5. Challenges to the Government’s approach

Introduction

5.1 Shortly after the emergence of BSE, the Government introduced control measures on the basis of work by government scientists who had identified the vector of disease and begun to characterise the nature of the infective agent. However, the conclusions reached were not universally accepted by all scientists, some of whom criticised government findings as well as the reluctance of the Government to allocate funds for research into alternative theories. Several scientists and farmers developed views about the disease, that ran counter to the Government’s views. They were often vigorous in seeking to persuade MAFF and DH of their views and concerns about the disease, including the implications for human health. The media regularly took up their cause.

5.2 In this chapter we look at the views of three microbiologists (Professor Richard Lacey, Dr Stephen Dealler and Dr Harash Narang) and one organic farmer (Mr Mark Purdey) and their interactions with MAFF and DH from the late 1980s up until March 1996. In the first section of this chapter we look at Professor Lacey and Dr Dealler, who have featured in other parts of the Report, eg, vol.6: Human Health 1989–96. Before BSE emerged they had worked together on promoting food safety, and often shared the same views on the Government’s response to BSE.

5.3 We then turn to Dr Narang who developed several diagnostic tests for BSE and sought to promote their adoption by MAFF and DH. Finally we look at the Government’s response to the theories of Mr Purdey regarding the relationship between BSE and the use of organophosphates (OPs) in agriculture. Further details of Dr Narang’s diagnostic tests and a scientific assessment of Mr Purdey’s theories are given in vol.2: Science.

5.4 Our intention in providing the above accounts is to explore the Government’s response to, and handling of, unsolicited independent views, particularly from those who pursued their case with persistence during 1986 to 1996. We are aware, of course, of other independent scientists, farmers and members of the public who were interested in BSE and voiced their concerns during this period. Indeed we have received evidence from many of these during the course of the Inquiry. Elsewhere in the Report, for example, we mention Dr Helen Grant (a consultant neuropathologist) who was often cited in the media during this period about her concerns that beef-eaters in the UK were consuming large doses of the causal agent of BSE which ‘would inevitably infect genetically susceptible people’. Her concerns were first raised shortly after the Southwood Report was published, as discussed in vol.6: Human Health, 1989–96.

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918 S410 Grant para. 4
**Professor Lacey and Dr Dealler**

5.5 Professor Richard Lacey was Professor of Clinical Microbiology at the University of Leeds from 1983 until his retirement in 1998. Prior to becoming involved in the BSE story, Professor Lacey was a vocal critic of the Government’s stance on food safety issues such as salmonella, listeria, cook-chill foods and the use of microwave ovens. Professor Lacey had been a consultant to the World Health Organisation since 1984 and advised MAFF as a member of the Veterinary Products Committee between 1986 and 1989 on preventing diseases in people that are derived from animals and food.

5.6 Dr Stephen Dealler is a consultant microbiologist and had previously been a medical senior registrar in Professor Lacey’s department. He worked with Professor Lacey on the study of listeriosis and listeria in cooked and chilled food.

5.7 Dr Dealler and Professor Lacey challenged certain aspects of the Government’s stance on BSE, in particular the view that ‘BSE was scrapie in cattle’, and raised concerns over the possibility that the BSE agent could be present in beef and pose a risk to human health. They also noted the risk of maternal and lateral transmission of BSE within cattle, questioned the Government monitoring of cases of CJD and BSE and challenged conclusions based on experiments on the infectivity of cattle tissue. Coverage of their views in the media is described in the context of unfolding events in vol.6: Human Health, 1989–96.

**Views from Professor Lacey following the discovery of FSE, May 1990**

5.8 Professor Lacey told the Inquiry that he first took a particular interest in the emerging reports of BSE when he was researching for his book Safe Shopping, Safe Cooking, Safe Eating in early 1989. He looked at various documents including the Southwood Report and the Tyrrell Committee Report in January 1990. On 11 May 1990, the day after the Government’s announcement that a spongiform encephalopathy had been diagnosed in a domestic cat, Professor Lacey was interviewed by a journalist from the Sunday Times and said that all herds infected with BSE should be destroyed. The paper ran the story on 13 May 1990.

5.9 Professor Lacey also told us that he was aware of the difficulties involved in raising the profile of BSE in the media:

The rising incidence of salmonella and other food poisoning, listeriosis, prompted me and my Department in Leeds in the mid 1980s to research the reasons why this was occurring, with a view to trying to reverse the trends. It was evident from the outset that there would be a conflict between the interests of the food producers and that of the consumers, particularly as both these were the responsibility of the same Ministry. I have adopted the attitude that fear of such a conflict should not be a deterrent to researching, analysing and raising the issues. The outcome of this research has in some
areas been gratifying with hazards from listeriosis, cook-chill and microwave use, now considerably reduced compared with 10 years ago. Unfortunately food poisoning from salmonella has remained high. The concerns, which I expressed from 1988 about salmonella in egg, inevitably led to criticism from the egg farmers and their Parliamentary supporters. Similarly when I made known my concerns in 1989 about processed foods, including cooked chicken, soft cheeses and cooked chill food, this led to similar people disparaging me. I also pointed out at this time that \textit{E. coli} 0157 was a potential problem with cooked meat. Therefore I was aware that with this background, raising the profile of BSE would generate anger from those with a vested interest. Despite that, my training and professional responsibility required me to give priority to the welfare of the public.\footnote{S10 Lacey para. 4}

\textbf{5.10} Dr Dealler told us that Professor Lacey considered that ‘there was no chance of getting any effect on the management of BSE unless it was done through the media’.\footnote{S22 Dealler para. 52} During oral evidence Dr Dealler said that he had discussed this approach with Professor Lacey:

\begin{quote}
I said to Lacey at the time, ‘You are being more aggressive than will be effective’. I think that was one of the times that he said to me, ‘You will get nothing done going through official channels.’ I think it was right back then. He said it a number of times that to try to get things done through official channels would be ineffective. The only way to get things done would be through the media. I found he was right in the end.\footnote{T15 Dealler p. 32 lines 8–16}
\end{quote}

\textbf{Agriculture Select Committee Inquiry into BSE, May to July 1990}

\textbf{5.11} On 16 May, the Agriculture Select Committee decided to hold an inquiry into BSE following an increase in public anxiety about the disease.\footnote{IBD1 tab 7 p. ix} Further details about the Select Committee’s inquiry are given in \textit{vol.6: Human Health, 1989–96}. Professor Lacey was invited, in collaboration with Dr Dealler, to produce a memorandum on BSE and related issues for the Select Committee.\footnote{S10 Lacey para. 13; S22 Dealler para. 19} Professor Lacey and Dr Dealler together produced a paper entitled, ‘The risk to man’.\footnote{IBD1 tab 7 p. 19} The paper outlined three reasons why there was concern over the infective agent in beef:

\begin{itemize}
\item[i.] Peripheral nervous tissue may contain infectious agent. For example, it is well known that the sciatic nerve from scrapie-infected sheep can transmit the infection to goats (Pattison et al, 1962).
\item[ii.] Lymphatics in channels and nodes around beef tissues may be infectious.
\item[iii.] During the processing of the carcass after slaughter, the procedure to remove spinal cords, brains and other ‘high-risk’ tissues could easily contaminate the rest of the carcass. In particular, the use of mechanical saws to remove bone could be hazardous in this way.\footnote{IBD1 tab 7 p. 20}
\end{itemize}
5.12 Professor Lacey appeared before the Agriculture Committee on 13 June 1990 along with Professor Ivor Mills, Emeritus Professor of Medicine at the University of Cambridge, Dr Helen Grant, a retired neuropathologist, and Dr Gareth Roberts from the Department of Anatomy and Cell Biology at St Mary’s Hospital Medical School. In oral evidence to the Select Committee, Professor Lacey said that the process of removing the heads of cattle with saws may cause soft brain material to ‘be spread around the slaughterhouse and contaminate other food items, in particular beef’. He also said that there was a dose/time relationship to the disease in contrast to the generally accepted dose-only relationship. He stated that very small doses of infectious material could produce clinical symptoms many years later. Professor Lacey suggested that, given that scrapie-type agent had been shown to be transmitted through peripheral nerves, there might be a danger from eating beef, since muscle is intrinsically linked with nerve tissue.

5.13 A Committee member criticised much of Professor Lacey’s submission as being ‘speculation and supposition and conjecture’. Professor Lacey was also asked by the Committee to comment on statements attributed to him in the press, including the statement that beef ‘may be from BSE infected herds, which are most herds in Britain’. Professor Lacey said in relation to the statement that he believed ‘that for every animal that has clinical BSE there are a substantial number that have sub-clinical, are infected but do not appear ill’. Professor Lacey continued that this was an opinion and remained so because the Government had failed to undertake ‘proper tests on the brains and other tissues of the non-clinically infected animals’.

5.14 Professor Lacey was also asked to comment on a further statement of his that ‘we cannot rule out the possibility of the disease spreading to humans, particularly pregnant women and young children’. He denied being ‘sensationalist’ and said that his opinions were formed on the basis of scientific knowledge.

5.15 When asked about the ‘difference between the incidence of human CJD vis a vis scrapie and the possibility of human CJD from bovine spongiform encephalopathy’, Professor Lacey told the Select Committee:

I think the question of sheep scrapie going to man is unproven. The evidence is either uncertain or weak. There is no really good evidence that sheep scrapie goes to man and we have eaten a lot of sheep’s brains, nerves, meat for many years. That is not in doubt. Some people say it is but I believe the evidence of sheep scrapie to man is small. The problem with BSE, we do not know if it has come from sheep in the first place. We do not know where it has come from. We do not know what it is. We do not know its host range. We have to look at BSE entirely as a new disease and I am not going to say it will go to man, I am not going to say it will not, I am saying we do not know.

934 S10 Lacey para. 14
935 IBD1 tab 7 p. 43 para. 48
936 IBD1 tab 7 p. 43 para. 49
937 IBD1 tab 7 p. 45 para. 64
938 IBD1 tab 7 p. 46 para. 72
939 IBD1 tab 7 p. 46 para. 69
940 IBD1 tab 7 p. 46 para. 69
941 IBD1 tab 7 p. 46 para. 72
942 IBD1 tab 7 p. 48 para. 85
5.16 The submission to the Select Committee by Professor Lacey and Dr Dealler contained several recommendations for further research. During oral evidence to the Select Committee, Professor Lacey highlighted two particular research projects which he considered were ‘serious omissions’ from the Report of the Tyrrell Consultative Committee on research:

One, the testing of brains and spinal cord from slaughtered cattle and cows that are clinically well, looking for the various proteins associated with this disease. I want to know why this has not been done before, it is the first thing that should have been done to identify the scale of the problem. This is a very serious and major omission. The second point is if we are going to address the most dreadful question, which is to what extent is the human population at risk, then there are various things that could be done. For example, people dying could have their spleens examined. If this infection gets into the human population we can assume its progress will resemble that of other spongiform encephalopathies with the spleen being one of the first organs.

5.17 Professor Lacy also commented during his oral evidence to the Select Committee that ‘if our worst fears are realised, we could virtually lose a generation of people’.

5.18 In his statement to the Inquiry, Dr Dealler said in relation to Professor Lacey’s appearance at the Select Committee Inquiry:

I was shocked by the way in which Professor Lacey was questioned. It was as if the Members of the Committee had not been informed adequately about this kind of disease. I left and went over to MAFF and asked to be able to give information to the people involved. They refused to see me personally and after about 20 minutes sent a secretary to collect the information. I sat and wrote notes on each of the scientific articles I had brought over, which were then taken by the secretary. The Ministry returned the articles to me 3 weeks later. The articles showed that in other species muscle and other meats that we were continuing to eat had been shown to be infective.

5.19 On 10 July 1990, the Agriculture Committee produced its report. This included a passage about Professor Lacey’s evidence:

That not all scientists carry equal authority was amply borne out in our evidence. Professor Lacey, in particular, showed a tendency to extrapolate sensational conclusions from incomplete evidence in order to publicise his long-standing concerns about food safety. The result was a mixture of science and science fiction – a quite unsuitable basis for public policy. When he told us that ‘if our worst fears are realised, we could virtually lose a generation of people’ he seemed to lose touch completely with the real world. We do not doubt the sincerity of Professor Lacey’s concerns, but we must question the judgement of television producers and newspaper editors who beat a path to his door as an authority on all aspects of food safety.

943 IBD1 tab 7 p. 51 para. 102
944 IBD1 tab 7 p. 55 para. 131
945 S22 Dealler para. 19
946 IBD1 tab 7 p. xxi para. 74
Dr Dealler consults other scientists with an interest in TSEs – summer 1990

5.20 In summer 1990, Dr Dealler visited several groups both in Britain and the USA to discuss BSE. At the Neuropathogenesis Unit (NPU) in Edinburgh he met Dr Hugh Fraser and Dr Moira Bruce. In evidence to the Inquiry, Dr Dealler said that Dr Fraser was concerned about the research that was being done and about the length of time taken for the offal ban to be introduced. Dr Dealler also said that Dr Fraser was concerned that tissues other than the specified offals may present health risks.

5.21 The research being carried out by Dr Fraser at the NPU involved inoculating mice with BSE infected tissue. Dr Dealler stated that this research would not necessarily give any indication of the infectivity present in those bovine tissues.

5.22 Dr Dealler visited Dr Stanley Prusiner’s laboratory in San Francisco during the same period, where he met and spoke to Dr David Westaway who worked there. Dr Dealler reported Dr Westaway as saying that it was difficult to get bovine tissues from MAFF in the UK.

5.23 Dr Dealler also visited Dr Bruce Cheesebro at the National Institute of Health (NIH) in Montana. Dr Cheesebro similarly disputed that the mice experiments would show the infectivity of different bovine tissues.

5.24 Dr Dealler sought a meeting with Dr Joe Gibbs of the NIH in Maryland. Prior to the visit, Dr Gibbs wrote to Mr Ray Bradley of the CVL enquiring both about the identity of Dr Dealler and the prospect of undertaking collaborative research with Mr Bradley. Mr Bradley replied saying that Dr Dealler worked with Professor Lacey and that:

As a result of the interviews of Professor Lacey by the [Agriculture Select] Committee, he has been very much discredited as an alarmist.

5.25 However, Mr Bradley did not know where Dr Dealler stood on the issue of Professor Lacey’s ‘high profile and sensationalist approach’. He advised:

. . . caution in forming a close liaison with the Department [of Microbiology, Leeds], particularly on the subject of meat and bone meal analysis, if that is the intention.

5.26 Mr Bradley also added that:

We are always willing to consider collaboration and supply of materials to institutes of excellence like your own, provided the objectives are sound and there is approval of importation of materials by the Federal Authority.
5.27 Dr Dealler said that at their meeting, Dr Gibbs was determined that BSE presented no risk to humans at all, although his researchers did not agree. 955

MAFF/DH response to Professor Lacey’s and Dr Dealler’s article in *Food Microbiology*, August 1990

5.28 Professor Lacey and Dr Dealler published an article in the journal *Food Microbiology* on 30 August 1990 entitled ‘Transmissible Spongiform Encephalopathies: The threat of BSE to man’. 956 The paper was a general review of TSE knowledge to date and provided views on histopathology, epidemiology, possible sources of infectivity and the risk to humans. They concluded that while there was little reason to suspect the presence of the TSE agent actually within or around muscle fibres, peripheral material – such as nervous tissue, lymphatics in channels and nodes around the beef tissue – was certain to contain infectious material if it was present. They considered that the means of processing beef, specifically the use of mechanical saws, would not remove these tissues and would increase the likelihood of contaminating the meat with brain and spinal cord. Thus there was a distinct possibility that man could acquire spongiform encephalopathy from consumption of contaminated beef. In addition, calculations were made regarding the infectivity of beefburgers, on the basis of an estimation of the amount of BSE infected brain tissue present in a beefburger. Professor Lacey and Dr Dealler concluded that the crucial question was:

Is a high infective dose required to provide the certainty of the infectious agent entering a cell at a specific receptor? If this were the case, then the consumption of small amounts of agent on numerous occasions could provide the same risk as consumption of the total number of particles on more than one occasion.

They thought that:

More research is required to assess the risk to man, and to develop methods for early detection of TSEs before the onset of clinical symptoms.

5.29 This article was circulated within MAFF and DH 957 on 19 December 1990 by Mr Maslin, who commented that the article contained ‘numerous errors and misrepresentations. I recommend that it not be read by those with high blood pressure.’ 958 Dr Pickles replied that the paper should be circulated to SEAC. 959

5.30 Mr Kevin Taylor prepared a critique of the article at the request of Mr Gummer, which included a note from Dr Pickles. Dr Pickles noted, amongst other points, that:

This paper is a mixture of scientific review, comment and speculation . . . This is not an authoritative work . . . There are several errors of fact . . . Apart from the flavour of scaremongering, it is not easy to extract the main points the authors are trying to make in this paper. 960

955 T15 Dealler p. 48
956 S22 Dealler para. 34; S F Dealler and R W Lacey, *Food Microbiology*, 1990, 7, 253–279
957 The article was circulated to Mr Meldrum, Mr K Taylor, Mr Lowson, Mr Lawrence and Dr Matthews in MAFF and Dr Pickles and Mr Murray in DH
958 YB90/12.19/2.1
959 YB90/12.19/2.1 (manuscript note)
960 YB91/1.14/1.6–1.7
5.31 On 15 January 1991, Mr Meldrum sent Mr Gummer a critique based on Mr Taylor’s and Dr Pickles’s critiques. On 1 March 1991, Mr Lowson wrote to colleagues in MAFF and DH asking for views on a possible public response from SEAC to the *Food Microbiology* article. He enclosed a draft version of the response to the Dealler/Lacey article which had already been shown to Ministers.

5.32 At their meeting on 7 March 1991, SEAC concluded that it did not want to respond to the article and that there was nothing in the article causing them to question their previous advice.

5.33 A critical response to the article was, however, submitted to *Food Microbiology* by Mr Taylor, and was published as a letter to the editor in September 1991. Mr Taylor wrote to the CVO and Mr Bradley informing them of his response, stating that:

I see little point in further argument about factual errors, most of which Dealler and Lacey claim not to be errors anyway, and propose to do no more than suggest that black will still be black however often you say that it is white.

5.34 In his letter published in *Food Microbiology*, Mr Taylor stated that:

The major shortcoming of the paper is not, however, the cavalier attitude to well-established facts that these errors reflect, but rather the assertions that are made about the implications of BSE for human health.

5.35 He added,

. . . the ‘estimates’ of BSE infectivity in human food are entirely misplaced. On the few experiments in natural scrapie, in the few subclinically infected British cattle now being slaughtered and in those parts which are permitted for human consumption, we would not expect any ‘infective units’ even by assay by intracerebral infection into susceptible mice. The added safety margin of oral exposure, which usually requires a dose $10^3 – 10^9$ higher to lead to infection, and a species barrier, was found entirely reassuring by the Tyrrell Committee.

5.36 Professor Lacey and Dr Dealler replied to Mr Taylor in the same edition of the journal. Their response contained point by point rebuttals of Mr Taylor’s arguments. In particular they asserted their own accuracy in calculating the infectivity in food and argued that their assumptions for the relationship between oral and injected doses were accurate. The two letters disputed each other’s facts. Professor Lacey and Dr Dealler demanded that the Government initiate experiments with transgenic mice bearing the human PrP gene. They said that if, when injected with BSE infected material, these mice succumbed to disease and produced human PrP, it would show that BSE was infectious to humans.

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961 YB91/1.15/8.1
962 YB91/3.1/7.1
963 YB91/3.07/2.8
964 K C Taylor, *Food Microbiology*, 1991, 8, 257–262
965 YB91/8.14/3.1
966 K C Taylor, *Food Microbiology*, 1991, 8, 258
967 S F Dealler and R W Lacey, *Food Microbiology*, 1991, 8, 259–262
5.37 On 24 August 1991, Mr Bradley minuted Mr Meldrum and Mr Taylor recommending a brief response correcting major errors and a general comment on disagreement with the Dealler and Lacey reply. He said that the response should avoid personal criticism and base all criticism on scientific facts, supported where possible with published papers. He continued:

. . . I advise caution here because this is essentially Dealler’s article (and he has even more extreme and bizarre views than Lacey judged on a meeting JW [John Wilesmith] and I had with him). . .

5.38 On 30 August, Mr Meldrum wrote to Mr Taylor about this correspondence. He said:

The response from Dealler and Lacey makes inflammatory reading and you will need to respond in your inimitable style knocking down the main issues raised.

5.39 Mr Meldrum suggested that Mr Taylor should seek input from Mr Bradley and from Dr Richard Kimberlin in putting together a further response.

5.40 On 4 September, Dr Pickles sent a note to Mr Taylor on Dr Dealler’s and Professor Lacey’s reply saying:

The latest note from Dealler and Lacey is so full of errors yet again I agree there is no purpose in a point by point rebuttal. But in not doing so, we need to point out that this should not be taken to imply that either their facts or arguments are any more valid this time than last.

5.41 Professor Lacey repeated his concerns about contaminated beef in a book published in 1991 entitled *Unfit for Human Consumption*.

5.42 In oral evidence, Dr Tyrrell was referred to the August 1990 article of Professor Lacey and Dr Dealler in *Food Microbiology*.

MR WALKER: . . . If I could just take you to the last sentence of the summary at the beginning? It is:

‘There is a distinct possibility that man could acquire spongiform encephalopathy from consumption of contaminated beef.’

Is that a conclusion that members of the Committee disagreed with?

DR TYRRELL: I do not think so, sir. What we have said already round this table, we always thought it was a possibility. I think our disagreement with a lot of the Lacey and Dealler statements, and some of their papers, was that in elaborating their ideas, which after all, in many cases, were just restatements of the data which had been collected by MAFF, they then added hypotheses about, for instance, the amount of infectivity to be found in various tissues, using for this, in almost every case, the worst case, which

968 YB91/8.24/1.1
969 YB91/8.30/1.1
970 YB91/9.4/1.2
could be extracted from research publications. I mentioned this in my statement. Our philosophy was not to deny unpleasant possibilities occurring, but to try to use a biologist's feel, if you like, for the overall situation, and say that ‘the most likely thing to happen is the following’.

It was when we got to that point that we parted company with Dealler and Lacey, both of whom are very able people in many ways. But that particular statement, as I have said, would have fallen into the category: ‘Well, what is there more to say? They have looked at the data, we have looked at the data. We come out with different views. They know what our views are, we know what theirs are.”

Professor Lacey proposes that BSE is endemic in Great Britain, February 1992

5.43 On 7 February 1992, Mr Kevin Taylor sent a minute to the MAFF Parliamentary Secretary, Mr David Maclean, informing him that Professor Lacey had written to the *Veterinary Record* using answers to Parliamentary Questions (PQs) to suggest that BSE was established as an endemic disease in Great Britain. The editor had invited Mr Taylor to reply and was intending to publish both letters in the 15 February issue. Mr Taylor attached copies of Professor Lacey’s letter and his intended reply. In his minute to Mr Maclean he said that:

> It is clear that Lacey is attempting to start a story. We know that he has contacted a number of journalists, including [James] Erlichman of the *Guardian*, saying that the information revealed by the recent PQ answer is cause for concern.973

5.44 On 15 February 1992, both letters were published in the *Veterinary Record*.974 Professor Lacey said:

> Three disturbing trends are seen: the total number of BSE cattle in 1991 is over threefold that in 1989, the mean age in 1991 is somewhat lower, and some BSE cows in 1991 could well have not been fed with contaminated offal. This suggests that BSE is now an established endemic, even though it was initiated or aggravated by feeding offal.975

5.45 Mr Taylor replied that:

> I do not believe that any evidence exists to justify Professor Lacey’s suggestion that BSE is now an established endemic. Nothing has yet happened to alter the view that sources of infection other than food are unlikely to be important for the future of the epidemic, and continued monitoring of the epidemic, though not yet conclusive, gives cause for optimism.976

5.46 Professor Lacey presented figures published in Hansard to compare the ages of death of BSE cattle in 1991 and 1989 and concluded that BSE was an established
endemic disease even though it was initiated or aggravated by feeding offal to cattle. Mr Taylor’s response supported the feed hypothesis and the view that cattle had received MBM when they should not have done. He expressed concern that Professor Lacey was being selective in his choice of data and said that inferences drawn from incomplete data were ‘fanciful’.

5.47 On 14 and 15 April 1992, Professor Lacey was reported in the media as claiming that an epidemic of BSE was ‘likely to hit the human race by the end of the century’. He was reported in the Evening Standard on 14 April as suggesting at a conference organised by the Townswomen’s Guild that there was a 70 per cent chance of the disease being transmitted to humans and that children were particularly at risk.977

5.48 Mr Lowson briefed Mr Gummer on Professor Lacey in a minute on 22 April 1992:

Professor Lacey’s latest speech is based on no new information; his assertions remain as questionable as they have always been.

. . . nothing has happened to suggest that BSE is likely to transfer to humans; the 70 per cent chance that he mentions is pure fantasy.978

5.49 On 9 June 1992, Professor Lacey wrote to Mr Wilesmith with comments on an article by Mr Wilesmith which had appeared in the Veterinary Record of 30 May.979 Professor Lacey voiced concerns about the data, namely that:

The suggestion that the offal banning of July 18th 1988 can only be expected to have an observed effect during 1992 would seem to me incompatible with the large number of 3-year-old animals that develop the disease.980

5.50 He suggested that urgent research was necessary to identify the number of animals at slaughter which were infected but not clinically ill. Mr Wilesmith drafted a response which he copied to Mr Taylor and others for comments on 11 June 1992.981 This contained rebuttals of Professor Lacey’s concerns but no comment on the suggestion of research into identification of non-clinically ill cattle at slaughter.982 On 19 June 1992, Mr Meldrum commented on the draft response and said:

. . . we should be aware that this letter is likely to get into the public domain and therefore in my opinion should be shown to Ministers and Press Branch before despatch.983

5.51 Mr Gummer approved the draft response on 26 June 1992.984

977 YB92/4.14/5.1
978 YB92/4.22/2.1
979 YB92/6.09/6.1
980 YB92/6.9/6.1
981 YB92/6.11/3.1
982 YB92/6.23/5.1
983 YB92/6.19/8.1
984 YB92/6.23/5.1 (manuscript note)
Further developments in 1992

5.52 On 12 June 1992, David Hinchliffe MP asked a Parliamentary Question which Professor Lacey told the Inquiry had been on his behalf. He requested details of the animal experiments which had been or were being carried out for the Government. The information was placed in the Library of the House and Mr Hinchliffe provided Professor Lacey with a table of results.985

5.53 On 29 July 1992, Mr Lowson informed the Minister that Professor Lacey had asked Dr Tyrrell for a meeting to discuss BSE. Dr Tyrrell had replied that he would be happy to arrange a discussion and suggested that someone from MAFF, possibly Mr Bradley, should also attend.986

Dr Dealler seeks MAFF’s views on the use of cattle brains for human consumption, early 1993

5.54 On 26 January 1993, Dr Dealler wrote to Mr Maslin asking about the amount of potentially infected tissue that would have been in the diet of the human population prior to 1989.987 Mr Maslin replied on 29 January 1993 and again on 11 March 1993 after consulting colleagues in MAFF about the use of cattle brain for human consumption. All agreed that cattle brains would have been little used before the specified bovine offals (SBO) ban. Mr Maslin also said that there was information that suggested that none of the offals specified in Schedule 2 of the Meat Products and Spreadable Fish Products Regulations 1984 were used before the SBO ban was enforced. Use of the thymus was also reported as rare.988

5.55 Dr Dealler replied on 12 March 1993.989 He was not convinced that bovine brains were not being used for human consumption before 1989, since he felt that wherever there was an economic benefit to selling even small scraps of material it would happen. He suggested that MAFF should fund work looking at treatments for Creutzfeldt-Jakob Disease (CJD) ‘if only to cover themselves’ in case of being found at fault for the whole outbreak of BSE. He said that the Southwood Report was ‘exceedingly misleading’ and that BSE should have been assumed to be infective to humans ‘because the risks were simply too high’. He recommended to Mr Maslin that results of experiments examining infectivity in different cattle tissue should not be published as they would be ‘shot down in flames’. He said that the amounts of tissue used were ‘very small and crossing a species barrier’, so that one would not be expected to see infectivity.990

5.56 Further Dr Dealler said he did not, at that time, intend to publish any further work on BSE that included information about human intake of tissue banned under the SBO regulations. He pointed out that Professor Lacey was still his ‘boss’, which meant he could not send Mr Maslin articles in advance of publication unless he was assured Mr Maslin would not ‘let anyone know about it’.991

5.57 Together with his own letter, Dr Dealler also forwarded to Mr Maslin a copy of an article yet to be published, entitled ‘BSE – The Offal Ban Fails’, which was

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985 S10 Lacey para. 21
986 YB92/7.29/3.1
987 YB93/1.26/3.1
988 YB93/3.11/8.1–8.2
989 YB93/3.12/7.1–7.3
990 YB93/3.12/7.1–7.3
991 YB93/3.12/7.3
written by Professor Lacey. The article discussed the spread of BSE, and possible maternal and horizontal transmission in cattle. It noted that BSE in zoo animals was disturbing and that experiments on other host animals led to the conclusion that ‘as with the spontaneously-infected animals, the potential host range for the BSE agent is very wide indeed’. After discussing the implications of the number of cattle with BSE to date, Professor Lacey concluded:

Thus the BSE epidemic is continuing despite the offal ban and must pose a very serious threat to many animals, including man. 992

5.58 Mr Maslin circulated Dr Dealler’s letter and Professor Lacey’s article along with a proposed reply to Dr Dealler to Mr K Taylor, Mr Lowson and Mr Bradley, amongst others, noting Dr Dealler was not ‘prepared to alter his view that we are all doomed’. 993

5.59 Mr Bradley said about the proposed reply to Dr Dealler that he had:

No comment, but frankly the gentleman has calmed down quite a lot since we first met him. This might be due in part to various contacts we have had over the years but I still rest uneasy. Is it all a timebomb waiting to go off or not? Caution is the word. If in doubt leave it out. 994

5.60 On 7 April 1993, Mr Maslin replied briefly to the letter (but not the article) based on comments from colleagues. 995 He advised that he had ‘not gone through’ Professor Lacey’s article, as it raised ‘issues that go beyond my expertise and policy concerns’, but that he imagined it would be refereed by ‘experts in the field of CJD’ before publication. 996

5.61 Mr Maslin suggested that there had been no economic benefit to removing and using brains in food, so suspected that the practice had not occurred. He also reminded Dr Dealler that considerable research into both BSE and CJD was being carried out by MAFF, the Medical Research Council and the Agriculture and Food Research Council as well as others. Mr Maslin denied the suggestion that the Southwood Report was misleading, since it recommended certain precautions to be taken in light of the fact there was a remote but potential risk to human health. With regard to the mouse experiments, Mr Maslin explained that the mice were a special laboratory strain particularly susceptible to spongiform encephalopathies, which had been used in scrapie research for many years. Given that bovine tissue was being injected into the mice intracerebrally, a route 100,000 times more effective than the oral route, Mr Maslin said that he believed that the experiments would give a good indication of the risk via the oral route to species not known to be susceptible. 997
Developments prior to the meeting between Professor Lacey, Dr Dealler and Dr Tyrrell – March to June 1993

5.62 On 25 March 1993 Professor Lacey forwarded a copy of his article, ‘BSE – The Offal Ban Fails’, to Dr Tyrrell, saying that he would be interested to have SEAC’s view. The draft began with a summary which stated:

Hopes that the offal ban in July 1988 would result in the resolution of the epidemic have not been fulfilled. It is proposed that the infective agent of BSE is primarily a cattle pathogen, perhaps initially spread by contaminated offal, but in recent years propagated chiefly by maternal (vertical) transmission with variable manifestation of the clinical disease.\(^{998}\)

5.63 In the draft article, Professor Lacey said that it may not be possible formally to prove his hypothesis but that ‘it seems to be the only explanation’.\(^{999}\)

5.64 We noted earlier that Mr Maslin circulated the draft article within MAFF. On 16 April 1993, Mr K Taylor replied to Mr Maslin saying that Professor Lacey’s paper was:

. . . a strange document, more polemic than scientific paper . . . \(^{1000}\)

5.65 MAFF sent the draft article to Dr Tyrrell and members of SEAC to discuss at their meeting on 22 April. The Committee considered Professor Lacey’s article and it was agreed that Dr Tyrrell should write to Professor Lacey ‘pointing out the errors on which his conclusions had been based’.\(^{1001}\) As agreed, Dr Tyrrell duly wrote to Professor Lacey with the views of SEAC on his article.\(^{1002}\) He said:

There are a number of errors in your article but the Committee did not feel that they should give a detailed critique of it. There are however two major factual points that need to be corrected as they are central to the arguments contained in the article.

5.66 The first suggested error was that Professor Lacey had confused the SBO ban for human and animal consumption with the ban on feeding ruminant derived protein to ruminants. The second was that a misunderstanding had led Professor Lacey to fail to appreciate that prior to the ruminant feed ban for ruminants, young animals were the major recipients of this feed.

5.67 Dr Tyrrell invited Professor Lacey to attend a meeting to discuss the issues he had raised on BSE.

5.68 Professor Lacey replied to Dr Tyrrell on 11 May 1993.\(^{1003}\) He thanked Dr Tyrrell for his letter and said that he had made appropriate revisions to his paper. He agreed to a meeting with Dr Tyrrell and Mr Bradley and restated his concerns regarding the epidemiology of BSE.
I am particularly concerned about the epidemiology of BSE and there seem to be two totally incompatible phenomena. MAFF’s proposal that the high number of cases in the last few years is due to re-cycling of bovine infectious agents which would suggest an extraordinarily high potency of infectivity for the oral route would seem to be incompatible with the claim made by Kimberlin, Ridley and members of your committee that these agents have very low infectivity by the oral route. Something is seriously wrong.1004

5.69 During May 1993, Mr Bradley spoke with Professor Lacey over the telephone on two occasions (10 and 13 May) and gave him some information on BSE statistics and experiments.1005 On the second occasion, Professor Lacey said he would welcome a visit and discussion with Dr Tyrrell and Mr Bradley and gave his available dates. Mr Bradley sent these to the SEAC secretariat so that they could arrange the meeting.

5.70 On 17 May 1993, Mr Meldrum wrote to Mr Taylor asking for a ‘list of the faulty arguments being deployed by Professor Lacey together with a brief rebuttal of each one in the form of a speaking note’.1006

5.71 On 18 May 1993, Mr Wilesmith sent a minute to Mr Bradley with comments on a revised draft of Professor Lacey’s article, now re-titled ‘BSE - The Gathering Crisis’. He noted that he had prepared the critique prior to the CVO’s request, but hoped that it would be useful nevertheless.

5.72 In the note, Mr Wilesmith referred to Professor Lacey’s article as ‘...Professor Lacey’s latest exposure of his ignorance on this topic’. He also said

...I feel that we have two basic options for dealing with this tendentious individual who has failed to apply any scientific scholarship to his so-called critique of BSE. The first is to ignore him and hope that he will keep digging his own grave. The second is to launch an all out attack at every opportunity particularly with respect to his published musings.1007

5.73 Mr Wilesmith said that he would ‘tend towards the second option’. He also mentioned the proposed meeting with Professor Lacey and suggested he might attend ‘otherwise, my fear is that the arguments may persist as he clearly feels that I am, personally, cooking the books’.1008

5.74 On 17 June 1993, Mr John Howard of MAFF’s Animal Health Division informed the Private Secretary to the Parliamentary Secretary, Mr Soames, that Dr Tyrrell would be meeting Professor Lacey on 22 June. The note said that Mr Bradley, Mr Wilesmith and Dr Dealler would also attend.1009 The note referred to the detailed critique of ‘BSE – The Gathering Crisis’ prepared by Mr Wilesmith, which Dr Tyrrell would use at the meeting.1010
5.75 The critique gave a detailed discussion of each paragraph, emphasising suggested inaccuracies and misunderstandings of information and epidemiological studies.

5.76 Professor Lacey’s article was published in the *British Food Journal* in July 1993.1011

**The meeting at Stoneleigh, 22 June 1993**

5.77 On 22 June 1993, Dr Dealler and Professor Lacey met Dr Tyrrell, Mr Wilesmith and Mr Bradley in the MAFF pavilion at Stoneleigh National Agricultural Showground.1012

5.78 Mr Bradley prepared a note of the meeting,1013 in which he said in summary that:

Professor Lacey’s views and the main points of controversy concerned:

i. His claimed ineffectiveness of the mouse model.

ii. The potential importance of cumulative exposure to BSE agent.

iii. The need to include offals of calves <6 months and bones of all cattle in the SBO ban with immediate effect.

iv. Maternal and horizontal transmission were important.

v. The recent data on age-specific incidence was not believed.

vi. [Bovine] Eyes were constituents of MRM though data was not quantifiable.

vii. Human exposure was still high from calf offals, bones and bone products.

viii. CJD should be made notifiable now or not at all.1014

5.79 The note of the meeting was not distributed at the time either to Dr Dealler or to Professor Lacey, who told us ‘. . . despite initiating this meeting I had not been informed of the existence of the minutes nor had I seen a copy until 26 February 1998’.1015

5.80 Mr Wilesmith explained that animals affected with BSE and born after the feed ban (BABs) were due to leftover feed. Professor Lacey said that there were too many BABs for that to be valid. He also made it clear that the material rots and that fungal spores would not allow the feed to be saved for any length of time. Dr Dealler asked for statistics ‘showing the age distribution of cattle with BSE and the age distribution of cattle in the bovine community’. These were provided.1016

5.81 Mr Bradley’s note of the meeting records that ‘. . . neither Professor Lacey nor Dr Dealler accepted the validity of the mouse model for establishing the safety of

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1011 S10 Lacey para. 23; R. Lacey, BSE – The Gathering Crisis, *British Food Journal*, 1993, 95 (4), 17–21
1012 S22 Dealler para. 69; S10 Lacey para. 22
1013 YB93/6.22/2.1–2.4
1014 YB93/6.22/2.4
1015 S10 Lacey para. 22
1016 S22 Dealler paras. 71–3
tissues for human consumption . . . Professor Lacey and Dr Dealler said that a mouse inoculation test could not give exact figures for infectivity unless the sensitivity of the mouse test was known. This was not accepted by Mr Bradley, who (according to Dr Dealler) said: ‘We should accept the mouse inoculation test as being the test that was available and that it was the right answer.’

5.82 Dr Dealler told us that ‘Professor Lacey decided that this was such bad science that it was totally unacceptable’. He added that Professor Lacey was determined that:

. . . inadequate research was being carried out to find out the level of infectivity in the tissues and until this was available we must not make statements indicating that beef was safe.

5.83 Professor Lacey also criticised Professor R.M Barlow’s recent paper in the *Veterinary Record*. He claimed that ‘the deductions of safety of certain tissues for consumption by man were misleading’, and that ‘in negative experiments mice could not live long enough to accommodate the incubation period’.

5.84 Professor Lacey said that since infectivity had been found in meat of hamsters, goats and mink, it should be assumed that infectivity was present in beef at a low level and it should be calculated whether the quantity eaten by humans represented a risk. Mr Bradley did not accept this and stated that if there was infectivity when inoculated into mice then ‘we should worry about it’.

5.85 Maternal transmission and evidence to support MAFF’s position were discussed. Mr Bradley’s note recorded that:

Professor Lacey stated his belief (without evidence) that maternal and horizontal transmission was important especially later in the epidemic. He dismissed the evidence that placenta was not detectably infected and thought this was the origin of both the types of transmission he favoured.

5.86 Because of this view, Professor Lacey considered that the specified offals from calves under 6 months old should be included in the ban. Professor Lacey and Dr Dealler also considered that there was a risk from bovine bones and gelatine and it was their belief that ‘bones of cattle should form part of the SBO ban and be instituted immediately’.

5.87 Dr Dealler spoke to Mr Wilesmith about the analysis of statistics. Dr Dealler said that he was surprised that the CVL had not done retrospective or adequate prospective analyses to calculate the number of infected cattle that were being eaten. They had produced predictive figures for the number of cases of BSE for the Treasury, but had not carried out the analysis which Dr Dealler had undertaken.

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1017 YB93/6.22/2.1
1018 S22 Dealler para. 74
1019 S22 Dealler para. 74. Volume 2: Science deals with the experiments which were in fact carried out to assess the sensitivity of the mouse bioassay compared with the calf bioassay. These began in January 1993 and some initial results were available around January 1995
1020 Middleton D J and Barlow R M Failure to Transmit Bovine Spongiform Encephalopathy to Mice by Feeding them with Extraneural Tissues of Affected Cattle, *Veterinary Record*, 1993, 132, 545–547
1021 YB93/6.22/2.1
1022 S22 Dealler para. 75
1023 YB93/6.22/2.2
1024 S22 Lacey para. 77
5.88 Dr Dealler gave Dr Tyrrell a copy of his paper, ‘Current and future dimensions of the epidemic of bovine spongiform encephalopathy, and assessment of its potential effect on the human population’. The paper said that scrapie could not be used as a guide to the infectivity of bovine tissues to humans. It concluded that the dose of infectivity for BSE in humans should not be assumed to be high. Dr Tyrrell passed the paper to Mr Bradley, who said that a response would come from SEAC soon.

5.89 Mr Bradley’s minute noted in conclusion:

Professor Lacey and Dr Dealler were satisfied with the data given to them and certain aspects of their understanding were undoubtedly improved. However unsound firmly held views were unshakeable.

He felt that to remedy this position they would need the results of:

i. the case control study;
ii. the comparative assays in mice and cattle;
iii. the placenta transmission in cattle;
iv. the pathogenesis study;
v. the attack rate study;
vi. more firm evidence of a continuing downturn in the epidemic.

5.90 Professor Lacey and Dr Dealler, for their part, left the meeting with the feeling that they had not been given a fair hearing. Dr Dealler told the Inquiry that:

It seemed initially that Professor Lacey and I were being treated as people who simply did not understand the subject and we had to be taught the truth so that we did understand. It was as if anything that we put forward was treated as invalid if it did not agree with their point of view whereas much of what they were saying the other way was accepted.

Dr Dealler added:

The impression that I got from the meeting was that Dr Tyrrell was accepting MAFF’s position on BSE as fact and as such it would be very difficult to get information to be assessed independently in the UK.

5.91 In oral evidence, Dr Tyrrell agreed that at the meeting that ‘he considered the points being put forward by Lacey and Dealler and that no other body pre-determined [the] role’. He also confirmed that:

It was sad that no more fruitful relationship grew out of this contact, since some of Professor Lacey’s ideas, such as the possible infectivity of bones have proved to be correct.
5.92 He added in his oral evidence that it ‘was something which he, like many of us, made a guess about and turned out to be right’.1031

5.93 Mr Bradley sent a minute to Mr K Taylor, Mr Eddy and Mr Wilesmith about the internal record of the meeting. He noted that:

Dr Tyrrell wished to send a copy to Professor Lacey as he considered the report may be a step towards resolving some of the irrational views Professor Lacey holds.1032

Mr Bradley said he had discussed this with Mr K Taylor and both had agreed that this was not the purpose of the minute, and it should remain an internal record of what took place.

5.94 Mr Bradley commented in hindsight about the meeting that:

There were several points at issue between us, but what we considered unsound views were vigorously upheld by Professor Lacey and Dr Dealler. I disagree that there was no scientific dialogue. I concede that the dialogue could have been better . . . particularly on some points like maternal transmission, but the entrenched views held permitted little helpful discussion. Perhaps a subsequent meeting after digestion of the first would have enabled a better dialogue to develop.1033

5.95 Mr Wilesmith discussed the meeting in a statement to the Inquiry and denied he played a ‘pre-determined role’ at the meeting.

As is evident from the record of the meeting, Professor Lacey and Dr Dealler did not suggest any additional epidemiological research within my domain and responsibility. In my view the record of the meeting, together with what is evident from my scientific publications would suggest that I (and my colleagues involved in research) kept an open mind in terms of the scientific hypotheses which I have investigated related to understanding the epidemiology of BSE . . . It is certainly not correct to say that I had, or have, a predetermined role; nor a closed mind. To the best of my recollection Professor Lacey did not make any practical suggestions as to further epidemiological research that should be undertaken at CVL.1034

Newspaper coverage of the Stoneleigh meeting

5.96 On 3 July 1993, an interview with Professor Lacey appeared in the Guardian, where he discussed the Stoneleigh meeting.1035 He said that experiments indicated that BSE could be contracted from the liver and bone marrow of affected animals, and that since these products were used so much more frequently than brain and spinal cord, there was a significant risk to public health. Mr K Taylor was reported as dismissing Professor Lacey’s arguments, saying that the high-risk tissues had been identified and removed from the human food chain. He was reported as saying,
‘We believe there is no cause for public concern as the dose of the infective agent is what is important’.

5.97 On 8 July 1993, Dr Tyrrell wrote to Mr Bradley about the Guardian article. In relation to the meeting with Professor Lacey and Dr Dealler, Dr Tyrrell said about Professor Lacey:

On the whole I regard this as an experiment to give an opportunity to a senior academic to return to a normal type of scientific interchange. It has failed, but we did express genuine goodwill and reasonableness and were unambiguously rebuffed. I don’t think we should waste any more time or energy on the matter. I certainly don’t wish to bother with any more letters. We may well have to expect more substandard behaviour in the future, and I suggest we just ignore it all.

5.98 In July 1993 Professor Lacey published ‘BSE: The Gathering Crisis’ in the British Food Journal.

Further correspondence with Dr Dealler after the Stoneleigh meeting

5.99 On 30 June 1993, shortly after the Stoneleigh meeting with Professor Lacey and Dr Dealler, Mr Bradley minuted Mr Eddy of the Animal Health (Disease Control) Division. He advised that Dr Tyrrell had read Dr Dealler’s draft paper and had asked for it to be reviewed by SEAC at the next meeting in October. He reported that Dr Tyrrell considered the paper interesting even if parts were ‘quite wrong’. He also commented that although Dr Dealler had agreed at the Stoneleigh meeting that the paper could be circulated to other members of SEAC, Dr Tyrrell wanted his agreement in writing and had asked Mr Bradley to deal with it. Dr Dealler told us that Mr Bradley sent him numerous requests for permission to distribute the document to SEAC and he sent repeated agreements that this should go ahead.

5.100 On 9 July 1993, Dr Dealler wrote to Dr Tyrrell following up the Stoneleigh meeting. He was concerned that ‘a lot of pressure may be put on Dr Wilesmith and Mr Bradley to try to rubbish the data that I have given you’. He also said that a number of scientists in the field were being told how to carry out experiments by ‘poorly qualified MAFF personnel’.

5.101 Dr Tyrrell responded on 19 July 1993. He asked that Dr Dealler send written permission for the circulation of his paper to SEAC as soon as possible. He also asked for specific examples of Dr Dealler’s criticisms which could be discussed at the next SEAC meeting.

5.102 On 23 July 1993, Mr Bradley wrote to Dr Dealler asking for written permission to distribute the paper. On 18 August 1993, Dr Dealler gave written permission for his article to be circulated to SEAC. He also requested that he be
present at the meeting when SEAC discussed his paper.1043 Mr Bradley sent a minute to Mr Howard on 1 September 1993 recommending that Dr Dealler should not attend the SEAC meeting.1044 Mr Howard agreed and wrote accordingly to Mr Eddy.1045 Dr Dealler’s paper was given to SEAC shortly before the meeting on 7 October 1993.

5.103 In August 1993, Dr Dealler published a method for calculating the number of cattle slaughtered for food while incubating the disease.1046 The underlying figures with which he worked had been obtained from Mr Wilesmith in 1992.1047 He claimed that approximately seven cattle with BSE infection were eaten for every one that died with symptoms. Dr Dealler told the Inquiry that the method was repeated more accurately by Professor John Kent, Professor of Statistics at Leeds University, and himself in 1995 (see paragraph 5.156).1048

SEAC discusses Dr Dealler’s draft paper, October 1993

5.104 On 7 October 1993, SEAC discussed the draft paper which Dr Dealler had given to Dr Tyrrell at the Stoneleigh meeting.1049 Dr Dealler was not asked to attend the SEAC meeting, although he had requested he attend to answer any criticisms of his paper. SEAC considered that the paper needed clarification regarding the assumptions on which estimates of infectivity were made. In addition, the paper was thought to contain too many assumptions acting together, that Dr Dealler had attempted absolute answers on non-absolute data, and had not related his assumptions to scrapie.1050

5.105 Dr Tyrrell wrote to Dr Dealler on 26 October 1993 and advised him that SEAC did not accept the validity of the paper. Dr Tyrrell explained that SEAC members did not accept the level of infectivity in the tissues and were unhappy with the methods used. They also did not accept the idea that infectivity was cumulative. Dr Tyrrell commented that if different experimental data had been used there would have been a ‘different conclusion about the hypothetical potential exposure of the human population to this agent’.1051

5.106 On 22 November 1993, Dr Dealler replied to Dr Tyrrell.1052 He did not accept SEAC’s points. The letter was circulated to members of SEAC on 3 December 1993.1053 The Committee added no further comments.1054

5.107 Dr Dealler’s paper was published in the British Food Journal in November 1993.1055 Professor Lacey had edited the article.1056 On 30 October 1993, an article by James Erlichman about Dr Dealler’s paper appeared in the Guardian entitled, ‘Research claims 1 in 10 could be at risk from beef-eating’. The article stated:
Dr Stephen Dealler says in the British Food Journal that very low doses of the infectious BSE agent in meat, liver, and kidney can become cumulatively fatal because we eat so much beef and live so long.

Many BSE-infected cattle will have been eaten because they were incubating the disease before slaughter but had not yet shown symptoms.

Kevin Taylor, the Government’s assistant chief veterinarian said: ‘Dr Dealler has been perfectly fair in setting out his assumptions but we do not agree with his conclusions.’

Dr Dealler rejects the government argument that sheep have had their form of the disease for centuries without passing it on. He says: ‘We eat more than twice as much beef as lamb, and BSE is 100 times more common in cattle’.

Mr Taylor said that meat from BSE-infected cows had been injected into the brains of mice, but tests completed last month showed no signs that mice had caught the disease. ‘We therefore remain confident that beef is perfectly safe to eat,’ he said.

[Dr Dealler] argues that, if man is susceptible to BSE, then at least 8 million adults are likely to have eaten enough to get Creutzfeldt-Jakob Disease (the human form) unless they die first from other causes. Mr Taylor said the assumption about cumulative consumption was highly speculative and not accepted.

5.108 On 3 November 1993, Mr Soames gave a Written Answer to a Parliamentary Question from Mr Martin Redmond MP about Dr Dealler’s paper. The reply stated that Dr Dealler’s paper had been considered at the SEAC meeting on 7 October and as a result, Dr Tyrrell had written to Dr Dealler:

That letter drew attention to the fact that Dr Dealler had drawn his conclusions on the basis of assumptions made about the BSE infectivity of various tissues by extrapolation from the disease scrapie in sheep. Using these assumptions he had gone on to calculate the potential numbers of people who had been exposed to various levels of BSE agent. The letter from Dr Tyrrell informed Dr Dealler that recent experiments conducted with tissues from BSE infected cattle have shown that only the brain and spinal cord have a detectable level of infectivity and that the range of other tissues considered by Dr Dealler including meat, liver, kidney and lung do not have any detectable infectivity. The implication of using these actual results, rather than extrapolating from sheep disease, is that there is no evidence of any significant risk from the consumption of tissues other than brain and spinal cord from adult cattle. Brain and spinal cord are among the specified bovine offals whose use is banned under the BSE Order 1991 and which do not therefore enter the human or animal food chain. The SEAC has not advised Government to amend their safeguards as a result of its consideration of Dr Dealler’s article.
5.109 On 9 November 1993, Mr Eddy wrote to Lord Howe, Parliamentary Secretary at MAFF, in response to his request for advice following Mr Erlichman’s article on Dr Dealler’s paper.\textsuperscript{1059} The note said Dr Dealler’s paper had been considered by SEAC before publication and that SEAC had not advised the Government to amend its safeguards as a result of the paper.

**Further input from Professor Lacey, Autumn 1993**

5.110 In autumn 1993, Professor Lacey became concerned about Government figures on the epidemic. He told us:

In the autumn of 1993 I became concerned that the Government was massaging the figures for BSE cases by back-dating deaths to earlier years. The *Lancet* on 25 September 1993 said that 51,875 cattle died from BSE between 1988 and 1991 but on 26 November 1992 a House of Commons answer to a question from Mr Hinchcliffe said 48,526 died in that period. I alerted James Erlichman to the discrepancy who published a story about it in the *Guardian* on 2 October 1993. He quoted me correctly as saying that presumably some recent cases of BSE had been added to the previous years to falsify the epidemic. I said that we needed an independent inquiry into the true state of the epidemic. According to Mr Erlichman, the Ministry accepted that there was a significant discrepancy, but the figures reported in the *Lancet* related to the date at which farmers first reported symptoms, while the deaths in the Commons answer were logged by the dates when veterinary surgeons ordered animals to be slaughtered.\textsuperscript{1060}

5.111 On 25 October, Mr Bradley minuted Mr Eddy, Mr Dixon and Mr Taylor about Professor Lacey and his interest in the safety of gelatine, liver and kidney from cattle.\textsuperscript{1061} Mr Bradley concluded by saying:

\ldots it would be helpful to be prepared for his next bombshell if it materialises. Knowing the enemy is the first step in developing a sound defence.

**Dr Dealler establishes the Spongiform Encephalopathy Research Council**

5.112 Dr Dealler told the Inquiry of his concerns that there was insufficient research on BSE. Further, he was concerned that ‘many ideas were just accepted as being valid and not tested (eg, tallow not being infective) even when there was little evidence to believe them’.\textsuperscript{1062} In 1993, as a result of these concerns and ‘after the failure to publish information showing that humans may have been exposed to sufficient infectivity to transmit the disease from cattle’, he decided to set up the Spongiform Encephalopathy Research Council (SERC), a registered charity.\textsuperscript{1063} The Trust Board was to include Dr Ann Maddocks, a retired medical microbiologist, as Chair and Professor Roy Postlethwaite, a retired virology professor, and Dr Martin Schweiger, a consultant physician in public health.\textsuperscript{1064}
On 21 January 1994, Dr Dealler wrote to Mr Michael Jack, MAFF Minister of State, about his intentions to establish SERC and to seek help. He said:

Research into the effect of BSE on the human population has been depressingly lacking. Now that it has become clear that the human risk from BSE may well have been unacceptably high for the past 7 or 8 years (see enclosed information sheet) this must change . . .

Could we ask you for some help? This would mainly be in bringing to view the realisation that BSE is becoming a medical rather than an agricultural issue and that research carried out so far gives little indication of the risks that are being taken by the human population of the country.1065

Dr Dealler told us that soon after the establishment of SERC, MAFF was coming under increasing pressure to expand its BSE research programme:

By the time that the SERC was registered as a charity, information was reaching the press in such a way as to put pressure on MAFF to increase BSE research. As a result SERC has not been a useful organisation except as an indicator to official figures that inadequate research is being carried out by official bodies.1066

Dr Dealler examines the death of a suspect BSE case

During mid to late November 1993, Dr Dealler became involved in the diagnosis of a suspected case of BSE in a cow that was the offspring of a confirmed BSE dam. On 22 November 1993, Mr K Taylor minuted Mr Soames about this case, warning him that Dr Dealler might be using the case to seek evidence with which to embarrass the Government:

[Dr Dealler] recently asked staff at the Central Veterinary Laboratory to carry out histopathological examination of brain tissue which he had obtained from a cow which had been reported by the owner as a suspect case, but which was not considered by the veterinary officers who carried out the diagnostic visits to be affected with BSE. They acted in close cooperation with the owner’s veterinary surgeon, and the decision to remove restrictions from the suspect was based on clinical observation over 4 days, an alternative diagnosis supported by biochemical evidence, the cow’s response to treatment, and the conclusion that the cause of the clinical signs was not BSE.

The owner, who farms in the York area, was not pleased by the decision . . . he refused an offer to have the cow destroyed voluntarily whilst under BSE restrictions (in which case some ex-gratia payment would have been made, the carcass would have been incinerated and the brain examined by MAFF) and instead arranged for the animal to be destroyed by his own veterinary surgeon, after restrictions had been removed, and the head made available to Dr Dealler.1067
5.116 In the event, BSE was confirmed in the suspect cow following diagnosis by a local veterinary laboratory in the York area, and by Dr Tony Palmer, a retired neurologist/neuropathologist at the University of Cambridge Veterinary School, and later by Mr Wells of CVL. On 10 December 1993, Mr Eddy updated Mr Soames about these developments, attaching defensive briefing for MAFF Press Office. In his covering minute Mr Eddy said:

. . . This [diagnosis] is unfortunate but we are satisfied that all the veterinary staff acted correctly, and that their judgement was justified by the alternative diagnosis which was supported by laboratory evidence, and the response of the suspect animal to treatment.

. . .

Dr Dealler will no doubt claim that the downturn in BSE cases reflects systematic attempts by the Department to hide the scale of the BSE epidemic by refusing to slaughter every BSE suspect which is notified. This is of course not true. In any case the headline figures which show the 12% decline in BSE are the figures for suspects put under restriction and this cow would have been included in that figure . . .

5.117 On 16 December 1993, Dr Matthews minuted Mr K Taylor to report, amongst other things, that Mr Wells had been given, in confidence, a draft letter from Professor Lacey and Dr Dealler to the *Veterinary Record* about this BSE case. The draft letter suggested that the case indicated the occurrence of vertical transmission of BSE. Dr Matthews commented: ‘. . . the letter is so full of errors that I feel it would be best to do nothing until published, and then to respond fully in the *Veterinary Record*.’

5.118 On 17 December 1993, Mr Wilesmith sent Dr Matthews his comments on the draft letter and included a draft reply calling into question the validity of Professor Lacey’s and Dr Dealler’s conclusions.

5.119 On 1 February 1994, Mr Robertson of the Animal Health and Veterinary Group minuted Mr Meldrum, enclosing a copy of the letter to the *Veterinary Record* on this BSE case from Dr Dealler and Professor Lacey. The letter had been sent to MAFF by the editor on a confidential basis. Mr Robertson noted that the letter was due to be published on 5 February and a response was being prepared with a view to publication in the issue of 12 February.

5.120 Dr Dealler’s and Professor Lacey’s letter, ‘Suspected Vertical Transmission of BSE’, was published in the *Veterinary Record* on 5 February 1994. The letter reported that BSE had been confirmed in a female Friesian-Holstein calf born on 4 July, 1989, nearly a year after the ruminant feed ban and that its dam had been one of the eight confirmed BSE cases on the farm in question. Professor Lacey and Dr Dealler described the initial confusion surrounding diagnosis, and went on to suggest that it was possible:
that young and vertically transmitted BSE cases show less prominent signs of disease in the standard histology; perhaps this is accentuated because farmers are becoming more experienced at recognising the early disease. We believe that BSE is much commoner than formally reported among animals under five years of age, and that the endemic is being maintained, at least in part, by vertical transmission.  

5.121 Dr Dealler told us that he was telephoned at home by the local MAFF veterinary officer after the publication of the letter in the *Veterinary Record* and that MAFF demanded the brain of the animal. Dr Dealler considered that this may have been to make sure that he did not carry out further research.  

5.122 On 19 February, the *Veterinary Record* published a response from Mr Wilesmith, Mr Wells, Miss Linda Hoinville and Mr Simmons. This set out to present the scientific perspective of the work that was being carried out on maternal transmission and the laboratory diagnosis of BSE. On the possibility of maternal transmission, Mr Wilesmith and his co-authors commented that for various reasons:  

. . . it has not been possible to assess the risks for individual cases of acquiring infection from sources other than feed. We therefore cannot support the suggestion made by the authors that the case described inevitably was the result of vertical transmission, although that explanation is certainly one possibility.  

An analytical epidemiological approach is required to estimate the risks of infection in this subset of animals from the three potential sources: feedstuffs, their dams and as a result of horizontal infection. To this end a case-control study of animals born after October 30, 1988, has been initiated at the earliest opportunity.  

5.123 On the laboratory diagnosis of BSE, Mr Wilesmith and his co-authors described the research in progress and commented:  

. . . there is no correlation between short clinical duration and mild or equivocal histological changes since there is wide variation in the severity of lesions irrespective of recorded durations of signs. There is therefore no evidence for the claim made by the authors that young, or any other subpopulations of BSE cases, have, preferentially, less prominent lesions. Indeed, the case reported was diagnosed using the routine method and the findings included severe spongiform change.  

In summary, we would urge caution in attributing specific sources of infection for individual cases of BSE and stress the remarkable consistency of the lesion pattern which has maintained the validity of this diagnosis throughout the course of the epidemic.

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1073 S F Dealler and R W. Lacey, *Veterinary Record*, 1994, 134, 151  
1074 S22 Dealler paras 118–20  
1077 J W Wilesmith, G A H Wells, L J Hoinville and M M Simmons, *Veterinary Record*, 1994, 134, 199
Correspondence between Dr Dealler and Mr Soames, early 1994

5.124 On 31 December 1993, Dr Dealler wrote to Mr Soames about his Parliamentary Answer of 3 November 1993 to Mr Redmond (see paragraph 5.108). He complained that it was misleading in the references it made to Dr Tyrrell’s letter to him concerning SEAC’s consideration of his paper. Mr Soames replied on 19 January 1994. He disagreed that the Parliamentary Answer was misleading and said:

Your letter clearly indicates that you disagree with Dr Tyrrell’s criticism of your paper but that is not of course the same thing as saying that the description of Dr Tyrrell’s letter in the Parliamentary Reply was incorrect.

5.125 On 21 January 1994, Dr Dealler replied to Mr Soames’ letter of 19 January. In it he restated his concern that there may be a more than one in two chance that millions of people had consumed fatal doses of BSE infected material. He also offered Mr Soames some advice, including:

Specific advice should be sought from a committee that is not organised by the MAFF but rather by the Dept of Health. People on this committee should not depend on MAFF for the income of their research groups or for goodwill.

Decisions should be made not to eat any further cattle from British herds that is over 1.5 years old, or another short figure. This should continue until minimal BSE is reported in the country . . .

Research MUST start in looking for rapid methods of diagnosis, methods of treatment, and epidemiological experiments to be further sure of the risks that have been taken already. This must not be funded by the MAFF or AFRC but by MRC. This is purely because many people on the AFRC committee are closely associated with MAFF . . .

Older cattle, when slaughtered, should be incinerated.

5.126 On 27 January 1994, Mr Soames held a meeting with Mr Capstick, Mr Meldrum, Mr Haddon and Mr Crawford to discuss this correspondence with Dr Dealler. The meeting agreed that the letter to Mr Soames was essentially seeking funding for research and should be given a ‘sensible response’. The response should include a statement on what research MAFF were carrying out and enquire what research was proposed. It was agreed that Mr Taylor should reply ‘as scientist to scientist’.

Dr Dealler complains about SEAC’s comments on his paper

5.127 On 11 February 1994, Dr Dealler wrote to Dr Tyrrell about SEAC’s comments on his paper (published in November 1993 in the British Food Journal – see above). He said that he intended to take legal advice as he felt his actions were

1078 YB93/12.31/1.1
1079 YB94/1.19/8.1
1080 YB94/1.19/8.2
1081 YB94/1.21/8.1
1082 YB94/1.28/9.1
‘wasted because of the way in which SEAC is responding to medical scientific advice’.

In his statement to the Inquiry, Dr Dealler says that in 1994 he realised that SEAC’s response was ‘by rights unethical’. He felt that they had responded saying they did not believe him, and that they should have provided evidence that he was wrong. He spoke with the Bishop of Wakefield on 11 October 1993, who suggested that ethics could be a way to press discussion of BSE risks to other parts of the Government.

5.128 Dr Dealler then approached Professor Harris and Professor Brazier, a Professor of Law, from the Centre for Social Ethics and Policy at the University of Manchester. They concluded that nothing could be done through ethical pressure or through legal action because the Government need not take any notice of it. He wrote to the major members of the UK Forum of Medical Ethics and the Institute of Medical Ethics in the UK. He published an article on this in the Bulletin of Medical Ethics. He wrote to the British Medical Association (BMA) in 1994. They replied that it was not their responsibility. He also approached the Nuffield Council of Bioethics in July 1994, and published an article in Nursing Ethics.

MAFF responds to Dr Dealler’s concerns

5.129 On 22 February 1994, Mr Eddy wrote to Dr Dealler at Dr Tyrrell’s request. He pointed out that Dr Tyrrell did not accept Dr Dealler’s findings and that the problems in the paper had been explained in Dr Tyrrell’s letter of 26 October 1993. He attached a list of project titles to give Dr Dealler an idea of the work already receiving MAFF support.

5.130 On 25 February 1994, Mr Lister (DH) wrote to Mr Eddy following telephone conversations with Dr Dealler. He suggested that Dr Dealler should be invited to discuss his paper at a full meeting of SEAC because:

. . . unless he is given this opportunity it will always be easy for him to claim that he has been fobbed off.

5.131 On 1 March 1994, Mr Bradley wrote to Mr Eddy recommending that Dr Dealler should not be invited to attend a meeting of SEAC and setting out the reasons why. There was a lengthy list of reasons including:

. . . to ask Dr Dealler to come at the end of the day so that there is a natural end to the discussion is a hostage to fortune. Anyone who knows Dr Dealler realises he will go on and on and on.

5.132 He also noted that

. . . if things backfired the SEAC could be brought into disrepute: Of course I could be wrong and they might like to clear the matter up once and for all. That is to my mind wishful thinking and the risks to the SEAC as a whole if they fail to deliver are not inconsiderable.

1083 YB94/2.11/4.2
1084 S22 Dealler para. 126
1085 S22 Dealler paras 126–33
1086 YB94/2.22/6.1
1087 YB94/2.25/2.1
1088 YB94/3.01/6.1
5.133 Mr Bradley said he would be happy to meet Dr Dealler personally to discuss his paper.

5.134 On 15 April 1994, Dr Dealler visited CVL and spoke with Mr Bradley. Most of the time was spent discussing the latest epidemiological and transmission data. They discussed maternal transmission where Dr Dealler claimed that the decline of the epidemic was due to the feed ban, ‘but this only operated by reducing new cattle infections and thus reduced maternal transmission.’ Dr Dealler also claimed that ‘maternal transmission for the most part occurs only in one generation ie that exposed via feed’. Dr Dealler expressed his intention to collect tonsils to carry out research into TSE infectivity. In one instance he admitted, when presented with data by Mr Bradley, that he had been mistaken in his interpretation of the information.

5.135 Mr Bradley asked what measures Dr Dealler would immediately take if he were Minister of Agriculture. He replied that he would immediately ban the human consumption of all liver and kidney from cattle over six months, and that he would ban all tissues from animals over 2½ years old from going for human consumption.

5.136 On 21 April 1994, Dr Dealler wrote to Mr Wilesmith asking for some assistance with the mathematics of trying to predict the cases of BSE. Mr Wilesmith replied on 4 May 1994. He concluded by saying that Miss Hoinville would not be able to provide Dr Dealler with any data as the time required to provide them was not justifiable.

Dr Dealler’s consideration of exposure to BSE by various occupations and through blood products

5.137 Dr Dealler told the Inquiry that in 1993 or 1994 he contacted Mr Peter Lister of the HSE asking about the occupational risk of BSE. Mr Lister suggested that he contact the Advisory Committee on Dangerous Pathogens (ACDP) which had been asked to produce advice on the handling of TSE-infected tissue. Dr Dealler told us that he spoke to the DH official responsible for this area about five times, but eventually she did not answer his calls.

5.138 When the ACDP advice ‘Transmissible Spongiform Encephalopathy Agents: Safe Working and the Prevention of Infection’ was published in 1994, Dr Dealler told us that he thought it should have had a dramatic effect as it indicated that ‘we should assume BSE to be infective to humans until there was evidence that it was not’. However, he was concerned that the advice was inadequate for the following reasons:

i. The ACDP advice did not state to which places of work their document applied – it should apply to all places of work (except abattoirs); and

ii. The advice stated that specific tissues (which were eaten by humans at the time) should be handled using gloves and in cabinets; and
iii. The advice did not seem to acknowledge that a proportion of the beef tissues being eaten at the time was from infected cattle and the number that would be late in the incubation period could be calculated, but that it was impossible to state which cow was infected.1095

5.139 Dr Dealler discussed the ACDP’s advice with Mr Peter Lister. He said that ‘clearly . . . a worker could be working with an infective tissue in a cabinet with gloves on and then go home and eat the tissue’.1096

5.140 In 1994 Dr Dealler wrote to the chairman of the ACDP explaining the problems. He did not receive a reply. He wrote to and rang DH, but once again he did not receive a reply.1097 In April 1995, he wrote an article on this subject for Medical Laboratory World – ‘Has BSE spread to Whitehall?’1098 It appeared to Dr Dealler that the ACDP were taking action that was practical for the industry to take up but could go no further.

5.141 In 1994, Dr Dealler also wrote a paper on the risk to humans of BSE through blood transfusion, calculating that a maximum of 60,000 people per year could be exposed to infectivity.1099 The publication was rejected by an American journal.1100 It was subsequently published, after amendment, in Transfusion Medicine in September 1996.1101

5.142 During the period between writing this paper and it being published, Dr Dealler gave a number of lectures and contacted a number of organisations with his theories about the risk of infection from blood transfusion. He published information as a poster at the Infection Society in London in 1995 and was approached by a number of groups, including a woman from DH who had been sent to see the poster. Dr Dealler was going to approach DH after the publication in Transfusion Medicine. We understand that he is now providing information to SEAC and DH, and has been involved in the risk analysis procedures of blood transfusion.1102

Dr Dealler and Professor Lacey keep up the pressure, mid to late 1994

5.143 On 21 July 1994, Dr Dealler wrote to the new Minister of Agriculture, Mr William Waldegrave, asking for a meeting to discuss BSE. Mr Eddy advised that this meeting should not take place and drafted a response to Dr Dealler stating that MAFF and DH received independent scientific advice from SEAC and that they would continue to rely on such advice.1103

5.144 On 19 September 1994, Mr Dale Campbell-Savours MP wrote to the new Parliamentary Secretary, Mrs Angela Browning MP, on behalf of Dr Dealler. Dr Dealler had expressed concerns about the export of beef from the UK. Mrs Browning replied on 19 October 1994 expressing surprise at some of Dr Dealler’s remarks on the application of the new EU rules on beef exports.1104

1095 S22 para. 106
1096 S22 para. 107
1097 S22 Dealler para. 109
1098 S22 Dealler para. 110
1099 S22 Dealler para. 90
1100 S22 Dealler para. 142
1102 S22 Dealler paras 87–94
1103 YB94/7.277.1–7.3
1104 YB94/10.184.1–4.2
On 29 September 1994, Mr Waldegrave became concerned about the book that Professor Lacey was due to publish (Mad Cow Disease: The History of BSE in Britain). Mr Waldegrave asked for views from officials on Professor Lacey, and in particular an assessment of where he had been shown to be wrong in his scientific judgements in the past. This briefing seems to have been provided by Mr Eddy in December (see below).

In October 1994, Professor Lacey and Dr Dealler published an article on vertical transmission of prions in Human Reproduction. In the same issue, a response was published from Dr Robert Will of the CJD Surveillance Unit and Mr Wilesmith, which claimed that vertical transmission was unlikely and that it should not be assumed for BSE. Dr Dealler said in his statement to the Inquiry that he thought these criticisms were reasonable at the time, but was concerned that there was not any discussion of the subject outside this publication.

Professor Lacey’s presentation to the Society for Microbial Technology

On 14 November 1994, Mr Taylor wrote to Mr J Jolly, the Chairman of the British Society for Microbial Technology, commenting on a presentation Professor Lacey had made to their AGM. He felt it was important to stress that Professor Lacey did not speak for many of the leading researchers and independent observers in the field, nor for the Government, its advisers, SEAC, the CVO or the CMO. Mr Taylor said that Professor Lacey’s suggestion that substantial amounts of infectious agent continued to be consumed by the human population, who might succumb to disease over the next 30 years, was completely unsubstantiated. He also emphasised that:

From August 1988 all cattle suspected to have BSE have been compulsorily slaughtered; their carcasses are incinerated so they can enter no food or feed chain.

Mr Taylor also said that infectivity had not been detected in tissues other than brain and spinal cord. Professor Lacey had, apparently, criticised the Southwood Working Party’s forecast for the total number of BSE cases for being based on a number of conjectures regarding the source of infectivity, the host range of the infective agent and the route of this agent into the human food chain. Mr Taylor said that Professor Lacey’s comments were a ‘conflation’ of animal and public health concerns and outlined the assumptions that had actually been made by the Southwood Working Party in calculating its estimates. With reference to the human health risk, Mr Taylor drew Mr Jolly’s attention to the work carried out by the CJD Surveillance Unit and reported the opinion of the CMO, Dr Kenneth Calman, that:

I continue to be satisfied that there is no scientific evidence of a link between meat eating and development of CJD and that beef and veal are safe to eat.

1105 YB94/9.29/4.1
1106 S22 Dealler para.120; R W Lacey and S F Dealler, The Transmission of Prion Disease – Vertical Transfer of Prion Disease, Human Reproduction, 1994, 9(10), 1792–1800
1107 Human Reproduction, 1994, 9, 1797
1108 S22 Dealler para. 120
1109 YB94/11.14/6.1–6.3
1110 YB94/11.14/6.2
Professor Lacey’s article in the *British Food Journal, October/November 1994*

5.149 On 17 November 1994, Mr Eddy circulated a copy of Professor Lacey’s latest article to appear in the *British Food Journal* (‘Bovine Spongiform Encephalopathy: A “Progress” Report’) and asked for comments.1111 Mr Bradley responded on 20 November 1994 saying:

This article is badly written, makes wrong deductions and exposes his own and Dr Dealler’s superficial and inaccurate knowledge of the subject area.1112

Mr Wilesmith’s views on Dr Dealler’s estimates of the scale of the BSE epidemic

5.150 In November 1994, Mr Wilesmith indicated his agreement with Dr Dealler’s estimates of the scale of the epidemic. He did so in a letter of 22 November 1994 to Dr Patterson, consultant in public health medicine in York, in which he commented on Dr Patterson’s recent paper on BSE and risk to human health.1113 He said, among other things, that he had a good estimate of the scale of the epidemic and:

On the veterinary epidemiological side this is necessary because of investigating the likelihood of maternal transmission and is an output from our modelling. I am afraid that I cannot admit this to people such as Stephen Dealler as he may use it against MAFF who naturally fund our research. Unfortunately the decision has been made by the powers that be that no quantitative estimate is made public. This is one that I continually strive to reverse, but have not yet had any success. However, all I can say is that Stephen Dealler’s ‘all time’ cumulative total is not far out.1114

5.151 He commented at the end of the letter that he considered that Dr Patterson’s paper was very balanced and:

I am certainly pleased to see a responsible action being made on the question of the human health issue of the risks of BSE. 1115

Professor Lacey’s book, *Mad Cow Disease: The History of BSE in Britain*

5.152 On 24 November 1994, Mr Hayward of MAFF’s Press Office sent a note to Ms Rimmington informing her that Professor Lacey’s book, *Mad Cow Disease: The History of BSE in Britain*, was about to be published.1116 The note said a journalist on *The Times* had been given a lengthy briefing on BSE by MAFF officials some time ago and would be using Professor Lacey’s book as the peg on which he could hang an article. In his statement to the Inquiry, Professor Lacey stated that:
Following immediate hostile comments by Ministry veterinarians in the *Times*, no book shop stocked it.\(^{1117}\)

### 5.153 On 2 December 1994, Mr Eddy sent a question and answer briefing to Mr Waldegrave in advance of the press briefing to launch Professor Lacey’s book.\(^{1118}\) MAFF had yet to obtain a copy of the book so the briefing was based ‘on the main arguments which Professor Lacey had recently been peddling in a series of scientific papers’. Mr Eddy also included an extract from the 1990 Select Committee report on BSE, and bullet points briefing as follows:

- Professor Lacey’s views on BSE are well known and are not shared by experts in the field either in the UK or abroad. In 1990 the Agriculture Select Committee considered evidence from him and others and reached the conclusion that he seemed to lose touch completely with the real world and showed a tendency to extrapolate sensational conclusions from incomplete evidence.

- Professor Lacey’s recent scientific paper was dismissed by a leading researcher as ‘a rather unappetising hash of misinformation and misjudgement’. This is not a comment from a MAFF scientist or even a UK scientist but one of the leading researchers in America.

- UK Government policy is based on independent scientific advice from a joint MAFF/Department of Health Committee chaired by Dr Tyrrell FRS. It is also endorsed by the independent scientific Committees of the European Community and by other internationally agreed guidelines. Ludicrous to think that all of these scientists world-wide are part of a massive conspiracy.\(^{1119}\)

### 5.154 Mr Eddy obtained a copy of the book on 22 December 1994 and sent Mrs Browning a minute about it the next day.\(^{1120}\) He said that the introduction (written by Mr Geoffrey Cannon of the National Food Alliance) was ‘extremely inflammatory and so extreme that it does a grave disservice to the rest of the book, which in comparison is relatively carefully worded’.\(^{1121}\) Mr Eddy was not surprised that the book had little impact with journalists as ‘it really contains nothing new’.\(^{1122}\) Mr Eddy sent Mrs Browning a more detailed critique of the book on 1 February 1995.\(^{1123}\)

### Dr Dealler prepares a paper on the epidemiology of BSE

### 5.155 On 3 June 1995, Dr Dealler wrote to Mr Wilesmith asking for more up to date figures for BSE in cattle so that he could work out the relative risk of continuing to eat bovine tissues against ceasing to do so.\(^{1124}\) He spoke to Mr Bradley in similar terms on 21 July 1995, stating that he wanted to publish a paper on the basis that he no longer believed that consumption of beef or cattle products passed for human consumption should cease.\(^{1125}\)
5.156 In 1994/5, Dr Dealler worked with Professor John Kent, Professor of Statistics at Leeds University, on a paper on the epidemiology of BSE.\textsuperscript{1126} Figures on age distributions were obtained partly from writing directly to Mr Wilesmith. Dr Dealler sent the article to Nature, but when it was not published after four months he withdrew it. The article was published in the British Food Journal in November 1995.\textsuperscript{1127}

5.157 On 21 November 1995, Mr Wilesmith wrote to Professor Kent about the paper saying he was ‘amazed that you allowed your name to be used in the authorship’, as he felt that they were working on the basis of a misconception.\textsuperscript{1128} On 8 January 1996, Professor Kent replied restating the paper’s main conclusions. He commented that he did not quite follow the points that Mr Wilesmith was making.\textsuperscript{1129} Mr Wilesmith replied on 16 January 1996 clarifying his objections to the paper.\textsuperscript{1130}

5.158 He stated that:

One aspect which Stephen Dealler does not understand is the absence of a change in the incubation period distribution over time and what I have termed the ‘packet’ theory. The latter is a simple means of saying that the increase in the prevalence of infected cattle, whose tissues could be included in meat and bone meal before the feed ban, resulted in an increase in the number of ‘packets’ of infected material and not an increase in the titre of the BSE agent within them. There is no reason therefore to suspect that the i.p. has changed throughout the major part of the epidemic which was cattle driven.

SEAC’s 22nd and 23rd meetings

5.159 At the 22nd meeting of SEAC on 23 November 1995, Professor Pattison, the new chairman of SEAC, reported that he had had two conversations with Dr Dealler. He said that Dr Dealler was mainly concerned that MAFF were not making information fully available to SEAC. Professor Pattison felt that Dr Dealler’s views were strongly held and should not be dismissed, and acknowledged that SEAC might need help in interpreting Dr Dealler’s mathematical calculations. He felt that it was essential to obtain an independent epidemiologist’s critique of Dr Dealler’s recent paper.\textsuperscript{1131}

5.160 At the next meeting of SEAC on 5 January 1996, Dr Dealler’s and Professor Kent’s paper was discussed, and it was agreed to set in motion a peer review of the paper. Dr Hueston agreed that this:

\ldots would add more weight to SEAC’s rebuttal of the paper (if that was in fact the outcome).\textsuperscript{1132}

5.161 On 20 January 1996, a letter from Dr Dealler entitled ‘UK Adults’ Risk from Eating Beef’ was printed in the Lancet.\textsuperscript{1133} Mr Taylor sent a minute to Mr Hayward
about the letter on 26 January 1996. He said that a critical review of the Dealler and Kent paper was being prepared on behalf of SEAC and that until that was completed, MAFF could not respond to the letter.

Professor Lacey’s article in the *Journal of Nutritional and Environmental Medicine*, December 1995

5.162 In December 1995, the *Journal of Nutritional and Environmental Medicine* published an article by Professor Lacey entitled, ‘Bovine Spongiform Encephalopathy – The Disputed Claims’. He considered six commonly presented claims regarding BSE and found ‘each to be untenable’.

5.163 These were that:

i. BSE was caused by sheep material infected with scrapie being fed to cattle.

ii. The total number of BSE cases would be 17,000–20,000 in total.

iii. The disease is in a dead-end host.

iv. It is not possible for BSE to enter the human food chain.

v. The risk of BSE for humans is remote.

vi. The number of BSE cases is now dropping.

Professor Lacey’s letter to the *British Medical Journal*, January 1996

5.164 On 20 January 1996, the *British Medical Journal* printed a letter from Professor Lacey entitled, ‘Bovine spongiform encephalopathy is being maintained by vertical and horizontal transmission’. Mr Wilesmith sent a response to the editor of the BMJ contradicting Professor Lacey’s conclusions and stating that not even Professor Lacey could draw conclusions from the incomplete results of the maternal transmission study. Mr Wilesmith’s response was published on 30 March 1996.

Discussion

5.165 The evidence that we have summarised is not a comprehensive account of the dealings between Professor Lacey and Dr Dealler on the one hand and MAFF officials and SEAC on the other. It suffices, however, to give a clear picture of an unhappy relationship. It was one of mutual suspicion verging at times on hostility.

5.166 The nub of the problem was what the Southwood Working Party had described as ‘the paucity of available evidence’. Risk evaluation depended to a large extent on individual judgements based on results of research into scrapie. The majority of scientists judged the risk of transmission to humans to be remote. A

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1133 S F Dealler, UK Adults’ Risk from Eating Beef, the Lancet, 1996, 347, 195–196
1134 YB96/1.20/5.1
1137 YB96/1.20/1.1
1138 J W Wilesmith, Cohort Study of Cows is in Progress, *British Medical Journal*, 1996, 312, 843
minority took a less optimistic view, including Professor Lacey and Dr Dealler. The rival views were to a large extent speculative.

5.167 Professor Lacey and Dr Dealler felt that MAFF officials were closing their eyes to some of the implications of BSE and not prepared to give adequate consideration to the views of other scientists – not least their own. At the extreme, they suspected MAFF officials of suppressing embarrassing evidence. MAFF officials considered that Professor Lacey and Dr Dealler were prone to making extremist and alarmist statements about BSE that were unsupported by the science and unsound.

5.168 Neither view was fair. MAFF officials were anxious, with the assistance of advice from the scientific committees, to pursue experiments that would help to inform them about the nature of BSE and its implications for human and animal health. They were particularly concerned to investigate whether BSE was maternally or horizontally transmissible.

5.169 Professor Lacey and Dr Dealler each gave evidence to us. Each impressed us as a serious scientist motivated by very real concerns about the hazards posed by BSE. Some of the conclusions they reached were speculative, some were extreme and some have been proved wrong. Many have, however, been vindicated as knowledge about BSE has increased. We have the following comments to make on some of the views that they expressed.

- **Professor Lacey’s suggestion that all herds infected with BSE should be destroyed:**1139 An extreme suggestion as a reaction to the first case of FSE, but an approach that was widely adopted in the European Union.

- **Concern that peripheral nervous tissue might contain infectious agent:**1140 A reasonable concern, subsequently proved correct in the case of dorsal root ganglia.

- **Concern that lymphatics might be infectious:**1141 A reasonable concern, shared by MAFF scientists, though not established by the pathogenesis experiment.

- **Concern about contamination of the carcass by slaughterhouse practices:**1142 A valid concern, addressed by MAFF and SEAC.

- **Dose/time relationship; a small dose could produce clinical symptoms many years later:**1143 Correct.

- **‘Most herds in Britain are infected with BSE’:**1144 This was an example of a tendency on the part of Professor Lacey to exaggerate, which did him no service. His conclusion that for every animal showing symptoms of BSE there were a substantial number of sub-clinical animals was, however, correct.

- **‘We have to look at BSE entirely as a new disease. I am not going to say it will go to man, I am not going to say it will not, I am saying we do not know’:**1145 Professor Lacey was justified in challenging the theory that

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1139 See paragraph 5.8 above
1140 See paragraph 5.11 above
1141 See paragraph 5.11 above
1142 See paragraph 5.11 above
1143 See paragraph 5.12 above
1144 See paragraph 5.13 above
scrapie was the origin of BSE. His statement represented a legitimate point of view.

- ‘If our worst fears are realised, we could virtually lose a generation’: We suspect that Professor Lacey must have regretted making this statement, for it was to be quoted as demonstrating that he was alarmist and unreliable. Yet, while extreme, this was a possible worst case scenario. There was a record of a colony of mink being virtually eliminated by infected feed.

- ‘There is a distinct possibility that man could acquire spongiform encephalopathy from consumption of contaminated beef’: This was the conclusion of Lacey and Dealler’s article in *Food Microbiology*. That article did not deserve the opprobrium it received from MAFF officials. It was a careful review of the subject and the authors stated in clear terms that ‘in discussing the possible effect on man, the most pessimistic view has been taken so far’. Most of the criticisms of the article do not have regard to that important qualification.

- ‘BSE is now an established endemic... propagated chiefly by maternal transmission’: The conclusion that BSE had become endemic as a result of maternal transmission was erroneous. It could have been dispelled had Professor Lacey known of the information about carry-over of ruminant feed available to MAFF.

- *Epidemic likely to hit the human race by the end of the century. 70% chance of the disease being transmitted to humans and children particularly at risk*: The first sentence has unhappily proved correct. We have not, however, identified any valid basis for the remainder of the statement.

- *High infectivity by the oral route*: Professor Lacey was perspicacious in identifying the implications to be deduced from the spread of BSE through feed on the amount of infective material needed to transmit the disease.

- *Risk from bones and gelatine*: This was subsequently recognised to be cause for concern.

- *Dr Dealler’s epidemiology*: Much of Dr Dealler’s conclusions turned on his calculations of the number of sub-clinical animals that the public would have consumed. We understand that his calculations were accepted as sound by Mr Wilesmith. It is a pity that it was not felt that he could be informed of this.

5.170 These examples illustrate the fact that Professor Lacey and Dr Dealler had a useful contribution to make to the debate about BSE. An attempt at collaboration was made at the Stoneleigh meeting. Unfortunately by this time those involved had developed entrenched attitudes. The meeting was not a success and was not repeated.
5.171 The precautionary measures that were taken to address risks to both human and animal health in the years after the first case of FSE in May 1990 were, in part, a response to public perception. The public concerns raised by media coverage of views expressed by Professor Lacey were unwelcome to MAFF. With hindsight it seems to us that they were beneficial.

Dr Harash Narang

Introduction

5.172 Dr Harash Narang is a microbiologist who was employed by the Public Health Laboratory Service (PHLS) at Newcastle General Hospital from 1973 to 1994. In his statement to the Inquiry he told us that he had spent some 30 years of his professional career studying spongiform encephalopathies. The emergence of BSE was naturally of particular interest to Dr Narang. A policy decision was taken that the PHLS should not be involved in investigating the potential implications for public health. Because of this there was no scope for Dr Narang’s employment to embrace research into BSE with the Neuropathogenesis Unit (NPU). He was, however, permitted to carry out such research at Newcastle in his own time.

5.173 Unfortunately a dispute developed between Dr Narang and the PHLS which resulted in Dr Narang’s suspension. We have not been concerned with details of that dispute. What we have been concerned with are allegations that Dr Narang was unfairly treated by MAFF and that potentially valuable scientific advances made by Dr Narang were disregarded, or failed to receive the support that they merited.

5.174 Dr Narang has, from the time of the emergence of BSE, had his own theory as to its cause, based on earlier research into scrapie. Unlike those who espouse Dr Prusiner’s ‘protein only’ theory, Dr Narang believes that nucleic acid is instrumental in producing TSEs and has coined the term ‘nemavirus’ to describe the agent which he considers responsible for the disease.

5.175 The claims made by or on behalf of Dr Narang have related to lack of support for three different tests of the presence of a TSE, which Dr Narang contended could be of practical value in diagnosing BSE or CJD. These were:

- a post-mortem ‘touch test’ involving taking brain slices and placing carbon-coated grids onto the cut surface of the slice. Following several processing steps the grids are observed under an electron microscope. Diagnosis of BSE would be confirmed by the presence of abnormal tubulofilaments, which Dr Narang considered to represent the infective agent.\(^\text{1152}\)

- a test involving the isolation of a single-stranded DNA(ssDNA) which, in BSE-infected brains, was claimed to have particular features that did not occur in unaffected brains; Dr Narang considered that this test would enable both ante-mortem and post-mortem testing.

\(^{1152}\) M37/88/1.14/1.1
iii. a live test involving concentrating the urine from patients suspected of having CJD and detecting the infective agent using electron microscopy.

5.176 We propose to examine some of the evidence relating to the consideration given by MAFF to requests from Dr Narang for assistance in developing these tests. The object of doing so is not to explore the technical aspects of Dr Narang’s work, but to consider the allegations that MAFF was at fault in failing to support this. At their most extreme, these alleged that MAFF were motivated by a reluctance to develop a test which would disclose the extent to which BSE was present in apparently healthy cattle.

5.177 We have not sought to set out a comprehensive account of dealings between MAFF and Dr Narang, but to give a sufficient account to give a fair illustration of the attitude of MAFF officials and Ministers to Dr Narang’s requests for assistance, making reference to internal documentation, including drafts for proposed correspondence about him.

The touch test

5.178 During 1985 and 1986, Dr Narang and his co-workers claimed to have developed a post-mortem diagnostic test for spongiform encephalopathies, in particular scrapie, and a description of this work was published in 1987.\(^\text{1153}\) The test, which we refer to as the touch test, offered potential for rapid routine diagnosis of SEs as an adjunct or alternative to histopathology (the study of microscopic changes in diseased tissues). Although the test had not been developed specifically with BSE in mind, Dr Narang told us that it could ‘provide a rapid means of diagnosis of BSE with very little handling and risk of exposure’.\(^\text{1154}\) Similarly he considered that it could be used to diagnose CJD.\(^\text{1155}\) The test methodology is described in more detail in vol.2: *Science*.

First demonstration of the touch test by Dr Narang

5.179 Dr Narang was invited to the AFRC/MRC Neuropathogenesis Unit (NPU) in September 1987 to demonstrate the test. He told us that the demonstration had been successful:

> Preparation and examination was carried out on six randomised scrapie and normal brains and correct results were recorded on all six brains within two and a half hours from start to finish.\(^\text{1156}\)

5.180 On 14 January 1988, Dr Narang presented a paper on the touch test to the 74th meeting of the British Neuropathological Society.\(^\text{1157}\) Among the other papers presented to the meeting on that day was an early paper on BSE by Mr Gerald Wells and Mr Tony Scott from CVL entitled, ‘Neuronal vacuolation and spongiosus: a novel encephalopathy of adult cattle.’\(^\text{1158}\)


\(^{1154}\) S113 Narang para. 1.10

\(^{1155}\) N K Narang and R H Perry, Diagnosis of Creutzfeldt-Jakob Disease by Electron Microscopy, the *Lancet*, 1990, 335, 663–664

\(^{1156}\) S113 Narang para. 2.11

\(^{1157}\) M37/88/1.14/1.1

\(^{1158}\) M37/88/1.14/1.1
Mr Bradley updates Dr Watson on the touch test

5.181 In a minute of 19 January 1988 updating Dr Watson on various BSE issues, Mr Bradley mentioned the paper given by Dr Narang at the meeting of the British Neuropathological Society, and his subsequent requests for assistance from SVS staff:

He has since contacted a large number of SVS staff including VIC and CVL staff. Gerald Wells has spoken with him and he also spoke to Tony Scott . . . He is particularly active in EM [electron microscopy] diagnosis (rapid) using fresh brain material directly onto grids – a halfway house between SAF studies and TEM of sections. These are called ‘Touch Preparations’ I believe. He is after some fresh brain material for trying his technique and also for purifying BSE DNA. I instructed Gerald Wells not to commit to collaborative studies and not to pass up fresh material until we had your/CVO’s agreement but to develop a working relationship for discussion purposes . . .1159

5.182 On 12 August 1988, Dr Narang contacted CVL from the USA, to propose the use of the touch test for diagnosing BSE.1160 He offered to talk to the CVL in Weybridge about his test. Dr Watson replied on 23 August, inviting Dr Narang to Weybridge to discuss the progress he had made.1161 He asked him to contact his deputy, Dr Brian Shreeve, on his return to the UK to arrange an appropriate date.

5.183 Dr Narang said in evidence that he did not receive this information at the time and that he did not hear from MAFF until 11 months later, when they invited him to come to Weybridge in August 1989.1162 We are satisfied that Dr Watson sent the letter. It is unfortunate that it seems to have gone astray.

CVL decide to collaborate with Dr Narang

5.184 Based on Dr Narang’s proposals, on 2 February 1989 Mr Wells, Mr Dawson and Mr Scott produced a minute entitled, ‘Summary of tentative proposals/requested collaboration with Dr H Narang’.1163 It detailed potential benefits to CVL/MAFF. These included the acquisition of a rapid diagnosis of BSE cases and, subject to the sensitivity of the test, a way of screening brains at the abattoir. The minute invited comment from Mr Bradley. The summary noted that conditions for collaboration had been raised by and discussed with Dr Narang.

5.185 Mr Bradley responded on the same day recommending that the CVL ‘immediately initiate’ experiments to enable Dr Narang to demonstrate his ‘touch preparation method’. Mr Bradley commented ‘overall this seems too good an opportunity to miss with many potential benefits for CVL/MAFF’.1164

5.186 On 8 February 1989, Mr Wells wrote to Mr Michael Dawson at the CVL Virology Department to discuss a project application from Dr Narang to the MRC, which Dr Narang had sent to Mr Wells for comment.1165 The application was to fund

1159 YB88/1.19/1.1–1.2
1160 YB88/8.12/3.1
1161 YB88/8.23/1.1
1162 S113 Narang para. 2.12
1163 YB89/2.2/5.1
1164 YB89/2.2/2.1
1165 YB89/2.8/4.1
the development of the touch test. Mr Wells explained that Dr Narang was in a hurry to get the application sent off. He said that:

I am unhappy about the lack of medical purpose behind the project application – it seems purely a veterinary aim. Harash may have specific reason for not including it but he did tell us that he saw a future for the touch preparation method in diagnosis of CJD.

5.187 Mr Wells also said:

I find it difficult to support whole heartedly, as written, because it suggests that the technique is vital to us, which is really not the case. It would, however, be a useful additional diagnostic and research tool.

5.188 On 13 February 1989, Mr Dawson wrote to Dr Narang, incorporating comments from Dr Wells and Mr Bradley on his MRC project proposal. They concluded that ‘the case would be more comprehensive and better balanced if there was more emphasis on the potential benefits for CJD diagnosis’.

5.189 On 18 March 1989, Mr Bradley wrote to Dr David Shannon, with copies to Dr Watson and Dr Kenneth MacOwan, about the BSE research and development programme. In relation to the proposed collaboration with Dr Narang on the touch test, he said:

This clearly has a potential benefit to MAFF and this and other work would be funded from his sources but the provision of material would remain with us . . . We should allow £10K for our expenses to support Dr Narang.

5.190 In June 1989 Mr Bradley chaired the third BSE research and development progress meeting at the CVL. In addition to others from CVL, the meeting was attended by Dr Hope and other representatives of NPU and by Dr MacOwan of the Chief Scientist’s Group. Collaboration with Dr Narang was briefly discussed. The note of the meeting recorded:

This so far had been of limited benefit. The poor success achieved on the initial examination of ‘touch preparations’ places the scientific merit of continuing the exercise in question. Dr Narang had reported that he had been unsuccessful in his application for funds from MRC. Dr Hope’s opinion is that the single-stranded DNA demonstrated by Dr Narang is an artefact. Further collaboration would not be encouraged. Dr Narang may seek funding from MAFF Chief Scientist.

5.191 Dr Narang told us that in August 1989 he visited CVL to present a seminar on his ‘touch technique’.

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1166 M37/89/2.13/1.1
1167 YB89/3.18/1.2
1168 YB89/6.13/3.11
1169 YB89/6.13/3.11
1170 S112 Narang para. 2.12
Dr Narang’s application for funding from MAFF is turned down

5.192 Dr Narang telephoned Mr Bradley on 7 November 1989 to request funding from MAFF for further studies and access to more cow brains, both infected ones and controls. Mr Bradley informed him that the CVL had no funds, and advised Dr Narang to discuss this with Dr MacOwan of Chief Scientist’s Group. On the request for the provision of further brain samples, Mr Bradley said that he would discuss this with Dr Watson and others at CVL.1171

5.193 On 16 November, Dr Narang submitted a research proposal entitled, ‘Development of routine diagnostic test for BSE as an alternative to histopathology’, to Dr MacOwan under MAFF’s Open Contracting scheme.1172

5.194 The minutes of the fourth BSE research and development progress meeting on 21 November 1989 record some discussion about Dr Narang’s request for access to brain tissue from three animals and abattoir controls.1173 The minutes said:

While CVL wished to be helpful he had caused some upset there when he visited and they did not want him using CVL facilities or the CVL name in relation to his work. Mr Dawson pointed out that he had at present no funding and Mr Bradley said this material would only be required if funding was forthcoming. Dr MacOwan said Narang had applied to the Chief Scientist for minimal funding and that if his request for money was granted any information his work produced would be for the Chief Scientist. While to date it was unheard of for the Chief Scientist to stop work being published this was possible. The contract made with the Chief Scientist when funding was granted was to protect the customer. Dr MacOwan asked the meeting to be more specific in their dismissal of Dr Narang’s work as this had a bearing on the result of the request for funding. Mr Dawson said he [Dr Narang] had come to CVL at his own request to show them the technique he had developed for finding fibrils using the electron microscope. He had used animal necropsy facilities, the senior electron microscopist and his deputy all day and had treated CVL personnel like servants. They had been understandably annoyed. Out of a whole day’s work he had found only 2 fibrils, 1 of which was on the first grid. He had only reported this first, immediate result and ignored all the failures. Mr Dawson said the technique was extremely insensitive even in Dr Narang’s hands for the diagnosis of clinical BSE.1174

5.195 The meeting agreed that ‘material should be released to him but no other facilities provided’.1175

5.196 On 27 November 1989, Mr Bradley wrote to Ms Claire Goodson and Mr Mark Hawkins of the Animal Health Veterinary Group (AHVG), to secure surveillance funding for costs the CVL would incur in cooperating in a proposed study with Dr Narang.1176 In his minute Mr Bradley commented:

1171 YB89/11.10/11.1
1172 YB89/11.16/8.1
1173 YB89/11.21/1.1
1174 YB89/11.21/1.26–1.27
1175 YB89/11.21/1.27
1176 S100B MacOwan para. 11
there is some scientific merit in initiating a small study under the supervision of MAFF staff who alone will do the histological diagnosis perhaps on 5 control and 5 diseased animals in the first instance. We will not accept Dr Narang here but will try to help in a suitable way eg. via a local VIC. 1177

5.197 Mr Bradley’s proposal differed from that for which Dr Narang had sought funding on 16 November 1989. 1178 Dr MacOwan told us:

The small study proposed was for the diagnosis of five control cattle brains and five BSE infected cattle brains. Mr Bradley was seeking funding from the AHVG surveillance budget for the costs which would be incurred by the CVL in supplying material to Dr Narang and in carrying out the histological diagnosis on the ten cattle brains to confirm Dr Narang’s results. Dr Narang obtained funding from a private individual, Mr Ken Bell, in May, 1990 and at that stage approval was given for funding of the CVL’s costs, I believe out of the AHVG’s surveillance budget. 1179

5.198 Dr MacOwan said in written evidence that Dr Narang’s proposal of 16 November was considered at an appraisal meeting early in 1990, which was attended by the Scientific Liaison Officers responsible for managing MAFF research, the Deputy Scientist and the Chief Scientist. 1180 He explained that these meetings considered large numbers of projects across the whole spectrum of MAFF’s work, and only brief minutes outlining the action points were prepared by administrative attendees from the Research and Development Division. Mr Bradley was later to explain that although he had proposed a blind trial, it seemed that the Chief Scientist’s Group decided not to fund the project since it did not fit in with Tyrrell proposals to develop a test in the live animal. 1181

5.199 On 25 January 1990, B Woodbridge of MAFF Chief Scientist Group wrote to Dr Narang to inform him that his application had not been selected for funding. 1182

Dr Narang receives private funding: reaction from CVL

5.200 On 30 May 1990, Dr Narang wrote to Dr Joseph Smith (later Sir Joseph Smith), Director of the PHLS, at PHLS Headquarters to inform him that he had not been able to carry out his research into work for a test for BSE, because MAFF had turned down his application for funding. Dr Narang informed Dr Smith that he had been approached by a private individual who was willing to provide up to £20,000 to the development of the test. He added that he hoped that ‘the necessary co-operation from all concerned will now be forthcoming’. 1183 The source of Dr Narang’s funding was Mr Ken Bell, the owner of a substantial food business and for whom the PHLS were undertaking private work at the time.
5.201 Dr Narang copied the letter to Mr Bradley, who circulated it to Mr Meldrum and others in MAFF, adding in a manuscript note that ‘this will undoubtedly lead to requests for brain material. We should decide how these should be dealt with’.\footnote{YB90/5.30/11.1}

5.202 On 5 June 1990, Mr Crawford wrote to Mr Meldrum to inform him that there was much media interest in Dr Narang’s private funding from Ken Bell and that the CVL would be cooperating with Dr Narang. They would provide him with fresh brain sections from five BSE-suspect cattle and five ‘normal’ cattle.\footnote{YB90/6.5/15.1} Dr Narang would then identify the BSE-infected samples and inform CVL of his diagnosis within half a day of receipt of the samples.

5.203 A minute sent by Mr Bradley to Dr Shreeve and others on 25 June 1990 described in detail the arrangements whereby Dr Narang would receive material from CVL, as agreed in November 1989 (see paragraphs 5.193–5.195).\footnote{YB90/6.25/20.1} On 29 June 1990, Mr Bradley sent a minute to Mr Holden of the CVL finance department, copied to Mr Shreeve, about the arrangements for collaborating with Dr Narang. He asked for any necessary costing and invoicing arrangements to be put in place, and advised that ‘Dr Narang on past evidence is likely to make a large number of demands upon us’.\footnote{YB90/6.29/6.1}

Progress with validation of the touch test

5.204 On 23 October 1990, a formal meeting was held to peer review Dr Narang’s work.\footnote{M37/90/10.23/1.1–1.39; M37/95/9.13/1.4} The review was set up by Dr J Smith, Director of the PHLS, in response to a request from Dr N Lightfoot of PHLS Newcastle, who was concerned that Dr Narang was working in isolation, with no peer review and without expert help. Dr Lightfoot supported Dr Narang’s work and wished to see the test validated with the help of others working in the same field. Dr Smith invited external experts to assist in the review – Professor H Smith from the Department of Physiology, University of Birmingham as Chairman, Professor Edwardson of the MRC Neurochemical Pathology Unit, Newcastle, and Dr Bostock of the IAH.\footnote{S114 PHLS para. 29} Dr Smith attended for the PHLS, and Dr Lightfoot and Dr Narang attended for part of the time.

5.205 The note of the meeting indicates that the discussions were wide-ranging. In relation to the collaborative work with CVL on the validation of the touch test, the notes record that ‘Dr Narang has been offered and received material from Weybridge but failed to give results’.\footnote{M37/90/10.23/1.2} The meeting broadly endorsed continuing the work, using external funding, to validate the touch test.\footnote{M37/90/10.23/1.35–1.36}

5.206 On 8 November 1990, Mr Ken Bell wrote to Dr Lightfoot, asking why he had not been informed of any progress on Dr Narang’s work, even though it had been six months since funds had been made available.\footnote{M37/90/11.8/1.1} He said that:
You will realise I will be very annoyed if anyone is wilfully holding up this work, I am being pressed by interested parties waiting for your report of progress.

5.207 Dr Lightfoot replied on 13 November, stating that ‘I wish to reassure you that no one is holding up Dr Narang’s research work and that at a recent review his work was supported’. He added that a full report was being prepared, which would include details of expenditure and would be forwarded to him within the next few days.

5.208 A few days later, on 16 November 1990 Professor Edwardson wrote to Mr Bradley, asking for the supply of brain material to Dr Narang to be speeded up. He commented:

. . . I have been a colleague of Dr Narang’s for the past ten years and have tried to provide advice and support for him in his relatively isolated research position during this period.

One of the major issues at the PHLS review was the need for Dr Narang to validate his ‘touch technique’ for the rapid post-mortem diagnosis of BSE in a blind trial, and I am aware of the proposed collaboration which has been set up with CVL to do just this. Yesterday Dr Narang came to see me about the slow pace at which this research is proceeding and after discussion with Dr Lightfoot, his Director, I agreed to write to you. I gather that, since the study was agreed, two brains became available in September, and there have been no more specimens since then. While I appreciate the various factors which need to be controlled for in this study, it is difficult to understand why the provision of tissue should be so slow, given what I understand is the prevalence of BSE locally.

5.209 Mr Bradley replied on 27 November 1990. He expressed surprise that Dr Narang had contacted the Professor rather than himself about the slow pace of research. He further stated that:

I have always had (and still have so far as I know) a good personal relationship with Dr Narang and I am very disturbed at the attitude you suggest he is taking. Research success depends upon good working relationships and I am anxious that any deficits are repaired as soon as possible.

5.210 He also stated that CVL would be willing to consider supply of material for further study, but that Dr Narang should write to him specifying the requirements.

Dr Narang’s progress in late 1990/early 1991

5.211 Dr Narang told us that he worked on the diagnosis of brain samples supplied by MAFF (CVL) between September 1990 and January 1991:
Between the 22nd September 1990 and the 8th January 1991 I was given 10 brain samples by MAFF to be examined to establish whether they were contaminated with BSE. The delay was caused by MAFF’s difficulties in finding ten control brains. The results of these tests were returned to MAFF by me on the day I received the specimens. Several months passed and the people who were interested in my researches were becoming increasingly concerned that MAFF had still not published the results of their tests on the 10 samples of brain. On the 13th March 1991, David Clark M P Shadow Minster for Food and Agriculture wrote to Dr Lightfoot asking why there was a delay. Shortly after this I received a report from MAFF confirming that my tests had positively identified the presence or absence of BSE in 8 out of 10 cattle brains. The test had not identified the presence of BSE in 2 of the brains. Although the score was not 100%, I viewed the results as encouraging. It has to be remembered that the test involved examining potentially infected material in order to identify whether or not Nemavirus/SAF were present. It is possible for the examiner to miss the presence, although the chances of this will be reduced by further refinement of the test and/or by identifying more precisely the areas of the brain to be examined. (The number and distribution of NVP vary from one area of the brain to another.) I had no control over the supply of the material and only very small quantities of material were supplied. If additional samples had been provided from each brain, it is likely that the two additional affected brains would have been positively identified.

Of great significance is the fact that none of the negative samples were identified as positive. 1196

Communications with Dr David Clark

5.212 On 13 March 1991 Dr David Clark, the Shadow Minister for Food and Agriculture, wrote to Dr Lightfoot expressing his interest in the work of Dr Narang, having received information from Mr Ken Bell on his work. He wrote of his disappointment in the ‘apparent delay’ by MAFF in publishing the results of Dr Narang’s tests on the ten brain specimens. He told Dr Lightfoot that he intended to put down a Parliamentary Question to find out why a delay had occurred. 1197

5.213 Dr Clark’s Parliamentary Question was tabled shortly after this and asked if the results of Dr Narang’s test on bovine brains supplied by MAFF were to be published. The reply stated that the publication of the results was a matter for Dr Narang, but that all the material and data requested by him, had been provided by the Department. 1198 The response to the PQ was provided by Mr Lowson, who in his covering minute commented that Dr Narang’s results ‘will show that he only identified three of the confirmed positive cases, and one of these three was originally reported by Dr Narang as being negative’. 1199
Results of the touch test

5.214 On 15 March 1991, Mr Anderson, Senior Veterinary Investigation Officer at Newcastle, sent Dr Narang the results of the histopathology and the analysis of scrapie associated fibrils undertaken by CVL on the ten samples of brains from ten cattle supplied to him. A few days later, Mr Bradley wrote to Dr Narang comparing the results from the CVL diagnosis and the touch test diagnosis on the ten samples of brain. The touch test had failed to identify two of the five BSE-positive brain samples:

BSE was confirmed in each case by brain histology and supported by the finding of scrapie associated fibrils in each BSE case. All studies were done blind as were your own.

In contrast your own studies aimed at detecting tubulo-filamentous structures failed to identify two brains as being from BSE-affected animals. The other animals were correctly identified.

I am quite willing to discuss the results with you but consider that the failure to identify two BSE positive animals seriously detracts from the value of your test as a practical substitute for brain histology in BSE diagnosis.

Please give me a call if there is anything you wish to discuss otherwise I feel we should consider the study concluded. 1201

5.215 In his evidence to the Inquiry, Dr Narang said that the fact that the touch test had not identified two brains as being BSE positive was because he had no control over the supply of the material he was given and he was only given very small quantities of material. He said:

If additional samples had been provided from each brain, it is likely that the two additional affected brains would have been positively identified.

5.216 He further said that the validity of the supply of material was a concern. In particular, the dates on the form that arrived with the samples differed from those that were shown on MAFF’s final report on 15 March. He therefore thought that there had been a mix-up of specimens in MAFF’s laboratory, so that the laboratories were perhaps not examining the same specimens.

Dr David Clark’s letter to the Independent

5.217 On 17 April 1991, the Independent published a letter from Dr David Clark on the subject of MAFF’s collaboration with Dr Narang. He mentioned that a private businessman had sponsored Dr Narang after his application for funding had been turned down. According to Dr Clark, MAFF ‘then systematically set out to scupper the research.’ For example after weeks of delay, samples of cattle brains were offered at £150 a piece for Dr Narang to work on, although the local abattoir would have happily supplied them at £1.50.
5.218 Dr Clark considered that the reason for this hostility was political:

The Government has consistently mishandled the whole BSE affair and is terrified that if a simple diagnostic test became available, random sampling could reveal the true extent of the disease in our cows.

5.219 In response, Mr David Maclean, the Parliamentary Secretary (Commons), wrote to Dr Clark on 18 April 1991, and copied the letter to the editor of the *Independent*. The letter asserted that an effective post-mortem test for BSE already existed and that when Dr Narang’s proposal had been compared to other applications it had not been considered of sufficient merit. He also said:

Although we have not provided funding we have co-operated fully with Dr Narang in his work by supplying brains from confirmed affected and unaffected animals. I believe that results of the study are now available and look forward to their being made public. The charge made only reflected the considerable amount of professional and technical time that went into ensuring he was provided with suitable samples to test his method. Brains straight from an abattoir could not have been used: had they been suitable no doubt he would have used them.1204

Dr Narang’s results are discussed at a joint CVL/NPU meeting

5.220 On 23 April a joint CVL/NPU meeting was held to discuss progress with BSE research and development.1205 The results of the touch test were discussed, amongst other things. In relation to these results, the minutes record:

Despite the difficulties faced by Mr Lowson and others because of questions raised in the House of Parliament on behalf of Dr Narang, the meeting did not think it ethical to divulge Dr Narang’s results – that was a matter for him.1206

Dr Clark continues to press the case of Dr Narang

5.221 On 12 June 1991, Dr Clark wrote to Mr Gummer asking whether the AFRC had received a grant application from Dr Narang.1207 It seems that the application had been submitted by Dr Narang to PHLS Head Office, for onward transmission to the AFRC. On 18 June, Mr Maslin sent MAFF’s Correspondence Section a draft reply for Mr Gummer to send to Dr Clark. In his covering minute, Mr Maslin advised that MAFF had no responsibility for grant applications to the AFRC, and continued:

I understand (by simply phoning them) that to date the AFRC has had no application from Dr Narang. It may however still be in the pipeline and I suggest that it is not for us to say so.

No application for work on a live BSE test has been made by Dr Narang under our Open Contracting Fund.1208
5.222 The draft reply to Dr Clark confirmed that the research councils were the responsibility of the Department of Education and Science, and that his letter had been sent on to a Minister in that Department.1209

5.223 On 18 June 1991, Dr Clark wrote to Mr Gummer alleging that Dr Narang had recently completed ten tests on bovine brains which had proved to be 80 per cent successful. Dr Clark made a request that ten more samples of bovine brain tissue be supplied to Dr Narang so that he could carry out another set of tests.1210

5.224 Mr Maslin consulted with Mr Bradley, Dr Macowan (CSG) and others in MAFF about the draft reply to Dr Clark. In particular he proposed advising Dr Clark about Dr Narang’s results:

I understand that it is not really ‘the done thing’ to discuss the results of research which have not been published and therefore made available for scrutiny by the scientific community. However, as Dr Clark appears to be aware of the results of Dr Narang’s research, I believe we should take the opportunity to try and lay this matter to rest by giving him the correct results and pointing out that the test used is simply not accurate enough.

At the same time we can express disappointment that Dr Narang has not yet made public the results of his research.1211

5.225 In the event the reply that was sent by Mr Gummer to Dr Clark on 6 July 1991, did not describe the results:

It is surprising that Dr Narang has not yet made public the results of his research. The absence of such information makes sensible discussion of the issues impossible.

If Dr Narang intends to proceed with further tests he should submit a detailed research proposal to us. In the light of this we will of course be prepared to consider a request for further brain material.1212

Dr Narang writes to Mr Gummer

5.226 On 17 July 1991, Dr Narang wrote to Mr Gummer informing him that he had seen the reply of 6 July to Dr Clark. Dr Narang enclosed various research papers and a draft paper on his validation of the touch test which, he advised Mr Gummer, was being ‘held for internal review’ within PHLS. He also commented that he had submitted an application for an AFRC research grant for further work to develop a diagnostic test in live animals.1213 Mr Adrian Dixon, of the Animal Health (Disease Control) Division sought comments from Mr Bradley and Mr Gardner in DAFS about this letter. Mr Dixon noted that DH had asked to be consulted on correspondence between Dr Narang and MAFF. He suggested that MAFF’s reply would ‘probably just be to thank him for the information which he supplied’.1214
5.227 Mr Gardner replied to Mr Dixon on 1 August 1991. He highlighted the fact that the scientific community did not generally agree with Dr Narang’s theories about the infectious agent. He also stated that the touch test would not be as effective as the existing histological methods. However he added:

Notwithstanding these comments I would think that research into EM [electron microscopic] diagnostic methods could be worthwhile. However, a first step would seem to be the publication of Dr Narang’s work and subjecting it to proper scientific scrutiny.

I would suggest that you draft a neutral reply thanking him for the information contained in his draft paper. 1215

5.228 On 9 August 1991, Mr Michael Harrison (Mr Gummer’s Private Secretary) replied to Dr Narang thanking him for his letter and research papers into SEs and stating that ‘we look forward to the publication of your results in the scientific press in due course’. 1216

Mr Dale Campbell-Savours takes up the case for Dr Narang

5.229 On 2 September 1992, Mr Dale Campbell-Savours (Labour MP for Workington) telephoned Mr Soames (MAFF Parliamentary Secretary), explaining that he was due to meet Dr Narang the next day. He asked Mr Soames about the reasons for MAFF’s lack of support for Dr Narang’s diagnostic test for BSE. Mr Soames undertook to provide Mr Campbell-Savours with a short note about this before the meeting. Mr Maslin was asked to provide the note. 1217

5.230 Mr Maslin’s note set out MAFF’s previous involvement with Dr Narang, starting with the application for funding in 1990. 1218 He stated:

[Dr Narang’s] proposal was turned down because an effective and accurate post-mortem diagnostic test already exists and, when compared to other project applications, it was not considered of sufficient merit to warrant funding. What was and is important is the development of an ante-mortem test and substantial funds are being devoted to research in this area.

In the event Dr Narang obtained private finance to conduct his research. But we have cooperated fully with Dr Narang by supplying brains from confirmed and negative cases of BSE. A charge was made for the time and technical expertise which went into meeting his requirements.

The refusal of MAFF funds led Dr Narang to the newspapers and Dr Clark, the previous opposition spokesman on agriculture, who both took it up as an issue.

As far as we are aware, Dr Narang has still not subjected his work to the normal scientific peer review by publishing it as a refereed article in a reputable scientific journal. (Dr Narang did supply a copy of his paper in
earlier correspondence. Although we cannot officially comment on it, scientific and veterinary colleagues had serious criticisms of the work.)

5.231 On 3 September, Mr Campbell-Savours wrote to Mr Soames to request a meeting between Dr Narang, himself, Mr Soames and other civil servants, with the aim of discussing Dr Narang’s research. Mr Soames replied on 29 September. He declined the offer of a meeting and went on to set out further details of why MAFF had not funded Dr Narang’s work. He said that although Dr Narang’s work had been carefully considered his application for funding was rejected on the following grounds:

i. an effective and accurate post-mortem diagnostic test was already in use; and

ii. speed of retrospective confirmation of clinical diagnosis was not important from a disease control point of view as the animal is slaughtered and removed from the farm once a clinical diagnosis of the disease has been made.

5.232 Mr Soames also declined to comment on the results of Dr Narang’s work since it had yet to be published. However, he continued:

... as a result of the Ministry’s involvement in the work, and since Dr Narang sent a draft of his report to the Minister, we are aware of some aspects of it. In general terms, the accuracy of his tests appeared to fall below that of the current method for confirmation of BSE by brain histology and that which would be necessary for any practical use. Let me stress again, however, that we have not seen the published results and are eager to do so.

Finally, I would re-emphasise that Dr Narang’s work relates to a post-mortem test, not to one that can be used in the live animal. The need for an effective diagnostic test in the live animal still exists and research has been underway for some time to address this. The Spongiform Encephalopathy Advisory Committee which reported in April 1992 has considered all areas of spongiform encephalopathy research and are satisfied that this important area is being adequately addressed.

Dr Narang and MAFF discuss a new ante-mortem test

5.233 Mr Campbell-Savours replied to Mr Soames on 16 October 1992. He commented that he had discussed the reply with Dr Narang who advised that MAFF seem to have misunderstood the position: Dr Narang wished to discuss a new test that he was developing for use ante-mortem. Mr Campbell-Savours reiterated his request for a meeting.
5.234 Mr Soames replied on 10 November:

It remains the case that we in MAFF have had no direct approach from Dr Narang about any research work other than the development of the post-mortem test which I mentioned in my earlier letter . . .

On the basis of what you say and of a number of indirect approaches that have been made to officials, it does seem that he is now pursuing a new line of enquiry. I am therefore asking the BSE adviser at the Central Veterinary Laboratory to contact Dr Narang to arrange for him to make a presentation about his work (possibly involving scientists from outside MAFF as well). If the work looks promising we would certainly want then to encourage Dr Narang to work up a formal research proposal for us to consider.1225

5.235 On 13 November 1992, Mr Bradley wrote to Dr Narang to invite him to attend a meeting to present and discuss the results of his recent research findings on TSEs in animals and man. The letter opened with the following remarks:

The Ministry of Agriculture places a high priority on the development of a cost effective test to detect infectivity in the live animal in respect of these diseases and in particular scrapie and BSE. Indeed, there is much work funded by MAFF and others to secure this objective. We are always interested to assess new ideas on the nature of the agents causing TSEs including the work done by independent researchers such as yourself . . . 1226

5.236 Mr Bradley went on to stress:

It would be especially valuable to present your ideas on how, and how quickly you consider the results of your work so far can be applied to detection of infectivity in the live animal and particularly in the pre-clinical phase of disease.1227

5.237 He accordingly proposed a seminar to be attended by a small group of researchers and others with a special knowledge or interest in TSEs.1228

5.238 The seminar was held on 17 December. In addition to MAFF and CVL representatives, attendees included Dr Tyrrell, Professor Jeffrey Almond and Dr Chris Bostock.1229 On 19 December Mr Bradley minuted Dr Shreeve, Mr Lowson and others in MAFF about the reactions to, and the outcome of, Dr Narang’s presentation. He said that the seminar went off without mishap and several members of the audience thought it was valuable. However, several members of the audience had reservations about some aspects of the science. He concluded:

I recommend that consideration be given to funding Project proposals aimed at verifying Dr Narang’s work so far and also to other projects aimed at extending this knowledge but without favour, and based upon their scientific
merits. Proposals should be peer-reviewed by MAFF and non-MAFF molecular biologists.\textsuperscript{1230}

\textbf{5.239} Shortly after the seminar, Dr Narang applied for a research contract with MAFF on 29 December for development of a live test for scrapie/BSE, involving the isolation of a single-stranded DNA which had particular features in BSE-infected brains.\textsuperscript{1231}

\textbf{5.240} On 27 January 1993, Mr Lowson reported to Mr Soames on the seminar. He noted that Dr Narang had acquitted himself well. However, there had been no attempt to draw any conclusions and no general consensus emerged; a number of members of the audience still retained their reservations about Dr Narang’s approach to TSEs. He also reported:

Before the meeting was held, we had received a research proposal from Professor Oxford, who wants to validate Dr Narang’s earlier work; and the AFRC had received another from Dr Narang and Professor Oxford jointly. After the meeting, we got one from Dr Narang himself. The first of these is a good proposal and, subject to some amendments, it could merit support. But it overlaps with the one submitted to the AFRC and over the next few weeks we will need to explore with them how any funding might be shared.\textsuperscript{1232}

\textbf{Arrangements for a jointly funded AFRC/MAFF project to verify the basis of Dr Narang’s proposed test}

\textbf{5.241} Shortly before Mr Lowson reported to Mr Soames about the seminar, he had minuted Dr MacOwan on 18 January 1993 to update him about the progress with the proposal submitted to AFRC by Professor J Oxford of the London Hospital Medical School (LHMS) and Dr Narang. He commented that a sensible way forward might be for MAFF and the AFRC jointly to fund the work, and acknowledged that this arrangement would reduce MAFF’s financial commitment.\textsuperscript{1233}

\textbf{5.242} MAFF and AFRC subsequently agreed to the joint funding of the proposal, and on 28 April 1993 notified Professor Oxford of his successful grant application.\textsuperscript{1234} The letter advised that funding would be provided for 12 months and set out the conditions under which the work would proceed. The aim was to verify the basis of Dr Narang’s proposed test. In essence the work would verify the existence of a scrapie-specific single-stranded DNA with an unusual nucleotide sequence; if verification was successful, this single-stranded DNA could be used as a basis of a test in the live animal. Dr Narang’s hypothesis in relation to this is described in vol.2: \textit{Science}. In essence, Dr Narang’s protocols were to be followed rigorously, and the work was to be conducted independently of Dr Narang.

\textbf{5.243} In the meantime, Dr Narang had made a further request for funding from MAFF. On 17 March he had written to Dr MacOwan seeking funds to make further progress on live test and for this work to be undertaken in parallel with the proposed
verification work on the test. In his letter he had also commented that he was not content with the conditions attached to the funding of Professor Oxford’s project, in particular his own role as a consultant.1235

5.244 On 15 May Dr Narang wrote to Dr MacOwan again, repeating that he did not accept the conditions attached to the funding of Professor Oxford’s project and seeking information on whether MAFF would fund his latest proposal. He commented that he would be presenting his latest results at various scientific symposia in summer 1993.1236

5.245 Dr MacOwan replied on 17 May commenting on the progress with agreeing the first proposal from Professor Oxford, and Dr Narang’s latest proposal:

In the interim you wrote to me and sent a revised proposal seeking funds to further your research from the point at which the first contract (with Professor Oxford, yourself and Dr Levantis, referred to above) will cease . . .

I am very pleased to hear of your successes with demonstrating ssDNA and I should be most interested to read your paper which you will present in Iceland. However I have to tell you that after very careful consideration further support for your research from MAFF will be contingent on the progress of your first contract with Professor Oxford and Dr Levantis. I realise that you will be disappointed but at the same time I know that you will understand that evidence of progress is very important.1237

Mr Campbell-Savours seeks an update from Mr Soames on Dr Narang’s applications to MAFF

5.246 On 24 May 1993, Mr Campbell-Savours wrote to Mr Soames seeking an update on the progress being made with Dr Narang’s applications to MAFF for funding.1238

5.247 Mr Soames replied on 2 June, and advised that MAFF and AFRC had awarded a contract to Professor Oxford:

. . . [the] Ministry and the Agriculture and Food Research Council have been in close contact to agree the details of the contract to be offered to Professor Oxford, of the London Hospital Medical School, for a 12-month project to validate Dr Narang’s findings, using Dr Narang’s experimental protocols. A letter awarding the contract to Professor Oxford has recently been sent, and a reply is awaited. Any support to further Dr Narang’s work will be contingent on the progress of this validation project. As soon as Dr Narang’s novel hypothesis has a substantiated scientific basis, his work will be considered further.

I would stress that this approach is in line with the views of independent assessors, and aims to ensure that Dr Narang is treated as impartially as any other notable research scientist seeking funding.1239

1235 M37/93/3.17/1.1
1236 M37/93/5.15/1.1
1237 M37/93/5.17/1.1
1238 YB93/5.24/3.1
1239 YB93/6.2/3.1
5.248 Shortly after this letter, Mr Soames agreed to a request from Mr Campbell-Savours for a meeting to consider Dr Narang’s research work on BSE. The meeting took place on 1 July. Mr Bradley was in attendance amongst others from MAFF. Mr Soames followed up the meeting with a further letter to Mr Campbell-Savours on 16 July to report on progress:

. . . Professor Oxford welcomes the opportunity for Dr Narang to attend his laboratory to ensure that the protocols are carried out to the letter, and is seeking authority from the Dean for that purpose . . . Finally the PHLS wish to contribute to the success of the project, and will favourably consider any application from Dr Narang to attend Professor Oxford’s laboratory during the course of the project for the purpose of instructing the research assistant in the details of method. The project work itself, however, will be conducted independently of Dr Narang as specified in the contract conditions.

5.249 Mr Soames also said that he would be writing to Mr Campbell-Savours again shortly about the ‘significance of any delay in developing a live test consequent on the need to validate Dr Narang’s work’. He did so on 27 July, enclosing a paper by Mr Bradley summarising ‘the issues and confirms the views expressed during the meeting’ on 1 July. The paper set out, among other things, the reasons why a one-year delay in progressing Dr Narang’s work was of little consequence:

To take all the work forward to reach a conclusion, and in the face of formidable competition from researchers who already have evidence from studies using transgenic mice that supports a protein (PrP)-only hypothesis, would take, at a conservative estimate, 8-10 years. In this context a delay of up to one year before further MAFF funding was available is insignificant.

5.250 The paper commented on MAFF’s role in progressing Dr Narang’s research:

No embargo has been placed by MAFF or AFRC to prevent Dr Narang in regard to developing his research. They have however, decided to independently validate the results of his work so far accomplished before making the substantial long term research investment necessary to bring his scientific approach to fruition. This is essential in view of the assessment of the proposals put forward by Dr Narang by in-house and external, independent reviewers and the fact that the scientific community with expert knowledge of the field is sceptical of Dr Narang’s results. In these circumstances the use of public money to progress the work before the primary findings have been validated would be indefensible. Validation by Professor Oxford is in hand and it would seem important that Dr Narang collaborates with him to the extent that Professor Oxford requires. This process has been facilitated by discussion with all the parties involved.
5.251 The paper’s conclusions stated:

i. The delay in funding to take forward Dr Narang’s work is insignificant in relation to the time required to develop and validate any test for use in the field.

ii. Dr Narang is at liberty to take his work forward via funding from alternative sources. CVL has prepared material at his request to enable him to progress some of this work.

iii. It is a MAFF objective to have a test to detect infection in the live animal and a great deal of work towards achieving this objective, some funded by MAFF, is already in progress.

iv. Dr Narang appears to have little understanding of the steps needed to get any test he may develop accepted by the scientific community. He has significantly, and consistently, underestimated the work required and the problems that have to be overcome.

v. MAFF will continue to see that Dr Narang and his project proposals are dealt with in an equitable manner, as it has done in the past.

First progress meeting of the joint AFRC/MAFF project with Professor Oxford

5.252 The first meeting of the monitoring group set up to review progress of the AFRC/MAFF project took place on 27 September 1993. Attendees were Professor Oxford, Dr Lynne Bountiff of the London Hospital, Professor Almond and Dr Oldham of the AFRC, Mr Bradley and Ms Helen Ainsworth of MAFF Science Division. The note of the meeting recorded that:

All present were very clear that the main aim was to repeat Dr Narang’s experiments as closely as possible.

There was discussion about which aspects of the protocols it was important to adhere to from a scientific viewpoint, and which were not likely to affect the final results. We all agreed that it was important that Dr Narang was satisfied that his protocols were being followed.

5.253 The meeting note also recorded that Dr Narang would be invited to attend the next progress meeting in November.

Mr Campbell-Savours writes again on behalf of Dr Narang

5.254 On 23 February 1994 Mr Dale Campbell-Savours wrote to Mr Soames on behalf of Dr Narang. He asked why Dr Narang had not yet been appointed to the LHMS, enabling him to carry out his research and to assist Dr Bountiff in repeating his work independently. In a hand-written note at the end of the letter, Mr Campbell-Savours asked:
Nicholas, can you put your hand on your heart and say ‘We are doing all we can to help this chap’?

5.255 A draft reply for Mr Soames to send to Mr Campbell-Savours was prepared by Mr Eddy. The actual reply, sent on 23 March, closely followed this draft. It informed Mr Campbell-Savours that there had never been any intention to appoint Dr Narang to the LHMS. The project at LHMS was designed to repeat independently the procedures Dr Narang had used to see if his work could be verified. He stated that Dr Narang’s presence there was for the purpose of assisting Dr Bountiff in using precisely the same protocols as he had used in his earlier studies. He highlighted the fact that MAFF was being extremely cooperative towards Dr Narang:

... everyone is doing their utmost for Dr Narang and for the funded project to be successfully concluded. Indeed additional funds for his travel expenses and accommodation have already been provided to assist in this process.

Dr Narang writes to Mr Waldegrave about his research

5.256 Dr Narang wrote to Mr Waldegrave on 1 August 1994. He described his interest in infectious diseases of the central nervous system and the tests he had developed to assist with the diagnosis of BSE. He commented:

I believe that the primary objective that we should all be working towards is animal health and the eradication of BSE from British cattle. However, for this to be effectively carried out we need to be able to identify the affected herd swiftly and relatively simply. This can be done using my diagnostic test. Rather than having a 6 years ban, one could certify the herd BSE free by this method.

5.257 He added that he had also ‘developed the basis of a live test which has been patented by the British Technology Group’.

5.258 Mrs Townsend of the Animal Health (Disease Control) Division replied on 24 August. In relation to progress with the various tests that Dr Narang mentioned, she said:

... colleagues at the Central Veterinary Laboratory and from the Veterinary Investigation Service have supplied brain material for your research. The results of that research, to the best of my knowledge, have not been published, even though we have supplied results of our studies on the same material. I understand that a further study, funded by MAFF and the BBSRC (then AFRC), designed to repeat independently your original procedures and to confirm, deny or modify the observations which led you to describe the existence of a scrapie-specific 1.2kb ssDNA with an unusual nucleotide sequence in affected hamsters, will be completed in a few weeks.
A *post mortem* test, even if shown to be effective on BSE affected cattle brain, is unnecessary for certification of the safety of beef. Since trade rules were agreed with Community Members and the Commission of the European Communities (CEC) there has been no obstruction to exports provided the certification conditions are satisfied. We are exporting beef to Member States and to third countries by complying with the certification conditions laid down by either the CEC or based on those recommended by the Office International des Epizooties (OIE), to which most trading nations belong.

This Ministry is not aware that you have developed a test that could identify infection in the animal while it is still alive. Our understanding is that your test has not been shown to identify an infected live hamster in the pre-clinical phase of disease. Indeed, I understand that you are insisting that in the present study being conducted at the London Hospital Medical School (LHMS) that hamsters need to be taken on to a terminal diseased state before ssDNA can be identified. Such a situation would be quite unacceptable in cattle for welfare reasons.

Even if an agent specific ssDNA was shown to be present in the brain (and experts do not subscribe to this view), for use in a test which would identify an infected animal which was showing no clinical signs it would also have to be shown to exist in a peripheral tissue that could be sampled in the living animal.\(^1\)\(^2\)\(^5\)\(^2\)

**Results of the jointly funded BBSRC/MAFF project to verify the basis of Dr Narang’s proposed test in the live animal**

5.259 On 21 October 1994, Mr Eddy reported to MAFF Ministers about, amongst other things, the results of the research project undertaken by Professor Oxford to verify the basis of Dr Narang’s proposed test in the live animal. By this time the AFRC had been replaced by its successor body, the Biotechnology and Biological Sciences Research Council (BBSRC). Mr Eddy commented:

The independent scientists have not been able to repeat Dr Narang’s results despite their best efforts and his involvement. He remains unabashed and has criticised some of the experiments and claims that at least one unnamed scientist in America is having more success in trying to repeat his work. Mr Bradley who, as Project Officer for MAFF, attended the meeting has a confidential report from a reputable American scientist in a TSE research laboratory of international repute which also rejects Dr Narang’s claims following experimental studies in hamsters and mice using Dr Narang’s protocols and reagents. Professor Almond (Project Officer for the BBSRC) and Mr Bradley were satisfied that Dr Narang’s protocols were followed as closely as could be expected and the conclusions of the researchers were scientifically, soundly based . . .

Dr Narang is likely to continue to press his case. I understand that a short scientific paper reporting the failure of Professor Oxford and his team to
reproduce his results will be prepared and submitted to MAFF by December 1994 for approval to publish as soon as possible thereafter.\textsuperscript{1253}

5.260 On 21 November, having seen Mr Eddy’s minute, and a later one from Mr Bradley, Mr Waldegrave asked for advice from Dr Peter Bunyan, MAFF’s Chief Scientific Adviser, ‘on the view of the Department that Dr Narang’s claims are insupportable and cannot be repeated in a laboratory of international repute’.\textsuperscript{1254}

5.261 Dr Bunyan replied to Mr Waldegrave on 1 December 1994 enclosing a detailed note on MAFF’s involvement with Dr Narang. In his covering minute, Dr Bunyan said:

I have made enquiries and I am satisfied that Dr Narang’s methods have been tested as thoroughly as possible and that the only conclusion which can be drawn as a result is that they are unrepeatable.\textsuperscript{1255}

5.262 In relation to the results of Professor Oxford’s project, Dr Bunyan’s note said:

Despite careful study and repetition of the experiments a dozen times using any and all of the variations which Dr Narang from time to time suggested to the Steering Group should be introduced, no scrapie-specific ssDNA was found. At a final meeting involving the Steering Group and a further independent, Dr Grant a retired neuropathologist, who had been a critic of the Ministry on BSE, it was agreed that Dr Narang’s claims were not repeatable. Dr Narang was present at this meeting and appears to have concurred with the conclusions, although he later questioned them in public.

... Dr Narang’s claims, which as so often happen in these types of cases seem to shift each time evidence suggests that they are insupportable, have been thoroughly tested in Professor Oxford’s laboratory by competent reputable scientists with negative results. Further studies have been undertaken at the Staten Island Laboratory in USA with apparently similar results. In the absence of any other convincing evidence the conclusion must be that Dr Narang’s claims in respect of identifying a scrapie-like specific ssDNA in hamster brains terminally affected with scrapie are insupportable.\textsuperscript{1256}

The urine test for CJD

5.263 During 1995, Dr Narang worked on the development of a urine test which he described in his statement to us:

... a simple method to concentrate the agent from CJD patients’ urine and I have by EM [electron microscopy] demonstrated the presence of both NVF [nemavirus fibrils] and SAF [scrapie-associated fibrils] similar to those seen in brain samples of CJD victims.\textsuperscript{1257}
5.264 He carried out his first urine test on a live patient with suspect CJD in 1995, and continued with ‘a number of live subjects who have subsequently died and were confirmed to have had CJD by the Surveillance Unit in Edinburgh’.1258

5.265 At the 22nd meeting of SEAC on 23 November 1995, under the item, ‘Any other business’, Professor Pattison asked Dr Wight to write to Dr Narang for details of his CJD test.1259 On 4 December, Mr G M Robb from DH wrote to Dr Narang on behalf of the SEAC secretariat. He said:

The SEAC has seen reports in the media of a potential diagnostic test for CJD which you are reported to have developed. Any such test, if clinically robust, would be valuable, and the Committee have therefore asked if you would be willing to send them details of this test, and, if possible, also of its use so far. Moreover, they would be interested to know whether you have yet submitted any papers on the test for publication, and if you have, whether you would be prepared to let them see copies.

The Committee will next meet early in January 1996, and it would, if possible, be very useful to have an initial response from you by the end of the year.1260

5.266 It would appear that Mr Robb did not receive a reply from Dr Narang. However, on 9 December, Dr Narang wrote to Douglas Hogg noting that he had heard Mr Hogg’s statement ‘on Channel 4 News on 6 December 1995 to the effect that you would like to try the brain test which I have developed for BSE.’1261 He went on to give a brief outline of his touch test and urine test for BSE.

5.267 On 18 December 1995 Dr Render wrote to Mr K Taylor, attaching Dr Narang’s letter to Mr Hogg. He stated that, in relation to the urine test:

The second test, the ‘urine test’, is, I presume, the one recently described in newspaper articles. DH have written to Dr Narang to ask for further details of this to put to SEAC. If Dr Narang responds to that request I hope it will be in considerably more detail than here, to allow for proper scientific analysis.1262

5.268 Dr Render subsequently drafted the reply to Dr Narang, which was sent by Mr Hogg’s Private Secretary, Marcus Nisbet, on 21 December 1995. He thanked Dr Narang for his approach and the offer on the tests. He continued:

As a scientist you will appreciate that all claims need to be rigorously tested and receive sound experimental support. This is, of course, usually seen to be achieved by publication of papers in independently refereed scientific journals. We are not aware that details of your tests have been published in this way and so would be grateful if you could provide more detailed information of a standard which can be properly assessed.

1258 S113 Narang para. 1.11
1259 YB95/11.23/1.1
1260 YB95/12.4/6.1
1261 YB95/12.9/2.1
1262 YB95/12.18/13.1
I understand that the Secretariat of the Spongiform Encephalopathy Advisory Committee has already written to you to ask that you provide the Committee with full details of your ‘urine test’. The Minister has asked me to urge you to provide them with full details to enable them to evaluate the tests to see if it could prove clinically reliable and robust.\textsuperscript{1263}

**Dr Narang continues to press his case with Mr Hogg and Mr Dorrell**

5.269 Dr Narang wrote again to Mr Hogg on 27 December 1995, having received the reply from Mr Nisbet.\textsuperscript{1264} He said:

> I would ask you, since MAFF have spent millions on this research, I do not see another instance where such claims were tested before grants were awarded. With all the effort so far no real progress has been made.

5.270 He noted that a company had recently been given a contract to develop a live test, ‘as far as I am aware this firm has not developed a test yet or even done any work in relation to spongiform encephalopathy research’. He continued:

> The Spongiform Encephalopathy Advisory Committee has asked for my experimental details of the live test. I need facilities so that I can make it available to farmers, I do not want some one else to exploit my test while I take a back seat and suffer.\textsuperscript{1265}

5.271 Two days later, Dr Narang wrote again to Mr Hogg, claiming that he had successfully used his urine test for CJD on a young patient who was suspected of having CJD. He invited Mr Hogg and Stephen Dorrell, the Secretary of State for Heath, to visit this young patient. Dr Narang considered that the visit would press home the importance of developing a rapid diagnostic test for CJD. He commented:

> It is strange that a number of patients under the age of 40 have contracted this disease in the past few years in the UK and therefore it is important to realise that this is a new phenomenon which suggests that there is a different source of infection in these patients.\textsuperscript{1266}

5.272 Mr Nisbet replied to this latest letter on 5 January 1996, advising Dr Narang that CJD was a matter for the Secretary of State for Health.\textsuperscript{1267} Accordingly Dr Narang wrote to Mr Dorrell on 8 January, enclosing a copy of the letter he had sent to Mr Hogg.\textsuperscript{1268}

5.273 Mr Dorrell’s Private Secretary, Shaun Gallagher, replied to Dr Narang on 19 January, turning down Dr Narang’s invitation to meet the young patient with suspect CJD. Mr Gallagher continued:

> The Government appreciates fully the seriousness of CJD. It is a rare disease, and, if a validated test were to become available, it would overcome current diagnostic difficulties. I understand the Spongiform Encephalopathy
Advisory Committee (SEAC) wrote to you on 4 December seeking details of your urine test. The Committee still wishes to give the Government an independent assessment of its clinical robustness and would welcome receiving the details necessary for this.1269

**Mr Hogg replies to Dr Narang’s letters of 27 December and 5 January**

5.274 On 21 February 1996, Mr Hogg replied to Dr Narang’s letters of 27 December and 5 January.1270 He had sought ‘expert advice on the highly technical issues’ Dr Narang had raised, and enclosed a copy of this advice with his reply. Mr Hogg’s reply focused on the various points Dr Narang had made about MAFF’s long-established mechanisms for funding research. Among other things he said:

I certainly agree that fairness is one criterion for awarding research funds but of equal or greater importance are the need for the work, the skill of the team, the resources required and available, the design of the study, its costs and priority in relation to other studies. Work with TSE is necessarily long term and usually demands a team approach in a centre of excellence though new groups have been encouraged to initiate work and some have creditable results.

I understand that any proposals from yourself have been examined in the same way as those from other individuals and institutes. If they have not been funded this is because they lacked sufficient merit or were not of sufficient priority.1271

5.275 Mr Hogg went on to respond to Dr Narang’s suggestion that a particular company had been given a contract to develop a live test without previously having experience in TSE research. Mr Hogg informed him that the company did have experience, particularly on working with blood and cerebrospinal fluid from patients with CJD. Mr Hogg continued:

It is clear to me from reading the papers that this Department and the BBSRC have gone to considerable lengths to try to confirm some of the experiments which you claim underpin your theories about the agents causing BSE and scrapie. I have to say that, despite the expenditure of considerable sums of taxpayers’ money, the results remain disappointing and I see no justification and none of my scientific advisers have proposed that we should spend more money to develop tests based on theories which are not accepted by the majority of workers in the field and which are not substantiated by research findings. I know that this will be a disappointment to you and I am prepared to ask my scientific advisers to look again at the matter if you can give us sufficient technical background for a proper research proposal. However, I am not prepared to ask them to depart from the normal procedures and to underwrite work which has only been reported in very general terms in the national press and not in sufficient detail in the peer reviewed scientific press and which you have subsequently refused to explain both to Departments and to the independent advisory committee, SEAC.1272
SEAC considers Dr Narang’s test

5.276 SEAC discussed Dr Narang’s urine test at their 24th meeting on 1 February 1996. Dr Will said that Dr Narang had allegedly diagnosed several CJD cases with his urine test based on the detection of either the nemavirus or single stranded DNA. The minutes record:

SEAC was told that Dr Narang has as yet been unwilling to give the Government details of his test. Mr Bradley expressed concern at possible safety risks involved in Dr Narang’s work, as we understand that he has no laboratory of his own and uses shared equipment. The Committee noted the major programme underway to develop diagnostic techniques, particularly for BSE, and felt that new avenues should be explored if they looked promising. This may well come about as a benefit from further basic research.

5.277 Dr Will wrote to Dr Narang on 16 February 1996 proposing that the CJD Surveillance Unit should attempt to replicate his urine test. He said:

I note from recent press reports that you have had some apparent success in using the urine test for diagnostic purposes in CJD. As I am sure you will recall from our previous discussions, it would seem to me that such a test is exceedingly unlikely to be helpful in CJD because of the wealth of evidence that suggests that conventional viruses are not involved in the pathogenesis of the TSEs. However clearly if there was any suggestion that there is a test that is of positive predictive value in the diagnosis of CJD this is something we must seriously consider here at the CJD Surveillance Unit. Accordingly, I do think it would be sensible for us to attempt here to replicate your test on samples of urine from patients with CJD.

I am sure you will agree that with any diagnostic test or development it is very important that results are replicated in other laboratories in order to achieve independent validation of any diagnostic procedure. What I am therefore writing to ask you is whether you would give us details of your protocol which I understand at least in part involves PCR of urine in order that we can test your interesting findings on an independent basis.

5.278 Dr Will suggested that they meet to discuss this proposal further. Dr Narang replied on 21 February agreeing to a meeting, stating that:

I note from your many public statements that you do not believe that BSE can be passed to humans. You also say in your fax that the urine test would be ‘exceedingly unlikely’ to be helpful in CJD cases. Given your clear beliefs in this regard, and your own department’s lack of experience in dealing with the Nemavirus, I think the best way forward would be for you and I to meet, with a view to discussing terms and conditions for having my test validated.
5.279 Dr Will and Dr Narang met on 13 March in Newcastle. They agreed to proceed with the validation of the urine test. Dr Narang wrote to Dr Will on 19 March to confirm that four points were agreed at the meeting. He said:

i. You would supply frozen urine specimens from confirmed cases of CJD to find out if frozen nature of the specimens does not interfere with the test result.

ii. That you cannot provide funding for this research. Mr Ken Bell of Ken Bell International has agreed that he would provide immediate financial needs.

iii. That you have no influence in securing lab facilities. Therefore, in order to arrange for lab space, I will write to individuals who may be able to help.

iv. If everything goes well, we could then arrange a blind study by coding specimens from suspect CJD cases mixed with normal specimens.

5.280 Dr Will replied on 10 April, advising that he would arrange the delivery of some frozen urine samples from confirmed cases of CJD. He sent a number of other faxes before he received a reply from Dr Narang on 7 June. Dr Narang stated that he had not been able to secure a laboratory space as yet, but was trying to negotiate with Newcastle University.

5.281 It would appear that the validation work proposed by Dr Will did not proceed. Dr Narang told us that it was not until 1997 that the ‘MRC funded a project at Leeds University to verify the urine test’. However, there were many problems with this project and at the end of the 18-month evaluation, no firm conclusions had been reached.

Discussion

5.282 Dr Narang’s experiences mirror those of many medical scientists who fail to attract research grant support from either the Research Councils or from the Medical Charities. The main grounds for refusal of peer-reviewed grant applications include lack of scientific merit and low priority in relation to other applications. Where an application is declined, reasons are normally given and the applicant is at liberty to reapply to the same or a different funding body. The system has stood the test of time and is considered by most scientists as being fair, and the best way of distributing limited funds so that the most deserving projects are supported.

5.283 Dr Narang only made one formal application to MAFF for research funding. This was the proposal submitted on 16 November 1989 under MAFF’s open contracting scheme. We are satisfied that this proposal received fair consideration in competition with the other applications for research funding from this source.

5.284 We wish to recognise Mr Ken Bell’s generosity in funding privately the development of Dr Narang’s touch test. The results of this were viewed differently
by Dr Narang and by MAFF. Of the ten samples, five were positive and five negative. Initially Dr Narang’s test only identified two of the positive samples, although he subsequently identified a third. He described this result as a positive identification of the presence or absence of BSE in eight out of ten cattle brains. MAFF officials considered that the test had only had a 40 per cent success rate, in making an initial diagnosis of two positive cases out of five. This was not considered sufficiently reliable to give the test any practical value.

5.285 Dr Narang gave evidence to us. We found him a sympathetic and moderate witness, devoted to his science and concerned that he had been unable to make the contribution to addressing the challenge of BSE and CJD of which he believed he was capable. The contemporary documents indicate, however, that he had difficulty in his relationships with some of his professional colleagues. More particularly, he appears to have irritated scientists at the CVL during his visit to the laboratory in 1989 which left the CVL scientists not merely sceptical of his technique, but reluctant to invite him back.

5.286 While Dr Narang’s touch test had not been a complete success, it was far from a total failure. Had he established a better working relationship with the CVL scientists, we cannot help feeling that there might have been scope for profitable collaboration between them. As it was we do not feel that MAFF officials can be criticised for insisting that Dr Narang should justify requests for further assistance by submitting the work that he had done to date for peer review and publication.

5.287 MAFF officials demonstrated that they were not turning their face against Dr Narang’s work when they agreed to join in funding Professor Oxford’s application to validate Dr Narang’s work using his experimental protocols. By October 1994 it was reported that Professor Oxford’s group had been unable to repeat Dr Narang’s results and that the claims made for the live test were unsupported. An independent study at the Staten Island Laboratory in the United States came to the same conclusion.

5.288 Dr Narang was involved in developing a third diagnostic test, this time for CJD using urine samples. EM studies on concentrated urine were claimed to be able to identify scrapie-associated fibrils and nemavirus fluids. He reported the success of this test on a young patient suspected of having CJD to Mr Douglas Hogg and Mr Stephen Dorrell by letter in December 1995. SEAC sought details of the test from Dr Narang who had not published on it and was unwilling to provide the method until he had completed his investigation on CJD patients and BSE affected animals. Dr Will offered to attempt to replicate the test at the CJDSU. But this did not happen, and it was not until 1997 that MRC funded a project on the urine test at Leeds University. This has not had a successful outcome.

5.289 We have concluded, for the reasons given above, that Dr Narang’s work received fair consideration by MAFF scientists. While we would pay tribute to Dr Narang’s dedication to research into TSEs, we feel that he had a fair opportunity to demonstrate the validity of his work but did not succeed in doing so.
Mr Mark Purdey

Introduction

5.290 Mr Mark Purdey is an organic farmer who for many years was concerned about the use of organophosphorus compounds (OPs) for the treatment of cattle for warble fly infestation. In the early 1980s, prior to the emergence of BSE, Mr Purdey had entered into correspondence with the Animal Health Division of MAFF, highlighting his concerns about the use of OPs and seeking permission to use alternative warblecide compounds (see vol. 12: Livestock Farming, for details of the use of OPs in warble fly eradication programmes). He did not accept the assurances given to him by MAFF and, in 1985, sought a judicial review of the Warble Fly (England and Wales) Order 1982. Following an out-of-court settlement, he was exempted from treating his cattle with OP compounds.1283

5.291 To the present day Mr Purdey continues to argue the case for an OP-linked aetiology for BSE.

Mr Purdey’s letter to Farmers Weekly proposing a link between OPs and BSE

5.292 Following the identification of BSE, Mr Purdey developed a theory that OPs had a possible role in the BSE epidemic. He set out his theory in a letter to the Farmers Weekly in January 1988. He highlighted the similarities between the pathological observations of BSE infected brain tissue and those of the nerve tissue of laboratory animals exposed to known OP neurotoxic agents. He wrote:

My own independent survey on some of the farms inflicted with this crippling neurotoxicity have revealed that warble fly liquids containing ‘phosmet’ and a brand of feeding stuff (compounded from insecticide-treated raw materials) are the common denominators upon these farms. Either, or both, of these inputs could serve as the delayed neurotoxic culprits to those genetically susceptible Holstein cattle bewitched by BSE.1284

5.293 Mr Purdey told the Inquiry that his independent survey consisted of telephoning five farmers who had cases of BSE on their farms in Devon.1285 He questioned the farmers about their feeding stuff policies and their use of warble fly treatment through the life of the cattle, including the time spent in utero. On the basis of his observations, he suggested a link between BSE and OP compounds.

5.294 The concluding paragraph of Mr Purdey’s Farmers Weekly letter read:

Is Farmers Weekly for free thought and development of the farmer or for shielding the embarrassing hiccups boomeranging directly from the compulsory clinical regimes resurrected from the multi-national ministerial alliance?
5.295 Mr Purdey gave us an explanation of this passage:

I suppose I was – even then had a rather cynical view over this. It is the way
that I perceived those ministerial bodies. I felt – I suppose I had a bit of a chip
on my shoulder, but I think there was some sense to what I was saying that
there did seem to be this cohesion between the interests of the multi-
nationals selling chemicals and the Ministry of Agriculture, particularly at
that time. A lot of the research that the Ministry was doing was always
tailed on the side of looking at effects of agrochemicals, rather than
looking at the alternative biological means of control. Being an organic
farmer, this was obviously causing me a sort of problem. 1286

5.296 Mr Purdey wrote a further letter to Farmers Weekly that was published on
3 August 1990 under the heading, ‘Look for missing BSE link’. He said:

I find it interesting that the 250 or so cattle that have grown up through my
dairy unit were consistently fed the brands of cake alleged to contain the
BSE agent, yet not one of them has developed BSE symptoms. Perhaps the
unique feature of my herd is that I have consistently refused and banned the
use of anti-cholinesterase-based insecticides and wormers in my
management. 1287

Suspect BSE case on Mr Purdey’s farm

5.297 On 14 July 1991, Mr Purdey wrote to Mr Keith Meldrum (CVO) outlining a
suspected case of BSE in one of his cattle. He sought a guarantee that MAFF, once
the afflicted cow was handed over for slaughter, would grant him access to the
animal so that he could carry out an independent examination of its central nervous
tissue. 1288 He recognised that such work would have to be carried out at a MAFF or
other specialist laboratory. He also outlined ‘various correlations which suggests
why BSE could be a sequel to an acute or chronic neurotoxic exposure some years
previous to the surfacing of the outward symptoms.’ He reiterated his claim that
there were no other cases of BSE on his farm although the cattle had been fed with
alleged contaminated feed.

5.298 Mr Meldrum replied to the letter on 25 July 1991 saying that under the terms
of the Form A (under the slaughter and compensation scheme) Mr Purdey must
surrender the carcass of the animal to MAFF and that they could not grant
independent access for examination of central nervous tissue. 1289

5.299 Towards the end of 1991, Mr Purdey wrote to his local DVO, Mr G W C
Wilson, at MAFF’s Taunton office. His letter included a suggestion that there was
a connection between BSE and warble fly treatment. The letter was received on
22 November 1991. In his reply dated 25 November, Mr Wilson commented:

You also suggest that there is a correlation between BSE and warble fly
eradication areas. I enclose extracts from the Chief Veterinary Officer’s
Annual Reports for 1984-1986, the years in which warble fly infected areas
were declared. As you will see these areas are clearly defined and

1286 T16 Purdey pp. 32–3
1287 YB90/8/3/1.1
1288 YB91/7.14/1.1–1.3
1289 YB91/7.25/5.1
circumscribed. The incidence of BSE, both in location and in the number of cases per 1,000 of the cattle population, show no correlation with the warble fly infected areas.

Circumstantial evidence continues to indicate a food-borne origin ie scrapie-contaminated meat and bonemeal in cattle rations. This would also account for the considerable number of BSE cases born after the final warble fly infected areas in 1986. 1290

5.300 In March 1992, an article by Mr Purdey entitled, ‘Mad cows and warble flies: A link between BSE and organophosphates?’ was published in The Ecologist. 1291 The article stated that scientific establishments were ignoring the importance of genetic and environmental factors that could influence the chances of an animal succumbing to infection by the BSE agent. He presented data that he said demonstrated that, in some cases, there was a correlation between warble-fly eradication zones and incidences of BSE cases.

5.301 In addition, Mr Purdey suggested that TSEs were linked to other degenerative disorders such as Parkinson’s and Alzheimer’s diseases in the way that they developed. 1292 He referred to a review article published in 1985 by Professor Gajdusek of the National Institute of Neurological Disorders, USA, which discussed the similarities between these diseases. Mr Purdey concluded his article by stating that ‘a link between BSE and organophosphate treatments is more than plausible, yet MAFF refuses to carry out the necessary research to prove or disprove the OP theory’.

5.302 Mr Purdey entered into correspondence with Mr Tom King, his constituency MP, informing him of his concerns and theories in order that these matters could be brought to the attention of the Ministry. 1293 Mr King subsequently wrote to Mr Gummer on 18 May 1992. In his reply of 8 June, Mr Gummer stated in relation to the suggestion that BSE was attributable to organophosphate compounds that ‘on the information currently available to us, it is not possible to draw any conclusions in support of Mr Purdey’s argument.’ 1294

Treatment of a BSE suspect with compounds used as antidotes for nerve gas

5.303 On 8 July 1992, Mr Purdey wrote to Mr Meldrum notifying him about a second suspect case of BSE in his herd. He demanded the right to treat the cow with compounds used as antidotes to nerve gas, to test it for OP toxicity, and, in the event of death, to examine the CNS tissue in a certified laboratory. 1295

5.304 Mr Kevin Taylor briefed Mr Soames on this request in a minute on 13 July 1992. 1296 He said of Mr Purdey that:

Although he claims that MAFF refuses to investigate the ‘link’ between BSE and OPs the original epidemiological study did in fact consider whether any
chemical or medical treatment was the cause of BSE, and found no evidence to support the theory.

The CVO will reply to Mr Purdey. We have no objection to samples being taken from the live cow for analysis, or to delaying slaughter provided that the welfare of the animal is not prejudiced. A veterinary officer will make regular visits to assess clinical progress, as is done for any suspect where slaughter is delayed. If welfare considerations require, the cow will be slaughtered whether Mr Purdey agrees or not. The question of access to tissues post mortem is less easily resolved: Mr Purdey will be asked to provide more detail of what he proposes and where work will be done. The prevention of any risk to human or animal health will be paramount in any decision which is taken.

On past evidence Mr Purdey is likely to seek publicity about this case to further his arguments, and we believe that he has already contacted the Times. The July issue of Dairy Farmer also carried an interview in which he expounded his opinions, although this was written before his second suspect case occurred. Press Office are aware of the situation.1297

5.305 In a letter to Mr Purdey on 14 July, Mr Meldrum agreed to the tests being carried out and treatment given, on condition that the animal’s welfare was not compromised:

. . . I do appreciate that your prime concern is that you are given the opportunity to carry out further investigations on the suspect to confirm your theories on organophosphorous toxicity. I have no hesitation in agreeing to your carrying out further tests on your animal while it is under restriction, with the proviso that it will be subject to regular clinical inspection by a veterinary officer. Our overriding concern is for the welfare of the animal, and if at any time it is deemed to be suffering unnecessarily, it will be slaughtered . . .

With respect to carrying out examinations on CNS tissue after slaughter however, I must ask for further information before agreeing to vary our procedures. Our primary concern at this stage is to investigate whether or not there are lesions in the brain which confirm the clinical diagnosis of BSE, and to conduct those investigations under conditions that do not prejudice animal or public health. We do this in our own laboratories by following strict protocols in controlled conditions, by exposing only those tissues needed for diagnosis, and by incinerating the headless carcass without further dismemberment . . .

I can assure you that we have not closed our eyes to the needs to individual farmers, or to the fact that symptoms characteristic of BSE may be caused by other syndromes. It is in our interest, as well as that of the owner, to permit differential diagnosis or treatment of the suspect while under restriction, subject to welfare considerations . . . None of our investigations, however, link either clinical signs or the development of lesions of BSE with prior treatment with organophosphorous drugs.1298
5.306 Mr Purdey’s treatment of this second BSE suspect was the subject of much press coverage during mid-July 1992. On 13 July, Mr Kevin Taylor minuted those SVS staff likely to come into contact with Mr Purdey, and others in MAFF, stating that:

All involved in this case will be well aware that the situation is being milked for publicity by Mr Purdey, with enthusiastic support from Professor Lacey. As a result all we do is subject to constant scrutiny by those who will not hesitate to use any unwise action or careless comment for their own ends.

It is therefore essential that all staff who visit the farm, or who may be asked to comment, act with the utmost circumspection. No interviews should be given to TV, radio or the press, and requests should be immediately referred to the Press Office in Whitehall. Action on the farm and contacts with Mr Purdey should be restricted to those necessary to observe the development of clinical symptoms in a suspect animal (as on any other farm with a suspect), and to protect the welfare of the cow. This implies more frequent veterinary inspection than is usual, but this is made necessary by Mr Purdey’s own demand that the animal be kept alive for treatment. The details and effectiveness of that treatment are a matter for Mr Purdey and his veterinary surgeon, not MAFF. Our concerns are BSE and welfare.¹²⁹⁹

5.307 Mr Purdey told the Inquiry that the DVO at Taunton had agreed to his request to carry out a course of treatment on the suspect BSE cow. Further:

The DVO also said that he had no objection to delaying the service of the Notice of Intended Slaughter to allow treatment to take place, provided that the cow’s welfare was not prejudiced. On 16 July I injected ‘Damson’ with oxime and atropine sulphate, pharmaceuticals carried by troops in the Gulf War as an antidote to nerve gas. Within 90 minutes the cow appeared to have remitted. On Friday 18 July Mr Budge, my vet and Mr Cohen, the MAFF vet, examined ‘Damson’. Over the following weekend ‘Damson’’s condition deteriorated. I asked my vet to continue with the injections. On Monday 19 July Mr Cohen visited my farm and told me that ‘Damson’ should be put down. Mr Budge later arrived and said that he had to take further advice before reinjecting ‘Damson’. On 20 July ‘Damson’ was down on the ground. I wanted to go to the High Court to force the Ministry to let me continue the treatment. Mr Cohen then paid me a further visit and I had no choice but to consent to ‘Damson’ being put down on welfare grounds.¹³⁰⁰

5.308 BSE was confirmed in this cow by MAFF on 20 August.¹³⁰¹ Mr Purdey wrote to the Mr Wilson on 29 September about various aspects of ‘Damson’’s case. Mr Bradley replied to this letter on 5 November with the aim of following up and clarifying some of the statements Mr Purdey had made:

That your Jersey cow, Damson (RBSE 92/23076) had BSE confirmed is beyond dispute. You say you had assumed the cow had chronic OP-induced spongiform encephalopathy (SE) and this was supported by blood tests and response to 2 PAM and atropine. If you are satisfied and agree the final diagnosis of BSE so be it. However if you are suggesting that the cow had
concurrent, chronic OP-induced SE we would like to see the supporting
evidence for this conclusion with the agreement of your veterinary surgeon.
This would include a clear description of the clinical signs (with dates of
examination) the nature, result and interpretation of the blood tests, and
when the blood samples were collected in relation to the clinical signs and
treatments.\textsuperscript{1302}

5.309 In his evidence to the Inquiry, Mr Purdey said that he supplied Mr Bradley
with the appropriate details.\textsuperscript{1303}

5.310 In his letter of 5 November 1992, Mr Bradley also referred to Mr Purdey’s
suggestions that MAFF should initiate some research into BSE. However, he said
that Mr Purdey had presented no new evidence that suggested this would be
necessary, but should he do so, his proposals could be reconsidered. He added:

However it would be helpful to us in considering any proposals for further
research to have a clear statement of the objective or the hypothesis to
test.\textsuperscript{1304}

5.311 When asked about his reaction to this, Mr Purdey said:

I felt sort of that it was putting the onus on me to do the research, which –
obviously I am a very small farmer and I have not got much money, so to do
the sort of extent of tests that would be necessary to provide some sound
scientific evidence that would in my mind spearhead MAFF to take it up was
obviously out of my pocket. All I was able to do was to do what I had been
trying to do, to test BSE cows on my farm in relation to the healthy cows on
my farm. I felt I was in a no-win situation. I was limited in what I could
actually do to achieve that requirement that Ray Bradley was putting on
me.\textsuperscript{1305}

Suggestions of maternal transmission

5.312 In April 1993, MAFF were notified by Taunton Animal Health Office that
the offspring of a confirmed BSE-affected animal in Mr Purdey’s herd, was
showing symptoms of the disease.\textsuperscript{1306} A veterinary officer at MAFF, Mrs Helen
Crea, and Animal Health Officer, Mr Keith Slater, examined the animal. They
found that whilst it was in poor condition, the symptoms were only suggestive
of BSE. A Form A notice was issued for the animal on 22 April, and its movement
restricted accordingly. Mr Purdey claimed that this was the first example of
maternal transmission and made this point to the \textit{Independent}.\textsuperscript{1307} Subsequent
inspections of this animal by MAFF officials on 10 May resulted in the lifting of
the restriction since symptoms appeared to have subsided.\textsuperscript{1308} Mr Purdey wrote to
Mr Prettejohn, his local Divisional Veterinary Officer, stating that he was unhappy
with this decision and he maintained his conclusion that the cow had been affected
\textit{in-utero}.\textsuperscript{1309} The cow however recovered and continued milking on the farm.\textsuperscript{1310}

\textsuperscript{1302} YB92/11.05/1.1–1.2  
\textsuperscript{1303} T16 Purdey p. 79  
\textsuperscript{1304} YB92/11.5/1.1–1.2  
\textsuperscript{1305} T16 Purdey p. 80  
\textsuperscript{1306} YB93/4.22/4.1  
\textsuperscript{1307} YB93/4.23/3.1  
\textsuperscript{1308} YB93/5.11/1.1  
\textsuperscript{1309} S23 Purdey para. 11  
\textsuperscript{1310}
Tom King writes to MAFF Ministers about Mr Purdey’s views

5.313 On 5 April 1993, Mr Tom King sent to Mr Gummer a letter that he had received from Mr Purdey. Mr Purdey’s letter contained details of his theory that OPs caused BSE, and suggestions of a link between CJD and the use of serotonin agonist drugs for the treatment of hypertension. He had also enclosed a letter from Professor Satoshi Ishikawa of Kitasato University, who considered that Mr Purdey’s description ‘about Mad cows to organophosphates compounds and warble fly is exactly true’ and expressed interest in the similarity between the histopathology of BSE and chronic OP poisoning. 1311

5.314 Mr Gummer replied to Mr King on 16 April, saying that detailed studies of cattle with BSE had ‘not shown any connection between the use of such chemicals, either in a primary or contributory role’. He also remarked, in relation to CJD, that ‘the Department of Health are not aware of any links between drugs used in the treatment of hypertension and the subsequent development of CJD-like illnesses’. 1312

5.315 In relation to Professor Ishikawa’s remarks, Mr Gummer commented:

> Whilst it is true that there are superficial similarities in the histopathology, as observed under light microscopy, of the lesions in brain tissue of animals affected by organophosphate toxicity and spongiform encephalopathy disease, there are clear and important differences in the ways in which the nerve cells are affected. These differences are agreed and have been extensively studied and documented by neuropathologists. 1313

5.316 Mr Gummer concluded his letter by commenting that much research was underway to investigate the many aspects of this group of diseases. He enclosed an interim report describing research that was underway which he thought Mr Purdey might find interesting.

5.317 In evidence to the Inquiry, Mr Purdey described his reaction to the letter:

> Well, when I read the list of research that was being carried out, I could not see anything at all that was relevant to my hypothesis, nothing at all. It seemed to be all tailored to investigate the official scrapie leap theory as the cause, and some work on the prion protein, but that obviously involved – you know, they were saying that the abnormal prion from sheep had jumped into cows and mutated, or whatever it was. 1314

5.318 On 17 May, Mr King sent a further letter that he had received from Mr Purdey, to Mr Gummer. Mr Purdey said that he did not think that the studies initiated by MAFF at the outset of investigating BSE were sufficient to determine whether chronic or delayed OP toxicity played a role in either triggering or exacerbating the disease. He also said that Mr Wilesmith’s report that no correlation between BSE and acute exposures to OPs could be identified, did not rule out what he considered to be the most likely cause, namely chronic OP poisoning. Further he said:

1311 YB92/12.22/1.1–1.2
1312 YB93/4.16/1.1
1313 YB93/4.16/1.1
1314 T16 Purdey p. 88
From international research into chronic OP toxicity in humanity (Professor Ishikawa is a renowned expert in this field), there are more than (as the Department terms it) just “superficial similarities” in the lesions and pathogenesis of BSE. 1315

5.319 On 16 June Mrs Gillian Shephard, the new Minister of Agriculture, replied to Mr King. She commented that:

Although OPs can react with a variety of biological substances, I am advised that OP poisoning differs so greatly from BSE that the link is most unlikely. Clinically, OP poisoning is so dissimilar to BSE that the two are unlikely to be confused, and there is no evidence to link any immunotoxic or mutagenic effects of OPs with BSE. 1316

5.320 Mr Purdey said in evidence that he did not agree with Mrs Shephard’s comments. He continued:

... she had not differentiated between acute OP poisoning and chronic OP poisoning, which show markedly different symptoms and pathology. It was a mainstay of my theory that this was a chronic problem, if you like, a high dose chronic problem. And this had been sort of ignored. This vague generalisation was in a sense a misrepresentation of what I was actually saying. Also, I can never recall having linked the immunotoxic effects of acute high doses of organophosphates to BSE because work has shown that in mice the immune system is actually required for the disease process to ensue, because when they have engineered mice with an immune knock-out, they cannot develop the disease. 1317

Article in the Sunday Telegraph about OPs

5.321 On 22 August 1993 an article by Mr Greg Neale, the Environment Correspondent, was published in the Sunday Telegraph. The article stated that:

The Government is reviewing the safety of chemical veterinary products, including some used in the home, amid fears that they could be linked to illness in farmers using sheep dip, or even bovine spongiform encephalopathy – BSE or 'mad cow' disease.

Gillian Shephard, the Agriculture Minister, has ordered the review of all veterinary products containing chemical organophosphates (OPs) including flea collars used on cats and dogs as well as on cattle and sheep.

5.322 The article continued:

An Agriculture Ministry spokesman said yesterday that the review did not mean there were grounds for linking 'mad cow' disease with OPs, as has been suggested by some researchers.

1315 YB93/5.17/5.3
1316 YB93/6.16/1.1
1317 T16 Purdey p. 89
1318 YB93/8.22/1.1
5.323 Mr Purdey commented in his evidence that:

I did not know anything about it at all. Certainly in relation to BSE, I was not aware that there was any proposed review into looking at the chemicals as a possible link to BSE or CJD.1319

Further correspondence with Mrs Shephard

5.324 On 1 Sept 1993 Mr King sent a letter to Mrs Shephard, enclosing a letter from Mr Purdey requesting a meeting with her to discuss his theories.1320 Mrs Shephard replied to Mr King on 17 Sept 1993 saying that:

In view of the highly technical nature of the issues involved, I suggest that Mr Purdey should meet my scientific advisors so that he can fully explain his views and they, in turn, can raise any points which they may have for Mr Purdey to consider.1321

5.325 Mrs Shephard also proposed that Mr Purdey:

. . . supply a pre-publication copy of his paper for distribution to the appropriate experts on a strictly ‘In Confidence’ basis (as is usual with such papers), then this would also facilitate the understanding of his hypothesis.

She suggested that he write to Mr Adrian Dixon of the Animal Health (Disease Control) Division to make arrangements for the meeting.

5.326 On 30 September 1993, Mr Purdey wrote to Mr Dixon as proposed noting that he had asked Mr King to forward his pre-publication paper. He also set out a suggested format for the proposed meeting.1322

5.327 In October 1993, Mr Purdey sent his pre-publication paper to Mr King, who in turn forwarded the paper to Mrs Shephard.1323 In her letter to Mr King of 14 October, Mrs Shephard confirmed that the paper had been sent to the Veterinary Medicines Directorate and that ‘his [Purdey’s] points will be noted and borne in mind during the consideration of the OP sheep dip issue by the Veterinary Products Committee’.1324

5.328 At around this time, MAFF complained to the Press Complaints Commission about a Sunday Telegraph article which, amongst other things, had advocated ‘a theory that the symptoms of BSE result from organo-phosphorus poisoning rather than from a transmissible disease agent’.1325 Mr Purdey told us:

The Ministry had complained to the Press Complaints Commission about articles by Christopher Booker in the Sunday Telegraph which had supported my theories on BSE and which had criticised MAFF staff. The Ministry included an evaluation of my theories and they included an extract of the paper, which I had sent to the Minister. Mrs Shephard wrote to

1319 T16 Purdey p. 92
1320 YB93/9.8/1.3
1321 YB93/9.17/1.1
1322 YB93/9.30/3.1
1323 This was later published in the British Journal of Nutritional Medicine, 1994, 4, 43–82
1324 YB93/10.14/1.1
1325 YB93/9.29/5.1
Mr King MP about this on 13 December 1993 and in the circumstances said that they did not regard what they had done as a breach of confidence. I did not accept this understanding of the situation.\textsuperscript{1326}

5.329 On 13 December, Mrs Shephard wrote to Mr King apologising for any misunderstanding about the handling of Mr Purdey’s paper. She said, however, that she did not construe the use of the material as a breach of confidence. She said that a meeting would take place on 17 January 1994 to enable Mr Purdey to discuss his paper. She noted that ‘[w]e have gone to a great deal of trouble to assemble a team of experts in the wide range of fields covered by Mr Purdey’s paper, including the flying in of an expert from the NPU in Scotland’.\textsuperscript{1327}

Meeting between Mr Purdey and MAFF

5.330 The meeting arranged by Mrs Shephard took place at the CVL on 17 January 1994.\textsuperscript{1328} It was chaired by Mr Eddy and attended by Mr Purdey, his brother Mr Nigel Purdey, Mr Wilesmith, Mr Bradley, Mr Jackman, Mr Livesey, Mr Austin, Dr Hope from the NPU, Mr Salahud Din, Dr Marrs from DH and Dr Woodward. Mr Purdey explained his theory that OPs had caused BSE by mutating the PrP gene or by action at the level of transcription or translation, and that the disease had become an epidemic because of recycling animal waste into cattle feed. Mr Wilesmith said that he had considered the use of OPs as a possible cause of BSE in his early epidemiological investigation, but had ultimately rejected it since no correlation between OP usage and BSE incidence was found. A lengthy discussion followed where Mr Purdey’s theories were considered individually in terms of existing scientific evidence. Mr Purdey said that his objective had been to stimulate MAFF interest in his theory and consider the possibility of research into it. He did not suggest avenues of further research. It was agreed to look further into Mr Purdey’s theories, especially in relation to pesticide-contaminated citrus pulp and cereal by-products, though specific research was ruled out.

5.331 A minute from Mr Eddy to Ms Mannix in the Press Office on 17 January 1994, described the meeting as amicable and as one which had resulted in agreement between MAFF and Mr Purdey on three matters, namely:

that the risk to humans is remote; that the present regulations are adequate to control the disease in cattle and that the number of cases is declining; on the importance of recycled animal protein in the spread of disease.\textsuperscript{1329}

5.332 In evidence to the Inquiry, Mr Purdey said in relation to the third point: ‘I did agree I felt it had a role, but I would not say I felt it was quite as important as the Ministry considered it.’\textsuperscript{1330}

5.333 In June 1994 Mr Purdey published in the British Journal of Nutritional Medicine his paper entitled, ‘Are Organophosphate Pesticides involved in the Causation of Bovine Spongiform Encephalopathy (BSE)? Hypothesis Based upon a Literature Review and Limited Trials on BSE Cattle’.\textsuperscript{1331} The paper described the
various elements of the theory of OP-induced disruption of prion protein synthesis as a cause of BSE, the different sources of OPs and the role of magnesium and calcium. Serotonergic deficits, genetic susceptibility, and possible auto-immune responses were also described. Mr Purdey concluded that the best way of preventing neurodegenerative disease would be by rigorous screening of chemicals before they are approved.

5.334 On 2 June 1994, Mr Eddy wrote to Mr Purdey, following up various issues raised at the January meeting along with further points raised by Mr Purdey in subsequent letters to MAFF. Mr Eddy said:

The idea that you mentioned in your letters of in vitro experiments is certainly an interesting one, but as you will no doubt recall at the meeting, there is no obvious model system which could be used at the present time to do these experiments, nor as I understand it, do you have any unique or particular chemical from the OP family which you hypothesise as the responsible agent which could be tested. Screening all OP agents, and all possible contaminants, and synergistic effects of combinations of chemicals would of course lead to an impossibly complicated set of experiments.

5.335 Mr Eddy went on to address specific points which he had promised to look at during the January meeting. Mr Purdey had been concerned about OP build up in citrus pulp, a major component of animal feed. Mr Eddy said that if the OP content of citrus pulp was responsible for the initiation of BSE, it was unlikely that BSE occurred only in the UK, since citrus pulp is an imported feedstuff. Mr Purdey had also expressed concern about pesticides binding to grain. Mr Eddy said that some residues bound to grain and some of these were toxicologically active if digested and absorbed. He went on to say:

‘Legislation is based on concentrations of free (unbound) residues. Research carried out by the Central Science Laboratory of MAFF has shown OP residues in grain could cause underestimation of total OP concentration by up to 1.5ppm. That is an acceptable error because the UK maximum residue levels (MRLs) for the OP commonly used in grain are 10 ppm except for malathion, for which the MRL is 8ppm.’

5.336 Mr Eddy concluded that:

I have to say that we remain of the view that the probability of OPs having caused BSE is very low, because the toxicology of OPs has been very extensively studied and their usage is worldwide. The epidemiological and pathological findings simply do not support this hypothesis.

5.337 Following this letter, Mr Purdey wrote to Mr William Waldegrave, the new Minister of Agriculture, on 8 July expressing his disappointment at the response from MAFF and his suspicion that his requests for further research were being deliberately misunderstood. He then went on to outline his theories.

5.338 Replying for the Minister, Mr Adrian Dixon assured Mr Purdey that his work was not being dismissed outright and that significant consideration had been given
to it. The main scientific points of Mr Purdey’s letter were considered and answered. Mr Purdey was reminded that the epidemiological evidence still supported the food-borne hypothesis for the origin of infection and was not discredited by the incidence of diseased animals born after the feed ban. Mr Purdey’s theory with respect to OP use could not, however, explain the epidemiological observations.

5.339 Additionally, Mr Dixon expressed confusion as to an apparent change in Mr Purdey’s hypothesis, which suggested that disease was induced by a chemical-only mechanism as opposed to earlier assertions that it was caused by the interaction of OPs with the scrapie-like agent. On the matter of similarities between chronic OP poisoning and BSE, Mr Dixon relayed his advice that major differences existed between the clinical and histopathological aspects of the two conditions. In reply to Mr Purdey’s request for PrPc, he was told PrP^BSE was available for research purposes and was advised that, should he wish to use this reagent, he should submit a written research proposal to the Ministry.

5.340 Mr Purdey wrote to MAFF again on 15 and 27 October 1994. Mr Dixon replied on 10 November 1994 explaining again that Mr Purdey’s theories could not adequately explain the occurrence of BSE. Mr Purdey was advised to apply for funding by contacting Dr MacOwan. He was also told that his research proposals stood a better chance of being viewed favourably if he would collaborate with a scientific body of reputable standing.

Correspondence with Dr Stephen Whatley

5.341 During 1995, Mr Purdey contacted Dr Stephen Whatley, a researcher at the Department of Neuroscience, Institute of Psychiatry in London. Dr Whatley told us in oral evidence that Mr Purdey had written to him asking if he was interested in testing Mr Purdey’s theories. This appeared to be as a result of MAFF suggesting that he ought to ally himself with somebody who was more experienced in the field of research. Dr Whatley looked at Mr Purdey’s paper and found it very difficult to understand, but felt that there was perhaps ‘perhaps a core there that required some explanation’. The paper was later published in 1996.

5.342 Dr Whatley recalled that in the course of correspondence with Mr Purdey, he suggested various avenues for him as to how the theory could be tested. Mr Purdey eventually decided that it might be possible to do some research in the laboratory. When Mr Whatley explained how things worked in a scientific laboratory, he thought that Mr Purdey had probably been dissuaded, and because of his inexperience, decided to try to raise some money to fund research instead.

MRC experiments into the link between OPs and BSE

5.343 On 28 April 1995, an article appeared in Farmers Weekly stating that the MRC would be involved in preliminary trials to test the link between OPs and BSE. The article stated that:
The trial will determine whether OPs can bind to normal prion protein and then convert it into the ‘mutant version’. If this is the case then it will also test whether the deformed protein is itself the BSE agent and if it is infectious.

5.344 Initially, it was proposed that the work be funded by Channel 4, with the intention that the results could be used in a programme about Mr Purdey as a lone voice against the scientific establishment. However, in contrast to the claims made by Farmer’s Weekly, the experiments would only test whether OPs bound directly to the prion protein.\[^{1340}\]

5.345 On 12 May 1995 Dr Ray of the MRC Toxicology Unit wrote to Mr Nigel Gregory of Lauderdale Productions, confirming that the experiment was still valid as planned.\[^{1341}\] He added that the statement made in Farmer’s Weekly that Mr Purdey had ‘strong support from the establishment’ was ‘rather overstated’. He also said that he had:

> . . . been in touch with MAFF to clarify that I am only involved in testing those first steps of his hypothesis which relate to the organophosphate-protein interaction, and not with the question of infectivity.

5.346 The report of the experiment was published on 28 July 1995.\[^{1342}\] It concluded that the experiment:

> . . . provides no evidence that any part of the protein would represent a particular target for organophosphorous compounds in general.

5.347 Mr Purdey disputed these results. In a letter to Mr Tom King, he voiced his concerns and requested that MAFF should appraise the MRC results and continue with further research.\[^{1343}\] Mr Purdey also suggested that MAFF was changing its own theories on BSE, by suggesting that very small oral doses of BSE were rendering cattle susceptible to infection. He proposed that this undermined the position of MAFF with respect to foreign cattle, which during the 1980s had been fed UK derived feed and had not shown signs of BSE. On 12 September 1995 Mr King wrote to Mr Douglas Hogg, who had succeeded Mr Waldegrave as Minister of Agriculture, Fisheries and Food, relaying Mr Purdey’s concerns.\[^{1344}\]

5.348 On 17 October Mr Hogg replied to Mr King expressing surprise that Mr Purdey should need MAFF to interpret the results of the experiment that he had designed to test his theories.\[^{1345}\] He also suggested that since MAFF did not subscribe to the OP theory, there was no reason to carry out further experiments to substantiate the MRC findings. Mr Hogg also rejected claims that MAFF had altered its theory on the cause of BSE.

\[^{1340}\] S23 Purdey para. 19
\[^{1341}\] YB/5.12/1.1
\[^{1342}\] YB95/7.28/1.1
\[^{1343}\] YB95/9.5/3.1
\[^{1344}\] YB95/9.12/1.1
\[^{1345}\] YB95/10.17/1.1
Channel Four programme on BSE and Mr Purdey’s theories

5.349 On 26 July 1995, Mr Hayward sent a minute to Mrs Browning making her aware of the forthcoming Channel Four programme which would discuss Mr Purdey’s theories on the cause of BSE.1346 It was noted that the programme would like to interview Mr Wilesmith. It was agreed that this would be agreeable as long as certain conditions were met, such as provision of a set of pre-interview questions.

5.350 On 25 September 1995, a further minute from Dr Render was sent to all interested parties, including the Minister and the Minister of State.1347 It described the background to the story and noted that:

It is unlikely that the programme will raise concerns about a link between BSE and risks to human health. Mr Purdey has previously confirmed to officials in MAFF that he does not feel that our measures to protect human health need strengthening.

5.351 The Channel 4 programme (‘Frontline’) was shown on 27 September 1995. In the programme, Mr Purdey asserted that the MRC results did in fact show the modification of PrP by OPs. Reviews of the programme appeared in The Times and the Daily Mail.1348 Following the broadcast, Dr Ray from the MRC was interviewed on ‘Farming Today’.1349 He expressed the opinion that the programme gave a distorted view of OPs, incorrectly linking the toxicity of OPs with long-term disease shown in only a few people. Dr Ray explained the experiment to us in the following terms:

We looked at the prion protein in two different forms, one more soluble than the other, and we made a comparison between that and acetylcholinesterase. Now acetylcholinesterase is the known target for acute toxicity. What we wanted to do was to make a relative comparison between the susceptibility of the prion protein and the acetylcholinesterase. Because it is possible to wind up concentration of the toxic agent so high that it becomes irrelevant to any kind of human or animal toxicology, it is best to have an internal reference. We used, therefore, acetylcholinesterase as an internal reference. If you look at the numbers, you can see that with acetylcholinesterase, there was a concentration of the protein-related increase in radio-labelling which went up to about 4,000 or 5,000 in one case, or about 8,000 counts in the other case.

MR WALKER: That other case is marked ‘A’ and ‘B’?

DR RAY: Acetylcholinesterase A and acetylcholinesterase B. When you look at the case of the prion protein, you will see there is not this large increase. There appears to be a very small, possibly suggestive, possibly significant, possibly not increase at the very highest concentration, but at most this is about 1,000 times smaller than the increase in counts with the
acetylcholinesterase. Therefore I concluded that there was no significant interaction with the prion protein.\textsuperscript{1350}

Further correspondence with MAFF

5.352 On 1 January 1996, Mr Purdey wrote to Mrs Angela Browning, the Parliamentary Secretary at MAFF.\textsuperscript{1351} He made an accusation that MAFF deliberately misrepresented his theories to others. In addition to restating his central theories, he criticised the MRC experiments, first for not using Phosmet and second on the basis that the experiments that were carried out would not address the mechanism by which disruption of the tertiary structure could affect PrP.

5.353 Mrs Browning replied on 1 February 1996.\textsuperscript{1352} The letter reaffirmed the previous position of MAFF and reasserted the advice received from SEAC. The letter also discussed the research carried out by the MRC in collaboration with Mr Purdey, stating that ‘it is not the case, contrary to what you allege, that the Government “hijacked” this research’.

Discussion

5.354 Mr Purdey believed that OPs could cause delayed neuro-toxic effects, and based his evidence of a link between OPs and BSE on the correlation between warble-fly eradication zones and the incidence of BSE. Later, he proposed that the OP-induced toxic effect led to the conversion of PrP\textsuperscript{c} to the disease-producing PrP\textsuperscript{Sc}. We are not in this volume concerned to explore the technical aspects of Mr Purdey’s theories about the connection between OPs and BSE. These have evolved over time, and are considered in vol. 2: Science. Here we are concerned with suggestions made by Mr Purdey and many of his considerable following to the effect that MAFF were blinkered in that they were not prepared to consider any possible cause of BSE other than their own theory that it resulted from contaminated feed.

5.355 Mr Purdey wrote to various MAFF officials and to his MP, Mr Tom King, in April 1993 about his theory that OPs caused BSE. He was concerned that MAFF were not responding to his ideas by initiating appropriate research, and wrote to Mr William Waldegrave in July 1994 indicating his disappointment.

5.356 In April 1995, the MRC Toxicology Unit agreed to undertake an experiment to determine if OPs could bind to prion protein and convert it to the disease-producing isoform, PrP\textsuperscript{Sc}. By July 1995, the result of this experiment was known and this indicated that OPs did not convert normal prion protein into PrP\textsuperscript{Sc}.

5.357 No further experiments were made to test the OP hypothesis until after March 1996 when Dr Stephen Whatley tested the effect of OPs on a neuroblastoma cell line. The only observed effect was an increase in the expression of normal PrP. If such occurred \textit{in vivo} in the brain it was theoretically possible that OPs could be a factor in susceptibility to the BSE agent. This type of effect could be tested by experiments in mice to determine if treatment with OPs reduced the incubation period following intracerebral inoculation with BSE.
5.358 Mr Purdey is an unusual farmer. Although not a scientist, he has mastered much of the complex science relating to TSEs. His general concern about the use of OPs as a systemic treatment of cattle is not irrational. Nor are his theories of the link between this treatment and BSE.

5.359 MAFF did not reject these theories out of hand, but considered that the epidemiology was not consistent with the observed cases of BSE being induced by treatment with OPs. Mr Purdey came to accept that feed was the significant factor in the recycling of BSE, but persisted in his view that OPs could have been responsible for the original outbreak. More recently, he has suggested that the effect of OPs could increase the susceptibility of cattle to contracting BSE. Dr Whatley’s limited experiment lends a modicum of support to this possibility.

5.360 We would commend Mr Purdey for the tenacity with which he has pursued his theories in the public interest. We understand that this has involved him and his family in financial sacrifice. We have considered the manner in which he, and his theories, were treated by Government scientists and have concluded that they received fair consideration. At times his persistence irritated MAFF officials who believed that he was barking up a number of wrong trees. To an extent they were correct, but the door is not yet closed on the possibility that OPs played a role in rendering cattle susceptible to BSE infectivity.