

IN THE HIGH COURT OF JUSTICE
QUEEN'S BENCH DIVISION

1990 R No 860
1989 H No 3689

ROYAL COURTS OF JUSTICE
 THE STRAND
 LONDON

Wednesday 11 November 1992

Before

THE HON. MR JUSTICE FRENCH

ELIZABETH REAY

Suing on her own behalf and as
Mother and Administratrix of the
Estate of DOROTHY REAY (deceased)
and as Widow and Administratrix of the Estate
of GEORGE REAY (deceased) (Plaintiff)

V.

BRITISH NUCLEAR FUELS plc (Defendants)

AND

VIVIEN JANE HOPE (Plaintiff)

V.

BRITISH NUCLEAR FUELS plc (Defendants)

APPEARANCES:

For the Plaintiffs:

MR B A HYTNER QC
 MR B F J LANGSTAFF
 MR G S READ and MISS T GILL
 (Instructed by Messrs Leigh, Day &
 Co. Solicitors, London)

For the Defendants:

MR K S ROKISON QC
 MR M G SPENCER QC
 and MR C J BUTCHER
 (Instructed by Messrs Freshfields,
 Solicitors, London)

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EIGHTH DAY'S PROCEEDINGSWEDNESDAY, 11th NOVEMBER 1992

A MR. JUSTICE FRENCH: Mr. Rokison and Mr. Hytner,
before we resume can I raise one or two matters, largely
of administration? The first is that as already
B announced the families are being spared the distress of
having to give evidence. The knock-on effect of that is
that I do not foresee, rightly or wrongly, that there
will be any evidence before the Court as to the general
background, dates of birth, parents, children, occupation
and so forth, those will not be contentious as I
understand it, and I was wondering whether the matter
could be dealt with neatly by an agreed timetable of such
events as that?

C MR. ROKISON: May I just mention in relation to
that, my Lord, your Lordship may have recalled but we
will come to it in literally a few moments, that for the
purposes of the assessment of dose to the individuals
from the environment Dr. Stather has, of course, had to
take that data into account and he does actually
summarise it in Chapter 5 of his report on page 44 and I
understand that aspect is not in issue.

D MR. JUSTICE FRENCH: Yes, but what I was hoping for
with the assistance of perhaps your learned junior is a
timetable which epitomises that so that

MR. HYTNER: Does your Lordship need anything more
than is in the opening because I am sure that the
Defendants will agree the accuracy of that which is in
the opening?

E MR. JUSTICE FRENCH: If that be understood then
that meets the

MR. HYTNER: If your Lordship wants more than that
we will get it.

F MR. JUSTICE FRENCH: No, it is not that I want
more, it is just that I would welcome a tabulation.

MR. ROKISON: The answer is we can very well
tabulate what is found in the opening and what is found
in anything else to the extent to which it adds to it, my
Lord, certainly.

G MR. JUSTICE FRENCH: I am really seeking help from
your labours to diminish mine. That is the first point
and obviously there is no urgency to that. The second
point is that we have here what you call the white
bundle, though I have it in black.

H MR. HYTNER: My Lord, I am calling it white but I
am the only one that has it in white - P1.

PROCEDURAL MATTERS

A MR. JUSTICE FRENCH: Yes, Pl. Only a very limited number of documents in Pl were referred to. Is it likely that more will be referred to or is it sensible to abstract from Pl those referred to and forget the rest?

MR. HYTNER: My Lord, I think the bundle included the documents that are going to be put to Dr. Stather and I think Mr. Read will be putting some in addition to Dr. Stather but my Lord other than that it would certainly be sensible to prune that bundle.

B MR. JUSTICE FRENCH: The third matter I thought it right to raise at this stage is this, that the agreement as to occupational dose concludes with a proper and no doubt necessary reservation that the agreement is for the purposes of the work to be done by the epidemiologist - I am summarising of course, rather than quoting. The dose will be a matter to be considered by the biologists, C geneticists, in due course, and I thought it right to raise now whether having regard to the reservation which appears at the end of the agreed statement

D MR. HYTNER: My Lord, I shake my head straightaway. My Lord, it was most certainly not an agreed statement. That is why Dr. Strong was brought for cross-examination. My Lord, otherwise his statement could simply have been put in. My Lord, the doses are agreed. My Lord, I cross-examined specifically on what the basis of the doses was because I was concerned about some of the reservations, as it were, put in by Dr. Strong. My Lord, the answers on which I rely are the answers to the questions I very deliberately put, and the answers were that these were Dr. Strong's best estimates of dose. My Lord, that is what I shall be relying on.

E MR. ROKISON: My Lord, may I just say that is not right. It is not correct, what my learned friend has said. It is true that they are, not Dr. Strong's best estimates of dose, they are Dr. Strong's on the one side and Dr. Dennis and Dr. Lambert on the other's best estimate. But the reservations which were inserted in F Dr. Strong's report to your Lordship were reservations which were agreed between the experts. In other words, although those figures which have been arrived at are their best estimates and may be used as best estimates not only for the purposes of epidemiology but also for the purposes of genetics, but what is important and we shall in due course, and no doubt my learned friend too in a different context may rely upon, is that the experts G are agreed that there is a considerable amount of room for doubt and argument in relation to what are the precise doses. What they have done is to arrive at a best estimate with the reservation that for the reasons there set out those doses cannot be regarded as precise, particularly in relation to those other than Mr. Reay and Mr. Hope. That was not Dr. Strong's evidence that was, H as I understand it, a matter which was agreed between the

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experts and then set out by Dr. Strong in that report, where what he was doing was recording the agreement of the experts.

MR. HYTNER: My Lord, I am happy to agree that statement. I think I was concerned, as your Lordship clearly picked up, that the reservation at the end of Dr. Strong's report related to epidemiology. My Lord, I am content to agree what my friend has just said.

MR. JUSTICE FRENCH: It is then understood, correct me if I am wrong in this statement, that the figures which have been achieved and which are the best estimate of Dr. Strong's and Dr. Dennis's evidence are to be used as such by the biologists and the geneticists for the purpose of their evidence.

MR. HYTNER: Yes, my Lord.

MR. JUSTICE FRENCH: It was simply that the reservation left open, subject to your agreement to the contrary, the utility of those figures when dealing with genetics.

MR. HYTNER: My Lord, your Lordship has picked up the one point on which I was concerned and why I was cross-examining as I did. My Lord, we seem to be agreed.

MR. ROKISON: Yes, we are.

JOHN WHARWICK STATHER Recalled

Examined by MR. ROKISON (Continued)

Q. Dr. Stather, good morning. I just want to pick up if I may just very briefly the topic which was being canvassed in your evidence last thing yesterday afternoon before my Lord rose, and you were being asked about, in particular, your Figure 4.2 which appears at page 41 of your report.

A. I have it.

Q. In particular you were asked questions about the peak which appeared on that figure at the end of 1954 and you told my Lord that you had made an assumption that the strontium-90 levels which had been found when monitoring was first started in 1958, you had assumed for the purposes of your assessment that had all derived from one discharge of uranium oxide in 1954, even though you thought that it had spread over a longer period. I had asked you, and I merely raise this again because I do not think you actually answered the question, at the bottom of page 80G of the transcript of yesterday's proceedings I ask you this:

"I just wanted to clarify this.

Q. I think you said in your evidence that making that assumption that it was one discharge in

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mid-1954, I think you said that was a cautious assumption to make."

and in that I was referring back to your evidence on the previous page between F and G, where you said:

"In practice some of the release would have occurred in later years but we felt this was a cautious assumption to make"

What I was trying to clarify - and again, my Lord, forgive me, it may be that it goes without saying and it is obvious but I just want to clear it up with Dr. Stather - I say:

"You cannot actually infer from what you find in 1958 when it occurred"

Just pausing there, would that be right, just by looking at the quantity?

A. That is correct.

Q. "... but if you take it back and assume it all occurred in 1954, then you are having a greater quantity with therefore potentially greater effect? Is that correct?"

A. Yes, it is a worst case assumption. I could draw it on the paper behind me if it would help.

Q. MR. JUSTICE FRENCH: Any clarification is welcome. (The witness drew a diagram to illustrate his evidence.)

A. The first year, as I said, for which we had comprehensive measurements of the levels of strontium-90 in milk was 1958, so clearly the release of UO-2 occurred before that, so we have to extrapolate that and we have a food chain model which will predict concentrations of strontium-90 in milk in previous years, and it would give a concentration that would change in time something like this. So if we assume the release occurred in 1954 that infers a higher concentration than if we assume the release occurred in, say, 1957, as the concentration falls with time. So if we extrapolate back to 1954 that is the worst case assumption. The only other point I made yesterday was if we assume the release was in mid-1954 in practice the concentration in milk tends to be somewhat lower in 1954 than in 1955, because in 1954 we have half a year, in 1955 we have a full year. So that would be the concentration in 1954, 1955, 1956 and 1957. I could usefully go on if you wish.

Q. Can I just interpose a question here? Again the answer may be obvious in the light of what you said but you have no reason to suppose that factually there was a large single release in 1954?

A. The information we had from BNFL was that there were substantial releases in '54, '55, with some releases into later years.

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Q. So you are assuming not one large release but a series of large releases and you centre them

A. Centred in the middle of 1954.

Q. You worked on the factual basis that there had been not a single large release in 1954 but a series of large releases centred upon 1954?

A. Mid-1954.

Q. MR. ROKISON: Just to clarify that, it may be that I am going to confuse things but I hope not, the factual information you have, you said, was that there were a series of releases probably from 1954, 1955 and some thereafter, and I think you said - the only question I want to clarify is this - you told my Lord I think that you were centring your release in 1954. Was that - it may come from terminology and the question of what you mean by an assumption - did you assume as a matter of fact that it had been centred in 1954 or did you make that assumption as a cautious assessment for the purposes of your assessment?

A. We made that assumption for the purpose of the calculations in the assessment.

Q. MR. JUSTICE FRENCH: The assumption is a theoretical assumption?

A. Yes.

Q. Not something which was based on information you received as to releases centred upon 1954?

A. The information we received was a guide in terms of how we would do the calculations and we made the assumption for the purpose of calculation that the release occurred in mid-1954.

Q. Yes, in order to give a focus to factual information that there had factually been releases straddling 1954?

A. Yes, and in practice they occurred in later years as well but we believed that assuming all the release occurred in mid-1954 for the purpose of calculation that would be a worst case assumption.

Q. You believe factually that there had been releases straddling mid-1954 and in later years for the purpose of figure 4.2, page 41?

A. Yes.

Q. A worst case assumption was made that the entire release had been mid-1954?

A. Right.

MR. ROKISON: My Lord, I hope that has clarified that point. I am sorry it took a little time.

MR. JUSTICE FRENCH: Not at all. I am bound to say I do not really see why it was useful or helpful to make that worst case assumption.

MR. ROKISON: I will ask Dr. Stather:

A Q. Why in making an assessment as you did for the purposes of the R171 Addendum did you make a worst case assumption?

B A. I think it is one instance of where we tend to make cautious assumptions when we do not have the complete set of data to work on. If we had had more comprehensive information we might have made the assumption that the release occurred uniformly over the whole period of time, if there was factual evidence to base that assumption for the purpose of calculation, but we did not have that comprehensive information so we made what we believed was a worst case assumption for the calculations that the release occurred in mid-1954.

C Q. MR. JUSTICE FRENCH: Can that be paraphrased in this way, that you were assuming that a very high release causing acute doses was more favourable to the Plaintiffs than a lesser dose occurring over a longer period of time?

A. These were calculations that we made in R171 Addendum for COMARE.

Q. I am sorry, can you answer the question

D MR. ROKISON: My Lord, forgive me, I am intervening only to try to clarify - your Lordship said "more favourable to the Plaintiffs" but this was not an exercise done for the purposes of this case this was an exercise that was done in 1984 and for the Addendum in 1986, for the purposes of the 171 and 171 Addendum.

E MR. JUSTICE FRENCH: Thank you, I now follow the defect of my question:

Q. Why was it more helpful to COMARE to make that worst case assumption?

A. I think whenever there are uncertainties in a modelling exercise the tendency is to err on the side of caution.

F Q. That leads to the next question: in what sense was it a worst case assumption?

A. Because it would give rise to higher doses than if we had made the assumption that the release had been spread over a period of time.

Q. That assumes then, does it not, that the acute dose then tailing off would be more harmful than if the dose were spread over a longer period?

G A. The way we calculate risk, the risk we calculate is simply a function of the radiation dose, so the greater the dose the greater the risk, in proportion.

Q. It is a dose related risk?

A. Straightforward dose related risk.

H

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A MR. JUSTICE FRENCH: That I think I can follow. Although wrongly expressed factually because it was a COMARE exercise, the inwardness of the question remains very similar.

MR. ROKISON: It does, except may I try to refine it even further through the witness, my Lord?

MR. JUSTICE FRENCH: Before you do that, let me continue and complete my note. I interrupted myself.

B Q. The worst case assumption was made because of the further assumption that risk to people from radiation was greater the more acute the dose?

A. No, the greater the dose - the greater the risk, the greater the dose.

C Q. Yes, but dose spread over is still the dose, dose which is large but relatively short in time is still the dose?

A. Yes.

Q. So isn't it the acuteness of the dose?

A. No, it is just the greater the dose, the greater the risk.

Q. Whether spread over minutes or years?

D A. In these cases for protection purposes and these types of calculations, they are all essentially low doses not acute doses that would cause acute effects in the short term.

Q. Perhaps I am using the word "acute" incorrectly.

E A. By high doses we would be talking about doses very, very much higher than the sort of doses we are concerned with here.

MR. ROKISON: My Lord, I do not think it will be controversial, may I try by leading? If my learned friend objects I will, of course, stop but I am seeking to save time to try to clarify the point.

F MR. JUSTICE FRENCH: By all means.

G Q. MR. ROKISON: Dr. Stather, in the diagram that you have drawn there your starting point is your measurement of strontium-90 in 1958?

A. Yes.

Q. In theory that measurement could be accounted for by strontium-90, subject of course to total decay, but having been deposited at any time before 1958?

A. Yes.

Q. But you looked to the available evidence to see when it is likely to have been deposited?

H A. Correct.

A Q. And you say you take a worst case because you then allocate it all effectively to one discharge which occurred at the beginning of the period over which you think it happened?

A. Yes, that is correct.

B Q. The result of that, as that shows, would show a greater deposition. In other words if, because of the decay, both physical and in relation to the decay of the radionuclide, the earlier it occurred in order to account for what was found in 1958, the greater that deposition will have been? Is that correct?

A. That is correct.

C Q. So that by making that worst case assumption one arrives at the theoretical greatest amount of deposition which you think there could have been in order to account for what was found in 1958?

A. That is correct.

D Q. However, that is to do with deposition and is not directly related to dose because since you were talking about strontium-90 being taken up through milk, once you have the deposition there that would be taken up through the milk over a period of time. Is that correct?

A. This curve gives the concentration of strontium-90 in milk as a function of time after that assumed deposition in 1954. It is intakes of strontium-90 in milk that give rise to dose.

E Q. Right, so that the mere fact that you postulate and assume for the purposes of your modelling that you have one big discharge in 1954 does not mean that the dose to the population which derives from that through strontium in milk was a dose that was all received in 1954?

A. The pattern of dose to the population reflects this changing concentration of strontium-90 in milk.

F Q. MR. JUSTICE FRENCH: I think I have no problem following the proposition that the extent of strontium-90 in milk declines with the passage of time. That is what the graph shows.

A. Yes.

G MR. ROKISON: The dose is not all received, as your Lordship put it, in an acute way when the discharge occurs because the pathway which Dr. Stather is considering is the pathway through ingestion of milk. So far as that is concerned, therefore, his assumption will only give you the greatest amount at the earliest time, so that it will maximise dose but that dose will nonetheless be received over a period of years:

H Q. Is that correct?

A. Yes. Strontium-90 has a physical half-life of 30 years so once it is there, it is there for some considerable time but because strontium-90 becomes progressively less available to the food chain the concentration in milk and in other foods will fall with time.

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A Q. MR. JUSTICE FRENCH: Yes, that I do see. Let us perhaps take a step back again. Is this proposition correct? The worst case assumption was made because of the further assumption that the risk to people from radiation is greater the greater the dose?

A. Yes.

B Q. Is this proposition correct, before I write it down? Figure 4.2 is a worst case assumption as to deposition rather than as to dose?

A. Yes.

B Q. 4.2 involves a worst case assumption.

C Q. MR. ROKISON: Can I take that a little further? Insofar as you make a worst case assumption as to deposition, does that in turn give you a worst case assumption in relation to dose from that deposition?

C A. Clearly you have a concentration of strontium-90 in milk in 1954 which is calculated using a model, and that relates to a deposition that you calculate but do not measure in 1954, and that calculated deposition is of the order of 5,000 Bq per metre squared. In practice the measurements suggest a value about half of that, so that is a calculated deposition of strontium-90 on the ground in 1954 that corresponds to this calculated concentration of strontium-90 in milk in 1954.

D Q. MR. JUSTICE FRENCH: Would you say that again so that I can record it?

A. We have a calculated concentration of strontium-90 in milk in 1954 and we can also calculate the deposition of strontium-90 on the ground that corresponds to that concentration.

E Q. That corresponds with that concentration of S-90 in milk.

F Q. MR. ROKISON: Going back one stage, you said to my Lord that directly - and I respectfully have no questions about that - what you were doing was taking a worst case assumption in relation to deposition but is the result of taking that worst case assumption in relation to deposition that you are consequently making a worst case assumption in relation to doses from that deposition?

A. Yes.

MR. ROKISON: That probably goes without saying as well.

G MR. JUSTICE FRENCH: I think one must follow from the other.

MR. ROKISON: Yes. I am grateful, my Lord:

H Q. Perhaps we can go on.

A. Maybe I should extend this one more stage. The essential point is that strontium-90 is not the only radionuclide

that is released with the uranium oxide particles. There are other radionuclides that are also important in terms of giving dose to people - caesium-137, for example. If we calculate the amount of strontium-90 on the ground in 1954 we know the ratios of different radionuclides in the uranium oxide particles, so we can then calculate the amount of caesium-137 on the ground in 1954 and we can use models to calculate doses.

Q. MR. JUSTICE FRENCH: Yes, I have, I hope, no difficulty grasping that but perhaps I had better write it down in case I forget. The assumed deposition of S-90 enables a calculation of the accompanying caesium-137.

A. And other radionuclides.

Q. And radionuclides which would have been present in the uranium oxide?

A. Yes.

Q. MR. ROKISON: I think you told my Lord that the calculated figure for the energy deposited being 5,000 Bq per metre squared, which you calculate for these purposes

A. Is about twice the measured value.

Q. Which you cross-check against other radionuclides found, is that right?

A. The measurement of strontium-90 is about half the value we calculated.

Q. So that tells you it is a cautious assumption, does it?

A. Yes, and the same is true for caesium-137.

Q. MR. JUSTICE FRENCH: So the 5,000 Bq per metre squared

A. Approximately.

Q. Yes, obviously, is the entirety of the radiation which you assumed from that amount of uranium oxide, or is it only ...?

A. It is the amount that we calculate for the area around Seascale.

Q. MR. ROKISON: But it is the whole of the energy spectrum? You are not just talking there in your 5,000 Bq

A. This is specifically strontium-90.

Q. MR. JUSTICE FRENCH: I see. It does not include the other ...?

A. No.

Q. 5,000 Bq per square metre around Seascale is attributable to strontium-90 and excludes the other radionuclides.

A. Yes.

Q. MR. ROKISON: And you said that is about twice what was measured in 1954?

A. In later years, in 1958.

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Q. MR. JUSTICE FRENCH: What is twice? The 5,000 Bq?
A. The calculated value is about twice what was measured some years later, in 1958.

Q. The calculated value and that is the 5,000 Bq per square metre?

A. Yes.

Q. I.e., is twice that found on the ground?

A. In the late 1950s. I forget the exact year of measurement.

MR. ROKISON: My Lord, I am going on from this topic now.

Q. Can I come back to page 34 of your first report please, Dr. Stather? There are some matters I think we can take quite shortly. You are still here, just to clarify it for my Lord, talking about the exercise that was carried out for the purpose of the R171 addendum?

A. That is correct.

Q. We have referred to the top of the page on page 34 which is where the Figure 4.2 is picked up. You refer to changes which are reflected in your figures over the years. Then you come on and deal with other main sources of radiation exposure, natural radiation, medical, radiation from weapons fall-out, which you took into account?

A. Yes.

Q. Then over on page 35, the first full paragraph there, you say:

"The levels of radiation exposure of the population from all the radiation sources, other than natural radiation, have varied with time over the period of operation of the Sellafield Plant and as a consequence the radiation dose to any child depends on its date of birth".

Then you refer to Table 4.1 which is at page 38, which gives a summary of the radiation doses by source for typical children born every fifth year between 1945 and 1975, and they are calculated to age 20 years or 1980. Are they total effective doses or are they also ---

A. These are doses to the red bone marrow.

Q. Are they committed doses?

A. They are doses up to age 20 years.

Q. You say this:

"The children receiving greatest doses were those born in the mid 1950s, mainly as a result of discharges from the Windscale piles. The cumulative doses resulting from the Sellafield discharges, including the Windscale fire, are always less than those from natural radiation".

We find that in Table 4.1. You have set out the Sellafield discharges and the Windscale fire, being the first two sources, and then you deal with weapons fall-out, medical exposure and natural background, and one can compare for any particular year?

A. Yes, and clearly natural background dominates the doses from the other sources.

Q. Now you say:

"As a consequence of the revised estimates of discharges from the Sellafield plant made in R171 Addendum there were a number of changes in the dose estimates given in that report compared with those given in R171."

We know that 171 was 1984 and the addendum was in 1986?

A. That is right.

Q. Which took into account, amongst other things, the Jakeman factor as we have called it?

A. Yes.

Q. You then refer to Appendix D. I do not want to refer you to much of Appendix D, but I would respectfully invite your Lordship to read Appendix D if you would be kind enough to do that. I do not want to spend time reading through it. It may be worthwhile simply reading the first paragraph of the introduction, which we find at page 1 of Appendix D, just to show what it was:

"In principle, the methods used to calculate radiation doses to persons living near nuclear establishments are straight forward. Discharges from a site can be either to the atmosphere, to the sea, to fresh water or to some combination of all these, and will result in increased levels of radioactivity in local foods, air and other environmental materials. Provided these levels can be measured, or predicted, and the habits of the population are known (the amount of air people breathe, the quantities of various foods they eat, the time spent on the beach, in the home, etc), intakes of radionuclides by inhalation and ingestion or levels of external radiation exposure can be determined. This allows the calculation of radiation doses to tissues, and hence the risk to individuals and populations. In practice though, a wide range of radionuclides in a variety of chemical forms can be released from nuclear sites. Their different behaviour in the environment, the range of potential pathways of exposure, difference in metabolism due to age, gender, etc, as well as uncertainties in the relationship between radiation dose and risk of radiation-induced disease, make any assessments fairly complex and subject to a number of uncertainties".

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A Then you deal with the various pathways, you discuss data in relation to the habits of the population, and you calculate doses and leukaemia risks. We find the conclusions which you have reached in Appendix D referred to in the main body of your report at page 35, where you say:

B "The doses calculated for children in Seascale were used for estimating the risks of radiation-induced leukaemia using the risk coefficients adopted in R171 and summarised in Appendix D."

B You refer to a total of 0.1 radiation-induced leukaemias being predicted in the study population of 1,225 children from all radiation sources:

C "The average risk to an individual is thus about 1 in 12,250. Natural radiation is the main contributor to the risk (66%) with weapons fallout and medical exposure each contributing about 9% and the Sellafield discharges, including the Windscale fire, 16%. For both Sellafield discharges and natural radiation high LET (alpha) radiation is calculated to contribute about 20% to the total risk".

D Then you deal with the Seascale population and you apply those factors and conclude at the top of the next page that the average risk of radiation-induced leukaemia in an individual child, from the Sellafield discharges (including the Windscale fire) was about 1 in 75,000?

A. Yes.

Q. In the next paragraph you say:

E "Within the Seascale population individual children will have received higher and lower doses than those calculated for the typical child. Radiation doses were calculated in R171 for individuals who could have received higher exposures than the average as a result of exposure to pathways that could affect only a few individuals in the population or as a result of extreme habits."

F In particular you refer to the high consumption of seafood and the disease known as "pica"?

A. That is right.

Q. A couple of paragraphs later, you say:

G "The increases in calculated doses even on a very pessimistic assumption were, however much less than would be needed to account for the observed leukaemias in the population".

H So your conclusion at page 36, paragraph 4.9, I think we should read:

A "It was concluded in R171 Addendum that while there remained some uncertainty about the releases from the Sellafield plant and concentrations in foodstuffs in the area, it seemed most unlikely that environmental activity resulting from the plant's operations could have resulted in the approximately tenfold increase in the incidence of childhood leukaemia observed in Seascale. For the five fatal leukaemias in the study population to be caused by the operations of the plant the radiation doses and associated risks from environmental radioactivity resulting from the Sellafield discharges would have had to be increased by a factor of about 300 for the whole period of the operation of the plant. This would have implied doses to all the children and young persons in the Seascale population of rather more than the annual dose limit for workers who are occupationally exposed The possibility that the doses calculated in R171 Addendum or the risk coefficients used for radiation induced leukaemia could be so substantially wrong seems most unlikely".

B
C
D You start that by talking about "there remained uncertainties", and you finish off by saying "seems unlikely". Has the conclusion which you there expressed changed in the course of work you have done since R171 Addendum?

A. I think that if we did these same calculations again there would be some small changes to doses, but nothing that would materially alter that calculation and that conclusion.

E Q. Thank you. Can we move on to Chapter 5? In Chapter 5 you discuss the methodology that you have used in the report to estimate radiation doses and risks. Again may we just read the introduction paragraph 1 together:

F
G
H "In this report, radiation doses are required for Dorothy Reay and Mr. and Mrs. Reay and for Vivien Hope and Mr. and Mrs. Hope. Radiation doses have been calculated for the main sources of radiation exposure. These are: routine and accidental discharges from the Sellafield site; fallout from nuclear weapons and natural radiation. Radiation doses to Vivien Hope from the Chernobyl accident have also been calculated. Radiation doses may be received from medical exposures but these are not considered. The doses calculated in the report that relate to the Sellafield site are concerned with those received off-site, i.e., from activity associated with radioactive materials released into the environment. No consideration is given to assessing the radiation doses that might have been received by Mr. Reay, Vivien Hope and Mr. Hope from occupational exposure at the Sellafield site."

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Then you set out - that was what I was referring to in answer to his Lordship's point this morning - briefly the relevant life histories of those involved. Then at paragraph 4.5 you explain the assessment procedure, the method of calculating doses which are referred to in Appendices D and E, and you have assessed radiation doses from both external radiation and incorporated radionuclides resulting from the release of radioactive materials into the environment from the Sellafield plant?

A. Yes.

Q. At the bottom of the page you refer to the various routes, the pathways, and then you say at the top of page 46:

"As far as possible calculation of doses has been based on measured values of the appropriate parameters When measurement data were not available, the discharge data were used ..."

I think you explained to my Lord yesterday that as far as the ingestion pathway is concerned, you relied upon measurements?

A. As far as was possible.

Q. As far as inhalation was concerned you had to rely more on ---

A. Much more dependent on information on discharges.

Q. Which of those two pathways is the more significant contributor towards dose?

A. Ingestion generally is the more important contributor.

Q. MR. JUSTICE FRENCH: Ingestion including that ingestion which is preceded by inhalation?

A. When I talk about inhalation, I mean all the dose that would come from the inhalation pathway, so that would include material that was cleared from the lungs and was subsequently swallowed.

Q. So that is inhalation?

A. That would still go down as an inhalation dose.

MR. JUSTICE FRENCH: Thank you.

Q. MR. ROKISON: Even taking that into account, ingestion is the major pathway?

A. Yes.

Q. Then you deal with the discharges from site in paragraph 5.3. Can I just pick it up at the bottom of page 46 to clarify a point perhaps which my Lord raised yesterday in the course of Professor Jones' evidence? You say:

"Additional releases of radioactive materials have occurred during various incidents, most notably as a result of the release of irradiated fuel particles

A from the Windscale piles in the mid 1950s and as a result of the Windscale fire in Pile No. 1 in October 1957. A list of accidents and incidents taken into account in the previous calculations by the Board are given in Annexes D1 and D2 in Appendix D".

B It may be of interest to just look at those to see the incidents that have been taken into account. They are to be found in Appendix D at page 19 onwards. First of all, Annex D1, "Incidents at Sellafield Involving Abnormal Releases of Radioactivity to the Environment, 1952 - 1981", and then there are set out in chronological order the various incidents, and then Annex D2 is "Summary of Data on Releases of Radionuclides from Sellafield Supplied to the Board by BNFL since July 1984". One can see that there is a substantial list of data which have been supplied which includes in particular at page 23 - and it relates to a question that your Lordship asked yesterday - atmospheric discharges of iodine 131, listed in 8.

Q. MR. JUSTICE FRENCH: There is nothing pre-October 1957 though, in either list?

MR. ROKISON: I think that is right, my Lord.

D Q. Were all these taken into account for the purpose of the exercise which you carried out?

A. For the present exercise, yes, they were.

E Q. There was a question raised - and we can go to the transcript if your Lordship likes but I do not know that it is necessary - about the possibility of short-lived radionuclides being discharged into the environment which would have disappeared in some way so that they were not susceptible to measurement.

MR. JUSTICE FRENCH: Disappeared because their lives were so short.

F MR. ROKISON: In one way or another, either through the half life of the radionuclide or perhaps because it is something which disappears by natural reasons.

G Q. My Lord asked Professor Jones yesterday - I do not know whether you were in Court - about the possibility that there may have been significant discharges from Sellafield of short-lived radionuclides which may have affected the local population but which would not have been measured or noticed at the time and would have disappeared, the evidence for them would have all gone. Do you have any observations about that?

H A. I really don't think I can add anything to Professor Jones's comments. He is familiar with the operations of the plant and that is not, I think, NRPB's expertise. Certainly you cannot look for information from environmental measurements in terms of whether there might or might not have been releases of short-lived radionuclides.

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- A Q. So far as iodine is concerned, would that be a relevant question for the purposes of the target tissue in this case?
- A. In that particular case, no, because iodine gives a dose predominantly to the thyroid gland and the dose to the thyroid is about 1,000 times the dose to other tissues, because radioactive iodine concentrates selectively.
- B Q. MR. JUSTICE FRENCH: I follow that, but have I got it right that the thyroid plays a part in the development of tissue?
- A. Well, it affects the metabolic rate.
- Q. It affects the metabolic rate but not the metabolic function?
- A. If somebody has an under-active thyroid, they will tend to be very slow and lethargic, so it does affect the general response of the individual; and the reverse is true with an over-active thyroid gland.

C MR. JUSTICE FRENCH: Yes, certainly.

D MR. ROKISON: My Lord, it is obviously to be contrasted with the thymus which, of course, is quite different. The thymus may be related to the production of cells but not, I think, the thyroid.

D MR. JUSTICE FRENCH: Yes.

MR. ROKISON: My Lord, we are not aware of anybody placing any reliance upon that point.

E MR. JUSTICE FRENCH: If no point is taken on it, then it is a point to be disregarded.

- E Q. MR. ROKISON: Then you deal on page 47 with discrepancies between discharge data reported by BNFL and environmental monitoring data which were noted. You say at the end of that second paragraph on page 47:

F "Measurements of plutonium in autopsy samples from people who had lived in the Seascale area suggested intakes of plutonium had not been underestimated. Similarly measurements of caesium levels in people living in the area were generally consistent with the discharge data".

G Then you refer to subsequent review of the methods and the fact that BNFL, as we have heard from Professor Jones, have reassessed estimates of discharge to atmosphere. In particular on the next page you refer to the B204 stack discharges and SEFs?

- A. That is right.
- H Q. That is a matter which was fully considered with Professor Jones. I do not want to ask you any more about that, unless you have anything to add?

A. No, I think we included all the information from Professor Jones in the assessment that we undertook.

Q. Then you deal in detail with the pathways of exposure, first of all discharges to sea and the measurements that are taken in relation to that. Then at the bottom of page 49 you deal with discharges to atmosphere and the important pathways which you set out at the top of page 50, that is inhalation of radionuclides in the atmosphere, ingestion of radionuclides in locally produced foods, inadvertent consumption of soil, and external dose from deposited activity and radionuclides in the air. You say:

"Other routes are possible, but these either lead to relatively small intakes and doses or apply to only a few individuals."

That is individuals with particular habits?

A. That's right.

Q. Then you say, which is a point that you have made before:

"In assessing the intake of radionuclides from each of the routes considered, the same approach was adopted as for liquid discharge pathways. Measurements of radionuclide concentrations in environmental materials were used where possible and supplemented by estimates based on discharge data supplied by BNFL as necessary The intakes of radionuclides via the various pathways were determined from the environmental concentrations using inhalation or ingestion rates that are considered appropriate for the cases and their parents and were based on information in the interrogatories supplemented by data on average habits of the population."

You refer at the bottom to the fact that you treated two incidents separately, that is uranium oxide and the Windscale fire. You deal with other pathways on page 51, in particular inhalation and ingestion of household dust, but your conclusion at the start of the second paragraph is:

"Both studies" - one of which is in evidence or is to be in evidence before my Lord, that is Professor Goddard's study - "suggest committed effective dose equivalents from intakes in a year in Seascale from both ingestion and inhalation pathways combined, of less than 10 μ Sv per year"

Your conclusion in the next paragraph is:

"Since these data indicate that doses from this pathway are likely to account for only about 1% or less of that arising from natural radiation doses from this pathway have not been included in the dose calculations given in this report".

A. That is correct.

A Q. Nor, you say, have you included the possibility of contamination by workers bringing contamination home with them?

A. That is correct.

Q. You say why you have excluded drinking water, finally in the last paragraph of this section?

A. Yes.

B Q. Then you deal with the other sources of radiation exposure, the habit data, which you used for the purposes of assessing dose, and then in paragraph 5.7 on page 53 you deal with - and I think this is something which Professor Jones did not do - the calculation of tissue doses?

A. Yes.

C Q. You first of all deal with intakes which, as Professor Jones explained yesterday, has to be contrasted with uptake. Is that a distinction that you would also recognise?

A. We do our calculations in terms of intake into the body either by inhalation in the air or by ingestion in food.

D Q. Then what you consider is the uptake to the relevant organs?

A. Then we look at the uptake into blood from the different intake pathways.

Q. You say that the methods which you have used to calculate doses resulting from intakes by inhalation and ingestion are in accordance with the recommendations of ICRP?

E A. Yes.

Q. You refer to the gut transfer factors that you have used, on which there is no issue as I understand it. At the bottom of page 53 you also say that you calculated radiation doses for Dorothy Reay and Vivien Hope in utero and that the dose models you used for that purpose were models which had been developed at NRPB for Report R171, but allowing for more recent information from animal studies on the transfer of radionuclides to the embryo and foetus from intakes of radionuclides by the mother, and you set out those in Appendix N?

F A. That's right.

Q. You deal with external radiation and then risk coefficients. You explain which target tissues you have taken. In the case of Dorothy Reay, acute leukaemia, you have taken the target tissue to be the red bone marrow, and in the case of Vivien Hope for NHL you have taken the target tissue to be the lymphatic tissue?

G A. Yes.

H Q. Then in Chapter 6, after you have explained your methodology, we find your dose assessments based on the application of that methodology. You say:

"This chapter reviews the results of the dose calculations undertaken for Dorothy Reay, Mr. and Mrs. Reay and for Vivien Hope and Mr. and Mrs. Hope. It also considers the consequence of dose that have been calculated for the risk of acute leukaemia in the case of Dorothy Reay and NHL in the case of Vivien Hope."

You say that you used methods which have been based on established procedures and similar to those which have been used by NRPB for the purpose of reports to both Black and COMARE. I think we can go on to page 60, where you set out again the relevant history of the individual cases and your conclusions in relation to the doses. You make the point again at the bottom of page 60 that you have calculated radiation doses separately for exposures in utero and those received after birth, and you have made the assumption in relation to Dorothy Reay that her exposure continued throughout the whole of 1962?

A. That's right.

Q. We find the figures for Dorothy Reay set out at the top of page 61 which are taken I think from the tables which follow this chapter of your report. In relation to Dorothy Reay, the Sellafield discharges (including uranium oxide release and the Windscale fire) give a total dose to the bone marrow in utero of about 7.2 μSv , and you say that it is less than the dose from weapons fallout, considerably less than that from natural radiation. We can pick up these figures on page 70, your Table 6.1?

A. That is right.

Q. Which is divided into foetal dose and infant dose, the left-hand side being the foetal dose, and your 7.2 is the total of Sellafield discharges, uranium oxide release and Windscale fire, in the third column, is that correct?

A. That is correct.

Q. One also finds the 38 for weapons fallout, 610 natural radiation, set out in that table. You explain the higher dose from uranium oxide, and then you again summarise the results of doses after birth which are set out on the right-hand side of Table 6.1, the dose from Sellafield discharges being 21.7 μSv , to be compared with natural radiation of 1078 μSv ; and you refer to the totals. Then at 6.2.2. you apply risk estimates to those doses and your conclusion at the bottom of the first paragraph of 6.2.2 is that the absolute risk arising from those doses of the risk of radiation-induced acute leukaemia is about 1 in 30m?

A. That's right.

Q. You relate that to risk from the normal incidence rate from background and the excess risk you say is about 2.3 in 1 million, at the end of the next paragraph?

A. That's right.

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A Q. You then summarise those results at paragraph 6.2.3. You then do a similar exercise in relation to Vivien Hope, and the relevant table for Vivien Hope is Table 6.3 on page 72, is that correct?

A. That's correct, yes. That includes the occupational dose and the dose from Chernobyl as well as the other sources that we had for Dorothy Reay.

B Q. Indeed. You set out some of the results on page 63. The in utero dose from Sellafield is 27.8 μ Sv and you compare that with weapons fallout of 88 and natural radiation of 598. Then exposures after birth, the Sellafield discharges - and of course we are now in the case of Vivien Hope looking at a period of many years - you assess her total dose from Sellafield as 3,729 μ Sv, of which about 40% from high LET radiation. To that you add the 11.7 μ Sv from occupational exposure from internally incorporated radionuclides on the Sellafield site, and you also build in external exposures. At the top of page 64 you summarise the overall picture. At Sellafield discharges, in total 3,924 μ Sv, as opposed to 1,064 nuclear weapons fallout, 188 Chernobyl, and 22,033 from natural background radiation exposure. Then you set out the risk of NHL arising from that on your assessment. If we can pick it up at the top of page 65, the cumulative risk is about 1 in 300,000. The excess risk, in the last paragraph, is about 1 in 20,000, is that correct?

D A. Yes.

Q. You then summarise your results in relation to Vivien Hope. You do similar exercises ---

A. The 1 in 20,000 is the risk from natural radiation.

E Q. I am sorry, forgive me. It is 1 in 200,000, the excess risk. Where is your excess risk for Sellafield?

A. 1 in 300,000. It is right at the top of page 65, and then I go on to the risk from the other sources.

Q. Thank you. You do similar exercises for Elizabeth and George Reay and Monica and David Hope?

A. Just in terms of calculating doses.

F Q. Indeed. We can perhaps go straight to the summary of that at page 68 at the bottom:

G "For both Monica Hope and David Hope the radiation doses received from the Sellafield discharges, including the uranium oxide and the Windscale fire, are considerably less than those due to natural radiation. For Monica Hope the dose to her ovaries is calculated to be about 29 mSv, with about 15% arising from the Sellafield discharges. For David Hope the dose to his testes from environmental radioactivity is calculated to be about 34 mSv, with about 5% of this dose arising from the Sellafield discharges".

H

A You make the point that as far as the Hope family is concerned you have assessed doses on the basis that they lived at Seascale even though up to 1971 they lived at Drigg?

A. That is correct.

Q. The next and last chapter of your report deals with the validation of those results, the validation of the calculations, from measurements first of all in the environment and secondly in humans and autopsy results, is that correct?

A. That is correct. We saw this as a very important part of the overall assessment that we did.

Q. One sees that although it is true, as you have told my Lord, that for the purposes of inhalation as opposed to ingestion doses you had to rely upon to a greater extent information provided by British Nuclear Fuels, you were able to validate your assessment by reference to measured air concentrations?

A. That's right, and for the important radionuclides the tendency is that the calculated values are more than the measured values.

Q. Indeed, and that is a point which you make near to the bottom of the second paragraph of 7.1, where you say:

"For the majority of radionuclides, model predictions are in reasonable agreement with measurements or tend to overestimate air concentrations. This is particularly the case for plutonium and americium where the overestimate is by a factor of about 5. The higher predicted concentrations than those measured suggest that, if anything, inhalation doses will be overestimated, at least for these years for which measurements are available".

You refer to measurements of human beings, the whole body content measurements, and you refer to measurements of caesium in adults resident near Sellafield?

A. There is really a substantial body of measurements of caesium body content over a period of years.

Q. Your conclusion is, near to the bottom of page 82, a few lines up:

"For the late 1950s the results show reasonably good agreement between measurements and predictions, with a tendency to overestimate intakes by about 50%".

You refer to measurements which have been carried out since 1977. Perhaps I can pick it up just the sentence before that. You refer to measurements made on a small group of adults employed at Sellafield whose work did not involve contact with radioactive materials, and you say:

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"Again there is reasonably good agreement with the predicted values although the intakes used in this assessment tend to give a higher body content than predicted, by a factor of about 2".

Is that a matter that surprises you?

- A. There is a tendency to overestimate intakes if you are using models, as I have already explained. Assumptions like, for example, all the fish that is consumed is locally produced at Seascale, or in the Irish sea, tends to be a cautious assumption, because we know that fish is brought in from other parts of the country.

- Q. Then you refer to measurements carried out by BNFL and again you say that the predicted body contents are generally greater than those measured for most years?

A. Yes.

- Q. You deal with the measurement of caesium in the Seascale population in 1984 on almost 300 persons resident in Seascale or the surrounding area?

A. They were measurements undertaken by NRPB staff.

- Q. One finds near to the bottom of the page that there is about a factor of 2 difference between predicted body contents of adults and the limit of detection, and you refer to the point that you have already drawn his Lordship's attention to, that it may be due to estimated consumption of locally caught fish, because a cautious assumption is made that all seafood is locally produced?

A. That is correct.

Q. You say:

"There is therefore no suggestion from these measurements of body contents of caesium for people in the Seascale area that intakes of caesium in the late 1950s, the 1960s or the 1980s were higher than those predicted from the intake data used in the report".

Then you refer to analysis of plutonium in autopsy tissues:

"Six sets of samples have been obtained for persons living in Seascale or the surrounding area ... and who are known not to have been occupationally exposed to plutonium during their lifetime The plutonium contents of tissues obtained from persons living in the Seascale area of West Cumbria are increased compared with those found in other parts of the country".

You point out in the last paragraph on page 84 that even on the assumption that all the increase in body content of plutonium is due to Sellafield operations, the measured body contents of plutonium in Seascale are about 10 times less than those predicted for the period 1980 to 1984?

A. I think that is more important if you are looking at people who have lived in the area since the start of operation of the plant, so that you are looking at cumulative intakes over an extended period of time, whereas body contents of caesium tend to reflect intakes in just maybe one or two years because caesium is lost from the body very quickly; plutonium is lost from the body only very slowly.

Q. You say again it might have resulted from the assumption that all seafoods is locally produced and:

"There is also the possibility that the calculated airborne concentrations of plutonium from the stack discharges and seaspray are also too high...

These calculations which indicate a lower body content of Plutonium-239 in persons in Seascale than would be anticipated from the parameters used in the assessment in this report are more significant than the Caesium-137 measurements as they integrate intakes over a lifetime."

Which is the point you have just made?

A. Yes.

Q. You conclude there was no evidence of significantly enhanced concentrations of plutonium in the tissues of three Sellafield workers' spouses. You refer to Prof. Jones' report and then finally, looking to our CONCLUSION, 7.4:

"The measured body contents of Caesium-137 in adults from the Seascale area, from measurements in the periods 1957-1968 and from 1979 to 1988 have been compared with those predicted from the intake data for Caesium-137 in adults used in this report. The comparison shows good agreement over the 1957-1968 and 1979-1988 time periods although there is a tendency for the predictions to overestimate intakes by up to a factor of 2. This overestimate in later years where much of the intake is calculated to arise from Sellafield discharges may be due to the assumption that all fish consumed is locally caught. Predicted levels of Plutonium-239 have also been compared with measurements of Plutonium-239 in tissues obtained at autopsy from six persons who had been resident in the Seascale area since the late 1940s or early 1950s. In general the measurement data for the 6 individuals were in agreement and suggested that intakes of plutonium could have been overestimated in this report by up to about a factor of about 10. A comparison of measured and predicted levels of airborne activity between 1978 and 1988 has also suggested that levels of activity in the air could have been overestimated. For Plutonium-239 and Americium-241 the overestimate is by a factor of about 5."

DR J W STATHER

Dr. Stather, to what degree are you confident that the figures you have arrived at in assessing doses to relevant tissues to the individuals that my Lord is concerned with, are figures which are at least as high as those which were actually sustained?

A. I think the doses assessment is a robust assessment. I think that is partly due to the fact there is an element of caution in some elements of the calculations. I think we have erred on the side of caution. I cannot see any reason why we should have underestimated doses. I can see a number of places where we could have included some overestimation of doses.

Q. May I just deal very briefly with your second report? You produced a second report for the purposes of these proceedings, as I referred to yesterday, signed and dated 3rd September, 1992. Do you have that?

A. I have that.

Q. Just looking at the Introduction to it and the end of the second paragraph is picking up the point I have just asked you about. You say:

"It is therefore more likely that I have overestimated doses than underestimated them."

You say in the last sentence of that introductory paragraph:

"In summary, nothing in the Plaintiffs' expert evidence causes me to change my approach to calculating doses and risks in my first report."

I am going to leave your comments upon the reports of Dr. Day, Prof. Harrison and Dr. Lambert. If you would go to page 10?

MR. ROKISON: My Lord, the position in relation to this is a problem which is going to arise, one fears, in relation to a number of areas of the case and that is because although we have done our best to segregate the evidence under the broad categories which were agreed at the beginning of the proceedings into dosimetry, epidemiology and genetics, of course in arriving at his assessments Dr. Stather has necessarily had to make assumptions in particular in relation to RBEs for the purposes of arriving at a dose expressed in Sieverts, micro-Sieverts or milli-Sieverts for alphas.

He therefore refers to evidence which is yet to be given by Dr. Lord in relation to that matter. What I was intending to do, and I have mentioned this to my learned friend, was to take Dr. Stather to this part of his evidence in chief but I understand that my learned friends are going to ask, and I cannot have any objection to that - I apologise to Dr. Stather - that they reserve cross-examination on this point until after Dr. Lord's evidence to which it relates.

MR. JUSTICE FRENCH: Yes.

A MR. HYTNER: My Lord, it seems logical in any event because whilst clearly my learned friend is entitled to adduce this evidence now, in the normal course of litigation a witness would not be asked to comment on the other side's evidence until it has been given.

MR. JUSTICE FRENCH: This relates to RBES?

B MR. ROKISON: Well, not directly, but it relates to the question of the sensitivity of the foetus and so on. It is a question that obviously Dr. Stather has had to take into account for the purposes of assessment of dose and therefore it does relate to dosimetry and is something which therefore, in our submission, his evidence on may be relevant at this stage.

C MR. JUSTICE FRENCH: Yes. If that is convenient to the parties I see no ground for intervening.

MR. ROKISON: Very well, my Lord:

Q. Page 10 you deal with the effects of alpha emitting radionuclides on haemopoiesis during development in adults. You say this:

D "In his report Dr. Lord considers the results of the research of his postgraduate student (Dr. Mason) in relation to which I myself was an external examiner."

You were actually examiner in relation to this particular exercise?

E A. Yes.

Q. You go on:

F "Dr. Mason has conducted high dose studies in mice that are related to the effects of alpha particle irradiation on the proliferative ability of haemopoietic stem cells in newborn mice following administration of plutonium-239 to the mother at various times during gestation. Dr. Lord's observation is that if plutonium is administered very early in pregnancy much less is transferred to foetal tissues, as measured by the activity in the newborn, than if it is administered later on. Despite this, Dr. Lord noted that the damaging effect on the proliferative ability of haemopoietic cells in the first few weeks after birth is about the same, regardless of whether the actinide is administered early or late in pregnancy suggesting a much greater sensitivity of stem cells, by up to a factor of a thousand following early administration. The basis of Dr. Lord's calculations has however been the uptake, and hence dose, to the foetal liver. The evidence from studies at NRPB..."

G

H

DR J W STATHER

Were these studies with which you were concerned?

A. Partly, yes.

Q. "...is that early in pregnancy there is a much greater accumulation of plutonium in the foetal membranes, which contain haemopoietic stem cells, than in other foetal tissues such as the liver. It seems likely therefore that the greater sensitivity of stem cells following administration of plutonium early in pregnancy may result from higher doses to stem cells in the yolk sac."

That is as opposed to higher sensitivity?

A. That is right.

Q. "For this reason I believe that it is essential to make allowance for the distribution of activity within the foetus in assessing dose. In the method used in my report for assessing doses to the haemopoietic tissue received in utero allowance is made for this early higher concentration of plutonium in the yolk sac."

So you have taken this into account?

A. We have taken the results of our own experiments, which give us information on the distribution of plutonium in the developing animal, as a function of time, and we have used that information to develop what we believe is a better model for assessing doses to the human embryo and human foetus.

Q. You say:

"In practice, for chronic intakes of plutonium the dose to the stem cells in the foetal membranes accounts for about 60% of the total dose to haemopoietic tissue received in utero.

The method I have used to estimate doses to the embryo and foetus from intakes of plutonium (and americium) by the mother makes full allowance for all the information presently available from human and animal studies including those considered by Dr. Lord in his report. The dosimetric model has been presented at a number of international meetings over the past two years and I believe has been accepted as an appropriate approach."

Again, is that something with which you have been involved?

A. Yes, I have presented a paper at two meetings in the past year, both on dosimetry, one in Germany and one in Toronto. The approach generally we are using for calculating doses to the embryo and foetus for radionuclides, and more specifically this particular set of calculations for plutonium.

DR J W STATHER

A MR. JUSTICE FRENCH: I imagine at some stage there will be at least a discussion, if not a dispute, as to the extent to which animal studies can be compared with the position in humans, but this is not the time to embark on that?

MR. ROKISON: I think not, my Lord, but I think the answer to your Lordship's question is yes, I am sure there will be.

B THE WITNESS: It applies equally to Lord's results as it does to results from NRPB on the distribution of plutonium.

Q. MR. JUSTICE FRENCH: Would you say that again?
A. It would apply equally to the results of Lord's experiments in mice and to the experiments that we conducted at NRPB on the distribution of plutonium.

C Q. MR. ROKISON: Then finally, I think this is a matter which we can leave over, if you don't mind, Dr. Stather, because I think it has been agreed that issues relating to dose and dose rate effect, is a matter which will be left over. You express your views here and perhaps it is not as relevant to ask you about at this stage because this is not a matter which has been taken into account, as I understand it, for the purposes of your doses. Is that correct?

D A. That is correct.

Q. I will leave that part, chapter 6 of your second report, to be dealt with at a later stage.

MR. ROKISON: Thank you very much.

E MR. READ: My Lord, as you are aware I will be cross-examining this witness on behalf of the Plaintiffs. Perhaps before I start it may be helpful, I may in the course of cross-examination have to refer to four volumes of the common bundle. It may be helpful if the witness has those four references ready.

F MR. JUSTICE FRENCH: First of all there will be P1, Mr. Hytner's white bundle. Secondly, my Lord, from the common bundle I may want volume C, dividers 29-51; volume H, dividers 102-124; volume O to R, dividers 196-215 and volume S, dividers 231-237. It would also be helpful if the witness could have a copy of Prof. Jones' first report near to hand.

G MR. JUSTICE FRENCH: When are we going to deal with Prof. Jones?

MR. READ: My Lord, later on. I think it will be after the short adjournment.

H MR. JUSTICE FRENCH: Very well, I won't bother about it now.

DR J W STATHER

Cross-Examined by MR. READ:

A Q. Now Dr. Stather, can I ask you some general questions about the way you have approached your calculations with R171 and R171 Addendum and the calculations you have put in your report? Am I right in saying that the method of calculating the dose levels that you have used in your report is basically the same as that which was used for R171 Addendum?

B A. That is correct. I think that is fairly clear from what you have here.

Q. Secondly, is it fair to say that the R171 and R171 Addendum are basically models which involve firstly a number of assumptions which we have already discussed in some detail, and secondly, raw data?

A. A combination of models and measurement data.

C Q. And, indeed, really the assumptions fall down into two categories: firstly, the modelling and secondly, straightforward assumptions you have made - for example, that all shellfish and crustaceans were landed at Seascale?

A. That is right.

D Q. So that people were in effect eating locally caught seafood?

A. For people in Seascale.

MR. JUSTICE FRENCH: I am sorry, what is the first assumption?

MR. READ: My Lord, firstly modelling.

E MR. JUSTICE FRENCH: He used a combination of models and measurements and he assumed, secondly, all fish eaten was locally caught. What was the first one?

MR. READ: Perhaps I will take it again:

F Q. Firstly there is the raw data. There is no dispute about that. What you have read in the soil, what you have found in the cows' milk, and, indeed, what you have been told by British Nuclear Fuels about the discharges?

A. Yes.

G Q. Secondly, you have assumptions. Would it be fair to split those down into two categories? Firstly, the straightforward assumptions, if I can put it that way, that all seafood was caught locally?

A. That is just an example of that type of assumption.

H Q. I was just using that as an example to get the overall framework of your modelling. Secondly, you have the assumptions relating to modelling. Those are mathematically derived, that is correct, isn't it?

A. This type of assumption?

A Q. Yes. Can I just run through the raw data element that goes into your modelling? Obviously there is the monitored releases that British Nuclear Fuels have supplied you with?

A. Yes.

Q. Secondly, and we have already heard this, there is the milk monitoring that went on?

A. That is right.

B Q. Just so that we put it in context. The first figures that are really used are from 1958 that relate to strontium-90?

A. For that release, yes. There were a few data measurements for earlier year, but 1958 was the first year for which there was comprehensive data.

C Q. The comprehensive data, perhaps I should have put it like that. In respect of caesium-137, when did that start, the constant monitoring of that?

A. If you are going to ask me details of when the monitoring started and every pathway, I am going to have to check...

Q. Let me ask you this: would you agree it was 1961?

A. I recall 1960/61, yes.

D Q. And iodine-131 I think was 1964.

MR. JUSTICE FRENCH: Caesium, constant monitoring as from 1961. (4) Iodine, constant monitoring? Is that right?

E MR. READ: My Lord, yes. They are set out in R171, and if I have derived the figures from there those figures will be right. I think yesterday Prof. Jones said that the monitor for milk with iodine started in 1958, but what is in R171 sets out the correct position on monitoring:

Q. Is that right, Dr. Stather?

A. I believe so, yes.

F Q. Returning to the raw data you use, you also have the releases you measure on the ground?

A. Strontium-90, caesium-137, plutonium - deposition on the ground?

Q. Deposition on the ground. You have measurements in other food apart from milk?

G A. Yes.

Q. But they start much later on, don't they?

A. Yes, that is right.

Q. 1978 is the figure that is in R171.

H A. Basically we use information on the levels of strontium-90 and caesium in milk to assess concentrations in other foods.

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Q. But the actual raw data for the other foods only starts in 1978 and then effectively you model backwards from what you know about milk?

A. Yes, that is right. Relationships have been established over a number of years between the concentrations of these radionuclides in milk and the concentrations in their foods.

Q. Seafood concentration, again is it fair to say there was an absence of actual measurements of those in the early years of Sellafield?

A. Yes.

Q. Just looking at the raw input data to your model, are there any other categories that I have missed out on that list?

A. Air concentrations.

Q. Yes, perhaps I had rather regarded that as the monitored releases, but we will put that in as a separate category. Those are the six categories of raw data that you use in your modelling?

MR. JUSTICE FRENCH: I have made it up to 8. Shall I read them out:

- (1) Measured releases from BNFL;
- (2) Strontium-90 in milk;
- (3) Caesium-137, constant monitoring as from 1961;
- (4) Iodine, constant monitoring as from 1964;
- (5) Measured deposition on ground;
- (6) Other foods, 1978 onwards;
- (7) Seafood concentration (absence of measurement in earlier years); and
- (8) Air concentration.

MR. READ: My Lord, I had simply categorised them slightly differently and lumped all the milk measurements together but, my Lord, that is, as I understand this witness, the extent of the raw data that actually went into his modelling.

MR. JUSTICE FRENCH: What I have written down will do?

MR. READ: My Lord, yes, I am more than content with that:

Q. That is right, Dr. Stather, that those are the 8 elements of the raw data you use in your modelling?

A. I think we covered terrestrial foods, marine foods, air concentrations and milk we have covered. I think we have covered the essential elements of the different inputs to the requirement for calculating doses.

Q. Can I look in particular at the early years, by which I mean up to a period of, say, about 1965? Your actual

raw data, the most important parts of your raw data fall into two categories? Firstly, the release information you are given by British Nuclear Fuels or their predecessors, and secondly, the milk monitoring?

A. Yes.

Q. Can I ask you this, and I use an example here, polonium-210, which, as I understand it, an alpha emitter?

A. That is correct.

Q. It has a fairly short half life - about 140 days?

A. Yes.

Q. In that period, how would that have been monitored in the environment?

A. It is very difficult to monitor in the environment, I think. It doesn't persist for long enough.

Q. MR. JUSTICE FRENCH: So you are saying it wouldn't have been monitored?

A. No. You have got to know what is happening on the plant to estimate what the discharges would have been.

Q. MR. READ: So you would accept, in essence, that for any information about a radionuclide like that, you are entirely dependent upon the information supplied by British Nuclear Fuels or its predecessors?

A. Yes, that is right.

Q. MR. JUSTICE FRENCH: You cannot calculate it from other nuclides?

A. No, it will be dependent upon information on the plant operations. Of course, there is a lot of naturally occurring polonium-210 in the environment anyway.

Q. That is a separate point.

Q. MR. READ: How do you measure for polonium-210 now?

A. Separate it, count it on an alpha spectrometer.

Q. When was that introduced around Seascale? Do you do that now around Seascale?

A. I don't do it at all around Seascale. I don't do it at all at NRPB. People at NRPB are measuring polonium-210 in tissues. They have been doing that for some years now.

Q. What are we talking about? Ten, fifteen years?

A. Ten years maybe. When BNFL started trying to measure polonium-210 I just don't know. That is not for me.

Q. MR. JUSTICE FRENCH: It can be measured now and has been so for about 10 years?

A. I suspect 10 years. I certainly know of published papers on polonium-210 that other people have done going back...

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Q. MR. READ: That is measurements in tissue?

A. Yes.

Q. Argon-41, again was it possible to monitor that at all before 1965?

A. It is a gamma emitter so it would have been possible to have monitored it to a degree, yes.

Q. It would have been? But was it actually monitored? I will re-phrase that. Was it monitored in the environment apart from British Nuclear Fuels?

A. The estimates of discharges of Argon-41 that I have included in this dose assessment are based on estimates from the Windscale piles, of throughput of air through the piles and calculations, rather than measurements.

Q. That is dependent on the information that British Nuclear Fuels or its predecessors gives?

A. Yes.

Q. MR. JUSTICE FRENCH: Argon-41 could have been monitored to a degree?

A. There would have been some gamma monitoring, yes.

Q. To a degree by reference to gamma monitoring?

A. Yes.

Q. However, in practice?

A. In practice the main release we have included here is the Argon-41 release from the Windscale piles.

Q. I think there is a stage which we are missing out. "In practice we were dependent on information from BNFL"

A. Yes.

Q. "And specifically information as to the Argon release..."

A. From the Windscale piles. These are calculated.

Q. "From the piles by way of calculations based on..."

A. The release is based on calculations based on air flow through the pile, through the system.

Q. MR. READ: Just to put this in context, I think it is Argon-40 that naturally occurs in air?

A. Yes.

Q. When you expose it to radiation it turns into Argon-41?

A. With a short half life.

Q. I think it is something like 1.8 hours?

A. As I recall - 1.8.

Q. It is fair to say that a predominant release of Argon-41 is far more concentrated where you have something that is air cooled rather than something that is water cooled, because the argon is in the air?

A. That must be true.

A Q. In fact is it also fair to say this - and maybe it is something you cannot specifically comment upon - but Sellafield has one of the highest concentrations of Argon-41 in the world?

A. I cannot comment on that.

Q. MR. JUSTICE FRENCH: You don't know whether that is right or wrong?

A. No.

B Q. MR. READ: This is true, Dr. Stather, that Argon-41 was one of the most important doses to the red bone marrow, certainly as far as R171 Addendum was concerned?

A. Certainly. That is a point we make in the document.

Q. Yes, you say it in your report.

A. Up to the period at which the piles closed, of course.

C Q. Yes.

A. Up to the end of October 1957.

Q. Well, it is to the 1950 cohort that you looked, because that was the one that was most immediately affected?

D A. Well, we looked at cohorts born in each year from 1945 through to 1979, but the cohort we chose to illustrate in the reports was the 1950 cohort because that was through the time period when a number of things were happening at the Sellafield plant and it illustrated very well the influence of different contributors to dose.

Q. It is correct to say that the estimate of the release of Argon-41 went up by something like 70% from R171 to R171 Addendum?

E A. I think exactly 70%.

Q. Of course, that error, if I can use that word, was something that could not possibly have been picked up by your environmental monitoring data?

A. Certainly not.

F Q. It was something that was entirely dependent upon the release information given to you by British Nuclear Fuels?

A. It is a point we pursued on many occasions with BNFL to ascertain that the value they gave for Argon-41 release was appropriate.

G Q. Before I embark on something that may turn out to be wholly irrelevant, can I ask you to look at common bundle S, file dividers 231-237? It is R171 Addendum. Can you turn to page 90, Table 3.16? You give there the gamma dose rates in air due to discharges to sea and atmosphere from the Sellafield site from 1951 to 1982. The middle column there relates to "Cloud-gamma"?

A. Yes, that is right.

H Q. Would that virtually all comprise Argon-41?

A. Most of it would.

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Q. Could you hazard a guess on percentages? Would it be 50%-plus or 80%, or something like that, or aren't you able to say?

A. Well, looking at the step function it is clearly 80%-plus I would say. There is clearly a change from 1957 to 1958, so that is resulting from the closure of the Windscale piles. That is implying that most of that is due to releases from the piles, so it must be Argon-41.

Q. You have anticipated what I was going to ask you to look at next. Can you look at Table 3.7, which is on page 79? That gives the information you have been supplied with, discharges of Argon-41 to the atmosphere. If we look for the first four years, the only part of the plant which was discharging Argon-41 was the piles?

A. Yes. Clearly in terms of the amount that dominates what happened in later years anyway.

Q. The Calder Hall reactor had not come on stream then, so of the total the discharges were all from the piles?

A. Yes.

Q. If one goes down to 1982 one sees under Calder Hall a figure, but nothing for the piles or for the WAGR?

A. Which had shut.

Q. What I am interested in, and so there is no mystery about this, is whether or not it is fair to look at Table 3.16 and assume that for 1982 the cloud gamma figure there - this is on page 90 - is in fact going to relate entirely to the Calder Hall reactor? On page 79, 1982, we have a sole entry relating to Calder Hall and then in 1982 we have the cloud gamma - this is on page 90 for that year - is it a fair inference to draw that the cloud gamma figure on Table 3.16 comprises virtually exclusively that 1982 figure for Calder Hall?

A. I think that is right.

Q. You think that is right?

A. Yes.

Q. You see, the reason I am asking this is because it is possible, from this, to work out the relationship between the discharge figure in Table 3.7 and the absorbed dose given in Table 3.16?

A. Right.

Q. Tell me if that is wrong because I would hate to pursue this if there is no mileage in it?

A. Are you going to say the ratio changes?

Q. What I am going to ask you....?

A. Because, of course, we have got releases from different heights and the height is taken into account in the calculation.

A Q. Yes, but, in fact, what I am going to put to you is that the ratio between, for example, 1952 - this is the ratio between the discharges and the dose - is more or less the same for 1982. I think the ratio between discharge and dose for 1952 comes out at something like about 0.24 millionths and, for 1982, it comes out at 0.36 millionths - 10 to the minus 6?

A. But we do make allowance for the different stack heights of the release. One is a high level release, one is a low level release.

B Q. I am not in any way trying to trap you, Dr. Stather, at all. I am simply trying to gauge whether or not, even after you have taken account of the various discharge heights, it is a fair inference to be drawn from the comparison of these two tables that the ratio between discharge in Table 3.7 and dose in Table 3.16 for the piles and for Calder Hall are very much the same?

C A. I do not see how you get them to be very much the same because in 1953 we have got 1.2, 10 to the minus 1. In 1982 we have 1.1, 10 to the minus 3. So there is a factor of 100 difference there.

Q. Yes, certainly, but the point I am making is, if you go back to Table 3.7....?

A. And there is a factor of 100 difference in the release.

D Q. Yes?

A. And the change occurs in 1957, when the Windscale power shut down.

Q. I am trying to concentrate on the years when we know only the Windscale plant was emitting Argon-41?

A. Yes.

E Q. And then to look at the year 1982, when we know that the Calder Hall plant was only emitting?

A. Correct, yes.

Q. To see whether or not the ratios between discharge that is given in Table 3.7 and dose in Table 3.16 are similar?

A. One reflects the other.

F Q. You think that is right, do you?

A. They seem to do. There may be some rounding errors in the numbers, but....

G Q. The only reason I ask you about that is because Prof. Jones said, I think it was the day before yesterday - if your Lordship wants the reference, it is Day 6 of the transcript, at page 74 - that you cannot simply apply the dose calculation for the release from the Calder Hall reactors and take it pro rata to the quantity emitted from the Windscale piles because of the different circumstances of the emission, in particular the difference in stack heights?

H A. We do allow for the difference in stack heights. Maybe

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the differences are not very great, but we do allow for the effect of stack height when we calculate the dose at 3 km distance, which is the distance of Seascale.

A Q. I see, but it seems from....?

A. The footnotes indicate that we are allowing for stack heights in the calculations.

B Q. Yes, but it seems from this data in R171 Addendum that the ratios, after taking into account the different stack heights, between discharge, on the one hand, and dose on the other are not that different for the Windscale piles or not that different between that and the Calder Hall reactor. You would accept that?

A. Yes.

Q. If you want to do any calculations over the short adjournment and want to come back on that, please say so?

C A. Right.

Q. Moving on.....

MR. JUSTICE FRENCH: Do we keep Common Bundle S by us or do we put it away?

D MR. READ: My Lord, if it can kept conveniently near to your Lordship.

MR. JUSTICE FRENCH: But put away for the time being.

MR. READ: It can be put away and shut up but, my Lord, if it can be kept close:

E Q. Dr. Stather, would you accept this, that really the two most important elements of your raw data are basically the discharge data linked in with the air concentration data and the milk monitoring?

A. And the seafood data, of course.

Q. And seafood. I see, you put seafood into that category as well?

F A. I think, clearly in later years, the concentrations of radionuclides in seafood are an important contributor to the dose, in the way that we calculate it, assuming that the fish is locally produced and locally eaten and 100 per cent of fish consumed is locally caught.

Q. But for the earlier years, of course, you did not have that data?

G A. No.

Q. It is very much confined to the later years?

A. Yes, that is right.

H Q. Also, for the earlier years, you do not actually have any air concentration measurements. That is right, is it not?

A. That is right.

A Q. So really, for the early years, one is looking primarily at the information you are given by British Nuclear Fuels and the milk?

A. And, as we have already covered, the autopsy data, which I think is very important for plutonium.

A Q. Just pausing there, that is really not something that is used in your model, is it? It is something that is used to check your model?

A. That is right.

A Q. Validate, I think, is the word. So, to that extent, it is something separate which you ultimately use to assist your calculations, to check your calculations, that you have used in your model?

A. And, hopefully, to demonstrate that we have not under-estimated the doses.

A Q. I will come back to the question of validation in a moment, but just concentrating on the input into your model, in the earlier years you have got the discharge data and you have got the milk. Obviously, in calculating those, you are dependent upon, firstly, the information you are given by British Nuclear Fuels and, in particular, more recently, the information you have been given by Prof. Jones regarding plutonium?

A. That is correct.

A Q. So it is not fair to say that your assessment has been carried out quite independently of Prof. Jones because part of your raw data is dependent upon him?

A. I think that is one of probably the first statements I said. We do rely on the operator for any of these dose assessments to provide us with information on discharges and we add to that as much information as is available on environmental concentrations.

A Q. Were you surprised by the increase in the plutonium releases?

A. Well, one of the uncertainties we pointed to in the Addendum was the core taken in Ponsonby Tarn and we felt at the time that the profile of plutonium in the tarn did not really reflect the information that we had at the time on discharges and certainly there was scope for looking at discharges through the '50s and seeing if there was any way that the profile in Ponsonby Tarn could be accounted for.

A Q. MR. JUSTICE FRENCH: Is the short answer "Yes" or is it "Not really"?

A. The short answer is I was not surprised to find there were some changes in the discharges.

A Q. Does that mean that the Ponsonby Tarn core had alerted you to the possibility that there might be changes?

A. Yes.

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- Q. "Alerted us to the possibility of changes in quantity of discharge of uranium oxide"?
- A. Plutonium. I was also aware of the work on sampling efficiency factors.

- Q. MR. READ: When were you aware of that?
- A. There was a presentation to COMARE, which I heard about, so that would go back two and a half years or something, three years.

- Q. This was before or after COMARE 1?
- A. Oh, well after COMARE 1, which was 1986.

- Q. There was nothing before that to alert you to the fact that the SEFs may change?
- A. I mean, it was the end of the '80s.

- Q. But it was after COMARE 1. You are quite sure about that?
- A. Clearly, yes.

- Q. Do you think you could have a look at Bundle P1 of the Defendants' documents. I hope that there is one up there for you. There should be a black file?
- A. This one?

- Q. I think that is probably it, yes. If you can go to - if I can just get the right reference.

MR. READ: If your Lordship would just bear with me for a moment while I find the right page reference. My Lord, I cannot find that document for the moment, so perhaps I will move on:

- Q. Putting it bluntly, Dr. Stather, the amount of release has gone up by a very large amount, has it not, of plutonium? This was not a small increase. This was a fairly large, substantial increase?
- A. That is right.

- Q. Did that surprise you?
- A. I think it probably did. I think it did, given that we had already looked at the autopsy data that we had available and we compared that with the predictions from our previous calculations in R171 and - no, sorry, it was in R215 we compared the autopsy data with the predictions of the calculations in R171 and we were already over-estimating body contents of plutonium from the discharge data. So any further increase in release was going to further increase the discrepancy between what we actually see in people and what we predict for the models, based on the information on discharges.

- Q. In order to use that information in your calculations for the purposes of this litigation, it is, of course, necessary for you to know where the emissions came from?
- A. The stack height.

Q. The stack height, and the chronology, of course?
A. And the timescale.

A Q. The timetable. In calculating your assessments in your report, have you centred the whole of the calculations for, if I may call it, the excess plutonium - that is the plutonium that is not directly referable to B204 - centred it on the pile ponds?
A. Centre of the site. The releases of, I believe it is all the radionuclides, are based on the centre of the site.

B Q. That applies, even though the release may have come from the base of the foot of the cooling tower?
A. The stack height is important. Whether it is a few tens of kilometres one way or another does not make that much difference when you are 3 km away or many kilometres away.

C Q. Does it surprise you that Prof. Jones yesterday could not identify precisely where this excess plutonium had come from?
A. We have looked, as I have said, to the operator to provide us with information on discharges. I think it is for you to ask Prof. Jones about the source.

D Q. MR. JUSTICE FRENCH: You are being asked about your subjective feeling, whether it was one of surprise or not?
A. Clearly there must be a number of possibilities for the source of the plutonium release. You presumably can decide that it might be one of two or three or a number of different sites of origin.

E Q. MR. READ: If we can go back in time, you start obviously by compiling R171?
A. Yes.

Q. That has to be revised because of a massive increase in the amount of uranium oxide that is discharged from the Windscale piles?
A. Yes, but not a massive change in dose estimate.

F Q. The environmental monitoring data that you had, had that highlighted in advance the possibility that the discharge of uranium oxide could be substantially greater?
A. No. I think the reason is we produced 171 in a period of two and a half months or so and it was produced for the Black Advisory Committee and it was very much taking information that was available to us from reports from Government Departments, from BNFL on discharges, and there was little opportunity in that short period of time to go to the environmental data and to try and validate the dose assessments. The sort of exercise we did in later years.

G H Q. I wonder if you can have a look at page 94 in Bundle P1. This is, in fact, a letter from Mr. Allday of British Nuclear Fuels to Mr. Russell of UKAEA?

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A. And shortly after 171 had been published.

A Q. And shortly after 171 had been published. Indeed, as we can see from the heading, it follows the intervention of Dr. Jakeman because it says "Dr. Jakeman: Windscale Pile Incident 1955" and then it goes on to list three documents there, which have been considered?

A. Yes.

B Q. Then, if one looks at the final paragraph on that page, it says:

"In order to provide the fullest possible information to the Black Inquiry we took steps to declassify these reports and they were sent to NRPB by Mr. Mummery on the 11 June 1984. The data in these reports were considered in the NRPB Report submitted to the Black Inquiry and which has now been published. The relevant section is the bottom paragraph of page 15. Here they state that 'The particle sizes of this material were about 50-700 μ Gy' and they conclude 'Particles of this size are not respirable and will therefore have little radiological impact.'"

C A. Micro-metres.

D Q. Sorry, micro-metres. Over the page:

"Thus you can rest assured that all matters relating to this incident were fully considered by the NRPB on behalf of the Black Inquiry."

E Is that right, that you had actually fully considered the matters relating to the uranium oxide discharge before submitting R171 to the Inquiry?

A. We had looked at this concentration of strontium-90 in milk in 1958 for 171 and we had assumed that that concentration remained constant over the years.

F Q. Yes, it is a horizontal line across the page, is it not?

A. Right.

F Q. Coming back to my question....?

A. So we had not specifically looked at the UO_2 in terms of activity, but we had looked at the measurement data for strontium-90 in milk and clearly the source of that, it turned out, was largely the UO_2 release.

G Q. You see, that letter is saying in terms there, is it not, that "You can rest assured that all matters relating to the incident were fully considered by NRPB on behalf of the Black Inquiry." This is on page 95. Do you think that is a fair comment or not?

H A. I think the answer is we were approached by Dr. Jakeman, who pointed out that there was more information about the releases from the Windscale piles, and clearly we had not fully taken account of the doses to people as a result of the release and we did that in a subsequent report.

- A Q. Putting it bluntly, you were entirely dependent upon what British Nuclear Fuels told you about that uranium oxide release, were you not?
- A. We were to a degree, but we were not because we based the estimates of dose on the environmental measurements of strontium in milk.
- B Q. But, Dr. Stather, your environmental measurements of strontium in milk did not tell you that there was such a significant release, did it?
- A. No.
- B Q. And, in fact, after you knew there had been such a significant increase, you had to re-interpret the milk data, did you not?
- A. Quite clearly we did.
- C Q. And that is what you have done. Instead of drawing a straight line backwards across the page, you have to have the peak?
- A. Correct.
- D Q. So, from that point of view, your milk data was not very efficient at telling you what was happening, was it?
- A. Well, it was efficient in that the release changed from 400 gm to 20 kg - the release estimate - and the effect on dose was actually very small.
- D Q. Leave dose aside for a minute?
- A. If we had relied solely on BNFL to provide us with information on the particles and to use that solely for the purpose of dose calculation, then clearly a change in the estimate of the release by a factor of 50, or whatever it was, would increase the dose by a factor of 50, but because we did not rely exclusively on BNFL, but we relied on the environmental measurement data, the increase in our estimate of dose was, in fact, quite small.
- E Q. But the one thing your environmental modelling data was not telling you was that there was a chance that there had been a considerable increase in the amount of uranium oxide that went up the pile chimneys. That is right, is it not, Dr. Stather? It did not highlight it?
- F A. It is right that at the time we had produced 171, we did not look to all the information on levels of activity in soil and try to interpret that in terms of what the release might have been. If we had done that, then we would have identified the problem that Dr. Jakeman pointed out to us.
- G Q. This milk monitoring data that we have heard so much about, it did not allow you to note there could be an uncertainty about this release, did it?
- A. Clearly not, because we just had information in 1958.
- H Q. In fact, if we analyse what you were actually looking at in that 1958 point on your graph, you are looking at two farms only, are you not?

A. Yes, the two farms nearest to Seascale.

Q. But that is what your data is based upon - two farms only - for that point in 1958, and is it also fair....?

A. I also refer in my report that there was a further farm, which was very near the centre of Seascale, where there were not comprehensive measurements, but the concentrations of strontium-90 in milk in that farm were the same sort of order as we got from the other two.

Q. The two farms you looked at to fix your point for 1958 on that graph, there was a ten-fold difference between them, was there not?

A. I remember it nearly a ten-fold difference.

Q. Nearly a ten-fold difference?

A. Yes, after correcting for weapons fallout.

Q. Nuclear fallout? Sorry, nuclear weapons fallout?

A. The farm nearest to Seascale had a high concentration and we used the average value of the two.

Q. MR. JUSTICE FRENCH: The 1958 milk strontium was based on two farms near Seascale. There was almost a ten-fold difference in the readings?

A. After correcting for the contribution from strontium-90 in weapons fallout.

Q. There was almost a ten-fold difference in the reading after proper adjustment?

A. That will do.

Q. MR. READ: You mentioned a third farm in your report?

A. Lane Head Farm.

Q. Yes, I think that was it. That did not have constant monitoring?

A. No, it did not.

Q. But it had a much greater concentration of strontium-90 in milk than the others. Is that right?

A. It was pretty much the same as we estimated from the other two. There is a typing error on page 7 of Annex D1. 1957 should be 1958. It refers to Table E2.

Q. MR. JUSTICE FRENCH: What manuscript page are you looking at?

A. On my main report.

Q. Page?

A. Page 7 of Annex D1 and it refers to Table E2. So the calculated value from the two farms in 1958 given in Table E2 is 1.3 Bq per litre, and Lane Head Farm in 1958 was 1.2 Bq per litre.

Q. MR. READ: Putting it bluntly again, Dr. Stather, there was a great variance between the very limited milk monitoring data you had in 1958, was there not?

A. There were a lot of measurements made on those two farms, but it was based on two farms.

Q. And those two farms had nearly a ten-fold difference between them. There was nothing that pointed out to you, from that milk monitoring data, that the overall model of R171 could, in respect of the uranium oxide release, be out by a factor of 50?

A. Of course not.

Q. And you had to re-interpret the data subsequently in order to make it fit with the later known....?

A. I think we explained that very fully in the addendum.

MR. READ: My Lord, I do not know whether that is a convenient point.

MR. JUSTICE FRENCH: Yes, that would be a good moment. Yes, two o'clock, please.

(Luncheon Adjournment)

Q. MR. READ: Dr. Stather, just to finish off where we were before lunch on the uranium oxide, you have indicated that you have made a worst case assumption about the 1954 release; the discharge of uranium oxide all occurred in the middle of 1954?

A. Yes.

Q. That assumption was presumably done on the basis of the information that you received from BNFL's predecessor?

A. From BNFL.

Q. From BNFL, yes. Did you critically review that at all or was it simply relying upon what they told you?

A. We had to rely on BNFL for information on what the discharge was.

Q. Yes. That is probably true throughout, is it not, that you have had to rely upon their discharge data?

A. Yes. That is not really our area of expertise, nor would you expect it to be.

Q. Would you be surprised, for example, to know that in August 1952 there were meetings minutes relating to cartridges being blown out of the back of a pile? Would that effect your assumption about the release all occurring in mid-1954?

A. It would depend whether that resulted in any release to atmosphere.

Q. Yes, but were you aware of the fact that cartridges were being blown out? Is that something you were specifically aware of when you made that assumption?

A. No.

Q. You must in looking at this data have known where the uranium oxide came from, i.e., out of these cartridges that were being discharged at the back of the pile?

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A. Yes, because we had to make assumptions about the burn-up of the fuel etc. We needed to know the radionuclide of the material that is released.

Q. Presumably you needed to make some estimations about the oxidation of the cartridges?

A. No.

Q. You relied on British Nuclear Fuels for that information?

A. We relied on British Nuclear Fuels for the estimate of what the release was from the stack.

Q. So at no point was it ever indicated to you that the release could have been before 1954, or the release could have started before 1954?

A. I asked a number of times, and I am sure there is correspondence on it, to the effect that when should we assume the release occurred, and the information I had was that there was minimal activity '52, '53 and releases were detected in '54, '55 and then subsequently to '55.

Q. MR. JUSTICE FRENCH: Just a moment. "I was told no significant releases '52, '53"?

A. '52, '53.

Q. Significant release '54?

A. '55.

Q. I am sorry, then?

A. Then some continuing releases after the end of '55.

Q. What about '54?

A. Significant releases in '54, '55.

Q. Significant releases '54, '55, then some releases in subsequent years?

A. Continuing releases in subsequent years, up to the time of the fire.

Q. In years '56, '57?

A. Yes.

Q. MR. READ: If the releases occurred prior to '54 then your graph would have to go up, that is right, isn't it?

A. It would depend very much on what the extent was. If they were just very small releases in '52, '53

Q. Yes, but if they were substantial releases then the graph would have to shift again?

A. If all the release was in '53, clearly.

Q. Yes.

Q. MR. JUSTICE FRENCH: The graphs would have to shift if there was significant release in '52, '53?

A. If it was substantial in '52, '53, yes.

Q. Significant, substantial?

A. Either word.

A Q. MR. READ: Anything more than a minor release is really what it comes to, isn't it?

A. No.

Q. You would not accept that?

A. No, because clearly there were releases in later years so this is a cautious assumption. I suspect if there were small releases in '52, '53 and also smaller releases in '56, '57 there would be some cancelling out of course.

Q. Let us just for the moment assume that all the releases occurred prior to 1955, or the vast majority occurred prior to 1955. Your assumption as it stands that the release actually occurred in mid-1954 would be all right for that provided there were no significant releases in the period prior to mid-1954?

A. Yes.

Q. But to sum it up, that assumption is based entirely upon what British Nuclear Fuels told you?

A. Yes, and of course the documentation would be provided with it as well.

Q. Were you provided, for example, with full minutes of all meetings of the United Kingdom Atomic Energy Authority, as it then was?

A. No.

Q. So you had none of that information?

A. We had papers by Dibben and Howells and so on.

Q. You see does it also come to this, that really the change from R171 to R171 Addendum came about wholly because of the change - on this uranium oxide point - wholly as a result of the fact that British Nuclear Fuels had to change the figures they gave to you?

A. Yes. Because of that we changed the way that we interpreted the data.

Q. Can I move now back to the plutonium issue from the plant and the revision of Prof. Jones. I have found the document that I wanted to go to before lunch. It is at page 69 in bundle P1.

MR. ROKISON: Before we get onto this new topic may I enquire of my learned friend, I do not know whether he is going to leave the topic that he has just left but if he is it is extremely unsatisfactory because it is based on a number of "ifs" as to which there has been no factual background laid at all. As your Lordship knows, my learned friend chose to call no evidence in relation to uranium oxide discharges, which includes of course the evidence in relation to what was found on site over the various years. My Lord, he may have given the impression through his questioning that there were significant

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A cartridges found at the back of the piles in the earlier years. If that impression has been given that is an impression which we would submit was wrong and is not supported by any evidence whatever.

MR. JUSTICE FRENCH: I have it in mind that it is an "if" question and there is no support for the hypothesis.

B MR. ROKISON: If he wants to pursue it in our submission the only way it could be pursued, if he wishes to pursue it in that way, is by putting all the relevant documentation to the witness and then asking the witness what if any difference it makes to his assessment. In our submission, to leave it as it is is wholly unsatisfactory in view of the stance which the Plaintiffs have chosen to take on uranium oxide.

C MR. JUSTICE FRENCH: I follow the point you make with respect Mr. Rokison, but it is an "if" question and if you wish to establish by re-examination and going through the documents that the "if" simply is baseless that is a matter for you. At the moment

D MR. ROKISON: No, my Lord, it is for the Plaintiffs to establish that the "if" has some basis.

MR. JUSTICE FRENCH: No, it is for the Plaintiffs to cross-examine and at the moment I am in the state of mind which I indicated to you, and if it goes no further well it goes no further. It gets the Plaintiffs nowhere.

MR. ROKISON: Very well, my Lord, thank you.

E MR. READ: My Lord, just so there is no misunderstanding about this, the documents that are involved in all of this are, in fact, included in the references. My Lord, they are documents they are not experts' reports and whilst I am not fully privy of any arrangement that was reached between Mr. Hytner and Mr. Rokison, the position as I understand it is that the two experts were not called who dealt with this specific issue, not necessarily because there was a dispute over the validity of the documents but solely over the validity of the interpretation of the documents. The documentary evidence is there and if my learned friend is suggesting we in some way be barred from putting that documentary evidence in as evidence

F MR. JUSTICE FRENCH: He is not suggesting that, he is suggesting you have not yet done it.

G MR. READ: My Lord, no. My Lord

H MR. JUSTICE FRENCH: I am going to permit the parties, unless something very strange happens, to conduct the case in the way they seek to do so. At the moment all you have got is an "if" question and an answer

based on the hypothetical "if". If you wish to pursue it, it is a matter for you. If Mr. Rokison wishes to demonstrate that the "if" is a baseless "if" then it is a matter for him. I am not going to direct the parties at this stage as to how they should conduct it.

Q. MR. READ: Dr. Stather, just so there is no doubt at all about this, you did not see any of the minutes, any minutes at all, relating to this issue of what cartridges were found where when you calculated your graph there?

A. Are you asking me up to 1984 or '86, up to the time we did the Addendum to 171?

Q. When you did R171 Addendum and you came up with your worst case assumption, at any stage then did you over-view, look through or otherwise go into the detailed minutes?

A. No, I did not go through the detailed minutes.

MR. JUSTICE FRENCH: Where are we? Are we back to P1, page 69, or are we going somewhere else?

MR. READ: My Lord, yes, I think if we go to page 69:

Q. Dr. Stather, one final question on that: have you ever seen any specific documentary evidence to support the information, the assumption, the worst case assumption that you derived?

A. I think the information I have got supports this approach that we took.

Q. What was that information?

A. That the substantial releases were in '54, '55.

Q. But what was the information, what you were told by BNFL?

A. I was certainly told by BNFL and there is documentation but I am not sure which particular piece of paper it is.

Q. It certainly was not contemporaneous minutes at the time?

A. No.

Q. Page 69, it is a British Nuclear Fuels document, the Introduction at paragraph 1:

"R and D"

- that is Research and Development -

"... investigations since April 1985 have shown that stack sampling procedures could be under-estimating routine aerial radioactive discharges by:

(i) between 50% and 5 times for alpha discharges, and

(ii) between 50% and 4 times for beta/gamma discharges.

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MR. ROKISON: What is this document?

MR. READ: It is page 69 in P1.

MR. ROKISON: What is the document?

MR. READ: It is a document that came on discovery from British Nuclear Fuels.

MR. ROKISON: What is it?

MR. READ: As I understand it, it is a contemporaneous British Nuclear Fuels document, isn't it?
A. Go to page 73, paragraph 14, and it says there:

"It is recognised that the above estimates provide only an approximate - although it is considered a realistic - guidance to enhanced doses from the present experimental reassessment of the sampling efficiency of routine aerial discharges. NRPB will be requested to do a more comprehensive reassessment using the computer models on which their previous dose calculations were made."

That would have been British Nuclear Fuels that would have been asking you to do that, would it not, Dr. Stather?

A. Yes.

Q. So going back to page 69, where one is looking at this introductory section, it would appear that the question of stack sampling procedures, SEFs, being under-estimating was on the cards from April 1985? Were you aware of that?

MR. ROKISON: My Lord, with respect this document is neither dated nor signed, nor has it been identified. We have no idea - it is referring to investigation since 1985 but we have no idea when this conclusion was reached at all.

MR. JUSTICE FRENCH: Is this a document which nobody on your side recognises?

MR. ROKISON: No, my Lord. There are thousands of documents in this case, as your Lordship knows, and we do not know where this has come from.

MR. JUSTICE FRENCH: I put the question: is there nobody on your side who knows where it comes from and you have given me the answer, there is no one, at this juncture.

MR. ROKISON: Not at the moment. We will obviously be able to find out.

MR. JUSTICE FRENCH: Are you proposing that I should stall this cross-examination until somebody has found out?

A MR. ROKISON: No, all I am merely asking is that my learned friend puts his questions upon an accurate, factual basis. He was asking a question on the basis that this shows that the question of stack sampling factors was on the cards from April, 1985. I was merely saying that it does not.

MR. JUSTICE FRENCH: No, because you do not know when the document was made.

B MR. ROKISON: That is all.

MR. JUSTICE FRENCH: I follow that.

MR. ROKISON: It is only the correct factual basis.

C MR. READ: My Lord, as I have already indicated, it is a document that has come on discovery from British Nuclear Fuels:

D Q. In order not to spend too long on this, Dr. Stather, whoever's document it is, if it is not a British Nuclear Fuels document - let us just suppose that for the moment - then the question I ask you is simply this: were you aware from April 1985 that the stack sampling procedures could have been under-estimating routine aerial or active discharges in respect of alpha, beta and gamma?

A. I was aware after we had produced the Addendum that there was a working group looking at sampling efficiencies and then I was aware that there was a meeting where COMARE was briefed, I think in 1988, that there were sampling efficiency factors which should be taken account of.

E Q. Yes, that confirms what you told me this morning that, in fact, it was not until after you had done the Addendum that you became aware of it, but of course you did the Addendum after 1985?

A. Yes.

F Q. Do you think if there had been any question of the SEFs under-estimating the discharge that is something you really ought to have known about?

A. If they were sure that the stack discharges were under-estimated, the answer is yes.

Q. Doesn't it go a bit further than that because if there was any uncertainty about it at all really that is something that you ought to have known about?

A. Yes.

G Q. Because your data on in particular plutonium needs to know, or involves knowing whether there has been a massive increase or an under-estimate in the amount of plutonium that has been coming out of the stacks?

H A. As the inhalation doses depend on a knowledge of stack discharges, yes.

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A Q. MR. JUSTICE FRENCH: When you say, "I should have known about it", do you really mean to say, "I should have been told about it"?

A. Yes.

Q. MR. READ: Because it is fair to say, is it not, that you had worries about the plutonium release before you did the R171 Addendum?

A. Before we finished it.

B Q. Before you really even started it. I wonder if you can look at page 103 in P1, Dr. Stather? It is a letter written by you to Dr. Anderson.

A. I remember it well.

Q. This clearly pre-dates R171 Addendum?

A. Because we had a lot of correspondence on Ponsonby Tarn.

C Q. It is clear from the letter that you had been talking to Mr. Anderson on Wednesday, 8th November, and the first paragraph continues:

"This concerned the difficulties we have found in comparing some environmental monitoring data with the recorded discharges from the Sellafield plant that we have available at NRPB.

D In the report recently published by Eakins and Cambray (AERE-R11182, 1985) on "Studies of environmental radioactivity in Cumbria Part 6: The chronology of discharges of caesium-137, plutonium and americium-242 from BNFL Sellafield, as recorded in lake sediments" information is presented on the concentrations of caesium-137, americium-241 and plutonium-239/240 at various depths in the sediment bed of Ponsonby Tarn."

E I do not think we have specifically referred to Ponsonby Tarn so far but

F MR. JUSTICE FRENCH: We had a core from Ponsonby Tarn referred to this morning.

Q. MR. READ: It was in effect a lake that one was able to take a core from the bottom?

A. That is right.

Q. Going back to the document:

G "This tarn is situated a short distance (< 1 km) due East of the Sellafield site. The profile of plutonium deposition in the bed sediments suggests that there was a substantial level of deposition of plutonium in the early 1960s with two other small peaks about 1952 and 1974."

H This is one sample, isn't it, that was taken?

A. Correct.

Q. And it showed by analysis that there were three peaks of deposition and those corresponded to the years '52, in the early 60s and '74?

A. You ought to go to page 132 in the Addendum.

Q. That is very helpful; I will come back to the Addendum in a minute but there effectively are, as you rightly point out from that, three points of interest. You go on in the letter to indicate:

"The peak found in 1952 corresponded to a single active particle and this could have arisen as a result of the release of uranium fuel particles in the mid 1950s. Julia and I have discussed the validity of the dating procedure, using lead-210, with Eakins and Cambray and they feel the dates given in the report are accurate to within about 3 years. What concerns us much more about these new data is the substantial peak in activity that was found in sediments deposited in the early 1960s. As you know we now have new information which suggests some significant atmospheric releases of plutonium in the 1950s. We also have information on discharges to the marine environment that peak in the 1970s and activity in land-blown seaspray will no doubt contribute to the total activity in the tarn. Some activity in Ponsonby Tarn will also have arisen from weapons fallout but this is a minor contributor. The only, and probably major, source of activity in the tarn must be atmospheric discharges from the Sellafield plant, and at present the data we have available suggest a fairly constant release over the whole time period from 1960 to 1982. There is clearly a discrepancy here that needs to be resolved and I would welcome any comments you might have."

So at that stage your concern was concentrating in particular upon the substantial level indicated by this sample for the 1960s peak, would that be fair?

A. Yes.

Q. It did not fit in with the data that you had been supplied by British Nuclear Fuels?

A. That is right, so we were questioning what we had been given.

Q. Yes. I wonder if you can go now to R171 Addendum which is in Volume "S" of the Common Bundle at divider 237, page 55, where you deal with the measurements of radionuclides in the lake sediment core?

A. The conclusion is really on page 56.

Q. I am glad you say that, it saves me reading page 55. Perhaps if we go to page 56 and if one looks at the second paragraph starting on that page, this is where you really start talking about the 1960s substantial peak, isn't it, Dr. Stather?

A. Yes.

Q. It indicates there:

"Although the substantial peak in the 1960s corresponds to the time of maximum fallout from nuclear weapons testing, sediment core profiles in lakes remote from Sellafield suggest that the plutonium-239 concentration in Ponsonby Tarn is too high by about an order of magnitude to be attributed to this source."

Of course, that is what you said in the letter, that you could disregard it as being nuclear weapons testing fallout?

A. Yes.

Q. It goes on:

"The pattern of air concentrations at Ponsonby Tarn resulting from Sellafield atmospheric discharges is shown in Figure 6.3. The calculated air concentrations take into account the effect of discharges from different stack heights, but there are a number of uncertainties that relate to particular releases. The total air concentration of plutonium resulting from the release of irradiated fuel particles in 1954 cannot be calculated because of the large particle size of much of the material released. The value given in the figure represents only the air concentration of the respirable fraction, taken to be 5% of the material released. Atmospheric discharges of plutonium-239 from Sellafield were reduced after 1960 when the pile cooling ponds were emptied and discharges up to 1968 were estimated to be fairly uniform with the exception of the release of 1.1×10^{10} Bq of plutonium from the cooling towers in 1961. During the two weeks when the major part of this release occurred the prevailing wind was in the general direction of Ponsonby Tarn. However, the concentration of plutonium-239 in air in 1961 shown in Figure 6.3 is based on the assumption that the release was equally dispersed in all directions. Although this is a conservative assumption for calculating doses at Seascale, it is likely to result in an underestimate of the air concentration at Ponsonby Tarn. This release might therefore have resulted in an elevated concentration of plutonium-239 in the part of the sediment core corresponding to 1961, and because of disturbance of sediments there would have been some vertical mixing in sediment profile."

Just pausing there, you are saying two things, I think, there, aren't you, Dr. Stather? Firstly, the wind may have been in that direction and that may account for the elevated peak?

A. To a limited extent possibly.

Q. And secondly, there could have been some disturbance of the sediments because of vertical mixing in the sediment profile?

A. That tends to average things out rather than leave peaks.

Q. In any event it may somehow be distorting the picture?

A. Possibly. The last sentence is the key one really.

Q. Let's read on:

"It seems unlikely though that it could have resulted in high sediment concentrations over a number of years, as is shown by the sediment profile in Figure 6.2 The calculated air concentrations in Figure 6.3 also show a peak in 1979 from the release of 1×10^{10} Bq of plutonium in that year. This was assumed to be a ground-level release that was equally dispersed in all directions. The sediment core profile in Ponsonby Tarn does not appear to match the profile of reported atmospheric discharges from Sellafield.

Discharges of plutonium-239 to sea are shown in Figure 6.4. They increased steadily up to the mid-1960s and then peaked at levels about an order of magnitude greater than discharges in the early 1960s. The pattern of liquid discharges of plutonium-239 from Sellafield also does not correspond with the pattern of plutonium-239 in the sediment core in the early 1960s."

Then as you rightly say, you conclude:

"Too much emphasis should not be placed on this sediment core analysis, however, as it is based on only a single sample. The analysis of plutonium levels in further sediment samples from Ponsonby Tarn and measurements of the contribution from earlier deposits brought into the Tarn by run-off water should give more complete information from which to estimate the pattern of plutonium releases from the Sellafield plant over the last 30 years."

I think what you are saying there is firstly it is only a single sample and one cannot necessarily rely on that?

A. True.

Q. And secondly I think you are also making the point there that earlier deposits could have been brought into the tarn by the run-off of water?

A. Yes, but I think the emphasis is basically on the sediment core profile and Ponsonby Tarn does not appear to match the profile of reported atmospheric discharges. I think that is a fairly clear sentence.

Q. Yes, so what it comes to

A. As is the pattern of liquid discharges of plutonium from Sellafield also does not correspond with the pattern of

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plutonium in the sediment core in the early 60s, so we are saying we do not get a match between the information on discharges and what is in the single core.

- A Q. Yes, it comes to this, doesn't it, that you had the information about discharges and it did not match this core sample?
- A. Correct, and we pursued this with Health on a number of occasions and also with BNFL.
- B Q. As you have raised that point maybe it is an idea to look at some of the correspondence, and I am afraid I am not necessarily sure whether this is the full correspondence that relates to it, but if you can go to page 159 in the bundle P1, this is a letter to you from British Nuclear Fuels on 21st April, 1986, where you deal with - you have given them a draft, haven't you, of your Addendum of NRPB R171?
- C A. Yes.
- Q. Unfortunately I do not think in the bundle we have got that draft, but in any event, if one goes to page 160 one deals with the measurements of radionuclides in the lake sediment cores?
- A. Yes.
- D Q. And this is dealing with Ponsonby Tarn, so Dr. Anderson is writing to you indicating:

"I remain somewhat unhappy about your treatment of the data relating to sediment cores from Ponsonby Tarn. While you have sounded a note of caution in placing too much reliance on these data I think such caution could have been expressed more strongly. Particular points which are relevant are as follows.

(i) The integrated plutonium data from Ponsonby Tarn are substantially less than those from the integration of soil core samples throughout the area around Sellafield. This therefore suggests that the plutonium data are not necessarily representative. We are all aware of the topographical factors which could lead to its known unrepresentativeness.

(ii) The recent note by Howarth and Eggleton has attempted to take account not only of current deposits but contributions from earlier deposits brought into the Tarn by run off water."

- G That is the point you were making in R171 Addendum?
- A. Yes, which is a fair point.

- H Q. "I think it might have been helpful if you had, with their permission, taken account of this recent work. It seems to me that in light of the various uncertainties such a statement as "it does not correspond with known releases from Sellafield" is probably a bit strong."

So there they are asking you to tone it down a bit?

A. But the sentence pretty well remained.

Q. Yes, it remains in a slightly different form though, doesn't it, I think?

A. It is still a pretty strong sentence, I think.

Q. Which sentence are you referring to?

A. There are two sentences saying the information on releases does not square with the information from Ponsonby Tarn.

Q. You are looking at page 57?

A. Page 56, bottom of the second complete paragraph on that page:

"The sediment core profile in Ponsonby Tarn does not appear to match the profile of reported atmospheric discharges from Sellafield."

That is not a toned down sentence particularly.

Q. No, I was not suggesting, Dr. Stather, you did tone it down. I was suggesting that the letter is saying to you, "Tone it down".

A. It may be suggesting that, yes.

Q. MR. JUSTICE FRENCH: The point you are making is the letter may have suggested that but you did not?

A. Precisely.

Q. MR. READ: Going on:

"(iii) It would also seem reasonable to me to add to the sentence at the end of the first paragraph on page 77 which begins 'There is no other peak ...' the words 'suggesting that the 1952 plutonium release has been overestimated.'"

That is dealing with the other peak, isn't it?

A. Yes.

Q. Then the fourth point is dealing also with the plutonium ratio in the 1975 peak. If we go back in the bundle but on in time to page 155 in P1, this is a further letter from Dr. Anderson to you thanking you for sending a copy of the paper:

"The general message which comes through is reassuring. However, as I have said previously I am still not entirely happy with your treatment of the Ponsonby Tarn data. In this respect I would like to suggest a more positive rewrite to the end of paragraph 5 for your consideration as follows.

After 'the total integrated activity' at the bottom of page 2 rewrite as follows:

'The release in 1961 is likely to have been the main contributor to the peak in the early 1960s as it occurred when the prevailing wind was blowing in the general direction of Ponsonby Tarn.'

So that is making the point that we see in R171 Addendum?

A. Yes.

Q. "Because of disturbance of sediment there would have been some vertical mixing in the sediment profile."

So that again is a point we have seen in the Addendum -

"Although the spread of high sediment concentrations over a number of years, as is shown in the sediment profile, is greater than expected. It might be explained partly by a run-off into the Tarn from the surrounding area. These results from Ponsonby Tarn are, however, based on only a single sample and more analyses are needed before any realistic attempt can be made to relate the reported atmospheric discharges of plutonium to the measured levels in the Tarn."

And would you accept that is a point also made in R171 Addendum, that you need more samples?

A. Yes. That would always be the case if you had just one sample. You cannot build too much from one sample. As you probably know more samples were in the end taken and we were very supportive of the fact that more samples should be taken and analysed.

Q. But it comes to this, doesn't it, Dr. Stather, that you really were touching on a point that has subsequently proved to be entirely correct, that the plutonium discharges were much bigger than the data you had already been given?

A. Yes, or have subsequently been calculated to be.

Q. Your calculations are coming from British Nuclear Fuels, I think you accept that already?

A. Yes.

Q. Of course, when you were doing R171 Addendum you were doing it then on the basis of the information that British Nuclear Fuels had already supplied to you. In other words, you had the core sample and British Nuclear Fuels information and at R171 Addendum stage you went more with British Nuclear than the one core sample you had? Would that be a fair way of putting it?

A. But clearly from this last sentence we were putting a lot of importance on that sample but saying more work should be done. I think the last sentence of that paragraph does stress the fact that we do see a discrepancy between what we see in the environment and what we were given.

A Q. But you do make the point, don't you, in that final paragraph on page 57 of the Addendum that, "Too much emphasis should not be placed on this sediment core analysis, however, as it is based on only a single sample"? Of course, perhaps if you had highlighted more strongly that single core analysis sample then it would have been more apparent that in fact British Nuclear Fuels discharges were wrong. Do you accept that?

B A. It did reproduce the figure. I think the fact that we included the figure on the Ponsonby Tarn profile, as well as explained it fairly fully that there was a discrepancy, is showing our concern, I think; that is demonstrating our concern.

C Q. You are putting the data in there but, of course, you are the expert and it is up to you to interpret that data, is it not? Would you accept that?

C A. As best we can. We couldn't interpret that single core profile in terms of discharges to atmosphere over many years, though.

D Q. Was it as a result of your concerns about this single sediment that British Nuclear Fuels put into effect their SEAM model?

D A. The SEAM model came into being three or four years later. I think the first stage of our concerns was for the Department of Health to fund more work to analyse the profile of plutonium in other cores from Ponsonby Tarn. A later core showed basically the same profile and that was followed by the implementation of SEAM 2 modelling.

E Q. The effect of the revisions of plutonium are quite significant, are they not, for the early years in the air concentrations?

E A. Yes.

F Q. In fact, I think we ought to have a look at the way the air concentrations have actually changed in respect of plutonium, because the air concentrations as I understand it are basically mathematical modelling dependent upon what you know from British Nuclear Fuels?

F A. Yes, in terms of discharge in each year and the stack height of the release.

G Q. There are three things I would like to look at. The first is Table 4.4 in R171; the second is Table 3.13 in R171 Addendum; and the third is Table H5 in your report.

G A. Table 4.4 first?

G Q. 4.4.

G A. In 171 or the Addendum?

H Q. In 171, which is file divider 236 in the core bundle at page 56, we have Table 4.14.

H Q. MR. JUSTICE FRENCH: I am sorry, I am getting a bit lost. Which are the ones you want me to fish out?

A MR. READ: My Lord, for this exercise I am afraid we are going to need two bundles. The first is the common bundle and the reference is S.

MR. JUSTICE FRENCH: I have got an S to U here. I have got an S216 to 230.

MR. READ: My Lord, it should be the one that you already have open relating to the R171 Addendum.

B MR. JUSTICE FRENCH: This is Stather.

MR. READ: I think it is the bundle that your Lordship has there.

MR. JUSTICE FRENCH: S231 to 237.

C MR. READ: My Lord, it is necessary to have file divider 236, page 56, which is Table 4.14. It is the same file as the R171 Addendum. R171 in the file immediately precedes the R171 Addendum in the references. My Lord, in R171, which is file divider 236, it occurs at page 56, the table that one needs to look at.

Q. Do you have that, Dr. Stather?

A. This is "Radionuclides in the Air 1952 to 1982"?

D Q. Yes. If one also has in the same bundle file divider 237 in NRPB Addendum at page 85, Table 3.13, which is the same table but updated, is it not, Dr. Stather?

A. Yes.

Q. So you have here the change from R171 to R171 Addendum, is that right?

E MR. JUSTICE FRENCH: If you want me to follow this, I am afraid you will have to be a little patient with me. What is the first folder that I must have in front of me?

F MR. READ: My Lord, if one starts with the common bundle references S, containing 231 to 237, there are two pages.

MR. JUSTICE FRENCH: I have got it here. Where?

MR. READ: My Lord, there are two pages that one will need to keep open in that. The first is in file divider 236, so if one looks for the file dividers down the side, my Lord, at page 56 ---

G MR. JUSTICE FRENCH: It seems to skip about from 136 to 237 in a curious way. Anyway it is 236 that I want, is it?

MR. READ: My Lord, yes, but it is also necessary I am afraid to look at 237. 236 is R171.

H MR. JUSTICE FRENCH: Do you think I could have a divider 136 when it ought to read 236? Is that possible?

A MR. READ: I think that must be the case, my Lord. My Lord, if one goes to the first page of the file divider and looks at the first page, it should read "The Risk of Leukaemia and Other Cancers in Seascale NRPB R171" at the front of the divider.

MR. JUSTICE FRENCH: So I have got mistakenly a 136 and it should be 236.

B MR. READ: Then, my Lord, it makes a great deal more sense now.

MR. JUSTICE FRENCH: I have got divider 236 and that is the one that you want me to have, is it?

MR. READ: My Lord, yes.

MR. JUSTICE FRENCH: What next?

C MR. READ: It is page 56 of that.

MR. JUSTICE FRENCH: That is Table 4.14?

D MR. READ: My Lord, it is necessary to go further on in the bundle, I am afraid, to the next file divider which was the Addendum you were looking at, and go to page 85 of that.

MR. JUSTICE FRENCH: Yes.

Q. MR. READ: Dr. Stather, do you have those two Tables?

A. I have got two tables.

E Q. One should be Table 4.14 in R171, and one should be Table 3.13 in R171 Addendum?

A. Yes.

Q. They both relate to the same matter, namely the concentration of the various radionuclides in air from 1952 to 1982? That is right, is it not, Dr. Stather?

A. 1951.

F Q. MR. JUSTICE FRENCH: 1951 in one case and 1952 in the other?

A. The addendum extended to 1951.

MR. READ: If one looks ---

G MR. JUSTICE FRENCH: Can I put aside anything else for the moment, or is there still something that I have got to have open?

MR. READ: My Lord, perhaps we can do the exercise just by looking at these two first, but then it is going to ---

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MR. JUSTICE FRENCH: You see, I want to know whether I can put these aside or whether I must keep them on my knee.

MR. READ: Yes, your Lordship is being snowed under! My Lord, do you have Dr. Stather's report?

MR. JUSTICE FRENCH: Yes, I have got Dr. Stather's first report.

MR. READ: My Lord, that is the only other document that will be necessary for this exercise.

MR. JUSTICE FRENCH: I shall keep that on my knee while I firmly keep this other one open like this.

MR. READ: My Lord, that is the part that you can get rid of.

MR. JUSTICE FRENCH: Yes, all right.

Q. MR. READ: If one looks at the discharge in 4.14 for 1952, one can see that that is given a zero in R171, is that right, Dr. Stather?

A. Yes.

Q. Because of the revisions for R171 Addendum, for 1952 that had actually gone up, or a figure had been estimated of I think 2.7? The photocopying is not very good on mine.

A. 2.7 10-to-the-minus-4.

Q. Which is 27 ten-thousandths, is it not, if one translates that?

A. 27 thousandths.

Q. If one goes on to 1953, which is the first year in Table 4.14 that there is a figure given, it is 1.5×10 -to-the-minus-6?

A. Yes.

Q. Again if one were to put that in layman's terms, that is 1.5 millionths, or is it 100 thousandths?

A. To-the-minus-6 would be a millionth.

Q. So it is 1.5 millionths, if one can put it in those terms. If one then goes to 1953 in Table 3.13, it is there 2.6×10 -to-the-minus-5?

A. Right.

Q. In other words, what we are talking about there is the concentration between R171 and R171 Addendum having gone up by a factor of about 20, is that right?

A. Yes.

MR. JUSTICE FRENCH: My copy, unfortunately, at page 85 is almost illegible. Can I make sure therefore that I am following this? Under plutonium 239 is the column that we are looking at, is it?

MR. READ: My Lord, yes.

MR. JUSTICE FRENCH: We get 1.50e minus-to-06?

MR. READ: My Lord, yes.

MR. JUSTICE FRENCH: I think I can just about read that. It is the same year, 1953. Is it 2.68 or 2.6E?

MR. READ: It is 6E, my Lord.

Q. Is that right, Dr. Stather?

A. It is always E-to-the-minus something.

MR. JUSTICE FRENCH: Yes, I now can read that with difficulty, thank you.

Q. MR. READ: I do not want to take you through all the other figures for the early years but it is about a 20-fold increase, is it not?

A. The increase from 2.6 to 2.7 is the result of the UO-2 particular release, assumed to be in 1954.

Q. The point I am making is that as we can see from these figures, the concentration of plutonium in air has gone up by a factor of 20?

A. Yes, for those early years.

Q. Before we leave R171, but unfortunately not the Addendum, can you also look at 1982, just so that we know the other end of the spectrum? That is 2.65 E-to-the-minus-6, so 2.65 millionths, and then the figure for the plutonium at page 85 in the addendum is 2.7, is it?

A. Yes, it is two figures rather than three figures. It is the same number basically.

Q. That is the point, it is the same number?

A. Yes.

Q. So for the early years one has an increase of 20-fold?

A. Yes.

Q. For the later years it does not affect it at all?

A. The reason for the increase being the information on releases from the pile ponds.

Q. Sorry?

A. It was plutonium released from the pile ponds, I believe, that was the reason for the increase.

Q. It has gone up 20-fold in any event

A. Yes.

MR. READ: My Lord, I have now finished with R171, if your Lordship wants to shut that part of it, but could your Lordship still keep page 85 of the Addendum open?

A Q. Dr. Stather, if one can go to your report as well, at Table H5, what I propose to do is simply to compare again what has happened since R171 Addendum in respect of the increase in the concentration of the radionuclides in the air. H5 occurs in Appendix H to your report and it is page 11 of Appendix H. If one now looks at 1953 again, the figure in R171 Addendum is 2.6×10^{-5} , and in 1953 in your report it has gone up to 2.8×10^{-4} ?

A. That's right.

B Q. So it has gone up tenfold, in other words? You are nodding your head?

A. Yes. You are just reading numbers off the two tables.

C Q. Just by way of comparison, if we look at 1982 in Table H5, we see that it is 4.6×10^{-6} , which I think has basically doubled or not quite doubled from what it was in R171 Addendum. There are obviously more figures in your Table H as time has gone on, but for 1982 the figure ---

A. It has roughly doubled, just under doubled.

D Q. So just taking the concentration of plutonium in the air, for the early years it has gone up 200-fold from what was in R171 to what is now in your report; for the later year, looking at 1982, it has gone up about twofold, something like that?

A. Yes.

E Q. Do you find such increases in the amount of plutonium in the air in the early years somewhat surprising, the way that it has had to increase from R171 to what is now in your report?

A. Yes, but it is the way Professor Jones has modelled the deposition close into the site that has resulted in this.

F Q. It comes to this, does it not, Dr. Stather: we all understand what you are saying about your dependence on British Nuclear Fuels' information, but that information has meant that you have had to increase the air concentration by a factor of 20 from R171 to R171 Addendum, and by a further factor of 10 from R171 Addendum to your present report, a 200-fold increase in the space of eight years? Over a period of eight years, is this right, it has been increased by 200-fold?

A. For those early years, yes.

G Q. Now you indicated in Chapter 4 of your report - I will not take you to it, but let's see if you agree with it - that for R171 Addendum about 10% of the dose related to alpha, I think it was, for the relevant cohort? Can you say "Yes"? The shorthand writer cannot see you nodding?

A. I realise that. Yes, that was a global figure taken over all the different cohorts.

H Q. What is the figure now? What are we talking about, on present information?

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A. I cannot tell you what it is for the global cohorts. I do not have a strict comparison, because all we have done for this report is to calculate doses to individuals.

Q. What do you think roughly it would be - 15%, 20%, 25%, 30%, 35%, 40%?

A. Maybe we should look at the calculations for Vivien Hope.

Q. MR. JUSTICE FRENCH: Where are you now?

A. I am on page 72, Table 6.3.

Q. Of your report?

A. Yes, of my report, page 72 Table 6.3. The way we have calculated the dose to the lymphatic tissue gives it about a third or so, I think, does it not?

Q. MR. READ: About 33%?

A. Yes.

Q. I appreciate that you have not done it for ---

A. It would be better to do it for bone marrow and then we would have a strict comparison. This is clearly a different tissue and not over the time frame that you are particularly interested in.

Q. In any event, what you are saying is that it has gone up from a factor of 10% to at least somewhere around 30% or 35%, something like that?

A. Maybe 20% or so. It is difficult for me to say what it would be to the bone marrow.

Q. I appreciate that. In any event, it is looking, is it not, as if alpha as the total part of dose has come up quite considerably from what it was when you were looking at R171 for the Seascale residents?

A. Yes. The counter, of course, is that the information from autopsy data is that we are overestimating body contents may be by an order of magnitude now.

Q. I will come back to that in a minute. Just looking at those figures, we are now looking at quite a substantial increase in the proportion of alpha in the dose figures?

A. I think that is probably right.

Q. I appreciate that you have been away out of the country, Dr. Stather, but are you aware that you have been asked to re-calculate the dose figures for the Seascale cluster?

A. Yes.

Q. Which, as I understand it, you have not had a chance to do yet?

A. That's right, or NRPB has not had a chance to do it.

Q. I think it is going to be necessary to go back to the R171 Addendum again which is file divider 237. I am sorry, my Lord, to have to keep juggling between documents.

MR. JUSTICE FRENCH: I am sure you cannot help it, Mr. Read. Where am I going to?

MR. READ: It is page 121 in the Addendum.

Q. Do you have that, Dr. Stather?
A. Is this "Doses to Lymph nodes"?

Q. Yes, that is what it was in R171 addendum. Obviously that dose will have changed as a result of the increases in the plutonium discharges?

A. Yes.

Q. Is it easy enough for you to re-calculate that figure?
A. We can do it. I think I have to say that to do these calculations - this is the way that people who do it express it - is not a trivial task, but we have obviously got the programmes to be able to do it.

Q. Are we talking about a matter of hours, days, weeks?
A. A week or so.

Q. So, for example, it would be possible for you to re-calculate that figure before closing submissions have to be handed in, within a week or so?

A. Yes. We have to do the calculations, to check them to make sure that they are right, and that that takes some time.

MR. READ: My Lord, it is obviously important that one should be able to put in perspective what has actually been happening in particular to the plutonium dose from R171 to R171 Addendum. Rather than have this witness have to come back and give further evidence on the point, obviously if it is possible for him and his colleagues at the NRPB to re-calculate that table so that one does have the updated figures before submissions are made to your Lordship, it would make the task of Counsel a great deal easier.

MR. JUSTICE FRENCH: Any objection, Mr. Rokison?

MR. ROKISON: I honestly say to your Lordship that I have no idea what the relevance of all these questions is. It is a very substantial task which is going to cost a lot of money to do. I would apprehend that there is no offer on the part of the Plaintiffs to pay for it in any event. My Lord, one looks in vain in my learned friend's opening and indeed in the lack of any positive case on this aspect of the matter, as to where on earth this leads.

The questions arising on environmental doses are, I suspect, twofold. One is, is the exercise so uncertain that one can make no assessment at all, which I understand is my learned friend's case, although it was not put in that way to Professor Jones, or are the estimates which have been arrived at by Professor Jones

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A and Dr. Stather in the respective ways in which they arrived at them the best estimates that one can make and, if so, what do those figures give you? My Lord, for the life of me I cannot see how it is going to help to go back over the question of whether R171 or the Addendum to R171 contained errors and, if it did, how much they have been corrected by.

B The position is that our evidence that we put before your Lordship is Dr. Stather's current report in which he sets out what his present assessment is, and the assessment that he may have made on different information in 1986 is neither here nor there.

MR. READ: My Lord, can I take some brief instructions?

C MR. JUSTICE FRENCH: Yes.

MR. READ: My Lord, perhaps I should ask Dr. Stather how burdensome a job it is going to be, because if it is going to be that burdensome, then we will take it no further.

D MR. JUSTICE FRENCH: Yes.

- Q. MR. READ: Dr. Stather, how burdensome is it actually going to be to re-calculate these doses?
- A. It is burdensome to the people who have to do it. Clearly they will spend quite a few days on putting the calculations together, as they always do when they do a set of calculations like this, and then it will fall on other people to check those calculations.

- E Q. It is a task that you have never done in order to see what the effect on R171 Addendum would be?
- A. We did some interim calculations in a preliminary way, but not any calculations that we would want to publish. At the same time as we were running these calculations, we did a quick look and see type of calculation.

F MR. READ: My Lord, I will not press that point any further.

MR. JUSTICE FRENCH: Very well, thank you.

- G Q. MR. READ: Can I now turn to the verification of your model?
- A. Maybe I should just point out that you are talking about lymph node doses here. They are quick look and see type of calculations, whereas red bone marrow doses are clearly what COMARE were interested in because of the effect on leukaemia, so they were not the calculations that you were ----

- H Q. So you have done some for the red bone marrow?
- A. The Table that I am looking at here is about lymph node doses.

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A Q. But you have the calculations to hand for red bone marrow?

A. We did some very preliminary but not final calculations for bone marrow.

Q. Obviously subject to that caveat, would you be happy to supply that in the course of this case?

A. It would still take a few days to do the calculations properly and completely and to have them checked.

B Q. MR. JUSTICE FRENCH: Have I got this right? You did a sort of preliminary check to see if you were in the right area?

A. Yes.

Q. Do the results of that preliminary check still exist?

A. Yes.

C Q. Would you be willing to hand them over?

A. I think I would not want to hand them over until we had run them properly to be sure that we were correct.

Q. They were good enough to check?

A. Yes.

D Q. Then why are they not good enough to hand over with all warts and it being understood that they are not complete and they were used as a check?

A. So they would be red bone marrow doses for the cohort born in 1950, with some explanation of the reasons for the changes?

Q. Yes. If you have got it, why not hand it over warts and all, it being understood how many warts there are?

E MR. READ: I am grateful, Dr. Stather.

F MR. ROKISON: My Lord, in those circumstances, if that is going to be done, I would respectfully suggest that what ought to happen is that NRPB through Dr. Stather would supply those documents to both parties because we have not seen them.

MR. JUSTICE FRENCH: Yes, certainly.

MR. ROKISON: Then Dr. Stather should return in order to be asked about them and explain them and what qualifications he would add to them.

G H MR. JUSTICE FRENCH: I think he has explained what the qualifications are, that they were simply a preliminary run to see if they were in the right area, and he cannot guarantee them without a proper run through, which, I agree, would be an onerous thing to do in all the circumstances, bearing in mind that we are dealing with doses which really are marginal when one is looking at the case as a whole, are we not?

MR. ROKISON: We would say irrelevant.

A MR. JUSTICE FRENCH: Never mind that. Even if they were relevant, they would be marginal to the case as a whole.

MR. ROKISON: I would think so, yes.

MR. JUSTICE FRENCH: Do you dissent from that as a proposition, Mr. Hytner?

B MR. HYTNER: My Lord, I have always said that this is a subsidiary issue.

MR. JUSTICE FRENCH: And you do not dissent from the way I have expressed it to Mr. Rokison?

MR. HYTNER: Not at all, my Lord.

C MR. JUSTICE FRENCH: There we are.

Q. MR. READ: Dr. Stather, can I now turn to the end of your model, which are the cross checks, the verification tests, Chapter 7? Just so that we have them in summary ---

D MR. JUSTICE FRENCH: Can I put R171 away?

MR. READ: My Lord, you will be glad to hear, yes.

MR. JUSTICE FRENCH: We are now going back to Dr. Stather No. 1 at page ...?

MR. READ: Page 81.

E Q. These in essence are the cross checks that you run against your model to see if it is performing properly?

A. Yes, the comparison of actual measurements, where they were available, with the model predictions. The doses in the tables are clearly based on the model predictions.

F Q. So you have your predictions which come from your model in the eight pieces of raw data that we discussed before lunch?

A. Yes.

Q. Together with mathematical modelling and assumptions?

A. Yes.

G Q. When those produced predicted figures, you then cross checked them against these items in Chapter 7?

A. That is right.

Q. Is that a fair way of putting it?

A. Yes.

H Q. What you are checking your predictions against are these three items that you have listed in Chapter 7. Just so

that we know where we are, those are the evidence from the environmental measurements, the whole body monitoring of caesium and the autopsy results?

A. That is right.

Q. From what you said earlier on, I think you would probably accept this, but it is fair to say that probably the autopsy data would in principle be the best check on your model?

A. For plutonium.

Q. For plutonium or indeed as a check overall. I know you are only checking the plutonium part but, for example, it does not have the problems that caesium or body monitoring has, which is that you are basically only looking at a particular year. With the autopsy data, you are looking over a cumulative period?

A. For plutonium, you are looking at cumulative intakes by different routes.

Q. You set out the autopsy data in Table 7.6 which is on page 93?

A. Right.

Q. There you list out seven cases, one of whom you indicate at the bottom, case 110b, was occupationally exposed?

A. Yes.

Q. So your cross checks that you have been looking at are six samples only?

A. That's right.

Q. You have actually put the residential documents down there, Seascale, Beckermest, Gosforth, Seascale, Gosforth, Seascale?

A. All either Seascale or the Seascale area.

Q. Why have you put case 110 in this Table?

A. I think it was the set of data that I had in front of me and it would have been more straight forward to have left it out, I suspect.

Q. Because there were at least three other cases, were there not, who were occupationally exposed?

A. We have got a number of other cases at NRPB that were occupational.

Q. I am just looking at the first column, because you list out there the concentrations in the plutonium in the various parts of the anatomy where it has accumulated?

A. Different tissues, yes.

Q. If, for example, one were to add in case 110 simply for the rib - I have not done it for all the others - it would bring the mean up from 8.5 to 25.82, would it not?

A. Surely.

Q. I am just interested why case 110 gets in there and the various other cases that were occupationally exposed do not?

A. It would have been better to leave it out, I suspect, but I think the Table it comes from had all the seven cases in.

Q. What I wondered was whether there was any question of case 110 perhaps having only worked part of the time at Sellafield and therefore there was a question mark over whether that case should actually be excluded or not?

A. No, I don't think so. It is certainly an occupational case and I cannot now remember the time period when the work at Sellafield was.

Q. One can see just by looking at case 110 that the effect it would have is that one case with a high dose content, or a high concentration in the tissue, would actually bring the mean up quite substantially, would it not, just looking at that case and using it as an example?

A. If I included other occupational cases, it would go up even more.

Q. The point I am making, Dr. Stather, is that this data here is really rather fragile, if I can put it like that. One high case distorts it right out?

A. I don't think it is fragile, because the six cases that clearly weren't occupationally exposed were very consistent one with another.

Q. The selection of samples for that autopsy monitoring had to exclude young people, did it not?

A. These were people born in 1949/1950, or lived in the area.

Q. You cannot actually get data on anyone under 50, can you, or rather it is not easy to check the data on anyone under 50?

A. These are accident cases, autopsy cases.

Q. I wonder if you can look at the bundle P1?

Q. MR. JUSTICE FRENCH: Page ...?

MR. READ: I am afraid I have lost the reference again. I am afraid I have taken you to the wrong file, my Lord. One will have to go to the common bundle to see this point. It is Volume O to R, 196 to 215, which I think your Lordship had out already.

It is volume O-R, 196-215. It is file divider 201, page 304. That is a report by Dr. Popplewell into "Plutonium in Autopsy Tissues in Great Britain". It was published in "Health Physics".

MR. READ: Your Lordship has the relevant article?

MR. JUSTICE FRENCH: I am looking at page 304.

MR. READ: My Lord, that is it:

A Q. If one looks at the heading "Materials and Methods", it indicates there:

B "Autopsy tissues were obtained from pathology departments in central Scotland, northeastern England, western Cumbria and Oxfordshire, England. The subjects were at least 50 years old at the time of death. The cause of death in most cases was from cardiovascular disease. Usually in the Cumbrian autopsy cases, positive information was available that the subjects had not been occupationally exposed to plutonium during their lifetime;"

C Then it indicates the way the data was obtained. The reason the subjects had to be at least 50 years old, as I understand it, was that it is very difficult for younger subjects than that to actually analyse their tissues for plutonium?

A. No.

Q. You don't accept that?

A. No.

D Q. Why was it then that Dr. Popplewell was saying the subjects had to be at least 50 years old?

A. I am trying to remember. I presume it is just because he wanted people who had been in the particular area for an extended period of time. Can you point to the sentence where he mentions 50 years... Oh, I have found it.

E Q. "...the subjects were at least 50 years old..."

MR. ROKISON: I was asking my learned friend where he said the subjects had to be at least 50 years old.

THE WITNESS: I think it just says they were.

MR. READ: My Lord, I put that as a question.

F MR. ROKISON: The question was: why did he say they had to be at least 50 years old.

MR. READ: Perhaps I phrased the question wrong.

THE WITNESS: I think it is just a factual statement.

G MR. READ: I think what I should have said is why the subjects were at least 50 years old.

THE WITNESS: I think it is just a factual statement by Dr. Popplewell that they were.

H MR. JUSTICE FRENCH: On the other hand, if one is dealing with accumulated dose, it is as well to have

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somebody who has had time to accumulate a dose, so there would be sense in it, although I am not saying that that is why it was done that way.

THE WITNESS: Autopsy cases that have resulted from problems with cardiovascular disease are likely to be people older rather than younger.

Q. MR. JUSTICE FRENCH: In fact, those who were at least 50 in 1985, if that be the relevant date, then they would have had 30-odd years under the influence, whatever it may have been, of Sellafield?

A. Yes. As all these cases in Table 7.6 clearly are.

Q. MR. READ: Looking at it in the round, though, what we are talking about here are six cases only? That is what we are talking about your validation exercise being done with six cases only in respect of the plutonium tissues?

A. I think that is a substantial number.

Q. MR. JUSTICE FRENCH: Never mind whether it is substantial or not, it is six cases?

A. It is six cases.

Q. MR. READ: It is rather meagre data, isn't it, Dr. Stather?

A. I think the answer is no to that.

Q. Have you still got Dr. Popplewell's report open? Go on to page 308, the final paragraph:

"The concentrations of plutonium found in the Cumbrian samples were compared with the concentrations for the other regions of Great Britain using a two-sample t test. The concentrations are generally higher for Cumbria with significant differences for femur, liver and lung. Clearly more autopsy cases from western Cumbria are needed to augment this rather meagre data available at present."

Dr. Popplewell seems to be thinking that it was rather meagre data?

A. Well, he would like more samples, I'm sure, but I still think six samples, with not much variation between them, is quite a good sample.

Q. MR. JUSTICE FRENCH: Well, the "meagre" has been augmented, hasn't it?

A. I think particularly, as they are people who lived in the area for really the time period we are interested in, from 1950 through to 1980.

Q. MR. READ: The fact of the matter is, as Dr. Popplewell said in a later report, there is not a reasonable number of cases upon which to base any conclusions? I will take you to the report if you need it. Let me read it out:

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"It was difficult to secure a reasonable number of cases from west Cumbria."

- A That is what he says.
- A. I am sure he would like more cases if he could get them.
- Q. What you are saying is six cases is sufficient to validate your model on plutonium? Is that what you are saying?
- A. I think given that there is a reasonable consistency between them and that we had people living in the area for the period in which we are interested, I think they are very useful data in this respect. I also believe measurements are much more useful than models if measurement data is available to you.
- Q. Undoubtedly. Dr. Popplewell's results were published in 1985? I think it was in fact received in 1984, that report. Why was it that one has this large revision in the amount of dose coming from plutonium, from 10% to approximately 35%, from R171 Addendum to your present estimates, and yet this autopsy data didn't suggest there might be anything wrong with R171 Addendum?
- A. Well, I have already said models tend to have caution built into them.
- Q. At the end of the day it proves the point that this autopsy data, helpful as it may be, cannot really establish anything because it isn't big enough as a sample size? If it was a large enough sample size then something ought to have told you that something was wrong with R171 Addendum?
- A. I think you have got six people here who lived in the Seascale area. They were breathing the air, eating the local food, and from those six people we see levels of plutonium in their bodies that are considerably lower than we predict, which to me indicates that there is caution in the models. The tendency is for the models to overestimate intakes, by inhalation or by ingestion, or by both pathways. That you cannot tell clearly from the autopsy data. You don't know where the source of the plutonium is.
- Q. I think I have made my point on that, Dr. Stather. Now can I turn to the measures of the air concentration?
- MR. JUSTICE FRENCH: If you are leaving autopsies could I interpose a couple of questions?
- Q. I just want to understand how accurate the autopsy data is and can I put it to you, as I understand it to be, and then you tell me whether that is right or wrong? You take a specific length of bone or sample of bone marrow or whatever it may be?
- A. Yes.
- Q. You then, using no doubt very sophisticated methods, not calculate but measure the amount of plutonium in that sample?

A. Yes, my Lord.

Q. You then say, "I have got X grammes of this tissue. This body is of a 6' man weighing X stones. These x grammes represent such-and-such a percentage of the total?"

A. Yes.

Q. So that you are actually measuring something and the only room for estimate, with the built-in errors that estimates have, is that you are getting the right relationship between the size of your sample and the true size of the total of that from which you have taken the sample?

A. Yes.

Q. Apart from what must necessarily be an estimate as to the size of the total of the tissue, it is a measurement, not an estimate?

A. It is a measurement, that is right, and it is usually to try and get a substantial sample of both the liver and the bones that are being analysed. Of course, it is these two tissues that accumulate the vast majority of plutonium that enters the body.

Q. Yes. So you take as much as the pathologist will let you?

A. That's right.

Q. MR. READ: If we can move on now to one of your other validation exercises, that is the measured air concentrations from 1978 to 1988, the first of these start in 1978, don't they?

A. Yes, they are later rather than earlier.

Q. Now we have already seen from the way the plutonium air concentrations went up that there was a substantial increase in the 1950s from R171 to R171 Addendum, to your present figures. We have seen that certainly in respect of 1982 it is basically gone up two-fold from R171 to your present estimates?

A. That is right.

Q. When one is actually measuring these air concentrations, what one is actually looking at is the later period?

A. Yes, that is true.

Q. It cannot help you to pick up this 200-fold increase that occurred from R171 to your present estimates?

A. No.

Q. So from that point of view the air concentrations cannot validate anything much that happened before the mid-1970s?

A. It cannot validate anything that is prior to the first day of samples.

Q. So it helps us not at all with the earlier period. Moving on to the caesium...

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C. MR. JUSTICE FRENCH: Are you agreeing with that?
 A. Yes. If you don't have any measurements you cannot use the measurements to validate the calculations.

Q. So measured air concentrations cannot validate any assessment before what date?

Q. MR. READ: 1978, I think you say in your report was the first measurement?

A. Yes.

B. Q. So we turn to the final validation exercise, which is the caesium estimates - whole body monitoring?

A. That is right.

Q. I think you have already indicated that caesium does not really stay around the body for a particularly long period?

C. A. It has got a half time of about 110 days in an average adult.

Q. Just so we have got this clear, that is a biological half time you are talking about, which is not the same thing as a radionuclide half time?

A. No, the physical half time for caesium is 30 years.

D. Q. In other words, the caesium will come out of the body more or less within a year?

A. Yes.

Q. If you can look at page 89 of your report, this is the first table of measurements that you actually have relied upon in your validation?

A. Yes. These were six residents near Sellafield.

E. Q. I think there were eight individuals?

A. Maybe eight. They were all employed at Sellafield.

Q. One of them was female?

A. That is right.

F. Q. They all come from Dr. Rundo's reports?

A. yes.

Q. If we need to check the precise numbers we can see that from his reports?

A. Yes.

Q. They only start from March 1957? That is the first one, isn't it?

G. A. I think you mean April, 1957?

Q. The first one is March, 1957, isn't it? They are done in chronological order. You are looking at table 7.2?

A. Yes.

H. Q. These are the results from Dr. Rundo's papers. The first estimate is March 1957, so it is going to tell us

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very little about the period before the beginning of 1957?

A. Yes.

Q. It cannot validate what was happening before that time?

A. No.

Q. You say, "no", do you mean...?

A. I mean it cannot validate what happened in, say, 1955, in terms of releases.

Q. Would it be fair to say we don't know much about where these individuals lived?

A. Yes.

Q. We know they all worked at Sellafield?

A. Yes, that is right.

Q. But we don't know where they came from?

A. Yes.

Q. From that point of view...

MR. JUSTICE FRENCH: Are you using where they came from as synonymous with where they lived?

MR. READ: My Lord, yes:

Q. Unlike the autopsy data, we haven't got anything in here that specifically tells us, for example, about Seascale?

A. No.

Q. The next set of data is on page 90 in your report, Table 7.3. That is obtained from Dr. Hesp?

A. Yes.

Q. Again, if we need to check it we can look at his report. My calculation makes it 20 males and 2 females we are looking at here in total?

A. There were groups of 4 to 11 I measured on different occasions.

Q. Anyway, the information will be in Dr. Hesp's report to tell us how many he was actually looking at?

A. Yes, that is correct.

Q. Again it is fair to say we don't know where these people lived?

A. They were employed at Sellafield and some of them obviously lived in the area.

Q. Well, how do you know that?

A. I cannot say where they lived.

Q. You can only say they must have lived within commuting distance of Sellafield, but apart from that you cannot say anything else from the reports? That is fair, isn't it, Dr. Stather? Again, that is not telling us

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anything specific about what might have been happening, for example, at Seascale? If we then go on to page 91, which is table 7.4, this is where we pick up again later on, I think it is British Nuclear Fuel's measurements, isn't it?

A. That's right. These are people from Seascale, Calderbridge, Gosforth, that area.

Q. Yes, we can identify them more readily now. The first of these measurements starts in 1979 and then the next set of measurements - there is a two year gap and then they continue annually thereafter. If one then goes on to page 95 in your report, one sees the predictions that you make of annual average body content. This is basically what you are using, this caesium whole body monitoring, as a cross-check, isn't it?

A. Yes.

Q. We know from the tables that the first one occurs in March, 1957. Is that right, Dr. Stather?

A. I am sorry, I lost the first half of the question.

Q. At page 95 what we are looking at here are your predictions of the annual body content?

A. Yes, from both weapons fall-out and from the Sellafield discharges, and a combination of both.

Q. We can actually see that on the graph? The black line is the total, the dotted line represents the Sellafield discharges, and the hatched line represents nuclear fall-out?

A. Right.

Q. If we look at it, the first two peaks as one goes from left to right across the page are primarily due to Sellafield discharges?

A. Yes.

Q. The second one, of course, being 1957, the Sellafield fire?

A. And the first one being the UO-2 particles.

Q. The next peak in 1963 is in fact nothing to do with the Sellafield discharges but to do with weapons testing, and then the next major peak takes place in 1975, which is the peak of the marine discharges?

A. Yes.

Q. Again that is Sellafield discharge driven rather than nuclear fall-out/weapons testing driven, because by that time the fall-out from nuclear weapons has gone down considerably?

A. That's right.

Q. If one just considers for a minute the dates of the data we actually have, the first one is in March 1957... I am sorry, Dr. Stather, you are obviously looking at something there?

DR J W STATHER

A. I was just seeing what else I had got on the individuals in the tables, and I am not sure I have got what I want with me.

A

Q. MR. JUSTICE FRENCH: Can I just understand Table 7.1, and make sure I am following it? Are the figures in thousands there GBq?

A. Becquerels.

Q. MR. READ: The first validation you have got for whole body monitoring is 1957?

B

A. That is right.

Q. So it completely misses the UO-2 discharge?

A. That is right.

Q. The final case, I think, was 1968 for that...

C

A. If it really was the peak in the way we have modelled it, of course.

Q. On your model?

A. Yes.

Q. What I was putting to you is that these caesium whole body monitoring doses could not validate that peak?

A. Correct.

D

Q. If we go back to the first batch of monitoring, that is from 1957 to 1968, it catches the tail of the Windscale fire and really does not pick up again before the marine discharge figure...

A. Starts to come up.

Q. The next figure we have is 1978, when, of course, the marine discharge figure is coming down again?

E

A. Yes.

Q. What we are looking at for your validation checks on this is really not a period of high discharges, but a period when on your predicted model the discharges are low? Would that be fair?

F

A. Certainly not at the peaks of discharges.

Q. It cannot validate those peaks of discharges, can it?

A. No. As I have said, it can only validate for the years we have body measurement data.

G

Q. Have you got Prof. Jones' report to hand there? The figure I want to take you to is Figure 12.2 which occurs at the end of chapter 12. It is about six pages back in before one then gets on to the annexes. He is doing the same thing here, isn't he, from his SEAM predictions? Is that right? The same exercise you were doing?

A. I think so, yes.

Q. He is?

H

A. Yes.

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Q. Again he is using data to try and validate this. Now the peak for the UO-2 discharge on your figure is predicted to be just over 2,000 Bq of caesium-137?

A. That is right.

Q. The peak for the corresponding point on Prof. Jones' model is 4.5 times greater, isn't it? No, it is not 4.5 times. It is 7,000 Bq of caesium.

A. That is three times greater.

Q. Three times greater. So the predictions from his model are three times as great as the predictions from yours? Would you accept that?

A. Well, I have to do from what I read on the Figure.

Q. Yes, but if these figures are right, then that is correct? For the fire, you have got a prediction of under 2,000 and he has got a prediction of over 10,000 Bq?

A. It would appear to be the case, yes.

Q. You see, both of you tried to validate your models by using caesium whole body monitoring, didn't you?

A. Yes.

Q. Both of your models have come out with wildly different predictions for those two peaks?

A. Certainly different predictions.

Q. Well, three-fold on one, five-fold on the other? You don't think that is wildly different?

A. Three-fold and five-fold is a difference of a factor of 2, isn't it? There are certainly differences.

Q. Yes. What this goes to prove, Dr. Stather, is just how careful one has to be of any modelling, that you can get wildly different predictions from the model and people saying that it is validated by more or less the same data?

A. Certainly I would agree that if different people build different models, they will come up with somewhat different predictions.

Q. At the end of the day would you accept this, that your model is nothing more than really a complex chain of reasoning with many uncertainties and basically sparse environmental monitoring? Do you want me to run through those again? A complex chain of reasoning, many uncertainties, sparse environmental monitoring...

A. It is certainly a chain of compartments that you are concerned about. You are using information on discharges combined with as much environmental measurement data as you have available to make predictions of doses.

Q. As you have already indicated, you are dependent on a great deal of BNFL's data?

A. For a number of the doses we calculate, certainly.

DR J W STATHER

A Q. Let me put this to you - it is what Prof. Jones said the day before yesterday. For your Lordship's reference it is Day 6 of the transcript, page 56E. I will read it for you. Mr. Hytner, who was cross-examining Prof. Jones, asked:

B "Q. Is it your case, in relation to discharges generally, that the exercise performed by Dr. Stather is so independent of your work that, however inaccurate were the figures you gave him, his results would be the same?

A. No, of course, that is not my case and that is not what I said when I presented my report in examination. I made it clear that Dr. Stather has taken my discharge figures and done his own assessment based on those."

C This is the bit I want to refer you to:

"In particular, the discharges of plutonium that I have written into my report must affect Dr. Stather's calculated doses because that delivers dose largely through inhalation and there were no relevant environmental measurements carried out in the 1950s."

D Would you accept that statement?

A. I haven't disagreed with it.

Q. You are not disagreeing with it?

A. No, but I would say we do have the autopsy data which provides information on people living in the Seascale area in the 1950s.

E Q. Which of course you had for R171 Addendum?

A. Yes.

F Q. You see, do you feel at the end of the day it is perhaps a little unfortunate the way this information has come to you because you are dependent upon BNFL to provide you with the information? Firstly, there is a massive change in the uranium oxide, and now we find there is quite a substantial change in the plutonium. Do you think that is rather unfortunate?

A. Clearly it would be better if you got all the information at one point in time and did one assessment, rather than having to revisit the data. Although you talk about a massive change in the release of UO-2, that in fact has not changed the dose calculations very much.

G Q. Of course you would accept that the plutonium has had quite a significant impact because we know from what you have told us earlier that the alpha as a component of dose has gone up from 10% to... Your approximation was 35%?

H A. We will have the figures in a week or so.

DR J W STATHER

A Q. You see, what I have to put to you is that we will never actually know the environmental doses of radiation because the data is so patchy for that period? I am talking about the period in the fifties and sixties.

A. The fifties you are talking about really?

Q. The fifties. We will never really know what the doses were for that period?

A. I am sure we will never know to within tens of percent.

Q. Within?

A. Tens of percent - clearly not.

Q. MR. JUSTICE FRENCH: To what? To within 20 or 30, 30 or 40?

A. I think we are talking about estimates to within a factor of 2 or 3, that sort of order.

Q. "We will never know the environmental doses"?

A. Yes, to better than a factor of maybe 2 or 3.

Q. MR. READ: Finally, Dr. Stather, can I ask you to go to volume C in the common bundle? It is 29-51. It is file divider 38, which is the COMARE 1 report. Perhaps we can go to the conclusions on page 23, paragraph 5.2 in the conclusions of COMARE 1:

"The way in which these data came to light is unsatisfactory and undermines our confidence in the adequacy and completeness of the available data. Although we accept that every reasonable effort has been made to ensure completeness of the information now available to us we feel that the monitoring programme and record keeping for the 1950s were such that we cannot be certain that all releases have now been recognised."

Would you accept that as a general comment?

A. As a general comment from when this was produced, or still?

Q. Still.

A. I think with the modelling exercise that Prof. Jones has done we are in a better position now than we were when this was written.

Q. But it still holds true?

A. There must remain some uncertainties.

Q. Paragraph 5.5:

"We believe that the dose and risk estimates presented by NRPB represent a reasonable picture based on conventional assumptions in the field of radiation protection. However, we have reservations about this conventional framework. A very complex chain of reasoning, involving many uncertainties, is necessary to go from the release

data and the sparse environmental monitoring data to a prediction of any possible adverse health effects."

A

Would you agree with that statement?

A. Well, it is certainly a complex chain of calculations that you need to go through.

Q. At the end of the day, Dr. Stather, it is nothing more than a model, is it?

B

A. It is a combination of models and environmental measurements, so it is not just a model.

Q. Can I briefly now turn to the relationship of British Nuclear Fuels and NRPB? There was a document prepared yesterday. I do not know if you have actually got a copy to hand.

C

MR. READ: My Lord, it is the same that Mr. Hytner yesterday put to Prof. Jones. It is annexed to the opening now, of course, my Lord. Your Lordship indicated yesterday that is where it should go.

MR. JUSTICE FRENCH: Remind me, how many points has it got?

D

MR. READ: My Lord, it has got seven points. My Lord, while your opening is being obtained, perhaps your Lordship can be given another copy. (Produced)

MR. JUSTICE FRENCH: Thank you.

Q. MR. READ: Dr. Stather, you were in Court, I think, yesterday when Prof. Jones was looking at that?

E

A. Yes, I was.

Q. So have you had an opportunity to consider it?

A. I looked through it at the time when it was being gone through in Court.

Q. Is there anything in that that you take distaste to, that you object to?

F

A. I just think the tenor of what is here is incorrect.

Q. I see. I do not want to take up time taking you through all the documents again because Mr. Hytner has obviously already taken you through that - sorry, Prof. Jones.

G

MR. ROKISON: My Lord, I do not know what they are going to say about this in their submissions but, if they are going to use it in any way to cast any doubts whatever on the evidence of Dr. Stather or documents that he has prepared, then I expect my learned friend to take him through it and put anything he wants to put. Whether Mr. Hytner has put it to Prof. Jones or not is neither here nor there.

H

DR J W STATHER

MR. JUSTICE FRENCH: Yes. Again I am not going to dictate how the case should be conducted, but you will have a very nice series of comments if the course is not taken.

MR. ROKISON: Thank you, My Lord.

Q. MR. READ: Dr. Stather, let me make it clear, as Mr. Hytner made clear yesterday, there is absolutely no suggestion that the NRPB have acted improperly in any way or in breach of the statutory function. Your function, the NRPB's function, is advisory rather than of an enforcement agency or a watchdog. So you accept that. You are happy with that part of the statement?

A. We are not a regulator. We are an advisory body.

Q. Sorry, you are not a....?

A. We are not a regulator. We are an independent advisory body.

Q. You are an independent advisory body. You are not a watchdog, if I can use the phrase there?

A. The 'papers term us a watchdog.

Q. So you do not object to that first paragraph?

A. No.

Q. "Paragraph A: The development of personal relationships between the personnel in two bodies."

Do you object to that?

A. Professional relationships is what I would say. I believe I recall Prof. Jones said the same.

Q. "B. A willingness on the part of the NRPB to remove from draft reports materials which might have been of interest to the public but embarrassing to BNFL."

Do you object to that?

A. Yes, I do. I believe you are referring to documents, I suspect, that have gone to BNFL for comment, documents which, I think, also went to the Department of Health for comment. Where there were technical issues, then if what we had in drafts was incorrect, we would change them.

Q. Let us just go to page 172 in the bundle.

MR. READ: This is the bundle P1, my Lord. Do you still have that bundle to hand? It is the black file.

MR. JUSTICE FRENCH: Yes, what page?

MR. READ: It is page 172, my Lord:

Q. Do you have that page, Dr. Stather? It is page 172?

A. Yes.

A Q. Perhaps before I take you through that, can I try and see if there is a way to short-cut this. You have already told us, accepted, that you were not a watchdog as such?

A. No, I said we are not a regulator. We are an independent advisory body, but we are not a regulator. The term "watchdog" is certainly used by the media for NRPB.

B Q. Which is wrong because you are advisory? The fact of your relationship with British Nuclear Fuels led you to accept their data. Would you agree with that?

A. Yes.

B Q. You did not investigate their data in great depth. It was not your duty to do that?

A. We are not in a position to investigate information on discharges.

C Q. Such was the relationship, therefore, between you that, from time to time, you took on trust the information they gave you where, if the relationship had perhaps been more of a watchdog body, you may have been more interested in ensuring the accuracy of their data?

A. We are not an organisation that can validate information on discharges that would be given to us by an operator. A regulator might be able to do that, but we cannot do that. We do not have the expertise. You have to know information about the plant operation, its characteristics, to come up with information on discharges, so we just cannot validate that information.

D Q. So would it be fair to say that your relationship was such that, from time to time, you took on trust, when given information, that information without probing further? Would you accept that?

E A. The way we have probed further is to look at the environmental data, which would or would not support the information we were given. In some circumstances, you can do it; in others, you cannot.

MR. READ: Yes, I do not think I need to take the witness further through this, my Lord.

F MR. JUSTICE FRENCH: Very well.

MR. READ: Thank you, Dr. Stather.

MR. JUSTICE FRENCH: Well now, Mr. Rokison, will you be more than five or 10 minutes?

G MR. ROKISON: Possibly, my Lord, yes. I would like to think about it and go through the evidence and decide. I think it would be probably more convenient, if your Lordship has any questions....

MR. JUSTICE FRENCH: No, I have not.

H MR. ROKISON: Well, my Lord, I think, if it is not too inconvenient to Dr. Stather....

DR J W STATHER

MR. JUSTICE FRENCH: No, I was only thinking of Dr. Stather's convenience.

MR. ROKISON: I know. So was I. If it is not too inconvenient for Dr. Stather perhaps to come back for half an hour tomorrow morning, I think that would be better.

MR. JUSTICE FRENCH: Yes. Very well, we will break off now and resume, please at 10.30 tomorrow morning.

(The Court was adjourned until the following morning
at 10.30 a.m.)

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