of risks, risk communications or any other scientific information is never, and can never be, a purely intellectual process, about reception of knowledge *per se*.<sup>5</sup> People experience these in the form of material social relationships, interactions and

interests, and thus they logically define and judge the risk, the risk information, or the scientific knowledge as part and parcel of that 'social package'. A corollary of this is that people do not simply not understand science when they are seen to disregard it; they do not recognize it, or identify with it, morally speaking.

In other words it is increasingly accepted that the issues of public understanding of science, and of public risk perceptions, are not so much about public capabilities in understanding technical information, but about the trust and credibility they are prepared to invest in scientific spokespersons or institutions. The unduly cognitive emphasis of much public understanding of science and risk perception work has been undermined and shifted through this social transformation, which is still far from fully developed.<sup>6</sup>

In this paper I draw upon fieldwork from the Chernobyl issue to attempt to take this sociological transformation one step further. This fieldwork involved in-depth interviews with hill sheep farmers in the Lake District of Northern England who received intensive expert information and advice about environmental hazards from radioactive caesium isotopes deposited as fall-out from the Chernobyl accident. This fallout caused restrictions upon the free sale of sheep in an area dominated economically and culturally by this particular traditional way of life.<sup>7</sup>

The conventional framing of the public understanding of science issue misleadingly reifies scientific knowledge, as if it were objective and context-free. The more recent recognition that trust and credibility are the basic dimensions in public 'understanding', now also risks reifying these concepts, which would be just as misleading.<sup>8</sup> Trust, or trustworthiness, and credibility are relational terms, about the nature of the social relationships between the actors concerned. They are not intrinsic to either actor nor to the information said to be transmitted between them.<sup>9</sup>

I disavow theoretical commitments to 'essentialist' models of beliefs, values and trust, and of the relationships between them.<sup>10</sup> Most research in public understanding of science involves observing or measuring what people believe after they have been exposed to scientific information of some kind(s). This is true of both large-scale, often quantitative survey research, and of more small-scale, qualitative research. An assumption tends to be made in both approaches, though it is not essential to the latter, that the beliefs or 'understandings' are internally coherent, with a primary existence in the sense that they are not derivatives of other factors. Whether the beliefs are measured quantitatively or qualitatively, and whether they are about electrons, genes, or trustworthiness of social actors such as scientists, they are taken to be unambiguously real.

In this analysis of the reactions of hill-farmers to the Chernobyl crisis I wish to go not one, but two steps beyond the cognitivist approach,<sup>11</sup> to show that the best explanatory concepts for understanding public responses to scientific knowledge and advice are not trust and credibility *per se*, but the social relationships, networks and identities from which these are derived. If we view these social identities as incomplete, and open to continual (re)construction through the negotiation of responses to social interventions such as the scientists represented, we can see trust and credibility more as contingent variables, influencing the uptake of knowledge, but dependent upon the nature of these evolving relationships and identities.

In the case described below, our interviews revealed the complex and multi-dimensional social basis of trust and credibility as a central factor in the reception or 'understanding' of scientific advice by the farmers. They monitored and constructed evidence on this trustworthiness factor from a far wider range of behaviour and

demeanour of the expert institutions—including long-past behaviour on related issues—than would be recognized by cognitivist approaches.

However the same interviews also indicate ambivalence in relation to trust, and to what—and who—they actually believed about the sources of the radioactive caesium which had damaged their livelihoods. This ambivalence of belief and trust reflects the multifaceted and plural social networks and identities which the sheep farmers inhabit, contradicting simple notions of an unreflexive traditional lay culture. The evidence suggests that the beliefs the farmers construct, including their beliefs about the credibility and trustworthiness of different scientific and other social actors, are functions of the social networks with which they identify. There is nothing intrinsically different in this to the basic structure of scientific belief and commitment.<sup>12</sup> 'Understanding' or knowledge, its precision and resilience, is a function of social solidarity, mediated by the relational elements of trust, dependency and social identity; constructing that 'intellectual' understanding should be seen as a process of social identity-construction.

### **Sheep farmers, scientists and radiation hazards: the background**

The hill sheep farmers near the Sellafield (formerly Windscale) nuclear fuels reprocessing complex in the Lake District of Cumbria, Northern England, have more than a personal health interest in radiation risk information. Their economic viability depends totally upon rearing a large crop of lambs each spring, and selling them in the late summer and autumn, before they run out of their meagre valley grazing due to the temporary overpopulation of lambs. The UK lamb industry exports heavily to continental Europe. Any public perception of radioactive blight on its product would destroy the industry, especially the hill sheep farming sector which is a key early part of the breeding cycle, but which is economically more fragile and offers the farmer few or no alternatives compared to lowland sheep farming.

The upland hill farming region in the Lake District is one of the few locations of relative solidarity and distinctive traditional cultural identity left in industrial Britain. Although (as shown later) this should not be overstated, these communities share an unusually demanding livelihood as a way of life; they occupy a distinct and sought-after geographical location, and have common historical traditions, linguistic dialects and recreational pursuits. They also share particular 'external' socio-economic threats such as subordination to tourism, landlords and authorities who appear to be more and more concerned with meeting environmental and urban recreational demands on the country than with sheep farming. All of this was an important context of the post-Chernobyl crisis.

In May 1986, following the Chernobyl accident, upland areas of Britain suffered heavy but highly variable deposits of radioactive caesium isotopes, which were rained out by localized thunderstorms. The effects of this radioactive fallout were immediately dismissed by scientists and political leaders as negligible, but after six weeks, on 20 June 1986, a ban was suddenly placed on the movement and slaughter of sheep from some of these areas, including Cumbria.

Although this shock was mitigated somewhat by the confident scientific reassurances that the elevated levels of caesium in sheep, and hence the ban, would only last about three weeks, at the end of this period the restrictions were instead imposed indefinitely. The confident dismissal of any effects only two months earlier had changed to the possibility of wholesale slaughter and complete ruin of hill sheep farms

at the hands of a faraway stricken nuclear plant. At the time over four thousand British farms were restricted. The initially wide restricted area in Cumbria (which included about five hundred farms) was whittled down within three months to a central crescent covering one hundred and fifty farms (see Figure 1). These farms have remained restricted, contrary to all the scientific assertions of the time. A recent review has indicated that they could remain so for years, overturning completely the scientific basis upon which the previous policy commitments were made.<sup>13</sup>

Very close to this recalcitrant central 'crescent' of longer-term radioactive contamination, almost suggesting itself as its focal point, is the Sellafield–Windscale nuclear complex. The stories of Sellafield–Windscale and Chernobyl are intertwined in ways which I now unravel.

Sellafield–Windscale is a huge complex of fuel storage ponds, chemical reprocessing plants, nuclear reactors, defunct military piles, plutonium processing and storage facilities, and waste processing and storage silos. It has developed from its original role in the early 1950s of producing purely weapons-grade plutonium into a combined military and commercial reprocessing facility which stores and reprocesses thousands of tonnes of UK and foreign spent fuel. It is by far the biggest employer in the area, with a regular workforce of some five thousand swollen by a construction workforce of nearly the same size. It dominates the whole area not only economically, but also socially and culturally.



Figure 1. Map showing the restricted areas of Cumbria, from June 1986 (the original area), and from September 1986 (the present one).

Sellafield has been the centre of successive controversies, accidents and events relating to its environmental discharges and workforce radiation doses, with increasingly powerful criticisms not only of allegedly inadequate management and regulation, but also of poor scientific understanding of its environmental effects, and of the economic irrationality of the recycling option in nuclear fuel cycle policy. In the early 1980s the plant was alleged to be the centre of excess childhood leukaemia clusters; these excesses were confirmed by an official inquiry chaired by Sir Douglas Black, which nevertheless expressed agnosticism as to the cause. This controversy continues, with every scientific report exhaustively covered in the local and national media.<sup>14</sup> The plant operators were later shown to have misled the Black inquiry, inadvertently or not, over earlier levels of environmental discharge of radioactivity. In 1984 the operators were accused by the environmental group Greenpeace of contaminating local beaches above legal discharge levels, and were subsequently prosecuted; and in 1986 they were threatened with closure after another incident and an ensuing formal safety audit by the Health and Safety Executive. Despite huge investments in public relations, they have suffered a generally very poor public image for openness and honesty over the years.

Before most of these controversies developed, in 1957 the Sellafield–Windscale site suffered the world's worst nuclear reactor accident before Chernobyl, when a nuclear pile caught fire and burned for some days before being quenched.<sup>15</sup> It emitted a plume of radioactive isotopes, mainly iodine and caesium, over much the same area of the Lake District of Western Cumbria as that affected by the Chernobyl fallout. This fire and its environmental effects were surrounded by a great deal of secrecy. Although farmers in the vicinity were forced to pour away contaminated milk for several weeks afterwards, at the time they reacted without any overt hostility or criticism of the industry. Even in 1977 when they had the opportunity during a public inquiry to join with an emergent coalition of various forces against a major expansion at Sellafield, the local farming population largely kept out of the argument.<sup>16</sup> Significantly however, it was only after 1987 that the fuller extent of the Windscale fire cover-up emerged into the public domain. In 1990 it was revealed in a television programme that the ill-fated pile had in fact been allowed to operate with faults which meant that highly irradiated spent fuel had been lying in the air streams emitted up the stack. Thus it was exposed that the fire had been a blessing in disguise for the authorities, since any discoveries of local environmental contamination could be attributed to the one-off fire itself rather than to longer-standing irresponsible management which had allowed routine uncontrolled radioactive emissions to occur for some time before. The parallels with the Chernobyl issue nearly thirty years later are remarkable, as explained below.

The farming population in the Cumbrian hills is relatively stable, most farmers having lived through these controversies and events as near neighbours. Indeed many of them have relations, neighbours and casual employees who also work at the Sellafield–Windscale site. Not only is it close physically, it is also never far away from contemplation. Far from giving Sellafield–Windscale some welcome relief, the Chernobyl emergency ironically brought it even more to critical public attention.

### **Scientific knowledge and social identities**

At first, the scientific advice was that there would be no effects at all from the Chernobyl caesium fallout. After six weeks these confident public reassurances were

dramatically overturned when on 20 June 1986 the Minister for Agriculture announced the complete ban on sheep sales and movements in several affected areas, including Cumbria (see Figure 1). However the shock waves from this reversal were contained by the strong reassurances accompanying the ban that it would be for three weeks only, by which time radioactivity levels in lamb would, it was confidently expected, have reduced beneath the level at which intervention was required. This short ban could be accommodated because very few if any hill lambs would be ready to sell before late August.

Yet after the three-week period, instead of lifting the ban the government announced an indefinite extension, albeit for a smaller area. This represented an altogether more serious situation in which the hill farmers faced ruin, because not only lamb crops but also breeding flocks faced starvation and wholesale slaughter due to inadequate grazing. The government introduced a scheme to remove this threat: it allowed farmers to sell lambs contaminated above the limit if they were marked, in which case the lambs could be moved to other areas but not slaughtered until their contamination had reduced. This blight factor collapsed the market price for marked sheep, and many lowland farmers bought them and then made handsome profits when they sold them after the sheep had decontaminated on their farms.

The hill farmers were left in a quandary. If they sold, they had to run the gauntlet of the threatening bureaucratic system which had been established to manage the restrictions, which consisted of prior notification, tests and controls and paperwork, and offered only a possible and partial future compensation for catastrophically low prices. If they held on to their sheep they risked ruin from starvation, disease and knock-on effects, or from the costs for buying in extra feed. Yet even after the initial contradiction of their scientific beliefs, the scientists advised farmers to hang on because, as they persisted in believing, the contamination would fall soon—it was just taking a bit longer than expected. When farmers did follow expert advice and waited, they found once again that the advice was badly over-optimistic, and had led them into a blind alley in which many costly complications to farm management cycles had been introduced, and compensation was cut off because they had not sold within the prescribed period. In the circumstances it was not surprising that our interviews found many farmers bitterly accusing the scientists of being involved in a conspiracy with a government which they saw as bent on undermining hill farming anyway.

Through the troubled and confused summer of 1986, in spite of mounting evidence and their own public embarrassment, the scientists persisted in their belief that the initially high caesium levels would fall soon. Only later did it emerge that these predictions were based upon a false scientific model of the behaviour of caesium in the upland environment. The prevailing scientific model was drawn from empirical observation of alkaline clay soils, in which caesium is chemically adsorbed and immobilized and so is unable to pass into vegetation. But alkaline clay soils are not found in upland areas, which have acid peaty soil. This type of soil had been observed, but only for physical parameters such as depth-leaching and erosion, and not for chemical mobility.

Thus the scientists unwittingly transferred knowledge of the clay soils to acid peaty soil, in which caesium remains chemically mobile and available to be taken up by plant roots. Whereas their model had caesium being deposited, washed into the soil and then locked up by chemical adsorption, thus only contaminating the lambs on a one-pass basis, in fact the caesium was recycling back from the soil into vegetation, and thence back into the lambs. This mistake only became apparent over the next two

years, as contamination levels remained stubbornly high and the reasons were urgently sought. What was not lost on the farmers, however, was that the scientists had made unqualified reassuring assertions then been proven mistaken, and had not even admitted making a serious mistake. Their exaggerated sense of certainty and arrogance was a major factor in undermining the scientists' credibility with the farmers on other issues such as the source of the contamination. In any case the typical scientific idiom of certainty and control was culturally discordant with the farmers, whose whole cultural ethos routinely accepted uncertainty and the need for flexible adaptation rather than prediction and control.

The structure of the scientific knowledge in play also embodied and, in effect, prescribed a particular social construct of the farmers.<sup>17</sup> To summarize this analysis, the degree of certainty expressed in scientific statements denied the ability of the farmers to cope with ignorance and lack of control; and the degree of standardization and aggregation of the scientific knowledge, for example the spatial units of variation of variables such as caesium contamination, denied the differences between farms, even in a single valley (and even within the same farm). At the same time the scientists ignored farmers' own knowledge of their local environments, hill-sheep characteristics, and hill-farming management realities such as the impossibility of grazing flocks all on cleaner valley grass, and the difficulties of gathering sheep from open fells for tests.

As a result the farmers felt their social identity as specialists within their own sphere, with its adaptive, informal cultural idiom, to be denigrated and threatened by this treatment. This was a reflection of the culture and institutional form of science, not of what specifically it claimed to know.

A graphic example of the scientists' denial of the specialist knowledge of the farmers was the scientists' decision to perform experiments on the value of the mineral bentonite to chemically adsorb caesium in the soil and vegetation, thus helping reduce recontamination of grazing sheep.<sup>18</sup> The bentonite was spread at different concentrations on the ground in different plots; the sheep from each plot were then tested at intervals, and compared with controls on zero-bentonite land. However in order to do this the sheep were fenced in on contained fell-side plots. The farmers pointed out that the sheep were used to roaming over open tracts of fell land, without even fences between farms, and that if they were fenced in they would waste (lose condition), thus ruining the experiment. Their criticisms were ignored, but were vindicated later when the experiments were quietly abandoned for the reasons that the farmers had identified. The farmers had expressed valid and useful specialist knowledge for the conduct and development of science, but this was ignored. Similar experiences occurred over other aspects of hill-farming and the scientific knowledge relating to the management of the crisis.

In respect of both the 'conspiracy theory' and the 'arrogance theory' of science, the Cumbrian sheep farmers felt that their social identity as a specialist community with distinct traditions, skills and social relations was under fundamental threat. These two models of science, which reinforced each other in the experienced threat to social identity, are mutually contradictory if taken literally. The former implies omniscience ('they knew all along that the high levels would last much longer than they admitted'); the latter implies unadmitted ignorance in science. This apparent anomaly exposes the futility of expecting consistent formal 'beliefs' about science in research on public understanding; coherence lies at a deeper level, of the defence and negotiation of social identities. We examine this dimension next.

### Public belief and private dissent

Before the Chernobyl plume deposited its radioactive burden on the fells of Cumbria, there had been little or no controversy about radioactive caesium and related contamination of sheep on the high fells. Amongst several other issues concerning Sellafield's environmental discharges, contamination of pastures and grazing animals along the lowland coastal plain near the plant had been found and debated, for example in monitoring by Friends of the Earth, the Sellafield operators British Nuclear Fuels, and the Ministry of Agriculture, Fisheries and Food.<sup>19</sup> But little or no scientific monitoring or public attention had been paid to the high fells and their sheep; and no allegations of contamination of the fells and their sheep had been made. When the Chernobyl restrictions were announced however, and then almost immediately extended indefinitely, questions were very soon circulating locally about the real source of the contamination almost centring on Sellafield persisted (Figure 1) against scientific long-standing, than was being officially admitted. The fact that a crescent of high contamination almost centring on Sellafield persisted (Figure 1) against scientific reassurances that levels would decrease within a few weeks, was *prima-facie* evidence of a hitherto hidden Sellafield dimension. The first national maps of caesium contamination measured after Chernobyl (in June and July 1986) had already shown remarkably high levels in West Cumbria, near Sellafield.<sup>20</sup> The fact that these measurements, which were taken from vegetation samples (Figure 2), did not altogether tally with the distribution as estimated from a combination of rainfall data during the crucial period while the radioactive cloud was over Britain, and models of rain-out of caesium from the atmosphere (Figure 3) also invited the question of whether a hidden factor, such as unacknowledged long-term Sellafield discharges, had created the differences.<sup>21</sup> This factor would be picked up by the vegetation samples method, but not by the rainfall-data method.

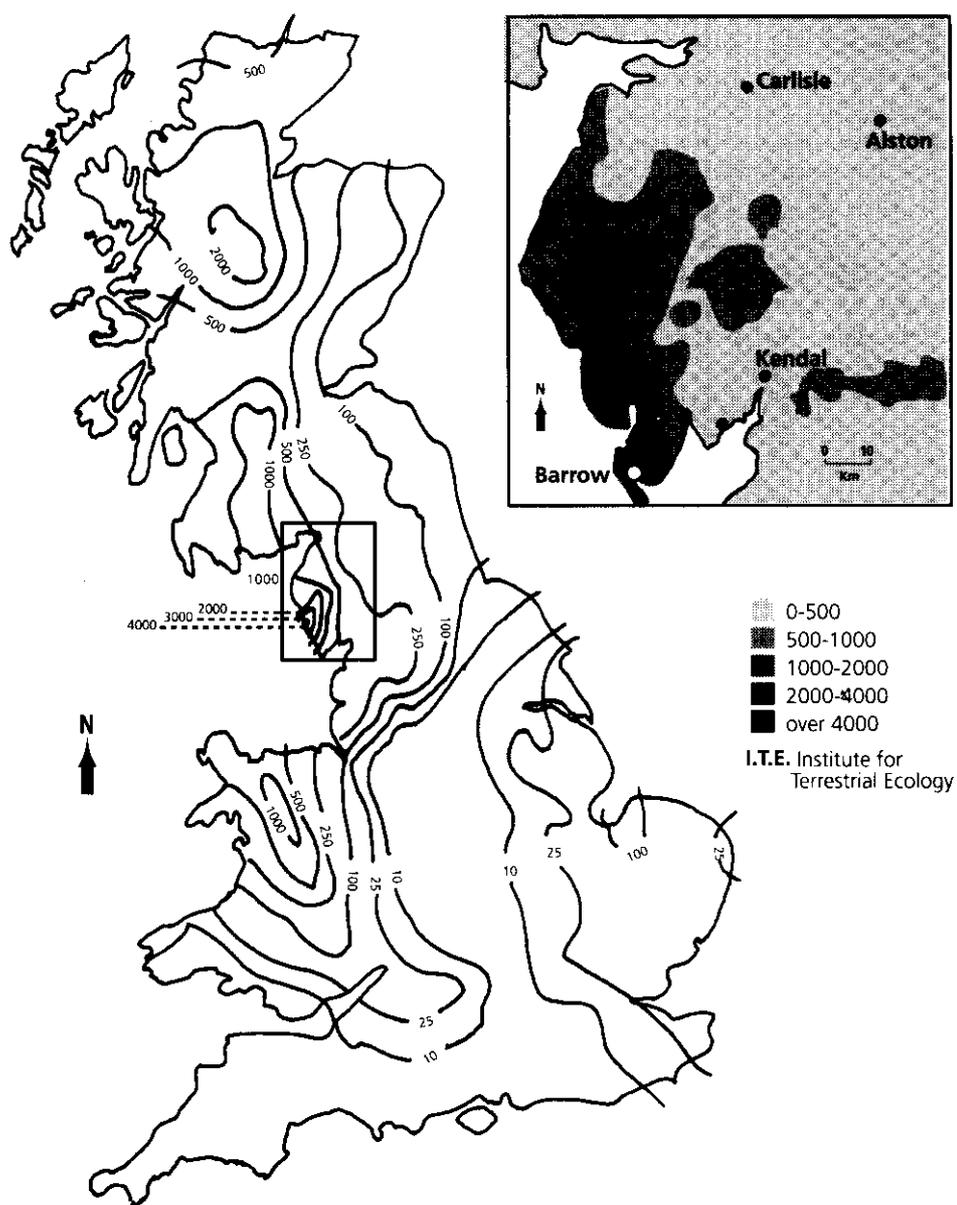
In the manifest scientific confusion and inconsistency, it was as if the farmers had suddenly found an outlet for fears and suspicions that they had previously entertained, but felt unable to voice. Ironically it was radioactive contamination which scientists confidently proclaimed was nothing to do with Sellafield-Windscale which gave the hill farmers their first embryonic voice about that local trouble-spot.

In our interviews, typical scepticism about the scientists' assertions of Sellafield's innocence was expressed as follows:

There's another thing about this as well. We don't live far enough away from Sellafield. If there's anything about we are much more likely to get it from there! Most people think that around here. It all comes out in years to come; it never comes out at the time. Just look at these clusters of leukaemia all around these places. It's no coincidence. They talk about these things coming from Russia, but it's surely no coincidence that it's gathered around Sellafield. They must think everyone is completely stupid.<sup>22</sup>

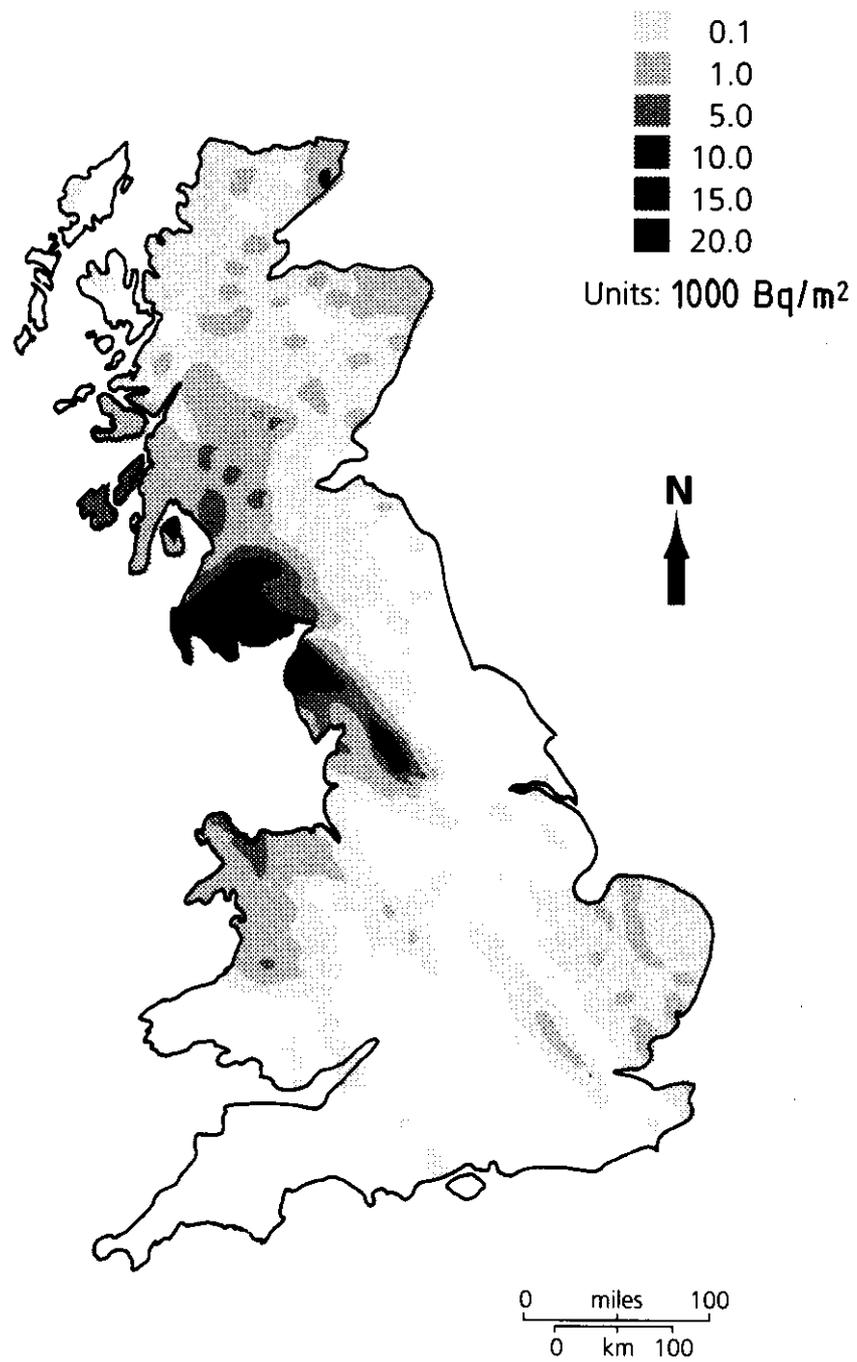
These immediate local suspicions were only strengthened by the Ministry of Agriculture maps showing the restricted areas (Figure 1). Other farmers reinforced this logic, as did experience of the continuing secrecy surrounding the 1957 fire:

It still doesn't give anyone any confidence, the fact that they haven't released all the documents from Sellafield in 1957. I talk to people every week—they say this hasn't come from Russia! People say to me every week, 'Still restricted eh—that didn't come from Russia lad! Not with that lot on your doorstep'.



**Figure 2.** Contours of UK radioactive caesium contamination measured from vegetation, June–July 1986. The data are in units of  $\text{Bq m}^{-2}$ .

The scientific view was that the Chernobyl caesium depositions could be distinguished from the caesium in routine Sellafield emissions, 1957 fire emissions, or 1950s weapons testing fallout, by the typical 'signature', in gamma-radiation energy spectra, of the ratio of intensities of the isotopes caesium 137 and caesium 134 (each isotope has a characteristic gamma-ray frequency or energy). The half-life of the caesium 137



**Figure 3.** UK radioactive caesium levels estimated from rainfall data, Chernobyl cloud movement data, and models of caesium rain-out from the atmosphere.

isotope is about thirty years, while that of the caesium 134 isotope is less than one year, and so the ratio of intensities of caesium 137 to caesium 134 increases with time. A typical Sellafield sample (from fully burnt-up fuel, usually stored for several years before reprocessing; or if from the 1957 fire, aged in the environment) would therefore show a greater ratio (about ten to one) than a Chernobyl sample consisting of fresh fuel and fission products (about two to one). Thus the deposits were said scientifically to show the so-called Chernobyl fingerprint, making an analogy with a form of evidence which is never questioned in law.

This scientific distinction, which exonerated Sellafield, was unequivocally asserted at public meetings and lectures with virtually complete consensus from scientists from the Ministry of Agriculture, Fisheries and Food (MAFF) and the other scientific organizations involved in the issue, at least for the first year or more of the crisis. However, it too turned out to be less clear cut than first claimed: it was later admitted that only about 50% of the observed radioactive caesium was from Chernobyl, the rest being from 'other sources', including weapons testing fallout and the 1957 Wind-scale fire.<sup>23</sup> Nevertheless, at the time the difference in the fingerprints was represented as a very clearcut scientific distinction, with Sellafield for once in the clear, and Chernobyl definitely to blame. Yet although it was against their economic interests to entertain thoughts of a longer-standing but neglected (or covered-up) blight from Sellafield, and in the face of this solid scientific consensus, many hill farmers were ready, at least in private, to implicate Sellafield. Their reasoning tells us a lot about the deeper cultural and social structures of expert credibility.

It was striking that when we asked farmers sceptical about the scientists' exoneration of Sellafield to explain their reasoning, many of them talked about the 1957 fire and the secrecy surrounding it. At first we did not see this as an answer to the question, but then we realized that it was—they were explaining the lack of credibility of the present scientific claim about the Sellafield–Chernobyl distinction as due to the untrustworthy way in which the experts and authorities had treated them over the 1957 fire, and the longer history of perceived misinformation surrounding Sellafield:

Quite a lot of farmers around believe it's from Sellafield and not from Chernobyl at all. In 1957 it was a Ministry of Defence establishment—they kept things under wraps—and it was maybe much more serious than they gave out. Locals were drinking milk, which should probably never have been allowed—and memory lingers on.

The farmers thus embedded their reading of the present scientific claim about the isotope ratio distinction firmly within the context of the unpersuasive and untrustworthy nuclear institutional body language which had denigrated them for thirty years or more. Their definition of risk was in terms of the social relationships they experienced, as a historical process.

They had a range of further reasons supporting this dissident logic. The empirical evidence of the maps (Figure 1, and contrasts between Figures 2 and 3) was powerful as far as they were concerned; and official disclaimers were ridiculed with a heavy irony only evident in a personal interview, such as (referring to a MAFF scientist) 'she said she couldn't understand why the heaviest fallout from Chernobyl happened to fall around Sellafield'.

Thus the farmers gathered evidence which was drawn from science, including scientific inconsistencies on which the scientists themselves did not focus. They entered

the scientific arena in this sense, re-drew its boundaries, and, operating with different presuppositions and inference rules, also re-drew its logical structures.

Other direct empirical connections were drawn which may not have made scientific sense, but which served to make a consistent explanatory picture to people who saw the science to be either politically manipulated or naively over-confident in its own certitude.

Most farmers believe it's really from BNFL [Sellafield]. You'd have great difficulty convincing them otherwise. The area is a kind of crescent shape. If you're up on the tops [of the fells] on a winter's day you see the tops of the cooling towers, the steam rises up and hits the fells just below the tops. It might be sheer coincidence, but where the [radiation] hot spots are is just where that cloud of steam hits—anyone can see it if they look. You don't need to be a scientist or be very articulate to figure that one out. And a lot of the farmers may not be very articulate but they've figured it out all right. I think there's been low-level fallout ever since that placed opened, and Chernobyl has gone on top of it.

Interestingly, the apparently unfounded notion that high deposits occur where 'the clouds' hit the fellsides is not unreasonable, because scientists themselves recognized the importance of intense 'occult deposits' of radio-isotopes direct from low-lying clouds and mists which are typical of the Lake District climate.

Other farmers seemed to be exercising a strong penchant for irony when they put into sceptical perspective the experts' claims about the 'coincidence' of Chernobyl deposition next to the local controversial nuclear site.

When you look at the stations around here, I said it was like a magnet, it just drew it in! [Then, relaxing the irony] I still think it was here before. They [the experts] won't have it. . . . We can't argue with them, but you can think your own ideas.

Often the justification for disbelieving the scientists on the Sellafield connection was simply that the same experts had very recently asserted, with similar confidence, first that there would be no effects of the Chernobyl cloud, and then that the restrictions which were imposed after all would be very short-lived. Since their self-confidence had been shown to be misplaced on those counts, why should they expect to be believed this time, especially when no admission of the earlier mistakes was forthcoming? The farmers scorned what they saw as the scientists' addiction to over-confidence and false certainty:

My theory, which is probably as good as anyone else's is this: we don't know. . . . They keep rushing to conclusions before the conclusion has been reached—you understand what I'm saying? They'd have been far better to keep their traps shut and wait.

And a farmer's union representative put it:

We may be on the eve of a new age of enlightenment. When a scientist says he doesn't know, perhaps there's hope for the future!

It is important to note that scientific credibility was influenced not only by the evidence which alternative logical presuppositions could select and render coherent,

and not only by the prior intellectual mistakes, but by the way they were handled socially. This gave impetus to the alternative constructs.

The farmers also came into direct contact with the conduct of science on their farms, as hosts to a proliferation of monitoring, sampling, field analysis and various other scientific activities. Again, they soon noted the inconsistency between the certainty pervading public scientific statements, and the uncertainties involved in actually attempting to create definitive scientific knowledge in such novel and open-ended circumstances. The experience of watching scientists decide where and how to take samples, of seeing the variability in readings over small distances, of noticing the difficulty of obtaining a consistent standard for background levels, and of gradually becoming aware of the sheer number and variety of less controlled assumptions and judgments that underpin scientific facts, corroded the wider credibility of official statements couched in a typical language of certainty and standardization. By accident, as it were, the farmers entered the 'black-boxes' of constructed, 'naturally-determined' science, and saw its indeterminacies for themselves.<sup>24</sup> Referring to the live monitoring of sheep which was now obligatory, one farmer indicated how doubts set in:

Last year we did 500 [sheep] in one day. We started at 10.30 and finished at about six. Another day we monitored quite a lot and about 13 or 14 of them failed. And he [the monitor] said, 'now we'll do them again'—and we got them down to three! It makes you wonder a bit . . . it made a difference . . . when you do a job like that you've got to hold it [the counter] on its backside, and sheep do jump about a bit.

These forms of reasoning were buttressed by further social evidence and judgment. There existed a widespread model of the capture of science by institutions with their own manipulative political agendas. Such judgments were supported by empirical observations, such as the refusal of MAFF officials to allow an American television team to film the lively debate with affected farmers at a public meeting in February 1987. The TV team was preparing a five-country documentary on the international response to Chernobyl. The producer's acid comment as he departed—that his team had received more open treatment in (pre-*glasnot*) Poland than in Britain—was widely quoted afterwards among the farmers.

The farmers drew similar conclusions from MAFF's response to their requests for pre-Chernobyl caesium data on the fell-top vegetation, soils and sheep; they asked for these in order to test MAFF's assertion that there had been no significant contamination before Chernobyl. However MAFF's reply was to refer first to an official document which contained only post-Chernobyl data,<sup>25</sup> and then to data which included pre-1986 monitoring on the lowland coastal strip, but still no fell-top data. The farmers saw this as evidence that the authorities were trying to cover up—either that they did have data which showed high fell-top levels of caesium before Chernobyl, or that they had no data at all! If the former, they were guilty of straightforward lying and conspiracy. If the latter, they were guilty of at least gross complacency and incompetence, but possibly also conspiracy to remain deliberately ignorant of the levels before Chernobyl forced them to look. In addition, the 1957 fire had provided an ideal opportunity—apparently neglected—to have done the necessary research which would have avoided mistakes in the 1986 prediction:

Going back to the 1957 fire, nobody really knows what that did, what effect it had on the land and that, because they never tested it. . . . A lot of people have

it in their minds that they [the UK authorities] were just waiting for something like this [Chernobyl] to blame.

This indicates a belief that the authorities had done secret research, had found high levels and had decided to cover up—waiting for the chance, which Chernobyl provided, to pass on the blame. It also encouraged the farmers to conclude that they and their families had been used as mere objects of scientific research. (In fact the question of whether the authorities had done previous research in the Cumbrian fells, and thus knew that the radioactive caesium contamination would last much longer, is extremely complex. What counts as ‘previous research’ is itself open to interpretive differences; some ecologists we interviewed said afterwards that they knew, and told the government at the time, but that they were ignored by the ‘physicalist’ ethos which dominated the official advisory mechanisms. This is the subject of further research. In evidence to the House of Commons Agriculture Select Committee in 1988, a local environmental group, Cumbrians Opposed to a Radioactive Environment, alleged cover-up, and also noted that the government’s advisory body, the National Radiological Protection Board, had promulgated emergency reference levels for environmental radioactivity, only a month before Chernobyl, which completely overlooked the central environmental medium and food chain in the Chernobyl emergency, namely sheep meat.)

These modes of reasoning interlocked with other judgments which the farmers made of the controlling institutions from which scientific claims were seen to emanate. Thus another farmer related what he saw as deliberate official ignorance to Sellafield’s denial of claims that the site caused leukaemia:

The Department of Health could body monitor but they don’t deliberately because if they did and found high readings then various ministries could one day be accused of irresponsibility in this regard. I think it self-evident that when BNFL [the Sellafield operator] were accused of being responsible for leukaemias they were quick to say ‘what evidence is there?’ I have been told that if I make an accusation that my grand-daughter has got leukaemia in the future and I suggest it was due to Sellafield they will say to me ‘what evidence have you?’ It is a deliberate policy of government not to do this appropriate monitoring and testing so that they can protect themselves against an accusation of this nature. I would suggest we have another Christmas Island situation. The first such situation was at BNFL [it was then the Atomic Energy Authority] in 1957. Now we have Chernobyl Cumbria, Chernobyl Wales, South Scotland and Ireland. . . . When you have bottomless financial pits like Sellafield sponsoring this, that and the other in order to blackmail local feeling, why could they not instead do something positive like supporting controlled experiments to answer all the questions that need to be answered?

Of course we can judge that these views were encouraged by probably unrealistic ideas about what can be expected of scientific knowledge in a situation such as the post-Chernobyl emergency. Even allowing for this factor however, the expressed attitudes reflect a rich supply of evidence to support a model of the subordination of science to untrustworthy institutional and political interests, and of a deep flaw in the very nature of science which drives it towards unrealism, insensitivity to uncertainty and variability, and incapability of admitting its own limits. (These can be seen as contradictory models of science, but are better treated as rhetorical stances which deconstruct and delimit the authority of the social control which the science represented in the

experience of the farmers.) Analysis of the logical structure of the farmers' responses to the scientific expertise indicates both a far greater open-endedness about scientific logical structures and its institutional and cultural forms than is usually recognized; and a greater need to acknowledge and negotiate these as a condition of science's social legitimation.

### **Credibility: the social dimension**

The way in which the farmers' scepticism was expressed suggests that Chernobyl acted to release a large historical backlog of more private disbelief, mistrust, and alienation from the authorities, which related to Sellafield, and which had been quietly simmering over the years as one experience of official perfidion led into another. This would also explain the apparently abrupt change in their position from acceptance to hostility: it was probably not nearly as abrupt as it may have seemed, because there was already a finely balanced 'private' ambivalence, and not by any means a complete uncritical acceptance of the site and its expert apologists.<sup>27</sup>

However the dimension of this issue which drew in the farmers, and on which they had the most confidence to judge the outside experts and to criticize them, was the fact that this time, expert responses to the crisis constituted massive interventions, disruptions and denigrations of their normal practices and livelihood. The administrative restrictions introduced by the Government to prevent contaminated lamb from reaching the market were tantamount to large-scale social control and reorganization, and denial of essential aspects of the farmers' social identity, to an extent that the outside experts and bureaucrats did not remotely recognize. The interventions required not only scientific understanding of the radioecology of caesium in this particular physical environment; they also required this to be integrated with knowledge of hill sheep-farming methods and decision-making processes, in what is a highly specialist and particular kind of farming.

Whereas the hill-farmers were quite reserved in their scepticism towards the scientists on scientific matters, they were abrupt and outspoken about them when they saw the extent of the scientists' ignorance of hill-farming environments, practices and decision making. Even worse was the way that the outside experts did not recognize the value of the farmers' own expertise, nor see the need to integrate it with the science in order to manage the emergency properly. An example which ruined the experts' credibility with many farmers was the advice given to farmers to keep their lambs a little longer on cleaner valley pastures so as to allow high caesium levels gained on the fell tops to decrease. This ignored the locally taken for granted fact that hill farming in such areas is organized around a severe short supply of valley grass, which would as one farmer put it, 'be reduced to a desert in days' (and with knock-on effects into future breeding) if it were not very carefully husbanded.

Naturally the farmers felt that their whole identity was under threat from outside interventions based upon what they saw as ignorant but arrogant experts who did not recognize what was the central currency of the farmers' social identity, namely their specialist hill farming expertise. This expertise was not codified anywhere: it was passed down orally and by apprenticeship from one generation to the next, as a craft tradition, reinforced in the culture of the area. The impact of the scientists' hegemonistic cultural orientation on their general credibility showed itself repeatedly:

There was the official who said he expected levels would go down when the sheep were being fed on imported foodstuffs, and he mentioned straw. I've never heard of a sheep that would even look at straw as fodder. When you hear things like that it makes your hair stand on end. You just wonder what the hell are these blokes talking about? When we hill men heard them say that we just said, what do this lot know about anything? If it wasn't so serious it would make you laugh.

Another derided the experts' ignorance of what were elementary facts of life to hill farmers:

If you start fattening lambs and sell twenty, the next twenty get fat quicker, because you've got more grazing. But if you keep them all . . . [gesticulation of disaster]. But that's the problem with the ministry—trying to tell them those sort of things. That's where the job has fallen down a lot. They couldn't understand that you were going to sacrifice next year's lamb crop as well. They just couldn't grasp that!

Scientists and Ministry officials were often seen as indistinguishable; indeed the most prominent officials explaining and defending official decisions were scientists. But there was also a deeper structural convergence between the forms of monopolistic scientific representation of the issues, and the forms of administrative intervention and reorganization of farming practices. The significant elements of scientific representation in this respect were:

- its standardization of local physical environmental variations, farming conditions and practices (hence farmers); and
- its ethos of prediction and control, which engendered an exaggerated sense of certainty, and which conflicted sharply with the farmers' ethos of adaptation and acceptance of intrinsic lack of control.

These coincided with the centralized formal nature of the administrative interventions, which reduced the long-established individualism, informality and flexibility of farm-management decision-making to an extension of bureaucracy. The farmers were quite familiar with uncertainty on several fronts and thus with adaptation to factors beyond their control. This deep cultural outlook—reflected in their intellectual frameworks as well as in their whole way of life—was simply incompatible with the scientific-bureaucratic cultural idiom of standardization, formal and inflexible methods and procedures, and prediction and control.

These dimensions of incompatibility and lack of mutual credibility existed at a structural level which was deeper than that of evidence and information. They lay at the level of moral, or cultural recognition. Each side only recognized even as possible evidence, claims expressed within its cultural style. Thus for example the scientists had an a priori credibility gap to overcome when they stated things so categorically and universally, before the substance of the statement was even reached. By the same token the farmers' expertise was not recognized because it was not formally organized in documentary, standardized and control-oriented ways recognizable to scientific culture; and their later claims for compensation encountered the inflexible bureaucratic demand for formal documentation, dates, details, proofs and signatures in a way which was entirely alien to their own culture.

This sense of being ensnared by an alien and unrecognizing combination of science and bureaucracy was neatly captured in two typical comments:

They've been watching too much of 'One Man and his Dog' [a popular national television programme where shepherds compete in driving and penning sheep, under artificially simple conditions]. . . . They think you just stand at the bottom of the fell and wave a handkerchief and the sheep come running.

Another, after a detailed explanation of complex differences between farming practices even within his own small valley, reflecting different microconditions, lamented:

this is what they can't understand. They think a farm is a farm and a ewe is a ewe. They think we just stamp them off a production line or something.

Thus underlying overt clashes of knowledge, information, evidence and belief were incompatible social and cultural structures, prescribed modes of social interaction. The scientific knowledge, in the levels of aggregation and standardization of data and parameters by which it was organized, also expressed commitments about the levels of political standardization and control of the farmers.

Thus the scientific perspective was just as socially grounded, conditional and value-laden as the other. Its credibility was influenced not so much by what it said directly and explicitly, as in the way it was institutionally and intellectually organized, including lack of recognition of its cultural and institutional biases—its own tacit social body-language. As explained later, it suffered from its own lack of reflexivity.

Analysis of this credibility gap allows us to identify factors which affect the social credibility of science. These are summarized in the table (see over), as criteria by which laypeople rationally judge the credibility and boundaries of authority of expert knowledge. It is easier to understand the resilience of disputes over the authority of scientific knowledge when these several layers of the social and cultural framing of expert and lay discourses are recognized. They are structurally identical to the factors shaping the logics of dispute and development within science; it is just that in public situations the prior mechanisms of social closure are by definition less powerful.

This analysis suggests that reflexive recognition of its own conditionality is a prerequisite for science's greater public legitimation and uptake; yet this requires more than an intellectual advance from science, but institutional reform of its modes of organization, control and social relations. This would involve, *inter alia*, recognition of new, socially extended peer groups legitimated to offer criticism of scientific bodies of knowledge from beyond the confines of the immediate exclusive specialist scientific peer-group. The social definition of such extended peer-groups would relate to the context of use of the scientific specialties concerned; and criticism would include explicit negotiation of the social criteria or epistemology of knowledge for the situation.<sup>28</sup> This approach to public understanding of science therefore underlines the point reflected in other sociological analysis of scientific knowledge, that the boundaries of the scientific and the social are social conventions, predefining relative authority in ways which may be inappropriate, and which are open to renegotiation.<sup>29</sup> The practical process of developing that negotiation first requires recognition that existing approaches and discourses misrepresent this conventional character as naturally determined.

### **Conclusions: lay reflexivity and social identities**

A productive way of analysing the interactions between hill sheep farmers and scientists in this case, is to see each social group attempting to express and defend its social

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**Lay criteria for judgment of science**


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- (i) Does the scientific knowledge *work*?  
For example: predictions fail.
  - (ii) Do scientific *claims* pay attention to other available knowledge?  
For example: scientists monitor sheep without paying attention to where they graze, whereas farmers know where on open fells they graze.
  - (iii) Does scientific *practice* pay attention to other available knowledge?  
For example: when scientists devise and conduct field experiments which the farmers know will not work.
  - (iv) Is the *form* of the knowledge as well as the *content* recognisable?  
For example: degrees of expressed certainty, standardization, aggregation.
  - (v) Are scientists open to criticism?  
For example: no recognition of other legitimate knowledges and expert actors; no admission of errors, omissions or oversights.
  - (vi) What are the social/institutional affiliations of experts?  
For example: imputed social/political biases and interests; historical track record of trustworthiness, openness.
  - (vii) What issue 'overspill' exists in lay experience?  
For example: from Chernobyl to Windscale-Sellafield; lack of rational connection for scientists because institutional dimensions defined out a priori, but for laypeople continuity depending on institutional models of agency and responsibility in decision and knowledge construction.
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identity. The farmers experienced the scientists as denying, and thus threatening, their social identity by ignoring the farmers' specialist knowledge and farming practices, including their adaptive decision-making idiom. They also experienced the scientists as engaged in a conspiracy with government against hill farmers, initially to deny any need for long-term restrictions and later to claim an innocent mistake in prediction—a combination of circumstances which influenced many farmers to make unfortunate decisions and to lose heavily as a result. Coming on top of the further hardships and external controls besetting the hill farmers in an area which is a tourist-dominated National Park, their treatment by the scientists and bureaucrats after Chernobyl was almost the final straw in a baleful succession of blows to their cultural and social identity.

The scientists on the other hand were expressing and reproducing their intellectual-administrative framework of prediction, standardization and control, in which uncertainties were 'naturally' deleted, and contextual objects, such as the farmers and their farms, were standardized and 'black-boxed' in ways consistent with this cultural idiom. Whatever private awareness they may or may not have had of the cultural limits and precommitments of their science, they successfully suppressed these.

These social identities were not completely predetermined and clear, nor were they immune to interactive experience and negotiation. My main point is that this dimension should be seen as the level from which explanation of lay responses to science is to be derived, and in which the factors and processes shaping credibility or 'understanding' can be identified.

The laypeople in this case showed themselves to be more ready than the scientific experts to reflect upon the status of their own knowledge, and to relate it to that of

others and to their own social identities. Thus, for example, the farmers implicitly recognized their social dependency upon the scientific experts as the certified public authorities on the issues, even if, as they indicated in interview, they held dissenting informal beliefs which they could defend along the lines described before. As one farmer put it: 'You can't argue with them because you don't know—if a doctor jabs you up the backside to cure your headache, you wouldn't argue with him, would you?', the suggestion being that when the expert tells you unbelievable things, you don't overtly argue, thereby inviting denigration. As another said: 'We can't argue with them, but you can think your own ideas. I still think it [the radioactive caesium] was here before.'

These more private beliefs were rarely displayed in public, and the farmers refused to confess to such dissent in media interviews. It was made clear to us that one reason for this was that the farmers identified socially with family, friends and neighbours who were part of the Sellafield industrial workforce. They recognized their own indirect and sometimes direct social dependency upon the plant—not only neighbours but also close relatives of the hill-farmers work there. Thus, underlying and bounding their expressed mistrust of the authorities and experts, there was a countervailing deep sense of social solidarity and dependency—of social identification with material kinship, friendship and community networks which needed to believe Sellafield was well-controlled and its surrounding experts credible.

Thus social alienation and identification coexist in the same persons and communities, leaving deep ambivalence and apparent inconsistency in relevant beliefs and structures of 'understanding'. These can only be understood by reference to the multiplex, not necessarily coherent, dimensions of social identities expressed in interleaved social networks and experiences.

All this could be interpreted as yet another example of the lamentable inconsistency and impossible fickleness of lay beliefs. The conventional model of rationality would include a principle of cognitive consistency as measured against some canons of abstract logic. However what is revealed in this case is a deeper and more complex consistency in public reasoning than that recognized by such simplistic models. In the real world people have to reconcile or adapt to living with contradictions which are not necessarily within their control to dissolve. Whereas the implicit moral imperative driving science is to reorganize and control the world so as to iron out contradiction and ambiguity, this is a moral prescription which may be legitimately rejected, or at least limited, by people. They may opt instead for a less dominatory, more flexible and adaptive relationship with their physical and social worlds. In this orientation, ambiguity and contradiction are not so much of a threat, because control and manipulation are not being sought or expected. This is no less legitimate a form of rationality than the scientists', and the 'public understanding of science' research field should recognize this, and build upon it.

The advance from focusing on cognitive dimensions (often assumed public deficiencies) to trust and credibility is important. But closer examination in this case-study of the basis of trust and credibility falsifies the predominant analytical tendency to treat these as unambiguous, quasi-cognitive categories of belief or attitude which people supposedly choose to espouse or reject.<sup>30</sup> My analysis suggests instead that 'credibility' and 'trust' are themselves analytical artefacts which represent underlying tacit processes of social identity negotiation, involving senses of involuntary dependency on some groups, and provisional or conditional identification with others, in an endemically fluid and incomplete historical process.

Thus what the hill farmers believed about the scientists and their assertions was rich in insights and refinement, on several levels beyond the one-dimensional reductionism of scientific logic alone. But this richness was also pervaded by an ambivalence reflecting their multiple and conflicting social networks and relations. It would have been easy to have marked them down as mere 'don't knows' in a more efficient attitude survey, even though this would have been a grotesque distortion of the true position.

Recognition of this multi-dimensional, even internally contradictory character of belief allows a more accurate perspective on the apparent fickleness of public responses to risks and scientific knowledge which is much lamented by authorities. If we assume that a widely observed lack of public dissent to expert reassurances equals (voluntarily espoused) public acceptance, then apparently sudden shifts to opposition and rejection seem capricious, irrational and uncontrollably emotive. If on the other hand we recognize the alienation and ambivalence often underlying surface quietude, we may see that what looks like a sudden shift of attitude, a 'betrayal', was nothing of the kind—it may have been only a very small shift in the balance of components of social identity which people are holding in tension with one another. This intrinsic instability of actors' 'loyalties' is something which is not fully recognized in Latour and Callon's theoretical vocabulary of enrolment and representation of actors by scientists, as they build intellectual-social empires by tying in those actors, appropriately defined, to their particular role in the edifice. Thus Callon's account of the 'betrayal' of the marine biologists by scallop fishermen of St Brieuc Bay who had seemed to have internalized the identity which the scientists had articulated for them, does not recognize the possible ambivalence of the fishermen about their designated identity even before the 'betrayal', which may thus have been much less of a shift than it appears in Callon's otherwise superb account.<sup>31</sup>

Thus the cognitivist presumption that risks, or scientific knowledge, exist independently as an object for measurable public attitudes or beliefs, is left at least two steps behind. The first step is the recognition that the trustworthiness and credibility of the social institutions concerned are basic to people's definition of risks, or uptake of knowledge, and that this is reasonable, indeed unavoidable. However the second step is to recognize that trust and credibility are themselves analytically derivative of social relations and identity-negotiation; thus, like risk, they too should not be treated as if they have an objective existence which can be unambiguously measured and manipulated.

Having advanced the case for social identity as the more fundamental concept for explaining responses to science and risks however, it should be accepted that this term is itself not unproblematic. To claim that it offers more explanatory depth is not to claim it is empirically pure, coherent and unambiguously identifiable.

The theoretical orientation of this paper coincides with postmodernist concepts which treat identities as intrinsically incomplete and open-ended, and as an endlessly revised narrative attempting to maintain provisional coherence across multiple social roles and reference groups.<sup>32</sup> Beliefs and values are functions of social relationships and patterns of moral and social identification. This stands in sharp contrast to the taken-for-granted (and hence rarely articulated) commitment underlying conventional approaches, in which values and beliefs are taken to be coherent, self-sufficient, and discrete entities, and where social identities are simply the aggregate of individual beliefs and values. In this perspective social interaction is recognized only as an instrumental device to maximize preferences and values, not as an activity with moral

and social meaning in its own right.<sup>33</sup>

The case shows the unacknowledged reflexive capability of laypeople in articulating responses to scientific expertise. They are able to reflect on and develop their own social position as part of a 'dependent' response in which they are supposed to enjoy no powers of independent critical rationality autonomous from 'proper' assimilation of scientific understanding. Indeed it is interesting that those who would be regarded as the representatives of traditional society showed this reflexive capability, whilst the putative representatives of enlightened modernity, namely the scientists, did not. The scientists showed no overt ability to reflect upon their own social positioning, that is upon the latent social models which their scientific interventions imposed on the farmers. Perhaps the distribution of reflexive capability (or impulse) is itself a contingent function of social relations of power.

It is not true to say that scientists are not reflexive, in that they do show a capacity to reflect upon the nature of their practice, its contingencies and limits. However this may (for all social groups) be brought about only by criticism and a related sense of insecurity, rather than by any intrinsic qualities of self-criticism. Thus the extent of such reflexivity in science is open to question, both in how deep it goes into examination of scientific founding commitments (hence identities) and in how openly such critical self-examination is expressed to other social groups, for example in public or policy debate. Such articulated self-criticism would display the uncertainties in scientific knowledge, and at the same time expose as negotiable science's definition and role in relation to other social groups. As I have suggested in this paper, ambivalence is usually treated as intellectual feebleness—the antithesis of rationality and 'clear thinking'. But it may be a necessary corollary of a social commitment to disavowing control of others, in which case the remit of scientific rationality (as usually conceived) is seen in a radically different light.<sup>34</sup>

The intellectual properties of reflexivity or its lack (or to put it another way, of the epistemological principles of science) are not independent of the institutional forms of science. Thus it becomes evident why the quality of its institutional forms—of organization, control, and social relations—is not just an optional embellishment of science in public life, but an essential subject of critical social and cultural evaluation. It is a crucial missing part of the contemporary non-debate of science's social purchase and legitimacy.

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- 31 Callon, M., 1986, Some elements of a sociology of translation: domestication of the scallops and the fishermen of St Brieuc Bay. *Power, Action and Belief*, edited by J. Law, Sociological Reviews Monographs, No. 32 (Keele University Press), pp.196–233.
- 32 For this process in historical perspective, see Hobsbawn, E. J., and Ranger, T., 1984, *Invention of Tradition* (Cambridge University Press). See also: Giddens, A., 1991, *Modernity and Self-Identity* (London: Polity Press); Shotter, J., and Gergin, K., 1988, *Texts of Identity* (London: Sage).
- 33 See, for example, Burke, P., 1991, We the people: popular culture and popular identity in Modern

- Europe. *Modernity and Identity*, edited by S. Lash and J. Friedman (Oxford, Cambridge, Mass.: Blackwell), pp.293–310. This capability could be equated with the informal resistance of people in solidary sub-cultures to meanings, identities or rationalities imposed on them from 'outside', as in Bailey, F. G., 1968, A peasant view of the bad life. *Peasants and Peasant Society*, edited by T. Shanin (Harmondsworth: Penguin Books).
- 34 For suggestive correspondence with feminist critiques of science, see Harding, S., 1986, *The Science Question in Feminism* (Milton Keynes: Open University Press). On the significance of ambivalence I have been helped by Leigh Starr and Vicki Singleton, though they should not be implicated in my particular rendering of it here.

### Author

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