Contexts and Culling[1]

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Abstract:

This article asks how contexts are made in science as well as in social science, and how the making of contexts relates to political agency and intervention. To explore these issues, it traces contexting for foot and mouth disease and the strategies used to control the epidemic in the UK in 2001. It argues that to depict the world is to assemble contexts and to hold them together in a mode that may be descriptive, explanatory, or predictive. In developing this argument it explores how contexts are assembled in a series of different descriptive and explanatory narratives in epidemiology, policy, critical social science and (feminist) social studies of science.

Introduction

How are contexts assembled in science and social science? And how does the assembly of those contexts relate to political agency? In this paper we explore these questions empirically by looking at the strategies used in 2001 in the UK to control a major epidemic of foot and mouth. This disease is a highly infectious viral condition that affects cows, sheep and pigs[2]. Infected animals often become quite ill, though mostly they don't die. However, from an agricultural point of view, the condition is important because it reduces animal productivity, and this has substantial economic implications. Rich parts of the world (including Europe, North America, Japan and Australia) have successfully eradicated foot and mouth over the last fifty years. Thus the European Union including the UK is normally free of foot and mouth, and it seeks to maintain this status with strict important controls[3]. When the condition does appear it is eradicated by culling and, sometimes, by vaccination.

The 2001 UK outbreak was very large. First detected in late February, it affected over 2000 premises over a seven month period and was eradicated only with difficulty and after the slaughter of over six million animals. The direct cost to the UK government was in the region of £3bn, and the total cost (including, for instance, the effects on the tourist industry) was close to £8bn[4]. After the first few weeks, with continuing dramatic daily increases in the number of new cases, a widespread view developed that the Ministry of Agriculture, Food and Fisheries (MAFF) and the State Veterinary Service (SVS) were being overwhelmed – and in the case of the former was simply managing the outbreak incompetently. The result was a decision to move control from MAFF to the government emergency control centre, the Cabinet Office Briefing Room (COBR). This was accompanied with two major shifts in culling policy which were in turn influenced by the predictions of several epidemiological models. Importantly for our story the latter were in some disagreement. In order to understand how context is used in social science – and other – forms of explanation, in what follows we attend to this disagreement, its links to policy, and the implications, political and otherwise, of the culling strategy as it actually unfolded.

Epidemiological Models Assemble Contexts

> 'Epidemiology is the multidisciplinary study of the distribution and determinants of disease in populations, from the molecular level through field studies to modelling. The quality of the field data collected is critical to the quality of the policy decisions made and the value of analyses further down the line. Ultimately the role of epidemiology is to turn data into information which informs action and policy. Knowledge of the relevant details of a specific disease is essential, because the course of the infection within a host and the routes of transmission from one animal to another vary from one pathogen to the next.' (Royal Society 2002, 57)

This comes from the Royal Society reflecting on the control of infectious diseases in livestock. In one reading it tells us that epidemiology assembles a spatial and temporal context composed of infective relations between members of a population

[2] For details of the disease see Royal Society (2002).
[3] For an historical account of the evolution of this policy see Woods (2004).
[4] There are many accounts of the 2001 outbreak. For the major government-sponsored report see Foot and Mouth Disease 2001: Lessons to be Learned Inquiry (2002). See also Royal Society (2002), National Audit Office (2002), and Policy Commission on the Future of Farming and Food (2002) for further official accounts.

by tracing, describing and characterising those relations. In 2001 the relevant population was susceptible animals or, more exactly, the population of premises with susceptible animals (farms but also slaughterhouses). This meant that the geographical location of animals and the period of time in which they were infectious formed part of context. In addition, and as a part of this, veterinary theory and practice, biology, cartography and meteorology, economics, and farming practice were also implicitly and/or explicitly assembled as part of the epidemiological context. Examples included: knowledge of prevailing winds (the virus might be carried in a plume from infected premises); ambient temperatures and humidities (the virus lasts longest outside its hosts in warm and relatively humid conditions); land-holdings (farms that have many widely dispersed land-holdings require more movement of animals, which tends to increase the probability of infection); patterns of human behaviour (for instance about the animal trading system, the movements of milk tankers, or contractors). In short, social and economic versions of expertise were embedded in the 2001 epidemiology. They formed a part of its context.

Modellers themselves talk of kernels and of heterogeneities:

> 'Spatial interactions … are governed by … kernels, symmetric, normalized, decreasing functions of distance that determine the probability or relative strength of interactions. Kernels have a functional form and a scale parameter (λ) that sets the spatial scale of interaction. Large values of λ mean strongly localised, short-range interaction.' (Dieckmann, et al. 1999, 392)

Kernels define relations in spatial and temporal terms. And, as the citation suggests, they may do so in a wide range of ways as they are modified by heterogeneities. These are the kinds of complexifying specificities that we have just been discussing: the weather, the patterns of land-holding, the sizes of farms, their specific locations, the numbers of animals, the distribution of those animals between different species, the infectiousness of animals with the virus, the length of time they are infectious, and all the rest. Such geographical, biological, economic and social heterogeneities specify the space-times proposed by the kernels. They build context by introducing a range of different kinds of expertise. But the kernels themselves are similarly dependent on statistical expertise. Here another family of contexts is thrown into relief by the need of those who practice this craft to make a series of statistical choices. For instance, they need to decide whether they are going to build a deterministic model, or one that is stochastic or probabilistic. Since there are arguments for both, the decision is a trade off. Deterministic models are quick and dirty (best in an emergency?) while stochastic models better mimic the uncertainties of the world (slow, and perhaps better at the tail end of epidemics when more or less random infection is a common pattern).

Different Epidemiological Models Assemble Different Contexts

In the UK in 2001 there were at least four teams of modellers at work and those models were important in forming culling policy. Here we focus on just two: a simulation from Imperial College London (we'll call this the 'Imperial' model); and a version of a commercial 'InterSpread' disease model being run by the Veterinary Services Agency at Pirbright in Surrey (which we'll call the 'VLA'). The two models assembled very different contexts, and their implications for culling policy were also quite different.

The Imperial model was deterministic and therefore relatively simple and easy to run. It also mobilised relatively few heterogeneities[5]. So, for instance, farm type, farm size and farm spatial distribution were combined in a single function (Kao 2002, 282). The different animal species were similarly turned into a single 'average animal' in terms of susceptibility to infection, infectiousness, and the length of time it was infectious. It also, and importantly, modelled spatial relations indirectly. To put it simply, it 'knew' nothing about the location of particular farms in the UK, so it 'knew' nothing about how close individual farms were to one another. Instead it created a context of 'implicit spatiality'. In practice there were two spatial terms in the model. First, it directly assessed the likelihood of the long-distance spread of disease by calculating a mean field value. This meant that the probability of a susceptible farm being infected depended on the total number of premises infected across the whole country at any given time. (The intuitive plausibility of this is greater than may at first sight appear: in the early stages of the epidemic most new cases of the disease were seeded by the long distance transport of infected sheep in the national market system). Then for local spread, it imported a technique from statistical microphysics called moment closure. It did this by treating farms as members of pairs ('doublets') and threesomes ('triplets'). This logic ran so. A susceptible farm was, it was argued, more likely to become infected if it was a member of a doublet in which the other farm was already infected. Then again, an uninfected doublet was more likely to be infected if it formed part of a triplet in which one of the farms was already infected. The logic can (and needs to be) extended. Thus an uninfected triplet was more likely to become infected if it was part of the quadruplet in which one of the farms was already infected. And so on (in principle this is a logic without end). So, as one commentator notes:

> 'the description of singlets requires knowledge of doublets, the description of doublets requires knowledge of triplets, triplets requires quadruplets and so on. To prevent the number of equations from becoming intractable, the equations are 'closed' by a relationship that fixes the highest-order moment considered (in this case, triplets).' (Kao 2002, 283)

This, then, is 'moment closure', a procedure for ending the series. The Imperial model generated local relations of infectivity statistically (working from mean field values). Then it closed off what would otherwise have been an endless (and calculatively increasingly labyrinthine) series by making an assumption about the proportion of triplets that were also triangles (meaning that the three farms of a triplet were also connected to one another to make a triangle) (Kao 2002, 283). What's important about this is that spatial context had nothing to do with geographical input about individual farms. Spatial location depended on mean field value and some conceptually difficult and quite indirect statistical calculations.

Contrast all this with the VLA model[6]. The latter was stochastic and it introduced no less than 54 heterogeneities (Kao 2002, 283). So, for instance, its context included GIS data about the distances between different farms (more on this in a moment). So too were farm size, field concentration, the distribution of different animal species, differences between those species in terms of infectiousness and susceptibility, together with such meteorological variables as wind direction. The spatial kernel was a matrix of contacts between different farms. Using GIS data it included the distances

[5] For publications from this model see Ferguson et al. (2001, 2001).
[6] For publications derived from this model see Morris et al. (2001, 2002).

between every pair of farms in the UK. Then it corrected this with the (heterogeneous) attributes of individual farms such as the numbers of and mix of animals. Finally the simulation worked by multiplying, and re-multiplying, the matrix.[7] Compared with the moment closure method this was conceptually simple. On the other hand the size of the matrix – with an entry between every single pair of farms in the UK – meant that it was calculatively extremely complex. If the model could claim to be more 'realistic' than the Imperial alternative, it was slow to calculate and also opaque: with so many variables it was not intuitively possible to understand the results.

If epidemiological models assemble spatial and temporal contexts by proposing space-time kernels, heterogeneities and statistical assumptions, (together with the expertise from which these come), they also include data. Perhaps we should count this as further version of context. So, for instance, the VLA InterSpread simulation, with its large number of heterogeneities, included much data of differing quality. While some of this (from meteorology?) was unproblematic for those involved[8], other parts of it were not. Were new incidences of infection being promptly detected and gathered by the SVS? Probably not. Was the quality of the data about species location good? With anecdotal reports about GIS data which (for instance) located farms in impossible places such as the North Sea (House of Commons Committee on Environment Food and Rural Affairs 2002, paragraph 22), the answer again was, probably not. And there were issues to do, for instance, with the relation between the GIS location of farms (usually the farm owner's household) and where the livestock was actually to be found on the farm (Kao 2002, 284). In sum, the models depended on or included uncertain data-contexts, but they were also differently and differentially sensitive to it.

Contextual Differences

We have argued that epidemiological models make contexts, and that they do so in different ways. Note that in making this argument we have also created our own context. This includes the models themselves, something about their origins, and something about their links with social, institutional, and political/historical circumstances. Before commenting further we will broaden that context with more historical specificity.

In the middle of March when the field epidemiological data was fed into the models they all predicted catastrophe (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 90). The modellers agreed that two thirds of the cases had not yet been identified. They agreed that the control measures was insufficient (National Audit Office 2002, 61) and that detection and culling needed to be speeded up. They also agreed that the epidemic was going to become very large. However, and importantly for our story, they differed about its likely ultimate size. On March 12th the VLA model predicted that there would be a total of 1000-2000 infected premises (IPs) (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 88). But the first Imperial predictions, which appeared on March 16th, suggested that unless

[7] 'A contact matrix entry m_{ij} thus represents the probabilities per unit time that a given premises Fi, if infected, will infect another other premises Fj in the matrix. Recalculating this matrix at each time step will produce a simulated epidemic.' (Kao 2002, 282).

[8] For an illustration of this, see PowerPoint slide number 11 in Donaldson (2002), which shows the airborne spread of the virus from the index case of the epidemic at Heddon-on-the-Wall, Northumberland.

drastic action were taken the epidemic would grow exponentially, and that by the middle of May there would be up to 1000 new cases each day. And Imperial made a specific recommendation. It argued that there should be a pre-emptive cull of all animals within 1.5 or 3 km of infected premises (National Audit Office 2002, 61). This – and the model itself – were controversial. So what was the basis of the disagreement?

This question was played out in a bad-tempered series of private meetings between the modellers and civil servants. So, for instance, Jim Scudamore, the Chief Veterinary Officer, was sceptical about the Imperial model. Though their modellers dissented, he claimed that the Imperial prediction didn't reflect the national stop to all animal movements that had been imposed shortly after the beginning of the epidemic on February 23rd (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 58). This, he reasoned, had cut the rate of infection, but given the delay in discovering infections it was not yet being fully reflected in the reports of new cases. Then again, he didn't like the fact (noted above) that the Imperial model incorporated an average or generic animal: possibly important differences between species were not being modelled. Next, he worried that none of the models had been validated. For the first time in history a foot and mouth epidemic was well established in – and being driven by – infection in sheep. The status of all of the models was quite uncertain. And finally, he thought that the Imperial modellers didn't understand foot and mouth in sheep. Here he made three points. First, sheep are less susceptible to foot and mouth than either cows or pigs. Second, their symptoms are less obvious: they are easily missed or misinterpreted. And third, equally importantly, they are less infectious.

Overall, Scudamore's argument was that since no animals were moving nationally there was no longer any risk of large-scale long-distance spread. Then he argued that the high headline figures for newly discovered infections reflected circumstances in the first week or ten days of the outbreak (when the consequences of unregulated national stock movements were still unfolding). Finally, he believed that the disease was well established among sheep, especially round Carlisle on the English-Scottish border, but (this is the crucial point) the rate of new infection was low. It was, in short, being passed on, but only slowly. The bottom line was that this reality was reflected in the VLA model whose heterogeneities included the species distinctions and real geography, but not in the Imperial model. To base policy on Imperial's predictions and recommendations would be a mistake. Indeed, it risked being counterproductive, because it might divert resources away from the real (sheep-related) problem.

Contextual Differences Revisited

The context that we have assembled thus far includes two models, the expertise that they embedded and a description of high pressure in-house debate between the two approaches. It also touches on their implications for policy. So how did the difference work through in practice? To understand this we need to layer in a further political context. This comes in two versions: the public and the private.

By the middle of March the government was under considerable pressure. The publicly available headline figures for new infections were alarming. 104 new cases were reported in the week ending 12th March, 153 the following week, and 289 in the week ending 26th March[9]. The press was becoming hostile: 'Foot and mouth 'is

[9] Figures from National Audit Office (2002).

spinning out of control'' reported The Daily Telegraph on March 12th (Brown and Graves 2001), while the normally pro-government The Guardian ran the headline 'Ministry Under Fire as Disease Spreads' (Branigan 2001) on the same day. As we mentioned in the Introduction, there was a general feeling that MAFF was handling the crisis poorly. In private the government was also chewing on the predictions of the epidemiologists. This combination of circumstances was to lead to two major policy changes in two weeks. First, on March 15th a preventative cull of sheep in Cumbria within three kilometres of infected premises was announced.[10] Then, as the daily headline reports of new cases continued to rise and the Imperial results circulated through government, on March 27th a cull of all susceptible animals on premises contiguous to those infected was announced (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 93, National Audit Office 2002, 107).

As is obvious, the two policies reflect the respective epidemiological contexts of (a) the VLA and Scudamore, and (b) Imperial. The 3km cull of Cumbrian sheep was intended to suppress the slow-spreading epidemic detected and feared by Scudamore and predicted by the VLA. By contrast, the much more draconian contiguous cull is an indirect expression of the Imperial recommendation of a so-called 'ring cull' of all animals within 1.5 or, preferably, 3km of infected premises. To be sure, the contiguous cull was not the same as the proposed ring cull, and how the one got converted to the other is unclear (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 92). However, it is highly likely that the contiguous cull can be understood as an administratively tractable way of implementing the Imperial recommendation. So why did the Imperial-related contiguous cull displace the VLA/SVS 3km cull of Cumbrian sheep? The answer is: insider politics. More context.

The modellers debated their differences in an ad hoc group chaired by the head of the Food Standards Agency, Sir John Krebs. Crucially, Krebs was an Oxford zoologist – but so too were two other key players. On the one hand there was Roy Anderson, the somewhat controversial head of the Imperial group of modellers. On the other there was Chief Scientific Advisor to the Cabinet Office, Sir David King. King was the most powerful scientist in the Whitehall machine. He had, if he needed it, a direct line to the Prime Minister Tony Blair. In the course of this crisis this link was mobilised. The message was that MAFF, a low status ministry at the best of times, was failing, or at least appearing to fail, in its attempts to tackle the crisis. The VLA, the VLA model, and the State Veterinary Service under Jim Scudamore, were associated with MAFF. All were already coming under a cloud of suspicion from the central government machine including the Cabinet Office and the Chief Scientific Advisor. This is where the Imperial model was best represented: its Oxford-related sponsors were well connected with the Chief Scientific Advisor. The consequence? The strong policy recommendation deriving from the Imperial model looked like the decisive action that was apparently needed substantively (if the existing strategy was failing a more energetic policy was obviously needed) but also politically (something needed to be done and be seen to be done). Exactly who spoke to whom and when is unclear, but the result was the contiguous cull. The VLA and its model were side-lined, and the Imperial model was turned into policy.

[10] House of Commons (2001, column 1200).The minister talked of 'animals' rather than 'sheep' in his House of Commons statement, which caused much alarm.

Policy in Practice 1: Was the Contiguous Cull Successful?

In our account so far we have followed a explanatory strategy that is standard in Science, Technology and Society (STS). What we have done is to explain the closure of a controversy, a decision, by identifying this in part as an expression of the interests of powerful social groups[11]. The explanatory assumption is that a narrative is satisfying when a context of such interests has been identified. It would be possible to stop at this point. But it is also possible to make further STS moves that work to complexify our understanding of context. To do this we need to talk about culling in practice.

A brief reprise. The discovery of foot and mouth in February 2001 triggered what we might think of as policy number one. Animals on infected premises were slaughtered, 'dangerous contacts' were traced, premises close to infected premises were placed under surveillance, and movements of stock from those premises were halted. Policy number two was the pre-emptive cull of sheep within three kilometres of infected premises in Cumbria announced on March 15th. Note the difference between this and policy number one. Sheep within the designated area were to be slaughtered rather than kept under surveillance. Policy number three? This was the contiguous cull announced on 27th March that we have just been discussing. Also called the 24/48 hour cull, the intention was animals on infected premises should be slaughtered within 24 hours, and those on contiguous premises within 48 hours. So what happened next?

The answer is that the moment the contiguous cull arrived was also the moment when the headline figures for new infections started to fall. The statistics report that the number of new cases peaked on 29th or 30th March (50 were reported on 30th March, the largest number for any single day during the whole of the epidemic.) The number of new cases for the week ending 9th April was 221, and the corresponding statistics for the following weeks were 190, 110, 77, and 48[12]. It was going to prove very difficult to eradicate the disease completely – it bumped along somewhat erratically but at a much lower level until the end of September – but it was clear by the end of April that an important corner had been turned.

How to interpret this? There's a straightforward story here, and it's the one preferred by government. Controversial though the contiguous cull was (and it was hugely controversial) the official line is that:

> 'On the basis of the advice from its veterinary and scientific advisers, however, the Department considers that at the time the contiguous cull was the only way to bring the disease under control. The Department believes that it saved many animal lives by containing the spread of the disease.' (National Audit Office 2002, 65)

'The Department' (meaning the Department for Environment, Food and Rural Affairs (DEFRA) which took over the functions of the discredited MAFF in June 2001) is not alone. Large parts of the farming industry share this view:

[11] This mode of explanation may be traced back both to Marxist-inspired attempts to understand the ideological character of scientific belief, and the more 'symmetrical' methodological relativist explanations developed in the sociology of scientific knowledge. For an instance of the latter see Barnes (1977). For discussion of their relations see Moser (2007).

[12] All these figures come from the appendices in National Audit Office (2002).

> ‘The contiguous cull was controversial. In a comparatively small number of instances in Wales, the cull was resisted by individuals keeping livestock who contended that topography and the farming circumstances in their specific locale did not warrant a cull of their stock. From an overall epidemiological perspective, however, there were concerns that, by holding up the contiguous cull, these appeals did create or threatened to create serious disease risks.’(National Farmers' Union Cymru 2002, 18)

As we have seen, the epidemic was eventually halted. And indeed, so far had the rate of new infections fallen that the contiguous cull policy was relaxed in late April. More local discretion was allowed. Hefted sheep and rare breeds were excluded, as too were cattle if certain biosecurity requirements were met (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 98, 128). But there are ways of messing with this official conclusion. One is to argue that it was not the policy of contiguous cull that actually did the trick.

We’ve seen the official position, and it offers the most straightforward answer. Since foot and mouth disappeared from the UK after September, it follows that the contiguous cull must have worked. But this is controversial. Some think that Jim Scudamore got it right all along. Though the history is contestable it is nevertheless instructive. We have mentioned that peak number of new cases was around March 29th or 30th(National Audit Office 2002, 70). Note the timing. This is three days after the 3km cull of sheep (sheep only) was announced. Perhaps it was this that tipped the balance (though perhaps not, for it took some while to organise, and it may well be that the halt on movements of animals and the tracing and slaughter of the first EU-derived culling policy that was working). Whatever the merits of this argument, on the most straightforward reading, these figures suggest that infection reached its peak rate nine days before the contiguous cull became policy. This suggests in turn that the contiguous cull was only successful in the sense that it was overkill, and quite literally so. One sceptic notes that the Imperial team:

> ‘later modelled the epidemic … and concluded that changes in culling policies explained less than 50% of the observed variation in transmission rates, which in turn indicated that effective movement restrictions and rigorously maintained biosecurity were equally vital in reducing disease spread. This would suggest that the role of the contiguous cull in controlling the epidemic was less crucial than proposed by the earlier model.’(Kitching, et al. 2006, 301)

The critics make other arguments too. They argue that the Imperial kernel overestimated short-distance disease transmission, and underestimated the importance of longer distance transmission caused by inadequate biosecurity (Kitching, et al. 2006, 301).

This, then, is an epidemiological – and policy – controversy. Retrospective versions of descriptive epidemiological context are being assembled by the critics to explain why the contiguous culling policy was mistaken. And then, and alongside this, versions of social, political and administrative context are being assembled to explain why it was actually implemented. Everyone accepts that there was confusion at the time. Nevertheless, a possible implication is that, albeit in a position of considerable confusion, good science and well-founded decision-making were displaced by bad science as a result of the working of possibly inappropriate administrative and political connections in the corridors of power. This explanation can be strengthened if we add another and economic aspect of context. This is that it was the sheep

farmers, smallholders, and those with sentimental interests in stock who most resisted the contiguous cull. Large scale cattle farmers whose interests are primarily economic were much more sanguine about it.

This resonates with a classic sociological and political critique of bad science in both liberal and radical mode. The root argument is that good science, a well-founded grasp of reality, is being subverted by the operation of inappropriate and distorting professional, political, and economic interests, or perhaps by an ideology that (for instance) emphasises the importance of firm state control[13]. We're attracted to this position ourselves[14]. On the whole, and with the benefit of hindsight, we prefer the science of the critics than that of the proponents. But stepping back and thinking about this analytically, it is clear that judgements about the quality of epidemiology, of science, are being informed by a narrative about the operation of legitimate and, more importantly, illegitimate social interests. Accordingly, there is a double contextual move here. The critical narrative depends on the intersection of a retrospective, almost Whig, account of the science, of epidemiology, with a theory of social power and an account of the operation of (im)proper social interests.

Policy in Practice 2: Was the Contiguous Cull Adopted?

So that is criticism. But there's an alternative version context to be assembled that messes with the official position in another way. Here is the Lessons to Be Learned Inquiry

> 'In practice, the 48 hour contiguous cull was probably never more than 50% implemented. Certainly, in the areas of the highest infectivity, the implementation rate was lowest.' (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 95)

The general point is quickly stated. Whatever was going on in theory, in practice there were always multiple and different versions of the cull. In short, there was never an effective contiguous cull at all. Here's the Inquiry again:

> 'MAFF now had a range of culling policies to be implemented on the ground:
>
> - Culling of all susceptible animals on premises with clinically confirmed cases within 24 hours of report.
> - Slaughter on suspicion.
> - Culling of known dangerous contacts.
> - Culling of sheep, pigs and goats within 3km of infected premises in Cumbria and Dumfries and Galloway.
> - Culling of all susceptible animals contiguous to infected premises within 48 hours.' (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 97)

These were all versions of official policy – and in fact the list is not complete. On Anglesey, for instance, there was blanket slaughter of all sheep in one particularly seriously affected area (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 98).

[13] For its most straightforward liberal mode see Merton (1957). For a version derived from the sociology of scientific knowledge see Barnes (1974). For a brief account of the Marxist version see Hess (1997, 116). On the concern for state control see Bickerstaff and Simmons (2004).
[14] One of us has argued this. See Law (2008).

This tells us that in practice the policy was a patchwork. And this is just a beginning. The 3km cull (policy number two) was started at different times in Scotland and England (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 89). The contiguous cull (policy number three) was implemented differently in the two countries too. In Scotland it was applied quite pragmatically in the form of a ring cull around the perimeter of the affected region rather than round each individual infected premises (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 97). The reason for applying it in this way was to stop the disease breaking out of Dumfries and Galloway and entering the cattle-intensive central lowlands to the north. It was also implemented against a background of much greater consent, in part because of there had been careful local consultation about the 3km sheep cull. The consequence was a level of trust between farmers, the National Farmers' Union, the local authority, and the Edinburgh national devolved administration that was absent in England. Perhaps not uncoincidently, the policy turned out to be much more effective too. In Scotland 'the disease', says the Lessons to be Learned report, 'was eliminated after 91 days' (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 97).

By contrast, in England and in Wales there was patchy but quite widespread resistance:

> 'There were numerous appeals against the contiguous cull and many of these were upheld. In several instances the farm in question was re-designated as not contiguous. A climate of confrontation and opposition to the cull was generated in many parts of the country.' (Foot and Mouth Disease 2001: Lessons to be Learned Inquiry 2002, 97)

With many complaints about 'carnage by computer' (Mercer 2002, 6), lack of local veterinary discretion, and insensitivity of the policy to topographical variations, there is evidence, for obvious reasons often circumstantial, that in England many animals due for slaughter simply disappeared from official view thereby escaping the cull. There were also cases of overt resistance, with violence threatened against Ministry officials (Goddard 2002). Sometimes those officials were seen off. Vets went away and did not return. Here, then, is the argument: the reality of the culling was much more complex than allowed for in the formal statements of policy.

STS scholar Vicky Singleton has noted for other contexts that policy in practice is much more complex than policy in theory (Singleton 2010). Her argument is that seemingly consistent application of policy may be achieved through the application of a kind of artful inconsistency that she calls ambivalence (Singleton 1995, Singleton and Michael 1993). In part the point is loosely Wittgensteinian: rules do not contain the conditions of their own application. In the present case arguments about the non-coherence of policy assemble a complex context that describes: variable interpretations of policy; operation of the kinds of ambivalences described by Singleton; the multiplicity of policy itself; and, finally, resistance to that policy. It is this context of complexity, the description of a patchwork, that allows us to mess with the official account in this second way. Note that unlike the critical account above, this context does not include a retrospective or Whiggish narrative about the epidemiology itself. The context is, so to speak, purely social. And its politics? These are debatable. However, in at least one version they would argue for the devolution of decision making, and a much greater level of trust on the part of the centre for the periphery. The argument is quickly put. If policy in practice is a heterogeneous patchwork anyway, then why not simply acknowledge that this is the case? Why not,

for instance, let local people including vets, farmers, and local government officials make decisions about how to eradicate foot and mouth in their area?

Conclusion: Context and Coherence

We have argued that to depict the world is to assemble contexts and to hold them together in a mode that may be descriptive, explanatory, or predictive. In developing our argument we have assembled contexts to do with foot and mouth 2001 in a series of different descriptive and explanatory narratives. We first described how epidemiology builds its (varying) descriptions statistically, professionally, and with respect to its data. Then we inserted a difference between two of the major epidemiological models into a series of policy, administrative and political contexts.

In a first version we did this descriptively. We asked how the difference between the models unfolded into differences towards policy. This version of contexting can be understood as a non-judgemental and perhaps 'symmetrical' version of the social shaping of expertise and policy in the tradition of the sociology of scientific knowledge (Bloor 1976). No interventionary politics were implied. In a second version we offered a critical account of policy as it unfolded from the dominant Imperial epidemiology model, and generated a context of illegitimate social interests to show that the success of the model had less to do with science than with the operation of such interests. This was a critical account consistent with a series of liberal and radical positions in STS which have often, if not always, suggested the need to shelter science and its practitioners from inappropriate political and economic pressures. In a third version we argued that the contiguous cull deriving from the Imperial model was simply unnecessary. In some ways similar to the second critical account, this worked by creating a context that combined the operation of illegitimate social interests with a retrospective and Whiggish account of the progress of the epidemic in relation to culling policy. Its politics were similar too, either liberal or critical. Finally, in a fourth version, we argued, following another line of work in recent STS, that there was no single policy. Here, then, we assembled a context that revealed this policy to be a patchwork in practice. Since the disease was indeed eradicated, this not only suggests that conformity is difficult to achieve, but also and politically, hints that acknowledging local variations and trusting local practitioners to make good decisions might be desirable.

In the writing of social science this tells us that there are many ways of assembling contexts and holding them together. The contexts and the narratives in which they appear reflect, inter alia, the traditions of intellectual scholarship and affiliation. Interests, the possible importance of distortion, and ambivalence, all of these have been prominent in the narratives of STS. But we want to conclude by making a final move that is also informed by current STS concerns. All the stories we have assembled above assume that the world is consistent enough to be described within a single framework: that a single narrative context can be generated. This is true even for the last of these accounts, for even if different bits of the world don't fit together they can nevertheless be assembled into a story to do with an ambivalent patchwork of different policies in practice. The assumption is that even contexts that do not fit together can co-exist, if only because they are described as being distributed across time and space. So, for instance, there were different culling policies at different times and in different locations. This might (or might not) be a problem for policy makers or disease control, but it does not pose a problem for

description. For practical purposes all these different bits and pieces can be assembled together into an overview. So what should we make of this?

One response is that it is a very strong narrative assumption. What is the basis for assuming that different bits of context can always be held together descriptively? A second is that it is also a metaphysical or ontological assumption. The presupposition that is at work here is that the world with all its different contexts is indeed a single world, that contexts, so to speak, belong together. This, too, is a strong assumption. But why do we take it for granted? A third response is to note that to write in this way, to smooth the contexts together to make a single narrative, has consequences that are potentially performative, and indeed political. Thus if it were the case that the world was essentially non-coherent, then writing it as if it were coherent would be to marginalise not only the particular non-coherences that do not happen to make it into the narrative, but, and perhaps more important, the very possibility of overall non-coherence. We would not know that the world was essentially non-coherent. And, at least to the extent that they circulated and were taken seriously, our accounts would also tend to reproduce a version of the world that held together.

What might this mean for foot and mouth? The answer is that we would need to think about quite different strategies for knowing it. Thus the epidemiology and the culling would not fit together into a single description even in the patchwork version that we have explored above. It would not be possible to assemble all the relevant contexts and patch them together. Instead we would have to invent ways of knowing contexts, the elements of reality, by distributing and relating them in non-coherent ways. Here we can only gesture. But we might, for instance, imagine putting them alongside one another without the attempt to offer a bridging narrative, in the form of parallel texts, or in the visualisation afforded by pinboards[15]. Contexts would relate, but uncertainly and indeterminately. Or we might imagine knowing them in different ways in different locations – which is, we think, what happened in practice in the course of veterinary involvement in the culling[16]. Again different contexts would not necessarily fit together. It would also be likely that knowing well would involve knowing contexts and stitching them together differently at different times as well as in different places. All of which suggests that knowing would become untidy. That it would be located and situated, and that there would be no overview. That it would respond to the possibility that there are relevant realities that cannot be necessarily be brought together within a single description at all.

We said just above that this is a move sanctioned by current STS. One of these – to do with situated knowledges – comes from the work for Donna Haraway (1991, 1997). Another, coming in part from the writing of Annemarie Mol (2002) explores the ontological performativity of practice. This line of work argues that practices, including representational practices, more or less precariously enact realities. It then suggests that there is no a priori reason to suppose that practices are automatically aligned. But if they are not automatically aligned, then neither are the realities enacted in those different practices automatically aligned. They might, but they might not, be assembled. They might come to form the more or less ordered contexts of a narrative. But they might not. And if they assembled together at all, this might, in any case, might by non-discursive means.

[15] On the possibilities offered by pinboards, see Law (2007).
[16] See, for instance, Law (2010).

The implications of this ontological multiplicity for both descriptions of context and for politics take us beyond the limits of this paper. But it does suggest, as we have above, that any overview is a precarious achievement. More important, it is also a situated achievement. Knowing, describing, explaining, assembling contexts, and intervening – all of these will be local, specific, and transportable only uncertainly, from one context to another. To know foot and mouth, its epidemiology, and its culling well will be to know in ways that do not and cannot cohere. This is the context of non-coherence.

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