

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

Monday 9th 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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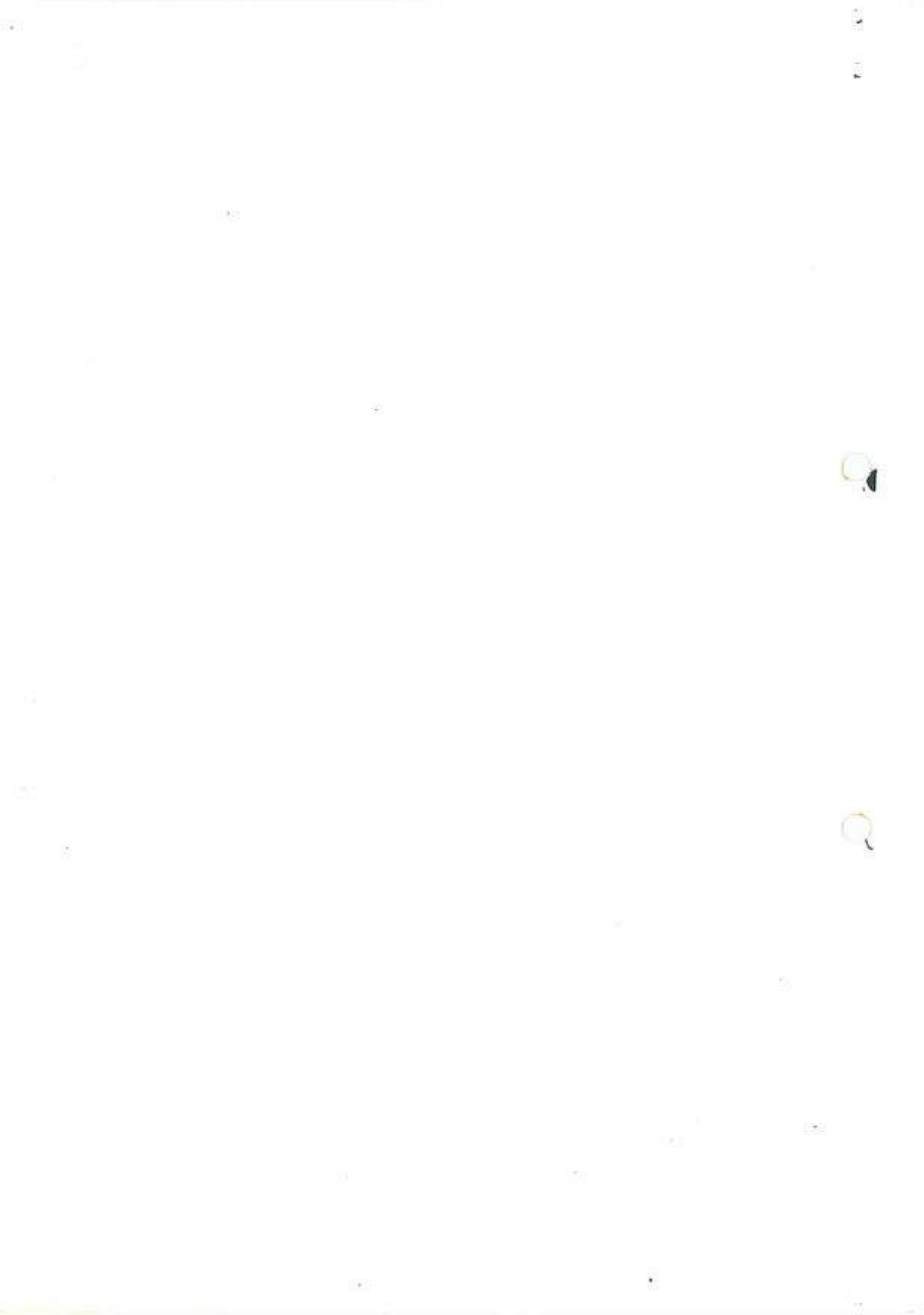
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STEPHEN RICHARD JONES Called (Sworn)

Examined by MR. ROKISON
Cross-Examined by MR. HYTNER

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SIXTH DAY'S PROCEEDINGSMONDAY, 9th NOVEMBER, 1992

A MR. ROKISON: My Lord, this morning we start the environmental dosimetry. My learned friend has confirmed he will be calling no evidence on that aspect of the case. In those circumstances I call Prof. Jones.

B MR. JUSTICE FRENCH: Before you do that I seem to remember there was a document outstanding which I think Mr. Hytner was hoping to receive and had not received.

C MR. HYTNER: My Lord, we received it a little late, true. I saw it on Sunday. My Lord, it is not a document which took long to digest and in the circumstances if they had sent it this morning I would not have been objecting. My Lord, we have got it.

C MR. JUSTICE FRENCH: In due course shall I have it or is it not necessary?

MR. HYTNER: My Lord, I think you probably will have it.

D MR. ROKISON: My Lord, it is a statement of a witness I was going to call in due course. It depends on timing, but whether after Prof. Jones or after Dr. Stather, but certainly in this part of the case.

MR. JUSTICE FRENCH: I need not be concern about it?

E MR. ROKISON: Your Lordship need not, I think, for now. I call Prof. Jones.

STEPHEN RICHARD JONES SwornExamined by MR. ROKISON

F Q. Your name is Stephen Richard Jones?

A. It is.

Q. What is your address?

A. My address is Summerhill, Fern Bank, Cockermouth, Cumbria.

Q. You are currently employed by British Nuclear Fuels Plc?

A. I am

G Q. In what capacity?

A. My job title is now different from that which it was when my first report was produced. I am now Director of Environmental Research at the Westlakes Research Institute, which is funded by BNFL in Cumbria, and Corporate Environmental Adviser to BNFL.

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- A Q. Could you just clarify what documents you are taking with you into the witness box?
- A. I have copies of my three reports that I have produced for this case, together with copies of a few of the supporting references which I have annotated and made notes in and highlighted and so on.
- B Q. Prof. Jones, you produced your first report in this case, which although not paginated throughout comprised 12 chapters and appendices and was signed by you on 16th June, 1992?
- A. That is correct.
- B Q. It is a lengthy report, Prof. Jones, but is there is anything in that report you wish to change?
- A. No, there is not.
- C Q. Do you adopt that report as your evidence to my Lord?
- A. I do, together with the other reports which I have produced and which are in evidence.
- C Q. Well, I will ask you about those in due course. I am not going to invite my Lord to refer to your third or fourth reports, both of which I think exclusively are answering points made by Dr. Philip Day?
- A. That is correct.
- D Q. Your second report, there is one small part of it which was not answering the Plaintiff's reports, which I will ask you about. In the first chapter of your report, Prof. Jones, you deal with your qualifications and experience and then you set out, starting on the second page, in summary the scope and contents of the report. In the first paragraph of that section you say that you have sought to make the most reasonable estimates which you can of the exposure to the individuals involved in these cases to radioactivity emanating from the Sellafield site. You refer for the first time in the next paragraph to the SEAM model. It may be very clear to my Lord, but could you explain what in outline the SEAM model is? It is obviously not a physical model. What is it exactly?
- E A. It is what is described as a mathematical model, which describes by a number of different means the behaviour of radioactivity emitted, whether to atmosphere or to the marine environment from the Sellafield site, and enables one to make first of all predictions regarding the concentrations of radionuclides in air, water, soil, agricultural foodstuffs, marine foodstuffs and so on, on the basis of the set of discharges which one has put in at the beginning of the model, as it were. Of course, it has been an important part of the exercise I have carried out, to check whether the concentrations so produced are in good agreement with measurements that have been made over a period of many years. In essence, the model is just a means of starting off with a set of discharges and then making predictions about what the concentrations of radionuclides in the environment,
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resulting from those discharges, will be. Then, of course, going on to assess intakes of radionuclides by the people involved.

- A Q. How substantial an exercise has it been to develop this mathematical model for this purpose?
- A. It has been very substantial. As I explain in the report, in many ways the individual components of the model do not break new ground. They are really quite well tried methods. However, all of those mathematical models or equations have to be assembled into the model as a whole and the whole thing has to be programmed on to the computer and tested and then, of course, we have the validation exercise that I described, the comparison of the model predictions with measurements, which was a major exercise. Altogether the construction of the model and the validation work has taken about four man years; two people working for two years pretty much full time on the model, with, of course, in addition to that, a very substantial portion of my own time to work on the report and to get involved quite closely with the validation exercise itself. So it has been a very substantial exercise.

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- D Q. At the top of page 3 you say the model is designed to be comprehensive. How confident are you that the model is comprehensive and that nothing of significance has been omitted from it?
- A. I am really very confident that what is in here is certainly the best assessment that has yet been made of the historic discharges from the Sellafield site and, if you like, the history of radionuclide concentrations in the environment. I am satisfied of that, partly by the amount of work that has gone into it, partly by the validation exercise which, as I said before, I regard as very important, and which has certainly been the first time it has been attempted to correlate environmental measurements with historic discharges to anything like the extent that has been done in this report. I am also reassured by the size of the discovery exercise that has gone on, really in parallel with what I have been doing in this case, and the fact that that discovery exercise, of course, has been carried out in such a way that issues relating to discharges from the site have been brought to my attention - flagged up, if you like.

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G The discovery exercise, of course, has been open to the Plaintiffs' experts as well. Out of that, really, there have been no significant new issues that have come out of discovery, which I would not have taken into account had I just been doing the model exercise without that.

H So all of that really gives me a substantial degree of confidence that what is in here is a good, complete assessment. Indeed, in certain respects I believe it is a cautious assessment with regard to certain of the discharges.

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Q. We have seen the word "cautious" used in your report and others. What do you mean when you say that it is a cautious assessment?

A. In this context I would define cautious as being tending to result in calculated intakes of radionuclides or doses to the cases involved, which are higher than those which are likely to have occurred in practice.

Q. You refer at the bottom of page 3 to the exercise carried out by Dr. Stather. To what extent have you and Dr. Stather collaborated together, or to what extent of the exercises you have carried out been independent of each other?

A. We have not really collaborated at all. I don't really think I have spoken to Dr. Stather more than a few words over the past year. The extent to which Dr. Stather and I have a common approach is that Dr. Stather takes the discharge scenario or chronology which I have developed in this by doing the comparison exercise I have described between environmental concentrations measured, environmental concentrations predicted and a particular discharge chronology.

Q. MR. JUSTICE FRENCH: I am sorry, could you say that again? He has taken the discharge chronology...?

A. That I have developed in this report and Dr. Stather has then taken that discharge chronology on to calculated doses by his own methods and means, as he thinks best. I have, of course, carried my calculations through as far as intakes, so there is some possibility of cross-comparison, if you like, at that point. Basically, Dr. Stather's estimates from the discharges that I have set out in this report are his own estimates from those discharges and do not depend in any way on what is in the rest of my report.

Q. MR. ROKISON: There has been a suggestion made on behalf of the Plaintiffs that the NRPB have perhaps a relationship to British Nuclear Fuels which is too close for them to be regarded as truly independent. Do you have any comment about that?

A. I simply do not accept that that is so. I think my comment can be as brief as that.

Q. In chapters 2 and 3 you first of all deal with radiation and radioactivity and the theory and practice of radiation dosimetry and dose assessment. My Lord, I was simply going to take those as read because they are really background. Having said that, may I just pick up on page 7 of chapter 3 under the heading of "Quantities Evaluated in this Report", you summarise the matters you have considered, but point out that you have not gone on to evaluate equivalent doses to relevant target organs?

A. That is correct.

Q. Because that is not a matter which you regard as being within your particular expertise, is that correct?

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A. No, insofar as the calculation of the doses to the very particular organs that are relevant to the particular leukaemias suffered by the individuals in this case are not organs which are covered by the standard ICRP metabolic models. Therefore I felt that calculations of doses to those organs required particular expertise in radionuclide dosimetry, which I simply do not have. Therefore, I have not attempted to do it.

Q. In chapter 4 you explain the exposure to radiation from sources other than Sellafield, both natural and artificial, and again that is not a matter upon which I have any particular questions to ask. In chapter 5 you deal with discharges from the Sellafield site and immediately after page 16 of chapter 5 you exhibit the Sellafield site layout. It is quite a complex figure. I don't know if my Lord has any questions that arise in relation to this. Is this your figure, or is it a figure which is reproduced from somewhere else?

A. It is my figure. The site drawing, of course, was something which was readily available, but the particular highlighting was intended to indicate the physical location of various buildings and facilities which were of particular relevance in the narrative. That is why certain buildings are marked in red.

MR. JUSTICE FRENCH: As far as I am concerned, if invited to ask questions, I should first of all need guidance among these various buildings, but it may be that is not necessary?

MR. ROKISON: I think it probably isn't. Your Lordship will have seen that Prof. Jones does refer to various discharges which emanated from various buildings or activities carried on in those buildings and as he has pointed out, those that are involved in his report have been highlighted in red.

MR. JUSTICE FRENCH: It is only right I should say, Mr. Rokison, that although I have read this report more than once I have not sought in the course of my reading to marry up any described building with any location on this plan. Is it necessary for me to do so?

MR. ROKISON: In the light of the way in which the evidence, or lack of evidence, has developed, I think it is unnecessary, but I was merely pointing out that if your Lordship wanted to find where a particular stack was then it would be marked on this diagram:

Q. You go on in your chapter 5 to describe the history of monitoring and on page 6; just above the middle of the page where you are referring to aerial effluent discharges, you refer to the fact that:

"In the early years of plant operation, much more attention was paid to measuring and recording liquid effluent discharges than was the case for aerial

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effluent discharges, since the former were numerically much greater and were considered to be of greater environmental significance."

Did that view change or did that remain the overall assessment?

- A. Well, the monitoring of discharges to atmosphere progressively became more comprehensive, leading, as I have indicated, in 1964 to there being systematic and, indeed, continuous, on many outlets, measurements of individual radionuclides from individual outlets, which were calculated through to make a quantity discharged, and which were submitted in records and reports to the authorising government departments.

Prior to 1964 many of the outlets were sampled, although sometimes not continuously, but the figures that were obtained were only recorded as the concentration of radioactivity in the effluent from the stack. They weren't taken through by a multiplication by the volume of effluent discharged to be recorded as a quantity. Nor, indeed, were individual radionuclides systematically or continuously determined, so that one would only have, as I indicate in the third paragraph from the bottom on page 6, these measurements of "total alpha" radioactivity and "total beta" radioactivity, measured as a concentration in the effluent, but not evaluated in terms of a discharge.

Therefore, progressively over the years the monitoring of aerial effluents became more comprehensive and complete. In the early years there was some monitoring, particularly of the major outlets, but that wasn't systematically compiled into a discharge record, which listed quantities discharged.

- Q. MR. JUSTICE FRENCH: I see the word "estimated" is used in the last line of the third paragraph under the sub-heading:

"...more detailed internal records were kept which include data on the estimated discharge..."

- A. Yes. I perhaps wouldn't lay too much emphasis on the word "estimate", in the sense that those were quantities which were arrived at by applying to a measured concentration in the effluent an assessed or, in some cases, measured volumetric flow, to arrive at a quantity of radioactivity discharged.

- Q. It is an estimate in the sense that it is based on a presumption that what you are measuring between hours A to B is the same as that you would have recorded if you had measured between hours B to X? Is that right or wrong? I have put it as a proposition. It is meant as a question.

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A. In the period after 1964 the samples were run, to all intents and purposes, continuously, so that where you measure a concentration, that would be, say, averaged over an entire week or for however long the sample had been run. Then by multiplying that concentration by the volume discharged within that week you come up with an estimate of the discharge. It relies on the volume having been correctly measured and in many cases the volume was estimated from plant performance data such as fan capacity and so on, not precisely measured. I guess in that sense the word "estimate" would be appropriate, and as I develop the account later, it also depends upon the assumption that what was taken as a sample from the effluent was a representative sample.

Q. You are putting neatly what I was putting in a rather more complicated way.

A. I think it is an issue of whether the sample is representative rather than whether it covers comprehensively the period of time involved.

Q. So it is whether the sample you are taking is representative and whether the assumption as to the volume throughput is correct?

A. That is correct, my Lord.

Q. MR. ROKISON: In chapter 5, page 7 onwards, you deal with the additional sources of aerial effluent discharges and in particular on page 8 you deal with the matter which has, as it happens, taken up rather a lot of time in various reports which have been filed, namely, the discharge of uranium oxide?

A. Indeed.

Q. In the penultimate paragraph of sub-section 1, the large paragraph about two-thirds of the way down page 8, you express the view that the accuracy of the quantitative assessments of those discharges only has a limited effect on radiation doses. Could you explain why you formed that view at this stage?

A. This is because whatever the quantities emitted might have been, the actual effects of the releases are considerably constrained by the fact there are environmental measurements which bear on the effects, in particular, the measurements of strontium-90, a specific radionuclide in milk, which I say in that paragraph were made from 1958 onwards. Whilst this was not concurrent with the actual emissions occurring, strontium-90 after deposition on the ground feeds through into milk over a period of time and therefore the levels in 1958 were potentially, and on the basis of the measurements that were made, were clearly affected by the level of uranium oxide emission. So that whatever you take to be the case for the quantity emitted, you were constrained to arrive at a concentration of radioactivity in milk which was as it was measured in 1958.

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Since the levels of radioactivity in milk are responsible for quite a significant proportion of the dose that arise from the emissions, then that means the dose resulting from the emissions cannot vary very much if you make the discharge bigger, because if you do you simply have to change some of your assumptions that relate the quantity emitted to what is in the milk, because what is in the milk was actually measured and everybody relies on that.

That is why I say there is a constraint there which has to be met even if you assume the discharge was larger.

Q. MR. JUSTICE FRENCH: I want to see if I can epitomise that answer in a way that Prof. Jones would agree with. Strontium-90 in milk was measured from 1958 onwards?

A. That is correct.

Q. The levels were low?

A. The levels were significantly elevated by the effects of releases. However, those measured levels may be taken really as a matter of fact. That is what the concentration in milk actually was.

Q. The measured levels can be taken as some guide to the deposition, whatever the emission may have been?

A. That is quite correct.

Q. MR. ROKISON: I think you said earlier in your answer that the levels of strontium in milk, one would take as being a question of fact and that that, effectively, is the major pathway whereby those living in the environment would actually receive their radiation does? Is that right?

A. That is correct. Also, as my Lord has said, since those levels also give a guide to the level of deposition, they also place some constraint on how much dose might have been delivered from external radiation.

Q. MR. JUSTICE FRENCH: In addition to what I have written, I have simply added this: "milk is a major pathway. Milk is also some guide to environmental effects generally"?

A. That is correct.

Q. MR. ROKISON: Now you have raised this question of uranium oxide discharge, and the views of Dr. Jakeman on page 8. Was that something you raised before you were aware that the Plaintiffs were emphasising this point in the evidence which they subsequently served?

A. Well, first of all in terms of my interaction with Dr. Jakeman, that goes back a long time before this matter was raised. In terms of the rather summary treatment I have given here of the uranium oxide question, that, of course, was written in my first report before I had seen the first reports of the experts for the Plaintiffs.

A Q. You refer to the discussions with NRPB in the third paragraph on page 8, which resulted in the figure of 20 kg being adopted. In the last paragraph of that section of your report, that sub-section, you express the view that that estimate might, if anything, be rather higher than that which actually occurred. Is that a view which you still adhere to?

B A. Yes, I think so. The reason for the comment in the last paragraph of that sub-section was that with a 20 kg emission I had to alter certain parameters in the environmental model to make calculated levels of radioactivity in milk as low as the observed levels. In other words, with a straightforward unmodified model, and 20 kg of uranium oxide emitted, we were calculating concentrations of activity in milk which were substantially higher than those observed.

C Of course, in addition to that, having gone into the matter in substantially more detail, in my second and third reports I have done some rather more careful integration of the deposition measurements and I am coming up with figures of around 15 kg of uranium oxide based on deposited caesium and strontium.

D On the basis of all that, 20 kg, with the other associated assumptions I have used, seems to me to be a very good basis for making an assessment of the effects of that particular release.

Q. You go on and deal with other sources of aerial effluent discharges and the sampling system. Page 12 you deal with the discharges from the B204 stack, and the effective height and so on:

E MR. ROKISON: My Lord, I am not going into the detail of this in view of the fact that Prof. Harrison is not being called by my learned friends:

F Q. On page 15 you summarise the differences which have been introduced for the purposes of the exercise you have carried out from the discharges which were considered and reported in NRPB 171, Addendum. You summarise those on page 15. In chapter 6 you deal with the behaviour of radionuclides in the environment and again I am not going to ask you further about that as no evidence has been tendered to the contrary effect.

Then in chapter 7 you deal with environmental monitoring at Sellafield. You start this chapter by the statement:

G "Measurements of radioactivity in the environment are the most satisfactory starting point of which to make estimates of radiation exposure of the public."

Why do you say that?

H A. I think it is a very important point in that I would consider the ideal way of making assessments of the radionuclide intakes and radiation doses to people

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A involved in this case would be if I had available a complete set of measurements of all relevant radionuclides in all relevant environmental materials at the particular locations that were involved over the entire relevant period. If that were the case then many of the uncertainties and difficulties, if you like, that arise in making such an assessment, would be avoided.

B However, although there has been a great deal of environmental monitoring around the site, it is not complete in that very idealised sense that I have stated. Certain of the radionuclides which were more difficult to measure in environmental materials, a particular example being plutonium, were not measured extensively in the environment before the mid-1970s. Equally, while environmental monitoring might well have been carried out at locations which were relevant to assessing doses to the most exposed individuals, the so-called critical groups, they were certainly not necessarily carried out in the specific locations in which the particular people we are concerned with were resident.

D There are gaps that have to be filled which are both gaps in the spatial coverage, in terms of where measurements were made, and gaps in the temporal coverage in the sense of whether particular measurements for particular radionuclides were made at a certain time or not. It is really because there are those gaps that I felt it necessary to construct the SEAM model at all. What the SEAM model does is to provide a basis for filling in the gaps. Since it has been validated against actual environmental measurements it is filling the gaps in a way which is consistent with measurements which are available, but that really is the role of the model, to fill in those gaps.

E Q. In this chapter, Prof. Jones, you summarise the monitoring programmes at Sellafield in relation to marine (environment and terrestrial environment over the years and then on page 5 you refer to research studies which were being carried out in the environment, and you explain those. Then your final paragraph on page 6 of chapter 7, you refer to the extensive programme of environmental research which is being currently funded.

F So far as the monitoring of the environment generally is concerned at and around Sellafield, are you aware how that compares with monitoring around other sites within the United Kingdom and elsewhere?

G A. Do you have in mind other nuclear sites or other industrial sites generally?

H Q. Either or both, Prof. Jones.

A. I think in relation to other nuclear sites, the monitoring around Sellafield is generally more intensive and comprehensive. It is the biggest environmental surveillance programme in relation to a nuclear site in the UK and that, of course, is because the emissions from

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A Sellafeld, by virtue of the nature of the processes that are being or have been carried out, are such that extensive monitoring is appropriate. Other nuclear sites have smaller programmes but nonetheless ones that are considered appropriate by the authorising government departments to their particular circumstances.

B In relation to other industrial sites generally, if you look at non-nuclear sites, then I believe the information we have available here by way of monitoring is vastly in excess of anything which would be available in regard to a conventional non-nuclear industry making discharges into the environment.

C Q. Chapter 8, you deal specifically with the Drigg low level waste disposal site and your conclusion in relation to that, on page 6, at the bottom, is that any radiation exposure to individuals involved in these cases at the Drigg site would be extremely small - indeed, so small, I think, that you did not consider it necessary to evaluate those exposures explicitly. Is that a view that you still adhere to?

D A. It is. I simply draw attention to the fact that in what goes before I have made an assessment of the exposure which might have resulted from a particular fire in the Drigg trench in 1964. I have, however, concluded that the figure for dose that I arrive at is, firstly, very small in itself and, secondly, likely to be a gross over-estimate of what the dose really was as a result of that fire. I suggest that that just be borne in mind more or less, if you like, as a sensitivity exercise in relation to the doses to the cases; that the doses to the Hope family could possibly have included this extra exposure, but probably did not.

E Q. And the figure that you put on it on page 6 is 10 μ Sv?

A. That is correct, a committed effective dose of 10 μ Sv.

F Q. Then, in Chapter 9, you summarise the modelling comprised within SEAM and you describe, at the bottom of page 1, how the structure of the model is determined by the pathways through which people might be exposed to radioactivity from discharges to a significant extent and you summarise, on the next page, what those pathways may be?

A. Yes, that is correct.

G Q. By way of inhalation, ingestion and external irradiation, and the various pathways which have been taken into account. On page 5, near to the bottom of the page, under "Model Outputs", you explain what the SEAM model is capable of telling you?

A. That is correct.

H Q. Then on page 6 you deal with the validation of the model, which, as you say, is very important, and you deal with that in the following chapter, Chapter 10?

A. That is correct.

S. R. JONES

A MR. ROKISON: My Lord, I was not going to invite Prof. Jones to explain Figure 9-1, which follows after page 6 of Chapter 9 because it is extremely complex, but it shows how data flows around the SEAM model. I am very disappointed my learned friend will not be cross-examining on this, but it shows how the data all inter-relates in order to be able to arrive at the model's conclusions.

B MR. JUSTICE FRENCH: Mr. Rokison, unless either you or Mr. Hytner seek to guide me through this, I am very happy to turn over the page!

MR. ROKISON: Perhaps your Lordship would turn over three and we can come to Chapter 10:

C Q. Where you deal with the way in which the model was validated and, in particular, in the first paragraph, the necessity to establish whether it correctly represented the distribution of radioactivity in the environment resulting from discharges and, in the second paragraph, you deal with the way in which that is done by seeing the extent to which the calculations match the measured values and that that would be a test of the validity of the model. I think I can go quite a long way forward through this exercise where you describe the validation by the measurements in the environment and the conclusion that you reach at page 13, where you say that, overall, you regard the results of the model validation exercise as highly satisfactory. You say that it has been thoroughly validated against relevant local environmental monitoring results to a far greater extent than is usually the case for models used in radiological assessments and you say that the use of the SEAM model, together with the preferred discharge scenario, successfully explains all the major features of both the temporal and spatial distribution of radionuclides in the environment around Sellafield and you express confidence that it constitutes a sound basis for assessing the exposure of individuals in this case.

F Then, Chapter 11, you deal with some further environmental measurements made for validation purposes, which I do not want to ask you about, and you set out a number of tables.

Chapter 12, which is after the tables, my Lord.

MR. JUSTICE FRENCH: Yes.

G Q. MR. ROKISON: Chapter 12 is headed "Case Assessments" and the exercise you there carry out is to use the SEAM model to calculate concentrations of all relevant nuclides and to calculate, where appropriate, gamma dose rates for locations relating to each of the individuals concerned in the litigation. You take into account, as you say on page 2 of Chapter 12, a number of specific indicators, which you there set out, and on

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page 3, near to the top of the page - perhaps I just ought to refer so that my Lord picks it up that, on page 1 of Chapter 12, in dealing with the questions of ingestion and inhalation, you say that you have not - I am looking at paragraph 4 - you say you have not used the house dust model built into SEAM, but what you have done is to use surveys carried out by the NRPB and Imperial College showing contamination levels of house dust in communities at Drigg, Seascale, St. Bees and so on. Why did you use that dust model rather than that which was built into SEAM?

- A. The reliance that I placed on the NRPB and Imperial College report was to establish the point that radionuclide concentrations in house dust are generally very similar to radionuclide concentrations in surface soil - in fact, generally rather lower than those - the only exceptions being houses which are very close to a shoreline where there is fine grained sediment. So, in other words, if fine grained sediment is transported into the house in quantity, the concentrations of radionuclides in house dust will be higher, but given that that does not seem to be the case in regard to the people we are assessing in this case, the concentrations of radionuclides in house dust you would expect to be similar to outdoor surface soil and the SEAM model already calculates that and takes into account inadvertent ingestion and inhalation of dust arising from surface soil. So that, if I set the model such that it is effectively calculating doses from those sources at all times when the individuals are assumed to be at home, then I will get an entirely adequate estimate and, if I use the house dust model as well, I would effectively double count a source of exposure. So that is why I decided not to use it for these cases.

- Q. This is the work done by Profs. Goddard and Thornton?
A. That is correct.

Q. Adopted?

- A. Yes, Prof. Goddard is the author, or one of the authors, of the Imperial College report, as is Prof. Thornton.

- Q. Thank you. On page 3 of Chapter 12, near to the top, the second paragraph, you refer to the fact that the results of the dose and intake assessments are given case by case for each individual involved in Annex 12C.

Your Lordship will find that in the last divider of the report, which is divider 12, and about a third of the way through that your Lordship will find Annex 12C.

MR. JUSTICE FRENCH: How many pages of Annex 12C?

MR. ROKISON: How many pages does it have? Well, my Lord, it has a lot of pages because it runs to the end of the report because it has attached to it, in tabular form, radionuclide intakes for the members of the relevant families.

S. R. JONES

THE WITNESS: Yes, there are a large number of tables.

MR. ROKISON: And they are summarised on page 1 on Annex 12C. When I say "summarised", there is a list of them, my Lord. Does your Lordship have that?

MR. JUSTICE FRENCH: Yes:

Q. In the nature of things, of course, these are estimates?
A. Yes, of course, they are estimates arrived at through the model. They are not quantities that you could really expect to measure specifically in themselves.

Q. MR. ROKISON: And, indeed, your conclusions on page 7 of Chapter 12, which is the last chapter of the first report - and perhaps one should read that, my Lord - your conclusions are:

"The assessments of intake and external dose which I have reported result from a detailed and careful assessment, fully described in this report, of the discharge history and environmental behaviour of all those radionuclides emitted from Sellafield which are likely to be of any significance in regard to the radiation exposure of individuals living nearby."

First of all, how confident are you, Prof. Jones, that you have not missed anything significant?

A. I am very confident that I have not missed anything significant, largely because, as I indicate, where I can make a cross-comparison between the amount of radioactivity actually measured in people and the predictions of the SEAM model, the SEAM model predicts substantially too much radioactivity in the people, not too little, and these comparisons are given in Figures 12-1 and 12-2 respectively for plutonium measured in Seascale residents in samples taken at autopsy and caesium-137 measured in Seascale residents over a period of years by a technique known as whole body monitoring. In both cases, the SEAM model is predicting substantially too much radioactivity in these people.

It would, of course, be very nice to have a lot more specific measurements of radionuclides in people to make those comparisons, but where there are such measurements, the SEAM model is, as I have said, rather cautious.

MR. JUSTICE FRENCH: Can I just ask a question here?

MR. ROKISON: Of course, my Lord.

Q. MR. JUSTICE FRENCH: Where you say, for example, if you have a comparison between the prediction of the SEAM model and what you find at autopsy?

A. Yes.

S. R. JONES

Q. Does that take into account the excretion which, to some extent at least, must go on all the time between ingestion and autopsy?

A. It does, indeed, my Lord. The component of the SEAM model that deals with the calculation of dose includes all those aspects of behaviour of radionuclides in the body, including clearance by excretion, which are recommended by ICRP and NRPB, so that, where we are calculating the amount of radioactivity in a person at a particular time, that depends both on the rate at which they have taken it in in each particular year and the rate of clearance.

Q. But when you are looking at, say, a small quantity at autopsy, that might imply, might it not, a large dose 15 years ago or a small dose three days ago?

A. Yes, for certain radionuclides, my Lord, that certainly is the case. The particular comparison I have made with autopsy data is for plutonium, which is retained for a very long time in the particular organs that I have used as the basis of comparison - that is bone and liver - so that the quantity measured at autopsy tends to reflect the cumulative intake almost over the entire lifetime, corrected....

Q. Could you pause a moment? What I have written is this: the finding at autopsy of plutonium tends to reflect a lifetime intake, and then I interrupted you?

A. That is correct. As I say, it tends to represent a lifetime intake, but there is a correction that you must make for slow clearance. However, that correction is not dramatic, so the plutonium result at autopsy tends to represent - and, strictly speaking, it is not the cumulative intake. It is the cumulative uptake into body fluids, which is less than the cumulative intake.

Q. What you have added, does it really significantly amend the answer which I have recorded as this: the finding at autopsy of plutonium tends to reflect a lifetime's intake, but there is correction for slow clearance?

A. Yes, I am being perhaps a little pedantic, my Lord, but intake and uptake are different quantities. Intake is what is taken in through the nose or through the mouth. Uptake is that fraction of intake which actually passes through into body fluids and goes into systemic circulation.

Q. So you would rather I put "uptake" than "intake"?

A. But if you inserted "uptake", that would be correct, my Lord. I apologise for having been loose with the term initially.

Q. I am not sure you were.

Q. MR. ROKISON: In your Figure 12-1 you deal with plutonium and in Figure 12-2 you deal with caesium. Do similar comments apply to caesium or is there further adjustment to be made, for example, by reference to decay?

S. R. JONES

- A. No, the comments that my Lord made about a measured body content at autopsy - in a sense, a small content at autopsy might reflect a very large intake or uptake earlier in life - would be quite correct for caesium. The clearance time for caesium is quite short. However, the measurements shown in Figure 12-2 on people for caesium are not autopsy measurements. They are measurements made in live people at a particular time by a technique whereby the person actually lies within a large and rather sensitive radiation detector, so the amount of caesium present in their body at that time is measured and, therefore, the caesium results show the variation in the caesium content of people as a function of time over a lot of the relevant period. They are not measurements made, as is the case with plutonium, effectively right at the end of the relevant period.

MR. ROKISON: Does your Lordship follow those diagrams on Figures 12-1 and 12-2?

MR. JUSTICE FRENCH: I think so, but I cannot be confident. Perhaps some elucidation might be wise.

- Q. MR. ROKISON: Could you just explain to my Lord by reference to those figures what each of the colours represents? Unfortunately, the red and the pink come out as really being pretty similar under artificial light?
- A. Yes, they are rather similar, aren't they? If we take Figure 12-1 first, this deals with the plutonium content of individuals and, in this case, the available measurement information rests on a relatively small number of samples taken from the tissues of individuals who have been resident in Seascale for a long period from, say, 1950 through until.....

- Q. MR. JUSTICE FRENCH: Are we talking here about autopsy or body samples?
- A. We are talking about a sample taken at autopsy. These are actual samples of bone and liver and so on. So the vertical bar on the bottom right corner of each graph indicates the range in the measurements that were obtained by analysis of these autopsy samples, so the samples in individuals range from the bottom of that bar up to the top of that bar and that reflects the plutonium content of that particular organ, as measured in 1984.

The rising line on the graph shows the quantity of plutonium calculated to be in that organ by the SEAM model, with all the discharge chronology and so on that I have used, and that shows the content of the organ rising as a function of time because, of course, plutonium is being taken in all of the time and a portion of the plutonium that is taken in goes into systemic circulation and it is retained quite effectively in bone or in liver.

- Q. I thought this was caesium-137?
- A. Figure 12-1, my Lord, is plutonium.

S. R. JONES

Q. Oh, sorry. Yes?

A. So, as I said, the bar bottom right of the graph indicates the range of values obtained in measurements made in autopsy samples in 1984.

Q. I want to make sure I am looking at the right document. I have got here - I was looking at the wrong document.

A. Yes, I think you were. Figure 12-1 looks like that.

Q. This is Table 12-1. Where do I find Figure 12-1?

A. I think, if you go back in the report, my Lord....

MR. ROKISON: Figure 12-1 comes immediately after page 7, I think, my Lord.

MR. JUSTICE FRENCH: Thank you, I have got it now:

Q. Yes, sorry, start again, if you would?

A. There are two graphs on the page. The top graph relates to measurements and predictions of the plutonium content of the liver, and the lower graph represents predictions and measured values for the plutonium content of bone. In each graph there is a vertical bar in the bottom right of the graph, which is a sort of pink or magenta colour, and that vertical bar represents the range of measurement results that were obtained from the samples that I have referred to, which were obtained in 1984 from a number of long-term residents of Seascale.

Q. How big is the sample, roughly?

A. It is only, I think, four or five individuals, my Lord. It is a relatively small sample, but those individuals from whom tissues were obtained were long-term residents of Seascale and were present right from the early 1950s through to 1984. The samples were obtained by the National Radiological Protection Board and I know Dr. Stather also refers to these data in his report, but the comparison I have done is to take the predictions of the SEAM model in regard to the concentrations of plutonium in these two organs - bone and liver - for an individual assumed to have been living in Seascale throughout the relevant period of time, with average habits in regard to the consumption of foodstuffs and so on. The results of those predictions are given in the rising lines on the two graphs.

The line rises because, of course, the plutonium content of a particular organ increases with time as the individual continues to take radioactivity in and as a portion of that radioactivity continues to pass into systemic circulation, to be taken up by bone or by liver and be retained by those organs for really quite a long period of time.

So the rising curve takes account of year by year intakes and also year by year clearance due to excretion and the line continues to rise because the rate of intake is greater than the rate of excretion, the rate of clearance being quite slow.

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A The split in the graph, where the graph breaks into two lines, that is simply showing the distinction between the amount in the organ arising from inhalation and the amount arising from inhalation plus ingestion. The fact that the two lines are rather close together up until 1965 - in fact, they are very close together up until 1965 - indicates that up until 1965 the dominant route for uptake of plutonium for these hypothetical individuals was inhalation, not ingestion, and at the end of the period, at 1984, when the autopsy samples were obtained, the predicted levels of plutonium in either bone or liver is very substantially higher than the values that were measured at autopsy on this, admittedly relatively small, number of individuals.

B If it had been the case that ingestion of plutonium was accounting for most of the calculated body content, then that excess may possibly have been due to the assumptions about food consumption habits that were made in order to do the calculations, but since a lot of the body content results from inhalation and since we can be quite sure that everybody breathes, then that really does imply some genuine conservatism or caution, rather, in the model.

- C
- D Q. MR. ROKISON: Figure 12-2 is a similar exercise in relation to caesium but, as you have told my Lord, that does not relate to autopsy, but relates to whole body monitoring that was carried out?
- E A. It does, indeed, and in this case the circles towards the bottom of the graph are the measurements that are available. Sometimes these are the average of the measurements for a fair number of individuals - sometimes as many as a dozen individuals - and, in this case, because the measurements have been made on individuals at different times, you can see by measurement the way the caesium content of people varies with time, and again the key point is that all the circles are well below the lines which represent the calculated body content, distinguishing between basically two different sources - Sellafield discharges and fallout from weapons testing and the Chernobyl accident - but it is clear that the model is calculating substantially higher body contents of caesium than were actually measured. I believe that that probably results from conservatism in the food consumption habits that I have assumed.

- F
- G Q. MR. JUSTICE FRENCH: Figure 12-2 calculates higher body contents than those measured - measured how?
- A. The measurement, my Lord, is done by the individual who is being measured actually goes into a shielded enclosure, which has within it sensitive radiation detectors.
- H Q. Whole body detection?
- A. Whole body detection, that is correct.

Q. For short?

A. Yes.

Q. Measured by whole body measurement methods?

A. That is correct, my Lord.

Q. MR. ROKISON: To return to your conclusions on page 7 of Chapter 12, Prof. Jones, having looked at those figures, you say in the second paragraph of your conclusions that you believe that the results you have obtained are likely to err substantially on the side of over-estimating intake and external radiation exposure, and two paragraphs later you say you are further encouraged by the observation that, where data exist on the concentration of radionuclides in the tissues of people living near Sellafield, the intakes assessed imply radionuclide content in people substantially higher than those which are observed?

A. That is correct.

Q. Those are the figures to which you have referred. Can I just put this to you? It was suggested in my learned friend's opening submission to my Lord that, because there have been some errors and omissions in the past in relation to environmental monitoring or the monitoring of environmental discharges, that nothing that is now produced by British Nuclear Fuels should be relied upon. Do you have any comment about that?

A. I think it goes without saying that I would disagree with that statement. In regard to this assessment, I would say that the strength of the assessment is the extent to which, as I said, it relies on cross-checking discharge measurements, or estimated discharges in some cases, against measurements of radionuclides in the environment, and the extent to which the final discharge chronology that I arrive at, together with the SEAM model, successfully reproduces the environmental measurement data is, to me, a very good assurance that there cannot have been any much larger discharges than those which I have assumed or, if there were, then something quite remarkable must have happened to keep the environmental concentrations at the measured values. However, whatever, the environmental concentrations that I predict from the model, I believe, are an extremely good basis for making the assessments that I have made in these cases.

Q. May I just ask you very briefly about your second report, Prof. Jones. This was a report which was dated 2nd September, 1992 and which comprises four chapters, together with a number of appendices?

A. Yes, indeed.

Q. I am going to ask you nothing at all about the first three chapters because, in those chapters, you deal respectively with reports which were submitted by various potential witnesses on the Plaintiffs' side, who have not, in fact, been called to give that evidence, so I am

S. R. JONES

not going to deal with it at all. May I just ask you about Chapter 4, where you deal specifically with the deposition of radionuclides, particularly from uranium oxide discharges, within 1 km of the site?

A. Yes.

Q. You describe how you have reassessed that deposition by reference to a different model, which was not available to you at the time when you wrote your first report. Could you just briefly explain that?

I am sorry, my Lord, does your Lordship not have it?

MR. JUSTICE FRENCH: Yes, I have now got Chapter 4.

MR. ROKISON: It is the last two and a half pages of the report, my Lord, just before the divider which should be marked 1.

MR. JUSTICE FRENCH: Unfortunately, the second report of Prof. S. R. Jones has no dividers.

MR. ROKISON: May we help your Lordship? Does your Lordship have the Chapter 4 now?

MR. JUSTICE FRENCH: I thought I had got Chapter 4. Let me just check. Yes, thank you.

MR. ROKISON: I am sorry, my Lord, we will put some dividers in.

MR. JUSTICE FRENCH: That would be helpful.

Q. MR. ROKISON: I was merely asking Prof. Jones that, in Chapter 4, you describe how you have done a reassessment based upon a new model called the ADDOP model, which was not available to you at the time of writing your first report, and you have reassessed the depositions within 1 km of the site by using that model rather than the SEAM model?

A. Yes, the reason for my presenting this chapter was that I was conscious at the time of writing my first report that the model I have used within SEAM for describing the dispersion and deposition of material emitted from stacks has a limitation, in that it is not considered appropriate for dealing with the deposition of material which has a settling velocity due to gravity of more than a few centimetres per second.

There were two specific releases that were modelled in my first report which had such properties. One was the release of uranium oxide, which involved certainly a proportion of the material emitted having settling velocities well in excess of a few centimetres per second, and the other emission that I have modelled was the emission of, as I have described it, additional plutonium emitted from low effective height at the site during the early years of operation, which, on my

interpretation, was best described by a settling velocity of about 10 cm per second, which was just outside the range of applicability of the standard model.

I felt that what I had done in my first report was justified, insofar as, even though the model might not have been strictly applicable to the matter being studied, the model, nonetheless, produced levels of deposition in the environment which were very consistent with measurements and, therefore, I did not have any great concern about the subsequent assessment, particularly for individuals, of course, who were resident several kilometres from the site at minimum, as being in any way really unreliable, but, nonetheless, I had asked for some work to be done to develop a model which was capable of describing this particular class of released materials. This is the ADDOP model, the precise acronym for which escapes me at the moment, I am afraid.

Q. It is the atmospheric dispersion and deposition of particles model.

A. Thank you, Mr. Rokison.

Q. That is something that I am reading! I am looking at Annex 2a, page 2.

A. But the essence really of what I have done in Chapter 4 is not specifically to reassess the deposition within a kilometre of the site, although the ADDOP model does predict different depositions within a kilometre of the site to that which the SEAM model does, but simply to confirm that for distances greater than a kilometre from the site, which are the distances in which I am really interested, that the ADDOP model gives similar results to the SEAM model and therefore I do not consider it necessary to go back through all the calculations I have done using the ADDOP model rather than the particular atmospheric dispersion deposition model used within SEAM. The comparisons between predictions from the ADDOP model and the SEAM model in respect of uranium oxide are at Figures 421 and 422 - I beg your pardon, and 423 - and the comparison in relation to the assessment of the high deposition velocity plutonium is at Figure 4.3.1, the extent to which the various curves are in good agreement, particularly, as I have said, in relation to uranium oxide at greater distances from the site is the basis of my conclusion that had I had the ADDOP model at the start the results of my assessment of intake would not have been significantly different.

Q. Prof. Jones, my Lord has read your report and I have asked you some questions about it. Is there anything that you wish to add by way of further clarification of either what you have done or the results that you have arrived at?

A. No, Mr. Rokison, I think you have taken me through the key points in my report very systematically and I am sure there is nothing I can think of at this stage that I would wish to add.

S. R. JONES

MR. JUSTICE FRENCH: Mr. Rokison, I do not think I need have flags put in. I see that in fact I did flag Chapter 4 myself but I called it "SEAM" instead of Chapter 4. Yes, Mr. Hytner.

MR. HYTNER: My Lord, during the cross-examination I shall be putting a considerable number of documents to Prof. Jones and equally when Mr. Read cross-examines Dr. Stather a number of documents will be put to him. My Lord, rather than each time I put a document the document has to be taken from one of these files, what we have done is prepare a bundle of the discovery documents which is all in one file. There is a copy for your Lordship, a copy for the witness and of course a copy for my learned friend.

MR. JUSTICE FRENCH: Mr. Rokison had notice of what you have just explained to me?

MR. HYTNER: My Lord, he did not. My Lord, we were still collecting the file on Friday night.

MR. ROKISON: My Lord, I have no objection. It is obviously a convenient course.

MR. HYTNER: My Lord, the only thing I would put on warning is that because it was compiled late during the course of Friday the numberings on the bottom of the pages are not in my view in the clearest of figures. That is not a view taken by others on my side who think they are very clear, but if sevens look like twos, my Lord, I put your Lordship on warning. It is not my writing.

MR. JUSTICE FRENCH: What are you going to call this, Mr. Hytner?

MR. HYTNER: My Lord, I think all this contains are documents that are already in other bundles. Prof. Hylton Smith was D1 apparently, so this could be P1.

Cross-Examined by MR. HYTNER

- Q. Prof. Jones, I am going to take you chronologically through this - so that you can understand the scheme of the cross-examination I hope to keep it chronological - and you yourself have referred to the early days when there was little monitoring of aerial discharges because they were thought unimportant, that is correct, is it?
- A. Certainly they seemed to be regarded as relatively unimportant, relative to the liquid discharges, I agree, yes.
- Q. There was another problem, was there not, so that this can be understood and put in context? The piles - Pile 1 and Pile 2 - were effectively the earliest part of the Sellafield plant?
- A. That is correct.

S. R. JONES

- A Q. When Piles 1 and 2 were designed there was no provision for filters in the chimneys?
- A. That is correct, the filters, as I am sure you were about to say, were designed and added almost part the way through the construction which is why they are at the top of the pile chimneys rather than the base.
- B Q. You have anticipated the question. I have read somewhere in the vast literature that I have had to plough through that in fact they were suggested by Sir John Cockroft and they were actually known by those who opposed them as "Cockroft's Folly"?
- A. Yes, I have read the same suggestion.
- C Q. Far from being a folly they saved the countryside from a disaster in the 1957 fire, that is right, is it not, in fairness to Sir John Cockroft?
- A. Yes. Obviously the efficiency of the filters is not what one would expect in a modern system but nonetheless the principle of having filtration on that stream is inescapably correct, the stream must be filtered and it is a jolly good job it was.
- D Q. As I say, if there had not been filters in the 1957 fire the effects would have been far worse than they in fact were?
- A. Yes.
- E Q. You have anticipated again my next point, of course, because they were a late design there were two problems, firstly they were not as efficient as they might have been, but secondly they were right at the top of the chimney stack so they were very difficult to use for monitoring?
- A. That is correct.
- F Q. The B204 and B205 stacks, to the contrary, had their filters, as it were, designed in and the filters were therefore at the bottom of the stack?
- A. Yes, although in the early years particularly there were still streams going to the B204 stack which had little or no filtration. Filtration was by no means a universally applied principle.
- G Q. One of the problems with the lack of monitoring and the paucity or the poverty of the filters in the piles was the release of argon-41 in the early days?
- A. Yes, indeed, although argon-41 would have been released whether or not there were filters and whether or not there was any monitoring. Filters will not remove argon-41 from a gaseous discharge stream.
- H Q. It would not have removed it but if you had been able to monitor it there would have been great advantages, would there not?
- A. There would have been relatively more certainty as to what the quantity discharged was or might have been.

- Q. Can I just deal with the argon, very briefly I hope?
Argon is in fact a natural gas found in the air?
A. Argon-40 is a natural gas found in the air, yes.

- A Q. MR. JUSTICE FRENCH: What is the proper notation for argon? I think I have seen it sometimes as simply "A" and sometimes as "Ar".
A. It is "Ar", my Lord. That is the accepted notation.

- Q. And it is Ar-40 that you find in air?
A. Yes.

- B Q. MR. HYTNER: That is natural?
A. That is correct.

- Q. When it, however, captures neutrons it becomes argon-41 and becomes radioactive?
A. Indeed.

- C Q. I think the process simply is that the argon-40 is natural in the air coolant and as it goes through the plant it attracts neutrons and then goes into the atmosphere as argon-41?
A. Yes. Argon-41, of course, has a relatively short radioactive half-life, 1.8 hours.

- D Q. This is going to be a very easy and swift cross-examination, Prof. Jones, because you are always anticipating my next question!

- Q. MR. JUSTICE FRENCH: 1.8 hours?
A. 1.8 hours.

- E Q. MR. HYTNER: And it is a gamma and beta emitter?
A. Yes. The gamma irradiation is the radiation mode which is the more significant in terms of dose.

- Q. Being in the form of a gas cloud it can, of course, be inhaled?
A. It can be inhaled but the dose arises primarily simply from being immersed in a radioactive cloud. It is effectively a source of external gamma irradiation.

- F Q. But both, external and internal?
A. Yes, although the external component of the dose is by far the more significant.

- G Q. That was being emitted between 1951 and 1957 without any monitoring at all but is it right that some prediction had been made as long ago as 1948 or 1949 as to what amounts of argon-41 would be discharged to atmosphere?
A. I simply do not know, Mr. Hytner. You are better informed than me on that point, I believe.

- H Q. Do you know this, that when figures were being prepared for the R171 and then for the R171 Addendum the figures given for R171 were uprated by about two-thirds, I think by about 70%?
A. Yes.

S. R. JONES

Q. For the R171 Addendum?

A. That is correct.

A

Q. Do you know why it was thought that there had been an error of such magnitude in the provision of figures for R171?

B

A. I understand that the different discharge quantity arose from a reconsideration of a calculation that had been done on the piles to predict the rate of argon-41 production, the essential components of which, as I understand it, are the neutron flux and the free air volume within the piles which is subjected to that flux, and those two quantities combine to produce an estimate of the argon-41 released, or the rate of argon-41 production. I understand that the revision in the calculation effectively arose mainly from a reconsideration of the free air volume, but for the detail of that I would have to refer to documents.

C

Q. This is a point that I shall be making, I hope not with such length, from time to time during the cross-examination but we begin with the argon. Those who calculated the prediction would have been men first of all acting in total good faith and honesty, and secondly men who were very highly qualified?

A. I would imagine both, yes.

D

Q. Errors nevertheless of the order of 70% crept in to that calculation?

A. That would appear to be the case.

Q. Can you help me on this, and I need help because this may be a very bad point I am making through lack of understanding of the document. Could you look first of all at document 85 in your bundle?

E

MR. HYTNER: My Lord, I am very sorry to have to say that I shall be jumping wildly from page to page in this bundle. It is not in a particularly satisfactory order:

F

Q. Could we just go through this? This is "Discharge of Ar-41 from the Windscale Piles", and it seems to be dated November, 1987:

"John Stather (NRPB) rang this a.m. He is producing a paper on the errors associated with dose calculations in R171 and Addendum. He asked in particular about errors in estimates of Ar-41 discharged from the two piles.

G

I attach a copy of an error estimation done assuming a 10% error in all variables. In the majority of cases this will be an overestimate of the error. Only in, perhaps, the flux data could the error be larger. As you will see, the calculated results vary from"

H

and it is $3.5 \text{ to } 7.0 \times 10^{17} \text{ Bq/y.}$

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MR. JUSTICE FRENCH: Is that Becquerels per year?

Q. MR. HYTNER: Is that Becquerels per annum?

A. As written it appears to refer to Becquerels per year but I would need to check that.

Q. "I suggest we write to Stather saying something to the effect that 'errors are very difficult to estimate. However making realistic assumptions it is unlikely that there will be an error of greater than a factor of 2 in the quoted Ar-41 discharges', (discharge quoted was 5.0×10^{17} Bq/y). Do you agree?"

"DGP", is that Pomfret?

A. That is Dr. Pomfret, yes.

Q. And is "RSA" Anderson?

A. No, "RSA" would be Atherton, I believe.

Q. Then again this is a note from possibly Mr. Atherton to Mr. Pomfret:

"Do a draft letter for me to see - however, we must not give the impression that errors are only on the increase or otherwise you will undermine the R171 Addendum."

Now irrespective of what that precisely means could you just tell us, are these errors that were referred to in Mr. Pomfret's memorandum, which is over on the next page and which I am certainly not going to take you through in detail, I only want to know in broad outline, are those errors additional to the 70% uplift that was given for the R171 Addendum?

A. Mr. Hytner, it is a little difficult for me to give a definitive answer on this and it would certainly be impossible for me to give a definitive answer on the correctness of the calculations in this particular argument

Q. I am not asking you to do that.

A. Which was clearly something done in rather a hurry by the look of it, but what it seems to be asking the question about is, taking the answer that was in R171 Addendum, how sensitive is that answer to an error in some of the parameters used to calculate the value. If I read the figures correctly he is saying R171 Addendum says 5.0×10^{17} Becquerels per year and if he plays around with the input variables and alters them all by up to 10%, in either direction because of course the error is in either direction, he gets results ranging from 3.5 to 7, and that indicates the sensitivity of the stated answer to changes in the variables. It seems to me to be on the face of it quite a reasonable position, whereby the estimate given, of course since it is an estimate, is subject to some error, but Pomfret is not saying in his memo that the figure should be bigger. He is saying it

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could be smaller by a certain factor or it could be bigger by a certain factor, that is in the nature of an estimate.

A

Q. Have I interpreted this correctly, that there was a possibility at any rate, accepted by Dr. Pomfret, that the figures given in R171 Addendum might be out by a factor of 2? They might be doubled or halved?

A. That is what he is saying but it is not actually consistent with the numbers he has put down in the memorandum, is it?

B

Q. It is not. This really is a reflection, is it not, of the danger of looking at figures which are estimates and then assuming that they are set in stone?

A. Yes, I have to agree with that. One should never do that but one then gets down to the argument of whether one should give a central estimate or whether one should give the upper bound for absolutely everything.

C

Q. Leaving argon for the moment - we may have to come back to argon much later on - after 1964 sampling was done, was it not?

A. Sampling was done prior to 1964. You are now referring to particulate discharges?

Q. Particulate, yes.

D

A. Sampling was done prior to 1964. It was, if you like, the range of the measurements that were made on the samples and the explicit estimation of discharge from those measurements that was made after 1964.

Q. The stack B204 which discharged plutonium had come into, I think the technical term is on stream in 1952?

A. Yes, I think that is correct.

E

Q. Sampling of plutonium discharges prior to 1964, how effective had that been, that is the twelve years the B204 had been in existence?

A. As is explained in my report, prior to 1964 the routine measurements on the B204 stack amounted to taking a sample of gas and particulate from the stack and assessing the filter paper that was so obtained for total alpha radioactivity and total beta radioactivity. There was not a specific measurement made of plutonium. However, documents that I have seen from discovery indicate that those measurements of total alpha and total beta were made with a certain amount of care. The main drawbacks were that they were not radionuclide specific and in the earlier years neither were they continuous, because the sampling pumps that were available to move the necessary volume of air apparently would not run continuously for long periods.

F

G

Q. Again during that period highly qualified and honourable scientists were assuming that the efficiency of the stack filters was pretty high?

H

Q. I am not sure what you mean in this context by the efficiency of the stack filters. Could you be more specific?

A. There was no suspicion that in relation to discharge figures a stack efficiency factor, a stack sampling efficiency factor, need be applied.

A. Yes, you are referring to the sampling equipment?

Q. Yes, that is correct.

MR. JUSTICE FRENCH: I am not sure I have got that, Mr. Hytner - stack sampling efficiency factor?

MR. HYTNER: My Lord, can we leave it because we will be coming in detail to that in due course?

MR. JUSTICE FRENCH: I need not understand it now but ought to later?

MR. HYTNER: What happened much later was that it was appreciated that the sampling arrangements were not all that efficient and that in order to assess what really went into the atmosphere you had to multiply by a factor, which I think was called by the acronym SEF, which I have never understood - it is either stack efficiency factor or sampling efficiency factor but I think when they write it out in full it is both, stack sampling efficiency factor - you had to multiply by a factor to achieve the figure that really went into the atmosphere.

MR. JUSTICE FRENCH: Yes, and that was around 1964?

MR. HYTNER: No, my Lord, they did not know about it in 1964 and that is what I have just got from Prof. Jones. They had no clue that they needed to do it.

MR. JUSTICE FRENCH: No adjustments made for sampling efficiency factor until roughly when?

Q. MR. HYTNER: 1986?

A. The factors were really first systematically applied in 1987, retrospectively to the 1986 data.

Q. Retrospective to '86, yes.

MR. JUSTICE FRENCH: Until 1986-87

MR. HYTNER: My Lord, in 1987 they applied the factor for the 1986 discharges.

Q. MR. JUSTICE FRENCH: 1986 then, it was not realised that SEF needed to be taken into account?

A. That is correct, my Lord.

Q. MR. HYTNER: At the same time prior to the Black Committee the assessment of uranium oxide that had gone up the piles into the atmosphere had been 100 grammes?

S. R. JONES

A. That was the assessment made by Dibben and Howells in 1955.

Q. But I think it went up from 100 grammes and then I think in 1957 it went up to 440 grammes?

A. That is correct.

Q. So what was given in 1984 to the Black Committee was the figure of 440 grammes?

A. Yes, what was given to the Black Committee, to be specific, were the 1955 and 1957 reports. There was no attempt to reassess or validate what was in those reports simply because the amount of time within which the exercise for the Black Committee Report was to be carried out was extremely short, very short by comparison to this litigation.

Q. I think the way you have put it in your report at Chapter 5, page 8, is that all these assessments were passed without critical review and accepted by the NRPB?

A. Yes, that is effectively what happened. In relation to the piles particulates the 1955 and 1957 reports were, if you like, contemporaneous reports, reports made at the time, assessments made at the time, and those were simply passed over within the space of time that was available to put together the discharge chronology for the work for R171. Certainly the company was never asked to review any of these reports, nor indeed would there really necessarily have been the time to do so.

Q. I was not actually suggesting that the Company should have made a critical review and I am not sure what your words meant and I shall now ask you. When you wrote that on page 8 did you mean "without critical review by NRPB", or "without critical review by BNFL"?

A. I meant without critical review by BNFL.

Q. So far as you know were they critically reviewed by NRPB before they were accepted?

A. So far as I know, no, but that is doubtless a question you will wish to put to Dr. Stather.

Q. They were certainly accepted by NRPB?

A. Yes.

Q. Given to the Black Committee?

A. Yes, by virtue of having been included in the first report.

Q. Your suggestion is that there was very little time in order to do a critical review. So far as you are aware were there any doubts felt by those in control of BNFL as to the accuracy of those figures, as to whether a critical review would have been necessary?

A. So far as I am aware, no.

Q. Could you look now at page 409 to see how these figures were regarded at the time? Present you have got

S. R. JONES

Dr. Stather, Dr. Anderson - Dr. Anderson at that time was holding the post that you later were appointed to, is that right?

A. Not exactly.

Q. A similar post?

A. The post was different, very different really, but Dr. Anderson reported at that time direct to the Director of Health and Safety at the Company Headquarters.

Q. "The purpose of the meeting was to review the position regarding the BNFL search of the Windscale site records up to 1957, the resulting identification of any additional discharges, and the NRPB assessment of this data."

Then going down to paragraph 4:

"The search of the site records up to 1957 by BNFL has now been completed. Dr. Anderson felt 99.9% certain that they had checked all the available data and identified any additional emissions. The files had been looked at in depth and any factors likely to have resulted in unexpected emissions were identified, even if these were not directly referred to in the paper, and regardless of the trivial nature of the release. An independent check had been carried out on papers in the public records office: this had not revealed that any relevant information had been missed. However the usefulness of this public records office search was limited by its non-technical nature, and the fact that it was restricted to non-classified papers."

That would seem to suggest again that highly qualified and honourable men believed that in 1985 there would be no further information emerging from Windscale/Sellafield as to additional discharges over and above those which had been recorded for the NRPB Addendum for 1986?

A. I am a little confused because

Q. I am sorry, for the NRPB 171 in 1984.

A. You came to this from a discussion of the uranium oxide. Are you suggesting that at this time Dr. Anderson would still believe that the Dibben and Howells was correct?

Q. I was coming to it from all the data, both plutonium and uranium oxide, which had been given to Black for the 1984 R171 - well, for the NRPB R171 for the Black Committee's Report.

A. No, I do not think that is correct because this memorandum to me clearly refers to the exercise that was being carried out in preparation for R171 Addendum.

Q. That is fair enough, the point is precisely the same, whether for the original R171 or whether for the Addendum, perfectly honourable and highly qualified men thought that they had reached the definitive position for discharges in 1985?

A. That seems to be what Dr. Anderson is saying. I would just qualify that in terms of the fact that he was saying it in relation to his search of the documents for the material that eventually led to R171 Addendum, not to R171 original.

Q. But I have no doubt at all that the same position would have arisen for R171. Have you any reason to think that at the time the information was given to NRPB for the original R171, that anybody at BNFL had any doubts as to the accuracy of the figures that were being given?

A. No, of course they did not and as I see it the main defect of the work that was done for R171 was the great rapidity with which effectively the Black Committee was set up and then subsequently reported, meaning that the data gathering was necessarily very circumscribed and restricted in time.

Q. MR. JUSTICE FRENCH: There is no reservation though in that paragraph, is there? 99.9% confident.

A. No, my Lord, but I am making something of a distinction between the circumstances relating to the first report, R171, and the second report later published in 1986 which was R171 Addendum, and what Anderson is saying here is that he is 99% certain in relation to the information which would ultimately be fed into R171 Addendum.

Q. Yes, I see.

Q. MR. HYTNER: Prof. Jones, very often witnesses who are being grilled in your situation start wondering, "Well, what is behind all these questions", and it sometimes becomes uncomfortable. Can I put it straightaway to you so that you understand what is the purpose behind these questions, it is not in any way to suggest that anybody at BNFL has ever deliberately misled anybody about discharge figures? It is to suggest two things: firstly, that errors have arisen because of complacency; and secondly, that errors may have arisen through over-defensiveness. I am now coming to the over-defensiveness. Would you look at page 233? This is from Dr. Fulker to Dr. Holder and it is dated November 1983, so these figures would be in preparation, would they, for R171?

A. These must indeed be in preparation for R171.

Q. "The attached table is the best available aerial discharge data for the years 1964 to 1971. The table I gave you earlier was compiled in 1977 from UKAEA reports and is known to contain a number of errors.

The attached table is of course still subject to serious reservations about the accuracy of sampling and measurements. B204, B6 and B230 only are included."

B204 is a high stack, is that right?

A. That is correct.

Q. Discharging plutonium?
A. And other radionuclides.

Q. And others. B6 and B230 are what, low or medium?
A. B6 has one high stack and one medium; B230 is a medium stack. The discharges from all of those stacks are described as high stacks with 80 metres effective height in my report.

Q. "There may have been significant discharges from other sources such as B30 and B38 during this period.

Specifically, the table does not include discharges of Kr85, A41, Cl4, tritium and I129. I131 is shown but this is not included in the beta data."

That is a man who is then handing over the table on the next page, which I am certainly not going to go through in detail with you, to Dr. Holder for passing on presumably to NRPB?

A. I presume so.

Q. Now can we look at page 230, 4th April 1984? This is from Dr. Mummery, the Director of Health and Safety, to Mr. Dunster of the NRPB, and he is "confirming our conversation following a telex re your draft report". He encloses figures for annual discharges to sea back to 1954 and to atmosphere through measured outlets back to 1964:

"This is the data which was identified as reasonably readily available and I am checking separately what, if any, special wording"

Then he discusses the data, if you would like to read it through quickly to yourself. There is no suggestion in that letter anywhere, is there, that there are any doubts held by those at BNFL about the accuracy of the data which is being sent?

A. No, at a quick reading I can't seen any such doubts expressed. Equally, I can't at first reading see any extravagant claims for the total reliability of the data. I can't put myself in the mind of Mr. Mummery when he was writing that letter.

Q. One would not, of course, expect an extravagant claim of accuracy. That would have been dishonest and one would not expect at all to find that from Dr. Mummery. What I am putting to you is that the doubts which were held by Dr. Fulker do not appear to have been passed on?

A. That, on the evidence of this memo, would seem to be the case.

Q. The next thing that happens which affects the attitude of BNFL to the need to assess accurately their discharge figures and to provide them to other authorities comes in this sense, does it not: you will remember why Sir

S. R. JONES

Douglas Black's Committee was set up - that was because of the Yorkshire Television programme?

A. That is correct.

Q. If there had been no programme on Yorkshire Television, there would have been no Black Advisory Committee, would there?

A. I would presume not.

Q. If there had been no Black Advisory Committee, going on, there would not have been a Dr. Jakeman, would there?

A. Again, if there had been no NRPB report, there would not have been a Dr. Jakeman.

Q. Without Dr. Jakeman, there would not have been an addendum, would there?

A. I really think that is rather more speculation than I would be prepared to acknowledge.

Q. Why?

A. To say there would never have been a review of these matters would, I think, not be a reasonable extrapolation. I would agree that the cause of the addendum being prepared when it was was undoubtedly Dr. Jakeman's intervention, but to say that there would not by now have been an addendum for some other reasons would, I think, be unreasonable extrapolation.

Q. I think I would accept that, Professor Jones, but as we come on to the history of this matter it may well have been that subsequently some time later there might have been a reappraisal, but at that time certainly the addendum was driven by Dr. Jakeman?

A. That is correct, he certainly determined the timing.

Q. MR. JUSTICE FRENCH: It was precipitated by Dr. Jakeman?

A. That is correct, my Lord.

Q. Dr. Jakeman precipitated the R171 addendum, but a review of 171 by now would have taken place without him? Is that what you are saying?

A. I am saying may have done so, my Lord. I can't say positively that it would.

Q. A review of 171 may have taken place by now without Dr. Jakeman?

A. Yes, my Lord.

Q. MR. HYTNER: I would certainly accept that answer in relation to plutonium; whether it is right in relation to uranium oxide is a different matter. At any rate, the intervention of Dr. Jakeman at that stage enabled the NRPB and other authorities, and indeed the public, to have the record put straight in relation the emissions of uranium oxide between 1951 and 1957?

A. That is correct.

Q. That should have been a help also to British Nuclear Fuels because it is very important for them to know the truth about their own operations, is that right?

A. Yes, I certainly agree with the sentiment.

Q. What is therefore necessary is to see the attitude of BNFL to the intervention of Dr. Jakeman and to the effects upon the addendum. Could you look first of all at page 102? I am sorry, when I say Dr. Jakeman, first of all, chronologically we are looking at the intervention of Mr. Cutler of Yorkshire Television; I am so sorry.

Q. MR. JUSTICE FRENCH: Page 102?

Q. MR. HYTNER: Page 102. This is from Mr. Atherton, is it?

A. Yes, that is from Dr. Atherton.

Q. To Dr. Anderson and to other gentlemen, and he is reporting on Black, and then in paragraph 3, because this is an extract, he says:

"It is somewhat galling that Black acknowledges that YTV may have performed something of a public service, and I find it even more galling to have to accept that YTV has so easily identified what Black calls unusual mortality rates of leukaemia amongst young people, when the local health experts have failed to do so".

Then he goes on:

"Although Black explains carefully why cancer rates in small areas known to have cancer are bound to give statistically significant excesses, his 'qualified reassurance' to the people of Cumbria may not be very reassuring to ordinary people when it is coupled with Seascale's position in the 'league table', and his perfectly valid statement that 'the proposition (i.e. links to Sellafield) cannot be completely discounted'".

I make no point on the last matter because it conflicts very strongly with what BNFL are now saying. But there is Dr. Atherton finding it galling that BNFL are now appreciative of their own position because of the intervention of Mr. Cutler. There is very little gratitude there, would you agree?

A. I would agree that Dr. Atherton expresses very little gratitude. I would say I think a number of things in response to that. First of all people, I am glad to say, often put their personal feelings and views about things in memoranda; secondly, I don't think anybody in BNFL feels any particular gratitude to YTV or James Cutler, not because the issue of excess leukaemias was raised, because that was important, but I think the bad feeling which Dr. Atherton expresses rather clearly was due to

the manner of its presentation. I think there are two different things there which leads one not to draw anything particularly sinister from Dr. Atherton's apparent annoyance with YTV.

- A Q. There is nothing really sinister in any event, is there, Professor Jones, and there is nothing very peculiar about British Nuclear Fuels in this respect? Large organisations, indeed sometimes small organisations, do not like being caught out and become over-defensive if there is a risk of being caught out in error?
- B A. I agree that it is always unpleasant to be caught out and certainly organisations can react defensively.
- C Q. Let us now look to see how the over-defensiveness can work, as it were, in advance, that is when somebody suggests that somebody has made a mistake but the matter has not yet been determined. Let us see what BNFL's attitude was to Dr. Jakeman. First of all can you look at page 93?

MR. ROKISON: I was asking my learned friend if we could have the reference to the last document because, of course, all we have set out in this document is the third page of a three page ---

MR. JUSTICE FRENCH: Of course you must have it.

MR. HYTNER: It will be passed to you.

- D Q. So that my Lord can follow the saga and how Winfrith came to be involved, Dr. Jakeman had worked at Sellafield and in 1984 was in fact employed by UKAEE at Winfrith, is that right?
- E A. As I understand it, he had been abroad for some time and in or around 1984 he had returned and read NRPB report R171. I believe that was the sequence.
- Q. Dr. Russell is writing to Mr. Allday (page 93) at British Nuclear Fuels. He is the Chairman and Chief Executive:

F "I am writing at Mr. Allen's request" (presumably one of Mr. Russell's inferiors) "to pass you the attached note produced by Dr. Jakeman, a physicist at PSO level at our Winfrith establishment, for his divisional head. This sets out some recollections of his from his days as an SSO at Windscale in the 1950's and his concern about the possible implications of these for the current investigations into possible links between Sellafield discharges and the incidence of leukaemia in the past 25 years in West Cumbria.

G It is possible that Dr. Jakeman was working in what is now the Windscale Nuclear Laboratories and so we are similarly sending a copy to them.

H

S. R. JONES

It would be helpful to have your comments on the note since Winfrith will be able to assure Dr. Jakeman that it has been given due consideration".

Then the following letter comes from Mr. Allday, very swiftly, I think within ten days:

"Dear Mr. Russell,

The following is in response to your letter of 2nd August.

The incident which Askew refers to was due to the release of UO2 particles following damage to some cartridges on their discharge from the Windscale piles in 1955. There are three relevant reports ... These reports are initially Top Secret Atomic, Secret Atomic and Secret Atomic respectively.

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 11th June 1984. The data in these reports were considered in the NRPB Report 171 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 μm and they conclude 'Particles of this size are not respirable and will therefore have little radiological impact.'

Thus you can rest assured that all matters relating to this incident were fully considered by NRPB on behalf of the Black Inquiry. If Dr. Askew would like copies of the reports we will gladly send them to him. He should contact Dr. Anderson, Head of Safety and Environmental Protection in our Health and Safety Directorate here at Risley.

It is clearly important to assure anyone who enquires that the full range of information available to the AEA and to BNFL has been released to the Black Inquiry. To this end we are planning to publish the previously unpublished data which were given to this Inquiry".

Of course, when it states that all matters were fully considered by NRPB, that would not be quite right if it turns out that NRPB did not make their own critical review of the information?

- A. Other than that, all I would read into that letter is that Mr. Allday is saying that in regard to this matter BNFL has fulfilled its obligations and made available to the Black Inquiry those really rather important papers, which up until that time were still under some reasonably high level of security classification, I believe.

S. R. JONES

- A Q. The implication of that letter is clear, is it not? The implication is that Dr. Askew, who is probably Dr. Jakeman's superior, has nothing to worry about, all this was taken care of, that there is nothing in this report of Dr. Jakeman?
- A. That is an implication which you make from the letter and I do not think I wish to comment on it.
- B Q. I am asking you - I am not giving evidence - is it not a fair inference from that letter that Mr. Allday was trying to push that one away?
- B A. It is an attempt to be immediately reassuring, which is one of the things that is important in these sort of circumstances, and Mr. Allday is simply saying what I think I have said, which is that all the relevant material was disclosed, which it was.
- C Q. Then we turn to Dr. Anderson, who writes to Dr. Lowe at Winfrith with a copy to Mr. Russell at page 96. We are now in October:

"I am responding to Russell's letter of the 7th September enclosing copies of the three reports relevant to the release of oxide particles at Windscale in 1955. I am afraid these reports are rather old and do not photocopy very well but where things would otherwise be illegible we have written over in manuscript.

Do you know what use Jakeman intends to make of these reports other than to satisfy his curiosity? Even though they are now unclassified, I would be slightly unhappy (perhaps irrationally) if too much was made of these reports in the public arena".

E That is from Dr. Anderson. For the next letter in December, we have I am afraid to go back to document 11, for which I apologise. This is Dr. Anderson to Mr. Allday. This is a fairly high level memorandum, is it not?

A. Yes, it is.

F Q. Dr. Anderson to the Chairman and Chief Executive:

G "You will recall that in August this year you received a letter from Dr. Jakeman via Arnold Allen's office, commenting on the possible impact of a Windscale pile incident in 1955 on the conclusions of the Black Inquiry. I advised you on a reply which you sent on 13th August and offered Jakeman a sight of the relevant Windscale reports which had been declassified for the Black Inquiry. This offer was accepted and subsequently has led to a potentially embarrassing situation of which you should be aware. However I can assure you that I have the present position under control and hopefully the potential will not develop into reality.

H

S. R. JONES

A When I sent the reports to Dr. Jakeman, via Dr. Lowe, the Director of AEE, I offered the opportunity to Jakeman to discuss with me and others any further views he might have after reading the documents. I heard nothing further until a few days ago when I received from Dr Lowe a copy of a formal report (about 12 pages) by Jakeman concluding that the impact of this incident had been substantially under-estimated by the NRPB's assessment. Jakeman's arguments suggested that the risk associated with Sellafield discharges would be about double on the basis of his calculations.

B From subsequent enquiries I have discovered:

(i) that Jakeman has sent the report to NRPB;

C (ii) NRPB have written a five page critique pointing out the errors in his radiological assessment;

D (iii) NRPB have sent the report together with their comments to Dr. Eileen Rubery, Secretary to the Black Inquiry. They did this because they did not know the status of the report and were concerned that it might be published without prior knowledge of the Black secretariat. They had not considered informing BNFL until I made my enquiry.

We have substantial criticism of Jakeman's erroneous derivation of some of the source terms concerning the estimated release fractions.

E In the light of the above, I have taken the following actions:

F (i) I have spoken to AEE Directorate (Mr. J. Holmes) and to Jakeman who has assured me that he regards his report solely as a discussion document and has no intention to publish it, if at all, without further consultation. I said I would send him our comments and offer him again the opportunity for discussion.

G (ii) I have spoken to Roger Clarke (Secretary of NRPB) to discover that he is annoyed about Jakeman's activities. Clarke will send me NRPB's critique of the radiological assessments and I will reciprocate by sending Clarke our criticisms of the source term calculations.

H (iii) I have also spoken to Eileen Rubery to explain the position to her and she has agreed to do nothing with the report pending resolution of the situation.

I apologise for burdening you with a somewhat lengthy memo on what I trust will evidently turn out

S. R. JONES

to be a non-event. However, I feel it necessary to present the facts to you to avoid any embarrassment if the position is brought to your attention in any other way".

This does not seem to reflect the attitude of somebody with an enquiring mind wondering whether perhaps Dr. Jakeman may have stumbled on something true?

- A. Yes, it is not clear to me at this stage whether Dr. Anderson has seen Dr Jakeman's report or not, and certainly I am not clear at what stage within this saga, as you have described it, I came to have a personal involvement, but perhaps we will get to that.

- Q. I think you come into it a little later. The other matter which seems to be reflected in this memorandum or letter is that we have here NRPB and BNFL collaborating not to promote an enquiry into a suggestion that their calculations may have been wrong, but collaborating to do down the chap who is making that suggestion?

- A. Apart from perhaps a comment in which Anderson says he has spoken Clarke and he thinks he is annoyed, I am not sure that I see even that, in that NRPB seem to have done something with Jakeman in the Jakeman report, and COMARE, quite independent of and without reference to BNFL. I am not sure that that smacks of collaboration.

- Q. I am sorry, no suggestion is made of collaboration with COMARE or with the Black Committee; the suggestion is that here we have Mr. Clarke of NRPB and Dr. Anderson agreeing together that they will collaborate and exchange critiques not to promote a positive enquiry into Dr. Jakeman's suggestions but to promote their already formed view that he is in error?

- A. That letter again seems to me to be a considerable extrapolation from what is written in the memorandum. What the memorandum seems to tell us is that Jakeman has sent his report to NRPB; it is not clear to me at this stage whether BNFL has Jakeman's report or not. Maybe that will become clear, but at any rate Jakeman has sent his report to NRPB, they have written a critique of it, they have sent the Jakeman report and their critique to DHSS - I apologise in saying COMARE because COMARE was not then extant or may not have been extant - and subsequent to that, Clarke and Anderson have agreed to exchange their respective critiques, so I guess by that stage Anderson must have seen Jakeman's report.

- Q. If you look at page 11, you will find that that is so. I read it out but I cannot expect you to absorb everything that I read. Paragraph 2:

"I heard nothing further until a few days ago when I received from Dr. Lowe a copy of a formal report. (about 12 pages) by Jakeman concluding that the impact of this incident had been substantially under-estimated by the NRPB's assessment".

- A. So this was at least at that stage very much an initial reaction by Anderson.

S. R. JONES

- Q. It may be that it was an initial reaction; it is the initial reaction of Mr. Clarke of NRPB and they are getting down together to promote their initial reaction?
- A. I am sorry, Mr. Hytner, but that is a matter that I don't see I can establish by reference to this memorandum one way or the other.

- Q. That is a very fair answer. It is a matter of inference that anybody can either draw or not draw?
- A. It is.

- Q. I will not ask you about it further. Will you look at page 99, which is still later in December? This is again Dr. Anderson to Dr. Lowe at the Atomic Energy Establishment at Winfrith:

"Dear Graham,

Thank you for the opportunity to comment on this document. I note that it is for discussion at this stage and I have an assurance from Jakeman that he has no intention to publish the report, if at all, without further consultation with BNFL and NRPB."

This, of course, that he is referring to is the Jakeman report that we have referred to.

"The NRPB have made very detailed criticisms of the basis of some of his radiological assessments and therefore I am confining my comments to estimates of the amounts released, in the annex to this letter.

I think it important to recall that the three main references supplied to Jakeman were originally very highly classified so there would be no pressures on the authors to underestimate the effects or sizes of releases. I see no reason to assume, as Jakeman does, that the releases were significantly different from those estimated at the time by senior people with access to far more material than could be contained in formal reports. In so doing, it casts doubt on the integrity of several distinguished people. It is easy, but not helpful, to construct a range of scenarios as Jakeman does largely on the basis of what if? I think he recognises the large uncertainties inherent in his approach as he uses expressions such as 'estimates are therefore very uncertain', and 'others may wish to choose the most appropriate value'. In a letter to Adams on 29/10/84 he states 'Clearly one can go on juggling with figures indefinitely without achieving very much. I admit to having a very biased view'.

I agree with his sentiments about juggling figures, but I am still prepared to offer him the opportunity for further discussion if this would be helpful. I would also invite representatives from NRPB, although

I know they feel that some members of their staff have already spent a considerable amount of time dealing with Jakeman's hypotheses.

I would appreciate hearing from you after you have had time to consider the latest position. We must find some mechanism to bring this position to a satisfactory conclusion".

Then we turn again, if one looks at how they were approaching this intervention from Dr. Jakeman, to page 150. This is January 1985 that we are moving to, from December, from Mr. Curtis, or is it Dr. Curtis?

A. I am sorry, what page?

Q. Page 150:

"I received a telephone call this morning from Derek Jakeman, who is known to me from my time at Winfrith. He gave the impression of being dangerously unbalanced, and I think it may be necessary to consider what further action he is likely to take in relation to his views on releases from Windscale in 1954-1955".

I am sorry to go back again to page 13, in respect of which I would like your assistance because I am not sure that I fully understand what is there, and I certainly do not want to draw inferences from ignorance. This is a letter from Mr. Longley to yourself?

A. Yes, although it was addressed to myself, you will forgive me for having forgotten that I ever received this memorandum. Longley seems to be telling me what references there are to uranium oxide emissions and related environmental measurements such as Booker's measure, measurements of caesium in soil, and unless there is something I have missed, it is simply something from Longley which tells me where there is relevant information.

Q. The puzzling thing, which I am sure there is a non-sinister answer to, is on the first page, half-way down:

"The third draft of SERC Pt 2 (January 1980) contained discussions of emissions of uranium oxide particulate (paragraphs 28, 29, 37, 42) citing Booker's report as reference 17. All this was suppressed in the final re-written report AERE-R9873 published in July 1980".

What does that mean?

A. It certainly means that that reference to emissions of uranium oxide was not in the final re-written report in 1980, but it does not help as to what the reason was. "Suppressed" does indeed sound a bit ---

Q. It is pejorative, is it not?

A. It is a bit pejorative.

S. R. JONES

Q. You cannot help us there?

A. Beyond the use of that word, I cannot help you, I'm afraid.

Q. A non-pejorative verb would have been "omitted"?

A. Omitted, yes.

Q. You cannot assist?

A. I can't assist, I'm afraid.

MR. HYTNER: Is that a convenient time, my Lord?

MR. JUSTICE FRENCH: Yes, thank you. 2 o'clock.

(Luncheon Adjournment)

MR. HYTNER: Prof. Jones, following on from the last document and answer, and you will be relieved to know this is easier because it is page 1 of the bundle, could you again help us with this document, which may have an innocent explanation? We seek clarification. This is the 24th of January, 1986. I am still dealing with the Addendum. This is from Mr. Llewelyn to Dr. Anderson:

"Arthur Chamberlain has made a point to us which you should know about. He says that a Harwell report AERE-R4020 has been referenced in recent unclassified reports which were submitted to the DOE/BNFL/AERE Tripartite Committee which was set up to consider environmental measurements in West Cumberland some time ago and that the data in this report, of which BNFL were well aware, were inconsistent with the figure of 400 grammes oxide emission in 1954/55.

Dr. Chamberlain feels that, depending on how the COMARE Committee discussion develops, it might become necessary for him to mention the existence of this report, as it is available to anyone who might care to ask for it."

The first thing I ought to ask you, although it may be clear to those who are more familiar with BNFL, since this is a public court and people may make errors of identification: which Chamberlain is this? There are two important Chamberlains in the history of BNFL, aren't there?

A. The Chamberlain here mentioned is not a BNFL employee at all. He is a former employee of the Atomic Energy Authority who has done a great deal of environmental research in relation to atmospheric deposition and a wide range of other matters.

Q. It is not the present...

A. It is not the present Chief Executive of BNFL. That is Neville Chamberlain.

Q. MR. JUSTICE FRENCH: When you say a former employee of the Atomic Energy Authority, do you mean then "former" or now "former"?

A. In 1986, I am not sure, but he has certainly now retired. In 1986 I am simply not sure whether he was still employed or not.

Q. MR. HYTNER: Can you help us? We know that 440 grammes of uranium oxide was the figure provided to the NRPB for the original R171 report to the Black Committee. Can you help us with this, Prof. Jones, because the suggestion seems to be here that Dr. Chamberlain was suggesting that BNFL was in possession of information at that time which would have alerted them to the fact that the 400 grammes, as it is loosely referred to, was wrong?

A. Yes. I am rather puzzled by this reference. I can just tell you what I am aware of from my own recollection in the matter, and that is at this point in time, before I, amongst others, had carefully looked at the original Dibben and Howells work, nobody in BNFL had considered the data in document R4020 in terms of its significance regard the total caesium deposit and what that implied by way of the uranium oxide emission. That is my recollection and it is certainly the state of my own personal knowledge at this point in the story. I don't believe there were others in BNFL, certainly those who were involved at the time, who knew of any such thing.

The only thing I can suggest is that AERE R4020, as you know, simply contains data on the measured caesium deposit. It doesn't in itself make any reference to the quantity of uranium oxide, except that which you would derive from integrating the caesium measurements, as I understand it.

It may be that Chamberlain at that time was himself aware that the caesium measurements were inconsistent with 400 grammes of uranium oxide, but to my recollection nobody in BNFL did at that point in time.

Q. Except that he seems to think that somebody at BNFL was aware of it?

A. No. He refers to the data in this report, "...of which BNFL are well aware." The data in R4020.

Q. Which were inconsistent with the figure of 400 grammes?

A. Only if one has done some interpretation.

Q. I want to make it absolutely plain, for reasons which will emerge later it is very important I should make everything clear, I am not suggesting for a minute that somebody at BNFL dishonestly and deliberately suppressed crucial information. What I do suggest is that there were times when people were somewhat careless with information and there was no great anxiety to pursue the truth if that pursuit might be inconvenient?

A. I think you must develop your line of examination to make that point.

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Q. You then came on the scene because you say in your report that you readily agreed with Dr. Jakeman when you were brought into the matter?

A. Yes. I cannot now recall the timing but it seems to be some time fairly shortly after the point we have now reached.

Q. MR. JUSTICE FRENCH: Is this right: soon after January, 1986 you were aware of the Jakeman Report?

A. Yes, I believe that is correct.

Q. And saw no reason to doubt it? Is that how you put it?

A. No. If I may explain my own recollection, which, without reference to documents here I believe is correct, was that I was originally contacted by one of the Health and Safety Directorate's staff, that is, Anderson's staff, who consulted me about this matter. I imagine the memo I got from Longley must have been at about that time, because it would appear at that time I was expressing interest in what relevant information was available. At that point in time I read through the Dibben and Howells Report and possibly the later one, the 1957 one, and I concluded from my own experience that the assessment they had made, the 400 grammes as it finally was, was not likely to be the full picture, based on the methods which they had used. Therefore, I was very open to the suggestion that the caesium deposit around the site indicated a bigger figure, and that that was likely to be nearer the truth. So that was very much my state of mind soon after I became involved in the discussion.

Q. Soon after January, 1986 you became aware of the Jakeman Report - "I had independently come to doubt the figure of 400 grammes"?

A. No, my Lord. Because of the Jakeman Inquiry people had been asking me certain questions about whether this was a credible situation or whether it was not. My reaction then was to read through the Dibben and Howells report and related reports to see how they had come to the conclusion of 400 grammes.

Q. Would this be right then: "I had already by then come to question the figure of 400 grammes"?

A. Yes, as soon as I had read the Dibben and Howells report for the first time in real detail.

Q. "I had by then, by reason of the Dibben and Howells report, come to question..." Come to doubt?

A. Come to doubt.

Q. "...come to doubt the figure of 400 grammes and hence to consider that the Jakeman report might have substance"?

A. Yes, my Lord, specifically in the sense...

Q. Could you just pause so I can get this step by step? And specifically?

A. That by looking at the total caesium and strontium deposition around the site one could arrive at a better figure, whatever that might be.

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A Q. MR. HYTNER: Either I misunderstood an answer of Prof. Jones or Prof. Jones has misunderstood a question I put. Could I just clarify that? Basically, was it the Jakeman report which caused you to look at the Dibben and Howells report?

A. It was certainly the Jakeman Inquiry. I cannot recall at exactly what stage I saw Jakeman's report, and I think I looked at Dibben and Howells before I had seen it, but it was the Inquiry by Jakeman.

B Q. We have probably not heard in the evidence of Dibben and Howells, although counsel will have heard and read a great deal. Dibben and Howells, that was an Inquiry which resulted in the figure of 440 grammes being estimated, as opposed to the previous estimate of 100 grammes, is that right?

C A. I had thought it was the later 1957 report which was Farmer, Howells and somebody else who came to 440 grammes. I think Dibben and Howells, if I remember correctly, came to 100.

Q. They were the original...

A. That was the 1955 report.

D Q. The way you put it in chapter 5 of your report is that once you had read Jakeman, you readily agreed with his approach?

A. It was rather that once I had read Dibben and Howells and the related reports critically, I was open to the suggestion that the release might have been much larger than 400 grammes.

E Q. MR. JUSTICE FRENCH: So Dibben and Howells was only 100 grammes and later elevated to 440?

A. That is correct, my Lord.

F Q. MR. HYTNER: It is right that one of the things you readily agreed with when you read the report, was what has been called the pepper pot theory?

A. No, I did not readily agree with the pepper pot theory. As a result of the work I have done for my second report, I believe that in the particular circumstances that we see here the pepper pot theory while right in principle, was over-estimated in terms of its quantitative significance by Dr. Jakeman. However, as I have said, I soon became aware of the potential flaws in the quantitative assessments done in 1955 and 1957. Therefore I saw no reason why a higher figure should not be possible and should not indeed be considered.

G Q. Could I ask you this: were you very much better qualified to judge the Jakeman report and the Dibben and Howells report than the others such as Dr. Anderson, who made comments about them?

H A. Certainly in relation to the Dibben and Howells report I had much more practical experience of environmental monitoring and it was that which led me first to doubt

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that the results of their work could be quantitatively relied on in the way that they did, for a number of reasons.

- A Q. Without reading Dibben and Howells, just for reading Jakeman, were you far better qualified than anybody else at BNFL?
- A. Frankly, I am not clear at what stage I did read Jakeman. I couldn't now reconstruct that.

Q. Well, in 1986, or 1985?

B A. I think in terms of interpreting environmental measurements I certainly was the best qualified person in BNFL, if it was a matter of interpreting environmental measurements, which was the direction that I approached the problem from.

C Q. One thing that is quite clear is that nobody else readily agreed with Dr. Jakeman. To the contrary, everyone else's initial reaction, including NRPB, was that he was in error and furthermore there was some lese majeste involved because if he was right it would cast doubt on the integrity of distinguished people?

D A. Yes. I think it is as well to be clear about the extent to which I agree with the Jakeman report as finally written up as his assessment of the uranium oxide matter. I certainly agree that the quantity was higher than 440 grammes and as you will know from the reports I have provided, I consider that 20 kg is a very good round figure, arrived at from the environmental data.

E There are aspects of Dr. Jakeman's report which then go on from that to assess what the doses resulting to people might have been and there is a lot in that part of his report which could be disputed, and, indeed, which I believe I would dispute and NRPB would dispute now, if it came to that.

F I also, as I have said, recognise the correctness, in principle, of the pepper pot effect, but I consider that the magnitude of that effect has probably been over-stated in Dr. Jakeman's report for a number of reasons.

- Q. In fairness to you, Prof. Jones, let me make it clear: you weren't agreeing with his assessment, and you still don't. His assessment was something like 30 kg?
- A. That is correct.

G Q. You were even then contending for something like 13?

A. Something in that region.

Q. Even now you really don't accept 20, do you?

H A. I have used 20 for the purposes of the assessment. If you are asking what is my absolute best figure, then it would probably be somewhere between 15 and 20, somewhere in that region. Twenty seems to me to be a sensible figure to use, recognising the likely precision of such assessments.

- A Q. What I must suggest to you, and it may be you would prefer others to draw the inference rather than yourself, but you are there representing BNFL in the witness box, and I ask you: when you look back on it coolly and you see that Jakeman actually had a good point which resulted in uranium oxide discharges being increased by a factor of 50, that the negative reaction which he produced, at times indignant negative reaction, was over-hasty and not indicative of an objective search for the truth?
- B A. Well, I have to say I haven't found the evidence for that in the material you have put to me thus far. I will simply say that in any organisation and BNFL is probably not only normal in this respect but at the extreme, one gets a lot of propositions put which are extravagant, over-exaggerated, and turn out eventually to be wrong. In this case, Jakeman in certain respects turned out to be right. If matters were entirely as you are suggesting at BNFL, then I should have met with a great deal of resistance when I started to - if you wish to put it that way - half way agree with Jakeman. However, in fact, I didn't. Dr. Anderson, whom you have referred to a number of times, was certainly a man of some integrity and honesty and wished to see the matter resolved in a way that was technically satisfactory, without, as he would put it, causing a fuss. That is the way he worked. I found no difficulty in dealing with Dr. Anderson, or with anybody else in BNFL, regarding the possibility that Jakeman might at least be partly right.
- C Q. A slight difficulty with that answer, Prof. Jones, which I am sure is partly justified is, of course, by that time the cat was out of the bag because the efforts of BNFL and the AERE to keep Jakeman within the family had rather flown and Jakeman was writing to all sorts of people, wasn't he, including COMARE?
- D A. Well, Jakeman had sent his report to NRPB, which is what you would expect him to do, and they had looked at it. I am not sure to what extent that was an element. If Jakeman or anybody else continues to press their question, it might be there is some initial resistance, but if an organisation is basically honest it will seek to get to the bottom of it technically, and that is what eventually happened in this particular case.
- E Q. I don't press this because it really is a minor part of a subsidiary issue, but since you are, as it were, defending the attitude to Jakeman, perhaps we should look at one other instance of somebody else doing the same thing and seeing the reaction to him. Could you look at page 164? Here we have a Mr. Williams writing in. This is in 1986, writing to Mr. Harding, the Chairman of BNFL. He was an employee of BNFL prior to retirement, wants to draw attention to a memorandum he wrote to Mr. Mummery on the subject of cancer deaths in Seascale, "didn't feel my memo was taken altogether seriously". I am not going to read it out, it may be all wrong. It is
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unfair to read out something that turns out to be wrong and I don't know whether it was right or wrong. Then he goes on with his complaint. Then we look at page 166 and here is Mr. Mummery writing to Mr. Harding:

"I understand at the time that his attitude was considered to be something of a problem by his senior management."

He referred to a memorandum and so forth. Then at the bottom:

"I have omitted any reference to Mr. Williams' memorandum of 1977 and have avoided any reference to an entry point with DHSS about the progress on Sir Douglas Black's recommendations."

Then over the page we see the draft reply, which was along those lines:

"I trust that the attached information which has been prepared for me is of help...Implementation of the recommendations is not the responsibility of the Company..."

There again is somebody who writes with inconvenient information or suggestions and he is another one with an attitude problem. Is this endemic in a large organisation, that anybody who challenges the organisation from within is regarded as part of the awkward squad?

A. I really cannot comment on the individual case. It may or may not have been that Mr. Williams had an attitude problem, as you put it. I don't know Mr. Williams.

Q. It may be true. It may be that his memorandum was rubbish and he did have an attitude problem.

A. In regard to the content of his memorandum the answer seems to be quite straightforward that this follow-up was one of the Black Inquiry recommendations which was eventually followed up effectively by Prof. Gardner, and which resulted in the report to which you will be making much reference in the litigation. I cannot see any issues from the letter.

Q. No, it has no direct bearing.

A. As to whether Mr. Mummery was right or wrong to say that Mr. Williams had an attitude problem, which is what he seems to be saying, I really cannot comment.

Q. Just finally leaving the 171 Addendum, the fact at that time is that Dr. Eileen Rubery was not necessarily accepting that 20 kg of uranium oxide was the correct estimate, was she?

A. I haven't read that correspondence.

Q. Could you look at page 81? It is from Dr. Rubery to Dr. Anderson:

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"I enclose DHSS comments on the paper going to the Local Liaison Committee.

BNFL ACCOUNT OF HOW THE NEW INFORMATION CAME TO LIGHT

Para 1

The initial assessment (1955) of the releases given as 100 g whereas we quote 100-200 g in the COMARE report. (Para 3.2) this is only a minor point. Of more importance is the BNFL statement that this was based on a comprehensive environmental monitoring and investigation programme. I question that the environmental monitoring programme at that time could be called comprehensive. Indeed this is contradicted later in the BNFL account (para 811) where it is stated that little routine environmental monitoring was carried out up to 1958.

Para 3 and 4

It is stated that, following the meeting of 1 April 1985, it was agreed that 20 kg of Uranium Oxide was a better assessment of the releases (in the 1954/5 episode). This was agreed by all as the best estimate of the releases. Dr. Jakeman however stressed that this figure was approximate and that a range of 10-50 kg was possible and perhaps this should be indicated (see para 3.3 of COMARE)."

Then there is reference to the reassurance on the efficiency of filters with which we need not be concerned with now.

In relation to uranium oxide your view is that despite that statement the uranium oxide, certainly 20 is top and it is probably between 15 and 20?

- A. That is what I derived from the data, having considered it now in considerably more detail than I had up to the time of writing my first report, and noting that the 50 kilos suggested by Dr. Jakeman arises essentially because of the pepper pot effect, which I would conclude is not nearly so large as Dr. Jakeman suggests it might be.

- Q. Now can we turn from the Addendum to later events?
A. Yes.

- Q. It is right that in 1985 the government told BNFL that in future authorisations would be required for discharges?
A. There have been authorisations for discharges, as far as BNFL was concerned, ever since the company was formed. However, whilst the liquid effluent discharge was quantitative and had numerical limits for the amounts which were to be permitted to be discharged, the aerial effluent authorisation was not quantitative. It simply required the company to use best practical means to

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minimise discharges and to carry out certain measurements of both the discharges and the local environment, but it had no quantitative limits.

A Q. I am right in saying that in 1985 you were on notice that as far as aerial discharges were concerned specific limits would now be imposed?

A. Yes, numerical limits on quantity.

B Q. As a result, according to you on page 11 of chapter 5, a major programme was implemented with a view to maintaining your discharges below the proposed limits?

B A. No. The major programme was implemented in order to assess whether the sampling equipment in use was providing a correct quantitative measure. Up until that point in time the complete quantitateness, if you like, of the measure had not been such a pertinent issue because aerial effluent sampling was used to assess whether the particular process was running normally or better or worse and public radiation exposure was assessed largely from the environmental monitoring data.

C Having to comply with a specific numerical limit for discharge means that one has to be certain that the numerical quantity being quoted for the discharge is quantitatively correct.

D Q. I think the answer to my question is yes.

A. It may be but I thought...

Q. It was because you now had quantitative limits imposed on you that you implemented a major programme to look into the efficiency of your sampling arrangements?

A. That is correct, because we were about to have quantitative limits imposed.

E Q. Thirty years after plutonium began to be discharged through the B204 high stack?

A. That is correct.

Q. And at a time when it was believed, as we shall see later, that the total discharges between 1951 and 1985 were something in the order of 174 GBq?

F A. That may be the correct figure. Obviously I don't have that figure in my mind.

Q. I keep mixing 174 and 176, I may be out by 2. Let's say 176.

A. I doubt that the difference is significant.

G Q. That is right, isn't it? Wasn't that the figure which was believed to be the extent of the plutonium discharges in 1985?

A. Mr. Hytner, I simply don't know what that figure was because I haven't done this sum.

H Q. The major programme caused you to realise, or produced the discovery, that the sampling arrangements had not

been efficient and that in order to calculate from the filter papers the true discharges you would need to multiply by a factor of 4 to get to the actual discharge? Is that correct?

- A. Not in every case. For some particular radionuclides. No, not even on average. The situation was that the stack sampling systems which had been more newly installed on the newer plant, which, if you like, were able to take account of recommendations for sampling practice that were published in about 1975, generally had sampling efficiency factors close to 1. That is their required little correction. The sampling systems installed on the older plant, which did not have the benefit of being designed with that in mind, had generally much poorer efficiencies and by poorer I mean a sampling efficiency factor much bigger than 1, like 4. However, the initial results had a sampling efficiency factor of 4 as I think probably the maximum for any individual nuclide on any individual outlet based on the initial results.

Q. The trouble with that as an answer is this: that the major discharges of plutonium were older discharges, which would have been subject to poor sampling arrangements. By 1985 the discharges of plutonium were very, very modest compared with the discharges in the fifties, sixties and seventies?

- A. The sampling efficiency factor of 4 certainly does relate to the sampling efficiency factor which was determined at that time for plutonium from the building B204 outlet. It is certain, at least on the interpretation that I have made in my report, that that was the major quantitative source of plutonium emission. However, there were other factors involved in that, as you are aware, than simply just the sampling efficiency factor.

Q. Again I think the answer to my question was yes. Now just pausing there it was a pity if the Addendum had come eight months or nine months later of course COMARE would have known about the stack efficiency factors?

- A. I think it is probably correct to say COMARE knew about the stack efficiency factors anyway, but they knew them after the R171 Addendum was published rather than before, and that is correct, it would have been nine months to a year, I believe.

If I could just interject at that point, the matter of sampling efficiency factors is not really quite so straightforward as saying that because we have made a particular assessment of the situation in 1986 or 1985 or 1988 that that automatically applies to all the results that have been obtained over the past 30 years. The efficiency factor does depend on a lot of relevant conditions that apply to the discharge at that particular time. It might be right or it might be wrong to apply them retrospectively. In writing my report I have thought about that again and come to the conclusion that

it would be right to use a certain value applied retrospectively, but that is not a thing which follows necessarily or directly from the fact that a particular measurement has been made in 1988, or whenever.

A Q. You mean there is uncertainty about this?

A. Of course there is uncertainty. There is uncertainty about whether the factor should be applied at all or whether a larger factor should be applied.

B Q. I think I may have misled you over the figure for plutonium discharges in 1985. I think it may be right that the R171 figure was 65 GBq, and that it was Howorth and Eggleton in a draft report in 1986 who raised that to 176 GBq. Do you recall them?

A. I recall the Howorth and Eggleton report, but they had done no work on the B204 stack emissions. I cannot see any reason why they should raise the figure from any value to another value.

C Q. It may be they were confirming a previous figure of 176.

A. It is true that the figure was raised between R171 and the R171 Addendum because the pre-1964 discharges were reassessed by trying to do a scaling exercise based on the material throughput of the plant.

D Q. It may be that my notes are confused on this and you are absolutely right. It may have been that the 65 GBq was the R171 and the 176 GBq was the Addendum figure?

A. It may be.

Q. In 1987 we had the BNFL Annual Report for 1986. Do you recall that?

A. I do.

E Q. There was a reference in the report to stack efficiency factors, or sampling efficiency factors. It simply indicated that the figure for 1986 would be adjusted for a stack efficiency factor of 4, and there was absolutely no reference to what they were or whether the previous year's discharges had to be adjusted. Do you recall that?

F A. I certainly recall the Annual Report and I am sure you are correct to say that only the 1986 figure was adjusted. I again make the point I made that certainly at that time while the issue of retrospection was certainly in people's minds, I for one didn't think we had sufficient information at that time to apply them all retrospectively. I think the comment in the report is quite clear, that we have re-assessed the sampling method and that as a result of that the figure we quote for 1987 is not comparable with those for earlier years because they were derived on a different basis. We hadn't at that stage assayed an attempt to say what the discharges in earlier years might have been.

G Q. That was never done, was it? In each annual report thereafter, the - should I be saying stack or sampling

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efficiency factor for SEF?

A. Sampling efficiency factors, please.

Q. The sampling efficiency factor was applied to the existing year and all years, indeed, from 1986 onwards, but in no annual report was the sampling efficiency factor applied to any previous year's discharge?

A. That is correct.

Q. So any member of the public reading the annual reports and wondering what the discharge figures were would simply have assumed that all previous years from 1986 were to remain as they always had been?

A. I suppose that could be true, yes. I think the insertion in the 1987 report would make it fairly clear that that was not the case, that we were now using a new method, which gave a different answer, but we never went back formally to revise all the previous discharges.

Q. Then we come to 1988, when Howorth and Eggleton actually publish their draft report, in which again the figure of 176 GBq was given as the total discharges from 1951 to 1988?

A. That may be so.

Q. Do you not recall that at all?

A. I can recall the report, but the precise figure of 176 GBq is not one that I remember.

Q. What I then ought to ask you is this, since I am putting to you that that was the definitive figure until you published your report or, I suppose, more accurately, until possibly the authorisation application went in a month or so before your report. Do you recall what the total discharge figure you were working on had been before you calculated the new figure of 3,400 GBq?

A. I do not recall that figure because I have not, during the course of preparing my report, particularly looked at the total quantity emitted.

Q. You must have appreciated, must you not, that when you arrived at 3,400 GBq, you were causing a massive uplift in the previously believed figures?

A. Of course, I did.

Q. You did realise that?

A. Of course.

Q. In fairness, again I must make it clear that, if you had been looking at the figures, you would not have looked at 176. You would have multiplied that, presumably, by the sampling efficiency factor for each year?

A. I would certainly have been aware that that figure was based on the as measured values, yes.

Q. But, in any event, irrespective of that, you were aware of a massive increase in the figure?

A. Yes.

A Q. That massive increase in the figure came about because of this litigation, did it not?

A. In terms of making a positive decision to apply the factor retrospectively for the purpose of making an assessment, yes, that must be the case. Again it was the instigating factor. I had to sit down and try and arrive at a clear view on what should be done about retrospection and I did so.

B Q. We know that the writs were issued in 1989/90 and we know that, in the spring of 1990, there was an order for discovery, and we know from your helpful report that SEAM was set up because of the litigation. Could you tell us when SEAM began to be set up or when the decision was made to set up SEAM?

A. I think work started on it probably a few months before the writs were received. The initial work started then, I believe, and carried on over the following two years.

C Q. So this would be 1989/90?

A. Yes.

Q. MR. JUSTICE FRENCH: Were the two enterprises connected - the impending proceedings and the setting up of SEAM?

D A. Yes, the SEAM model was set up specifically to calculate doses for this litigation as an exercise I felt we had to do, to carry out the exercise that I wished to carry out, which was to try and correlate all the available environmental measurements that we knew about with all the discharges we knew about and to do that balancing and accountancy exercise. That is not an exercise that we have really had to do before. It is an exercise which, with hindsight, it would have been very valuable had we done so, but it was quite a massive undertaking and it is not one which previously had had the necessary priority attached to it.

E Q. MR. HYTNER: You seem to anticipate all my questions, so I am sure you know what is coming, Prof. Jones. You see, the point that I would make is this: that the major programme which was implemented, which disclosed the need for uplifts for SEFs, was triggered by the imposition of quantitative limits. The massive uplift in the discharge figures for plutonium were caused by SEAM, which was triggered by litigation. The point I make is this: that it is only when BNFL have their backs to the wall that they really make an effort to find out what the accurate discharge figures are and were?

F A. I am not sure that I would put it that way. I would simply observe that, as a company, BNFL has many obligations to meet, including statutory ones, and the prime obligations it has to meet are to carry out its operations now, in the present day, in full compliance with all relevant standards and statutory requirements. That is the overriding priority. I do believe we have a general moral obligation, as I think you have indicated,

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to be sure about our history, but that is not a statutory obligation and it is not, therefore, necessarily always the one at the top of the company's priority list.

A Q. Before I go on with SEAM, can I just pause there in parentheses to deal with three points that you made in the course of your evidence about SEAM. The first is that really what matters is dose and dose is measured by environmental monitoring such as milk. Of course, there was no environmental monitoring of milk between 1951 and 1958, which was precisely the period when the piles were in operation. That is correct, is it not?

B A. That is correct, and that is why I made the point about SEAM, that, had such monitoring been in place and had monitoring for all other relevant pathways been in place at all other relevant times, we would not have needed SEAM at all.

C Q. The other observation comes in the COMARE 1 report at paragraph 3.21. If I can just read it to you for your comments:

D "The dose and risk estimates in the addendum to R171 are higher because the environmental data - that is the milk levels of Strontium-90 - are now interpreted as the decaying tail of an earlier peak, whereas in NRPB R171 they are interpreted as part of a steady state level of Strontium-90 in milk. NRPB did not extrapolate back from environmental levels to calculate an estimate of the emissions from the stack. They did, however, use the 20 kg estimate for uranium oxide release as the basis of their inhalation dose estimates in the addendum."

E That is contrary, that is in conflict, is it not, that passage, with your assertion that the environmental monitoring conducted by Dr. Stather is wholly independent of discharge figures that you produce, or you predict?

F A. No, it is not inconsistent at all. The change in interpretation simply relates to the fact that - or it is assumed that, should I say, in the assessment in the addendum - that the uranium oxide was all released in the year 1954. Having made that assumption, the exposure to people from that release is constrained by the milk monitoring data in 1958. The difference in the interpretation is that, prior to assuming that there had been a large release of uranium oxide, the assumption had been made that the levels found in 1958 were not due to a particular release or a particular incident, but were projected back on a level basis, essentially, in time. G It is still the case that once one takes the uranium oxide release into account, the magnitude of that release cannot significantly affect the dose delivered through milk because, whatever the magnitude of the release is, the levels still have to decay to reach the 1958 levels and subsequently and, therefore, it is not in conflict with my statement. H

A Q. Is it then your case - I put it to you quite bluntly so that it can be yes or no - is it your case that Dr. Stather's monitoring exercise is so independent of your figures that, however inaccurate the figures you give him, if you did give him them, it would not matter because his exercise does not require them and would not be affected by them?

B A. First of all, let me just qualify what you mean by Dr. Stather's monitoring exercise because, of course, Dr. Stather did not obtain the milk data. The milk data are a matter of record, if you like, from monitoring that was carried out in 1958 and subsequent years, but it certainly is the case that the way that Dr. Stather has done the assessment - and I am sure he will explain it in much greater detail - the levels of exposure that arise through milk are determined, first of all, by the measured levels in 1958 and, secondly, by the assumption that a release of magnitude occurred in 1954. As you have indicated in the extract from COMARE, the magnitude of the release only comes into Dr. Stather's calculation of the inhalation dose, which, for this particular type of release, is a very small component of the total, and so it is correct to say that, once Dr. Stather has embarked upon the methodology he has used, as I understand it, his doses will be very largely independent of the quantity assumed to be released because he has based them simply on the assumption that a release did occur at a particular time and that the measured levels in 1958 were as they were.

D Q. So if you had never set up SEAM and if you had relied - or BNFL in this case had relied - on the Howorth and Eggleton release figures, with the old SEF of 4, it would not have mattered because the same result would have been achieved by Dr. Stather?

E A. No, you transfer now from uranium oxide.....

F Q. Oh no, I am dealing with all figures. I am sorry if you thought I was just asking you about uranium oxide. I am putting it generally. 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?

G 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. 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.

H Q. I have to apologise to you. It may be you were not as clear as you might have been because certainly everybody on this side thought that is what you were saying, but we

now have it clear. There is a clear relationship, an interlink, between the figures you have given to Dr. Stather and his own exercise and, if your figures for plutonium are wrong, then those errors will affect his exercise as well?

- A. Yes, there are some radionuclides where that will not be the case, where Dr. Stather has derived his measurements, his dose estimates, say, by reference to measurements for milk, but specifically in the case of the plutonium release as a particular example, if I am wrong about the discharge, then Dr. Stather will also be wrong about the dose.

MR. HYTNER: My Lord, I half withdraw the apology because what I was recalling was Mr. Rokison's opening:

- Q. The next thing I need to ask you about your exercise on SEAM, which again I may have misunderstood from your evidence in chief and I want to clarify it, is the effect of discovery on your computer program. Were you aware of the discovery programme and the exercise in producing documents and so forth?

- A. Yes, of course, I was.

- Q. So you will recall that there was an order for discovery in the spring and that in 1991, sometime on, before September, something occurred which caused the Plaintiffs' solicitors to swear an affidavit on 23rd September, called the eighth affidavit, in which Mr. Day expressed his suspicions that the Howorth and Eggleton figures were wrong. Was that brought to your attention?

- A. I cannot say. It is certainly not in my present recollection.

- Q. Did you know then that something had happened in September which resulted in a massive exercise by BNFL, which took some six months to produce documents relating to plutonium releases from B204 before 1964?

- A. I was certainly aware that there had been a large discovery exercise. My recollection at that time was that the queries from Leigh Day and Co., the Plaintiffs' solicitors, were, in large part, related to the emissions of uranium oxide, but did include the questioning of other matters such as plutonium, yes.

- Q. And requests were being made for documents relating to pre-1964 releases from B204?

- A. That may well be the case, yes.

- Q. What I would like to know, if you can help us, is this: we are in September 1991, some 18 months or so after writs, a long time after Howorth and Eggleton. Does it surprise you or dismay you that it took two solicitors with A levels in physics, which I agree to me is an awesome qualification but to you probably is not, to become suspicious from looking at documents that Howorth and Eggleton's figures were quite wrong and that there was something very odd about the existing plutonium release figures of BNFL?

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A. I have no knowledge, of course, as to what the thinking was of the Plaintiffs' solicitors or experts at that time. I have no idea. I was clear in my own mind that the pre-1964 discharge matters needed to be thought about simply because in R171 addendum they were based on an extrapolation with no reference back to measurement, and that is not a situation about which one can be entirely confident. So I, quite independently of anything that the Plaintiffs' solicitors might or might not have been thinking, was very interested in the question of what the situation regarding pre-1964 discharges and measurements of those might have been.

Q. When did you become suspicious that you were embarking on an exercise which would result in a massive uplift of the historic discharge figures?

A. I think, as far as I was concerned, the key event was the location of the detailed log books with the records of the B204 stack sampling in them, together with other associated documents, which gave one a degree of confidence - gave me a degree of confidence - that those figures, while they might not be, if you like, totally relied on as well as one might rely on more modern measurements, that they should not be dismissed.

Q. You see, those were the precise parts of the documents which six months later turned up in the last stage of discovery. Are you suggesting that even those documents were irrelevant to your computer prediction?

A. The documents relating to stack discharges are, of course, very relevant to what one puts in as the discharge chronology on which to base your calculations. They are quite irrelevant to the actual setting up of the program until one comes to use it and embark upon the validation exercise.

Q. Again it is very important that, because of perhaps my lack of understanding of scientific matters, I do not misunderstand your answers. I thought you had said in chief that your computer exercise had been unaffected by the documents disclosed on discovery?

A. I said there were no major new issues that came out of discovery. I clearly had in mind before I started that the pre-1964 discharges were not tied back to measurements, that they were based on extrapolation, and that it would be desirable to get some measurement data. So it was not a surprise to me that measurement data turned up because one would expect it to be there if one looked hard enough. I was not surprised when the measurement data showed results different from the extrapolation. I was, I confess, somewhat surprised by the magnitude of the difference, but it was a sequence of events that one could fairly readily have foreseen at the outset. If one could not foresee the magnitude, one could at least foresee the sequence.

Q. You see, this is all part of the general point that I am putting about BNFL. Unless this is contradicted, and it

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may not be be, discovery was going to be completed and was contemplated by BNFL as being complete after the fifth list. The B204 documents had not arrived. The B204 documents arrived as a result of an order obtained after what I call the eighth affidavit sworn by Mr. Day, saying that something was suspicious, something was odd about the B204 uranium oxide figures. If that affidavit had not been sworn and the later order in October 1991 obtained, why would those B204 documents ever have reached you?

A. Because I may well still have asked for such documents to be located for my own purposes and I certainly was interested in securing the stack discharge record as best I could.

Q. But this was October 1991. The documents had not turned up then, and turned up as a result of a massive, six month exercise subsequent to an order of the Court. Had you not asked for those documents before March 1992?

A. I cannot recall when I first specifically asked for those. I would simply observe that it was in the autumn of 1991 when I personally started working closely on this, the SEAM model having by then been largely constructed as a mechanism and when we were moving through to the validation phase. That is when, in any case, I would have wished to start serious work on establishing the proper discharge chronology.

Q. Can we be realistic and practical about this? Your draft report is dated 31st May, although it was not signed until, I think, June. Those instructing me received the documents, the B204 documents, the last of them, the bulk of them, in March 1992. It is inconceivable, is it not, that you first saw them after March 1992 if your draft report was dated 31st May?

A. Yes, I certainly saw them, if not the complete documents and the complete determination, at some time between September 1991 and March 1992. I cannot be positive when.

Q. As I understand it, your answer is well, if the Plaintiffs had not managed to procure them by the eighth affidavit, at some stage you think you would have demanded them?

A. I was certainly keenly aware that we needed to trace as much information as possible about the early stack monitoring because I was aware that that was a gap in the information that was available to me. I did not do it in the expectation that I would find a huge increase.

Q. What made you think that such documents existed because plainly those in charge of discovery - and there is absolutely no suggestion of any bad faith here - those in charge of discovery on your side certainly did not suspect that any such documents existed, otherwise they would have turned them up before Mr. Day's affidavit?

A. I find it hard to put myself in the minds of those who you describe as being in charge. I was certainly not

closely involved in the preparation of the various affidavits and counter-affidavits which were produced at that time. Certainly it was in my mind that some information existed, even if it was only fragmentary, because that had been looked at a number of years before and, at that stage, people had concluded that the measurements were probably unreliable. I looked at it afresh with the benefit of having the full log book and various other associated documents and took a different view.

Q. I am not going to ask about the details of discovery because, anyway, it is nothing to do with you and it is all comment. Let us go on to what actually happened. Whilst you were performing your exercise, there was another exercise going on in relation to the authorisation applications for 1992?

A. Indeed.

Q. Performed by Dr. Dickinson?

A. Yes, and others, but Dr. Dickinson primarily.

Q. We have got his name because he has given us a statement. Was there any cross-fertilisation between your two exercises?

A. Yes, to some extent. Not personally with Dr. Dickinson, but in, it must have been the December of 1991, I held a seminar, at which I described to various people involved in environmental work on the site where I was getting to with the assessment I was doing, and certainly at that seminar the subject of SEFs and how I treated it and the values I had used came up. So there was cross-fertilisation to that extent.

Q. Dr. Dickinson knew you had the SEAM model?

A. Yes.

Q. And that you were concerned about SEFs. He would not have known, because you yourself did not know at that time, that there was likely to be a massive uplift in the plutonium discharge figures?

A. That is right. Exactly where I was in that regard in December 1991, I am not quite sure. I think I might well have had it in mind at that time, or I might well at that time have had some early stack monitoring data, but certainly not a complete set, but, in any case, while I am sure Dr. Dickinson would be interested that the historic discharges had been massively increased, it would not be anything that had a particular relationship to the application he was making, which was concerned with discharges in the more recent past and the projected discharges over the next three years.

Q. Except, of course, if he was going to put in a document on what the historic discharges were, it would be natural, would it not, to put in the accurate figures as opposed to what were known to be inaccurate figures?

A. It entirely depends what he wanted to use the information for.

Q. Can you imagine anybody, for any purpose, other than carelessness, putting in inaccurate figures in any document, any public document?

A. First of all, can I say the accuracy or inaccuracy of the figures is something in the way of a matter of debate because what I have chosen to do in retrospect in the SEFs is my judgment and that is all it is. I believe it is the best thing to do for the purposes of this assessment, but it may or may not be right and that is something which, after we have got through the exercise for this litigation, I think the company has to think about, but the decision to retrospect was only taken quite late. It is in my proof. It is my opinion for the purposes of this litigation. To a certain extent, I had almost arrived at that view independent of BNFL and, although I am part of BNFL, BNFL has in due course got to think what it wants to do with that information.

Secondly, the discharges as measured do have some status, particularly those discharges which were measured in terms of the compliance with the authorisation that was issued in 1988, because the methods of measurement are a matter which are agreed, or set out quite specifically, in documents issued by the authorising departments and, purely on the basis of compliance with the set limits, those are the methods and those are the values that are to be compared against the limits. There is a certain formality is what I am saying in the method of measurement which is agreed between HMIP and BNFL and, for that reason, whether or not you now think that the discharges from 1988, say, to 1992 were larger or smaller than those that were recorded and reported under the agreed methods of measurement that were then set out by HMIP, those numbers do have some status as the values which would be rightly compared with the limits set at that time.

I am sorry that is a long answer. I do not want to prevaricate, but I simply want to make sure that it is not a simple matter of saying this new set of numbers that I have produced are suddenly and immediately obviously the right and correct values. They are a judgment that I have reached.

Q. The historic figures, as you have called them, that is not a matter for you anyway because you did not produce them in any document, but the rest of your answer would appear to be the Plaintiffs' case, Prof. Jones. Here you are, highly eminent. You do the best you can, but the figures you actually produce in the end may or may not be right?

A. That is correct, although I believe that they are right and I believe, if anything, they are conservative, but in the end it partly rests on my judgment and on the

judgment of those who I have discussed the matter with on the rightness or wrongness of applying these SEFs retrospectively.

A Q. We know what has happened. The authorisation application was made and the document was sent out in April, and there has been an amendment. There have been amended discharge figures for 1988 onwards and these, it is said by the Plaintiffs, as you know, do not correspond with your own discharge figures. First of all, can we find out if you agree with that, that the figures given for 1988 onwards do not accord with your own discharge figures?

B A. The discharges, as amended, are similar to my figures. They are not exactly the same and the reason for that is connected with the way I have applied the SEF values to the retrospection exercise, which is fairly clearly set out in my report, and we may refer to that, if you wish, because I think it would help us in the discussion.

C Q. We are going to have to refer to it because I have to tell you we need your help because the suggestion that the two sets of figures are the same are miles apart from the way in which we have calculated them. If I could hand in another small file? (Produced) Can we just look at document No. 1 just to see if you agree this is an accurate.....

D MR. JUSTICE FRENCH: Shall we call this P2?

MR. HYTNER: P2, yes, my Lord.

MR. JUSTICE FRENCH: Yes, and we are looking at the first document.

E Q. MR. HYTNER: Could you just look at that page 1 and tell us if you agree that it is an accurate pictographic, if that is the right adjective, representation of your own deposition figures, showing the peak over Seascale?
A. I do not recognise where the peak over Seascale is.

Q. Sorry, Sellafield?

F A. The peak is over Sellafield and I cannot say that it is exact in every detail, of course, certainly not simply by looking at it, but it is clearly about right, given that, from the centre of the large peak to the shoulder, where it comes down to close to the levels generally prevailing, is of the order of 2 or 3 km. I cannot really tell that just by looking at the scale because I am afraid I am not terribly good with converting grid references into kilometres unless I think about it quite hard, but that looks about right. Each of these divisions on the axis is about a kilometre, is it not? It certainly cannot be very much more than that.

G Q. I suspect, though I hope it will not be true, that you may be in the witness box overnight, but I am sure you

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can be here tomorrow and, if you have a look at this overnight and find it to be inaccurate, perhaps you could come back and tell us tomorrow?

- A. Yes, I am a little puzzled at the moment as to what it has to do with the authorisation submission, but no doubt you will make that clear.

MR. JUSTICE FRENCH: To the Tolkein aficionado, it looks rather like the Tower of Mordor!

THE WITNESS: The Dark Tower, my Lord, quite right.

Q. MR. HYTNER: Would you look at document number 2? Again, it may be that you have not got the figures completely in your head but this is an attempt by means of a bar chart, as I understand they are called, to illustrate the difference in magnitude between the plutonium emissions as reported to NRPB in 1986, that is the little black marks at the bottom, with the actual emissions which are the open bars. Does that look again to you to be about right?

- A. It certainly makes the point that there has been a very large factor of increase which I clearly agree with, and that that factor is particularly large for years prior to 1964.

Q. If you turn to page 3, this is where I am afraid we are - and I say because it is not just my own denseness but it is everybody on this side - where we simply are puzzled by the suggestion that your figures and the authorisation figures are the same.

- A. First of all, as I recall the correspondence I do not think you have been told that any of the particular quantities which you seek to compare here are the same. You have been told that the numbers in the original application are very, very similar indeed to the figures in Table 5D-1 of my report, which are the numbers that I set out as my starting point, that is the numbers in R171 Addendum with basically a sampling efficiency factor of 4 applied to the high stack emissions. Sorry, I have confused myself there. Table 5D

Q. Prof. Jones, I am more than fully aware that even somebody who is better at arithmetic than I am may find this difficult. What I want to do is not catch you out. We may be making a mistake or you may be making a mistake, and it is important to find out. Would you like to look at these figures together with your files in the comfort of a room rather than in the witness box?

- A. I think we can take it a little further. Obviously I am not necessarily going to come to a positive decision on all the specific numbers that you set down but let me first of all give you the general explanation. The figures in the original application were based on the sampling efficiency factors that were applied as agreed with HMIP from 1988

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Q. May I cut you short for a moment? It will help. This is not an exercise in trying to have a go at BNFL for mistakes, or they may not have been mistakes, in an original application. Can we just deal with the amended application?

A. Please let me make the point first that the original application was based on the sampling efficiency factors which were, and were actually still at that time, the agreed sampling efficiency factors set out by HMIP under the terms of the 1988 authorisation. Table 5D-1 of my report contained the original discharge data up to 1982 as NRPB R171 Addendum, together with the discharge information post-1982, as published by BNFL. In other words that was the base line, and if you were to have done the comparison between the original application and my Table 5D-1 you would have found what was essentially an exact match of the columns. Therefore, there is consistency in the base information from which we are starting.

In the amended application the revised sampling efficiency factors subsequently agreed with HMIP, and I believe agreed after April, were taken into account. However, they do not necessarily compare directly with the results that you would expect on the basis of the way I have done it and to understand that I have to explain how I have done it and why I have done it the way I have done it, and if that is acceptable to you I would propose to now do that and we will come to the question of whether I need to consider the numbers in detail in a moment.

If I can just have a moment to find the correct reference in my report, I think it starts in Chapter 5, page 11, going on to Chapter 5, page 12, and it is really the section at Chapter 5 page 12 that is the important part, I think. First of all, as I have already mentioned, sampling efficiency factors, as experimentally determined, are specific to a particular outlet and a particular sampling point, and also to a specific radionuclide or group of radionuclides, so there are a large number of sampling efficiency factors which could be correctly applied to individual outlets on the Sellafield site and to individual radionuclides, and that really is the basis of the figures that are used to demonstrate compliance with the authorisation, and that is the basis of the figures that are in the amended authorisation application.

However, I describe in Chapter 5, page 12, what I have done to this data for the purposes of retrospection and really the key point of philosophy here is that I do not believe it is justified, because of the uncertainties, to apply sampling efficiency factors on an outlet by outlet, radionuclide by radionuclide basis, back throughout the past 30 or 40 years. Indeed, in many cases the particular outlets for which efficiency factors

today might have been determined might not be the relevant outlets then and the sampling systems may have changed. So what I have done is to apply factors as described in the third paragraph of Chapter 5, page 12, which are based on the latest information and which take account of what the major source of discharge was or might have been historically, so that for alpha emitting radionuclides emitted from high stacks I have taken a factor of 8, which is the factor now considered applicable to discharge of alpha emitting radionuclides from B204, which was of course the major outlet. Similarly, I have taken an SEF of 4 as the basis of the calculation of the discharges of caesium and strontium from high stack outlets, and further a factor of 3 for iodine from those outlets.

I have further assumed, again to simplify matters as much as possible, that since 1972 essentially all of the caesium and strontium emission has been from a low effective height not from a high stack, because most of it was from the low effective height, and that the SEF to be applied to that was the one which was experimentally determined for building B38 when it was operational, and that is a factor of 2.

These are much simplified assumptions. They do not take into account in exact detail all of the different sources of radionuclide emission from all of the particular outlets. They are based generally speaking on the higher end of the range of SEFs that were measured, but set against that, in regard to the factor of 8 for example, it is quite reasonable to apply that retrospectively to all alpha emissions from high stacks simply because in the past most of those did come, as you have observed, from building B204.

For consistency, when I have put my final set of discharges in, in Table 5D-3, I have applied those same assumptions through consistently. I have not suddenly in 1988 switched onto a radionuclide by radionuclide, outlet by outlet basis. So on that basis one would not expect exact agreement between the numbers I have in my report and the numbers from 1988 to 1990 that are in the authorisation application, and generally speaking if my statement about the thing being a bit conservative is correct, I should find slightly higher discharges for the period 1988-90 than are in the authorisation submission, because of course we are not now in the situation where essentially all of the alpha is discharged from building B204 stack, a much lower proportion of it now is, and certainly in relation to the first figure that is really exactly what you find, that the figures in the amended application are somewhat but not drastically lower than the figures in my report, and that is precisely what you would expect from the way I have carried out the exercise.

Q. The answer then to my question is yes, there is a discrepancy between the figures and you have now given an explanation for the discrepancy?

A. That is correct.

Q. Can we deal with the discrepancy because that is what is puzzling us? We were told by letter, and indeed I have indicated this to my Lord in open Court, that the discrepancy was something between 20% and 25%. In other words, your figures were 20-25% higher than those in the authorisation application. We make the discrepancy something like 70% on average and that is what I was saying we were puzzled by and on which I required your assistance. Could you tell us now what the discrepancy is between your figures for those three years, 1988, 1989, 1990, and the figures in the amended authorisation?

A. It depends on which stack you wish to look at, of course.

Q. The total plutonium emissions.

A. I have not looked at the figure for the total plutonium emissions. I can tell you what it is nuclide by nuclide for the high stacks and the low stacks.

Q. I am concerned with plutonium. Would you like sometime to look at it?

A. My notes say that for high stacks my figures for plutonium in 1988, '89 and '90 are respectively 1.7, 1.2 and 1.3 of the figures in the authorisation submission, and the figures for low stacks are 0.99, 1.01 and 1.03 of the figures in the revised authorisation submission. I know that does not seem like an average of 70% so

Q. To help me, because I am not very good at translating one arithmetical concept to another, what does that come to in percentages?

A. In percentages relative to what is in the authorisation submission plutonium alpha from high stacks, my figures are for 1988 70% higher, for 1989 23% higher, for 1990 30% higher, and for low stacks my figures are respectively 1988 1% lower, 1989 1% higher, 1990 3% higher. Of course, if one wanted to have a global average one would have to take account of the relative quantities emitted from those two sources and that is not something which I have done.

Q. The bulk of the plutonium has come from B204, has it not?

A. In 1988 the bulk of the plutonium does seem to have been coming from B204. I would need to check that. In 1989, 1990, yes, that seems to be the case.

Q. Perhaps I ought to take you through the actual documents because the 70% certainly sounds reminiscent; the 23% and 30% do not.

MR. JUSTICE FRENCH: Where are we going to now, what document?

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A MR. HYTNER: I will be asking him at the end of all this, and it may be that I can ask him now - it may or may not help - what his actual figures are for discharges, based not on conservative or other estimates but what is his best estimate and which is correct, the authorisation figure or his figure because we want to know what the figures are.

B THE WITNESS: I think I can answer that question in a very straightforward way, that if you wish to know specifically the values for discharge in 1988, '89 and '90, then with two particular exceptions the correct figures are those in the authorisation submission, or the best estimate of them, simply because those have had the sampling efficiency factors applied in an exact manner on a stack by stack, nuclide by nuclide basis. As I have indicated I do not think that is a particularly sensible approach to take in retrospection, because of the other uncertainties, so I have used a simpler method which I think is, if you like, robust and perhaps a little bit cautious, in the manner I defined at the beginning, and for consistency I have applied that in my report right through the piece, I have not made any distinction between post-1988 and pre-1988, therefore post-1988 on the whole I record somewhat higher discharges than those which are in the authorisation submission.

- D Q. MR. HYTNER: Prof. Jones, we may not have to go to specific documents; I hope not. What I want to know is this. We have been given in your report figures for plutonium discharges from the high stacks and the low stacks at Sellafield. We have been given figures for depositions within 20 kilometres and 5 kilometres. Would you now tell us, not applying your conservative and cautious approach which for some reason you have done, but using the approach adopted in the authorisation application which you say is more accurate, what plutonium deposition from the high stacks and the low stacks are now in the 20 kilometre and 5 kilometre radii from Sellafield?
- E A. Of course, I cannot do that now for the reason that I have not attempted to do those calculations and because I do not have all of the data readily available to apply the methodology used by way of SEF correction for the authorisation submission to make such a calculation for the discharges. Indeed, it may be that such a correction is impossible in principle simply because the sampling systems in place, let us say prior to 1988 or prior to 1990 or whatever date you wish to choose, would not be the same as those which have been evaluated to arrive at the figures for 1988-1990, so I have had to make a judgment. I have to say in my best judgment it would not be very different simply because I have applied the current factor of 8 retrospectively to the B204 stack emissions, and those are the emissions which overwhelmingly dominate the 3,000-odd GBq total emission that you refer to. So I could not come to a figure very different from that on any basis.
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Similarly, the low stack emissions in total are dominated by the additional emissions which I derive from the environmental data so I really cannot see (a) that it would be readily possible to do the exercise, or (b) that it would be on the face of it reasonable to expect that you would get a very different answer.

Q. But you have said that the application of an SEF of 8 is likely to be on the high side. Indeed you have taken it because you think it is on the high side?

A. Yes.

Q. Is that correct?

A. That is correct, although as I say it is the experimentally determined one for B204 as of now, and since that was the major source of emission maybe it is not that much on the high side when B204 is the sole source of emission.

Q. Is a factor of 8 the factor that has been taken for the authorisation application?

A. For the amended application yes, I believe it is.

Q. So that you know where we are going, because I do not want you to think this is simply an exercise in teasing you because it is not, we know, do we not, from measurements on the ground, what plutonium there is within a 20 kilometre radius and a 5 kilometre radius of Sellafield?

A. Yes, I think I have only seen information put forward in the Plaintiffs' reports on the basis of what is within 10 kilometres and what is within 20 kilometres, if I recall the reports correctly.

Q. What about your own reports?

A. In my own reports, in my first report I did not rely on any estimate at all of the integrated deposition on the ground. I relied on matching the calculated deposition point by point with the measured deposition, which is a slightly different exercise but which does not result in working out an integrated quantity.

Q. You see, it is important for this reason, is it not: we know or if we know what measurement of plutonium there is upon the ground and if we know what plutonium has been discharged from the stacks at Sellafield, if there is a discrepancy, the plutonium has come from somewhere else?

A. In the way that you have described it doing the integration out to 20 kilometres, that is not a very precise way of doing things, for the simple reason that for the high stack emissions only a small proportion of that which has been discharged is likely to be deposited within 20 kilometres. Therefore your estimates of the total inventory or budget out to that distance will be very dependent upon the deposition behaviour that you assume, which is another element of the calculation which is quite uncertain, and that is one reason why I would

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not place too much emphasis on the exercise of integrating the total plutonium deposit out to 20 kilometres.

A Q. Either I have got an appalling memory or we are at cross purposes. Was it not you in your report who worked out that within a five kilometre radius of Sellafield there was a quantity of plutonium which, deducted from the amount that came from the high stack or deducting the high stack from the total, left 94 GBq of which, if you took 20 as uranium oxide, left 74 GBq to be accounted for?

B A. No, that is not correct. That is not how I arrived at the figure of 74 GBq. To the extent that I have done that sort of calculation at all, I did it in my fourth report, which I understand is not presently in evidence.

Q. Never mind whether it is in evidence or not. How do you do it?

C A. If I could explain the 74 GBq first, because that is a figure that you have fastened on, that is a quantity which I derived by the first exercise that I mentioned, that is seeing what sort of release, given certain assumptions about deposition behaviour, would be necessary to match the deposition on a point by point basis. I did not make an integration out to five kilometres and I did not subtract one number from another, and I did not come up with a figure of 74 GBq. I have made some estimates of that sort in my fourth report in response to what Dr. Day put in his third report. If we are to start discussing that, I would really need to refer to that report. It is not a straight forward exercise.

D Q. Yes, because I am going to ask you how you worked his figures out.

E A. In that case, may I please have a copy of my fourth report?

MR. JUSTICE FRENCH: Would you mind, Mr. Hytner, waiting until a copy of this witness's fourth report is brought?

F Q. MR. HYTNER: Have you now got the fourth report before you?

A. I have the fourth report before me, yes.

Q. Could you look first of all at your first report and the figure 10C7? It is in Annex 10, I am told.

G Q. MR. JUSTICE FRENCH: Whereabouts in Chapter 10 is 10C7?

MR. HYTNER: Annex 10. In mine, it is some way past the blue divider.

H Q. Have you got it?

A. I have, Mr. Hytner.

Q. Could you tell us what that top chart is? "Calculated plutonium alpha deposition - additional discharges", and then 7.37 GBq, I think it will be?

A. No, it is actually 73.7 GBq. Figure 10C7 shows the total calculated deposition from all the sources I have considered, which means to say the high stack emissions, the low stack emissions, including estimates of re-suspension from the Magnox ponds, including an emission of 20 kilogrammes of uranium oxide as modelled by C, and including the effects of sea to land transfer and weapons testing for that, together with an additional release of the 73.7 to the 10 GBq, which I say effectively accounts for the emissions from a number of other acknowledged sources of plutonium emission which were reported in R171 addendum and for which the quantity was assessed, but not in my opinion very reliably, so that I have used all of this new deposition data, because there is much here that was not there when R171 addendum was produced, to arrive at a better estimate for the magnitude of those sources. So the curve there is all the calculated plutonium deposition for all of the sources which I have considered, and the graph simply shows that it matches the measured deposition really quite well. I should add that in subsequent exchange of correspondence with Leigh Day & Co., the magnitude of that additional source was amended from 74 GBq to 66 GBq because when I did the assessment shown in Figure 10C7 I did not at that time have all the final numbers for the B204 stack emission in the calculation. This was at a relatively early stage of the validation exercise in the preparation of the report. So long as you do not lay too much emphasis on the difference between 66 and 74, Figure 10C7 is a good representation of what the calculated and observed deposition looked like.

Q. I am not sure that it is very far from what I was putting to you, unless I am still misunderstanding. There is within a distance of five kilometres or less than five kilometres from the pile ponds, a deposition of plutonium, which you now say is 66 GBq, which did not come from the high and low stacks?

A. The thing I query and correct you on is the statement that there is that quantity of material necessarily within five kilometres of the site, because you cannot say that from the way I have worked out Figure 10C7. I have not integrated the plutonium deposition out to five kilometres of the site, or to any other distance, in order to arrive at that figure. I have simply adjusted the magnitude of the additional quantity emitted, the 74 GBq, until the calculated deposition curve matches as best it can the observed deposition measurements. I do not wish to be too pedantic about it, but it is a different method of calculation which will not necessarily produce the same result as integrating the total deposit. There are two reasons why it may not. First of all, it may be that not all of that 74 GBq is actually deposited within ten kilometres or five

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kilometres; some of it may be deposited further away. Secondly, a rather more obscure point but one nonetheless which is relevant: the way that the exercise of fitting the curve to the data points is done may well produce different results, depending which way you do the calculation. That is why I am being pedantic in saying that that figure by itself does not establish that there is an excess of 74 GBq of plutonium, or any other figure, within five kilometres. It was not obtained with the intent of making such an estimate.

Q. I am still not sure what you are saying about the 74, now 66. Let us keep to the 66 as it is your present figure of GBq. If it was not within five kilometres, where was it?

A. Perhaps if I may refer to my fourth report, to first of all Figure 2 of that report, Figure 2 shows what I have described as the excess deposition of plutonium as a function of distance from the site, the excess being the amount of measured deposition less the calculated level of deposition from high stack releases, low stack releases including the Magnox ponds, sea to land transfer, weapons testing fall-out, but excluding the effects of the additional plutonium releases I have described and excluding the effects of uranium oxide deposition; so that is a representation of the plutonium deposition that you are seeking to account for by those two sources within any particular distance of the site. In Figure 3, I show the results that you get by trying to fit an arbitrary curve through that set of data points and then effectively to integrate the area under the curve, although there is a little bit more mathematical manipulation to it than that. That treatment shows an integrated deposit of 70 GBq in total within five kilometres from both the uranium oxide and the additional plutonium releases. I note in the report that that is actually rather an upper bound figure, in that depending on different ways you choose to do the curve-fitting exercise, you can plausibly get values as low as 30 to 50 GBq within that distance; and indeed the figure equates reasonably well with the figures that Dr. Day quoted in his third report, where he quoted 30 to 70 GBq in total within ten kilometres.

Q. MR. JUSTICE FRENCH: Shall we go back to the start of that answer? Figure 3 of Jones 4, if I can call it that ---

A. That's correct, my Lord.

Q. --- shows deposition of plutonium alpha?

A. Yes, that is correct, my Lord.

Q. I think you added also plutonium oxide?

A. No.

Q. Sorry, uranium oxide?

A. It is strictly the integrated or total deposition in GBq of plutonium, the alpha emitting radioisotopes of

plutonium, which is in excess of that which you would calculate from the recorded stack emissions and other sources in my report.

A Q. Wait a minute. Can we get the uranium oxide out of the way? You either said it or you did not, you either meant to or you did not?

B A. The point I was trying to make, my Lord, was that the two sources of emission which would explain that total are the emissions of uranium oxide and the additional emissions of plutonium from effectively unmonitored low level sources on the Sellafield site, in the areas of operation.

Q. We will start again. Figure 3 of Jones 4 shows the excess of ---

C A. The excess of plutonium alpha deposition. It is the excess of plutonium alpha deposition as measured on the ground, over and above that which you calculate as resulting from site discharges, with the exclusion of the emission of uranium oxide and early unmonitored releases, I suppose you could say presumed to be from near ground level sources, that is sources of emission which are from near to ground level, not from an elevated chimney.

Q. Is that completed?

D A. That is completed, my Lord. That is the estimate I have made based on the integration method. The reason why I was being pedantic was that my first set of figures in 10C7 were not based on integrating the deposit. These Figures 2 and 3 are. They do produce slightly different answers, and indeed the integration method seems to produce or imply a slightly lower value for the total excess plutonium deposit within five kilometres.

E Q. MR. HYTNER: I am not actually concerned about the pedantic differences. I want to know for the purposes of this case the material differences. Is it right that based on your conservative approach to SEFs and your generally conservative approach to stack releases, you have reckoned that within five kilometres or thereabouts there are 66 GBq of plutonium unaccounted for in the sense that they did not emerge from the stacks?

F A. Yes, that is correct.

Q. Does that include your 20 kgs. of uranium oxide?

A. The figure of 66 GBq does not include the 20 kgs. of uranium oxide.

G Q. So on that basis we have 66 GBq to be accounted for. If the correct approach is taken, that is the approach taken by Dr. Dickinson in his application for authorisation, it follows that the amount of deposition from the high stacks and low stacks must be reduced and the unaccounted for excess must increase?

H A. That is correct. I would qualify that by saying but not by very much.

Q. By the percentage difference between your figures and Dr. Dickinson's figures?

A. No, that is incorrect.

Q. Would you tell us by how much you say the figures should be increased if Dr. Dickinson's approach, which you have accepted as correct, is taken?

A. Just going back to the two assessments I have done before, the first assessment, which included an incomplete set of emissions from building B204, produced a figure of 74 GBq. The second estimate, which included actually quite substantially higher discharges from B204 because it included some higher discharges in the earlier years which had not previously been included, only went down to 66 GBq. I would estimate, just by judging from that exercise, that applying Dr. Dickinson's methodology, starting at 66 GBq would probably not even get me back up to 74. The reason why that is so is that the high stack emissions and indeed the other low stack emissions contribute relatively little to the calculated deposition within the important 0 - 2 kilometres region. So altering the B204 stack discharge by a large amount would produce a very small effect on this figure of 66 or 74. It would not have a major effect. So it would not be pro rata with the change in the figures. Furthermore, as I have said, it is my judgment that because B204 dominated the plutonium discharges from the site and because I have applied the same SEF, that is 8, as Dr. Dickinson had done, using Dr. Dickinson's methodology would in any case not produce a very different total discharge from the site.

Q. I am sorry it has taken a long run up to the wicket to bowl the final ball. Professor Jones, now that you have accepted this in principle, I have to tell you that I cannot challenge your actual calculation on my feet. My Lord, unhappily, Professor Jones will just be still in the witness box overnight. May I return to this tomorrow when perhaps I have had some assistance with calculations? It may be that I will not challenge the actual calculation. I do not know yet.

MR. JUSTICE FRENCH: Yes.

Q. MR. HYTNER: Now I want to turn to another topic which is the Argon.

A. The Argon 41, yes.

Q. Could you look at page 284 in the white bundle?

Q. MR. JUSTICE FRENCH: Is this P1?

MR. HYTNER: My Lord, yes.

Q. This is the authorisation application?

A. Yes.

Q. If you look down table 6 down to the fourth nuclide, these are the figures that BNFL are now applying to discharge to atmosphere, do you follow?

A. I do follow.

Q. They are applying to discharge annually 3,700 TBq?

A. Yes.

Q. Of argon 41?

A. Yes.

Q. MR. JUSTICE FRENCH: Just remind me how big a TBq is?

A. One TBq, my Lord, is 1000 GBq.

Q. A big unit?

A. Yes.

Q. MR. HYTNER: I am not sure whether you are prepared to take this from me at the moment or whether you want to look at a document to which I have not at the moment got the reference. In the R171 addendum, it was stated that the release per annum during the 1950s was 500,000 TBq of Argon 41. Do you recall that or not?

A. I am prepared to accept the figure. It may possibly shorten the discussion - because I can see where you are going - if I point out that this quantity of Argon 41 that you are discussing here in the application is a discharge at effectively ground level from the Calder reactors, and the doses that result to the critical group are calculated on that basis. The Argon 41 that was discharged from the Windscale piles was discharged up a high stack, and indeed there was probably considerable thermal rise of the plume, meaning that the height of the release was very much larger than is the case for the release from the Calder reactors. Therefore, you cannot simply apply the dose calculated here for the release from the Calder reactors and take it pro rata to the quantity emitted from the Windscale piles, because the circumstances of the emission are quite different. The Windscale piles per TBq released would result in a much smaller dose to individuals than would releases from the Calder reactors. Furthermore, the doses in table 6 are to the critical group, that is people who live very close to the site, whereas the doses in R171 and addendum are to people two or three kilometres away at Seascale. So if the argument that you wish to make is to say that a certain release of Argon pro rata higher from the piles would result in a dose which is higher pro rata, it would not be correct.

Q. Once again you have anticipated the next question, Professor Jones. Whatever credit has to be given for your two points, they could not account, could they, for a ninefold increase in the dose pro rata?

A. Very, very easily.

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Q. You think they would?
A. Very easily.

Q. That is your explanation?
A. That is my explanation.

Q. But you accept the actual figure, which is that if you take the pro rata dose, it would result in nine times the dose which was given for the 1950s as has been suggested by Dr. Stather?

A. It probably would, but I would not be at all surprised, and I would not think that cast any doubt whatever on Dr. Stather's calculated dose.

MR. HYTNER: My Lord, I am going on to another point. I am not sure what time your Lordship is going on until.

MR. JUSTICE FRENCH: I think that when dealing with matters as difficult as this, 4.15 is probably a good time to break off.

MR. HYTNER: Yes, my Lord.

MR. JUSTICE FRENCH: 10.30 tomorrow.

(The Court was adjourned until
the following morning at 10.30 a.m.)

