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

Tuesday 1 December 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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SIXTEENTH DAY'S PROCEEDINGS

TUESDAY, 1st DECEMBER 1992

MR. JUSTICE FRENCH: Mr. Rokison, I understand that there have been some considerations given informally to the timetable.

MR. ROKISON: My Lord, there have. We meant, of course, no disrespect to your Lordship in not involving your Lordship at the first stage in those discussions. Discussions have taken place between my learned friend Mr. Langstaff and myself, and also informally with Prof. Evans.

My Lord, the position is, as your Lordship knows, that it was indicated that once we got stuck into the epidemiological evidence, in principle your Lordship was in agreement that it would be sensible to be in Court for four days a week in order to allow time for preparation for the next witness and so on. In the normal course of events, your Lordship indicated that the reading and preparation day would be Wednesday. I think it very unlikely that I shall finish cross-examining Prof. Evans today. I think there is a possibility that I will finish by tomorrow, although it is very difficult to estimate. My guess is that I will just about finish cross-examining on those matters that I feel able to pursue in cross-examination at the moment by close of play tomorrow.

That fits in well with my learned friends and with Prof. Evans, because he has indicated that he would find it extremely inconvenient to be here on Thursday. In addition, Dr. Scott Davis, who I understand was coming over last weekend, has now been put off and is coming over on Wednesday, and my learned friends would therefore welcome Thursday in order to discuss matters with Dr. Scott Davis.

My Lord, what we had tentatively agreed upon, subject to your Lordship's approval - and indeed I was going to mention it to your Lordship first thing this morning - was that if we could continue sitting today and tomorrow with Prof. Evans, if there is anything left over on what I might call the first tranche of the cross-examination of Prof. Evans, if that can then spill over into Friday, that we do not sit on Thursday, that Dr. Scott Davis is called on Friday morning, with the possibility of Prof. Evans, who has a commitment on Friday morning, simply completing the first part of his cross-examination on Friday afternoon.

MR. JUSTICE FRENCH: You had better explain to me the distinction between the first and second tranches of Prof. Evans' cross-examination.

DISCUSSION

MR. ROKISON: It is simply this: there are two main areas which I am not in a position to pursue fully in cross-examination at the moment and therefore would rather hold over. Those are the re-working of the Gardner study, and as your Lordship knows, I have already mentioned to your Lordship that until I have had an opportunity of discussing that fully with my epidemiologists and particularly Prof. Howe from Canada, that is a matter which I simply could not cross-examine on fully ---

MR. JUSTICE FRENCH: Yes, I follow what you are saying.

MR. ROKISON: The other aspect of it, which arises out of Prof. Evans' fourth report, is his comments on the Canadian study, which is also a matter that I want to discuss with Prof. Howe before I pursue cross-examination of Prof. Evans in relation to it. I think it is only those two matters. I am perfectly happy to ask him questions about the Louise Parker paper and also about Draper 92. It is really those two areas which I would like, if I may, to hold over.

MR. JUSTICE FRENCH: I understand what the areas are. The next question is, hold over till when?

MR. ROKISON: My Lord, I have indicated to my learned friends that I would have thought after the evidence of Dr. Scott Davis and Dr. Kopecky. I think that they are going to follow one another and they basically cover overlapping areas. They are epidemiological studies relating primarily to non-UK studies. What I was hoping was that by the time their evidence had ended, which I would anticipate would be somewhere towards the end of next week, I would then be in a position to be able to complete my cross-examination of Prof. Evans.

MR. JUSTICE FRENCH: We hope that Prof. Evans is able to fit into this timetable.

MR. ROKISON: Subject to your Lordship's overall approval, of course, we will endeavour to co-operate in order not to make it at a particularly inconvenient time.

MR. JUSTICE FRENCH: What about re-examination? Where does that fit in?

MR. ROKISON: I do not know whether my learned friend wishes to reserve his re-examination until the total cross-examination is completed or whether he would wish to re-examine on the cross-examination so far. That is a matter for him. We have not discussed that. He may like to indicate what his position is.

MR. JUSTICE FRENCH: Have you any thoughts, Mr. Langstaff?

DISCUSSION

MR. LANGSTAFF: I think there is no time like the present, my Lord, when matters are fresh in one's mind. Since the two areas that my learned friend indicates he will not presently cross-examine about are, as it were, discrete areas, it would seem to us more appropriate that I should cross-examine on the evidence which Prof. Evans has given.

I think there is a second reason for that as well, which is this: it was anticipated by Mr. Hytner in dealing with the split nature of the cross-examination that Prof. Evans would be available to those instructing us for advice and consultation, and plainly one would not want anything that might be said to affect the re-examination on those matters upon which he had already given evidence and been cross-examined.

MR. JUSTICE FRENCH: I must leave that in your hands. As I say, in a case like this the normal rules as to access to witnesses must be stretched, if not strained, and I must leave it to the discretion and good sense of those concerned to ensure that no adverse effects follow from those stretching of the rules.

MR. LANGSTAFF: My Lord, may I say that my learned friend and Mr. Hytner have been good enough to agree what might be called ground rules which both sides, I am confident, will adhere to.

MR. JUSTICE FRENCH: I am content entirely to leave that to Counsel.

MR. LANGSTAFF: I am obliged.

MR. JUSTICE FRENCH: I would only intervene if asked to do so.

MR. LANGSTAFF: May I mention two further matters which arise? First of all, a small correction. I have told Mr. Rokison - and the fault is entirely mine - that Dr. Scott Davis was expected on Wednesday. In fact, it is going to be tonight. It is right to say, however, that Thursday would be the most suitable day to see him because plainly he is likely to be suffering from jet lag and the effects of travel tomorrow.

MR. JUSTICE FRENCH: Yes.

MR. LANGSTAFF: My Lord, the second matter which causes us a little concern is this: I had not anticipated until Mr. Rokison addressed your Lordship about it that he would not be in a position to deal with the Canadian study, the McLaughlin study. My Lord, that gives us a little concern, for this reason, that Dr. Scott Davis himself having been in the New Americas, as Prof. Howe, will be expected by us to comment on the McLaughlin study, and indeed it is likely that something of what he will say will be put in writing and given to the Defendants before he is called.

It is one of those circumstances which arise, I think, because of the very late timing of the service of Prof. Howe's report. It was not until Guy Fawkes' Day, less than a month ago, that his report was first received, formally producing the study and commenting on it. Perhaps it is in the nature of the case that these slippages are inevitable, but it would be most regrettable if by the time Dr. Scott Davis had finished his examination-in-chief my learned friend were not then in a position at least to begin cross-examining him about the McLaughlin study. All I can do, not having discussed it with my learned friend, is to ask him through your Lordship to make every effort that he can to obtain instructions in time to avoid the recall of Dr. Scott Davis on some later occasion.

MR. ROKISON: I will obviously do that. As your Lordship knows, the reason why the Canadian study as an appendix to Prof. Howe's report was not put in earlier was simply that the study was not published earlier.

MR. JUSTICE FRENCH: Yes.

MR. ROKISON: Of course I have discussed the McLaughlin study to some extent with Prof. Howe. The problem is it is the specific points which have been raised - and I think there are only two - by Prof. Evans in relation to that study which I have not had an opportunity of discussing and being fully instructed by Prof. Howe. I anticipate ---

MR. JUSTICE FRENCH: It seems to me they are relatively narrow points.

MR. ROKISON: They are indeed, but I simply think it better, rather than to flounder even more than usual by trying to cross-examine on those points without full instructions, if I am going to be permitted to leave over cross-examination on the re-working of the Gardner study, it makes sense that as far as Prof. Evans is concerned I ask him whatever questions I am instructed are appropriate in relation to that aspect of the Canadian study at that stage.

I am well aware of the difficulty that my learned friend mentions, and I would anticipate that I would be adequately instructed by the end of this week, but obviously to the extent to which Dr. Scott Davis will have specific comments on the Canadian study, again it is no complaint, it works both ways - and I am sure my learned friend will appreciate this - that the sooner we have notice of those, the better. Of course, we are still waiting - and again this is no complaint - for Dr. Thomas's analysis of the re-working, which is another reason why it is not really appropriate to pursue that with Prof. Evans at this stage. My Lord, we will do our best to sort it out.

MR. JUSTICE FRENCH: I am sure you will. There has been a good measure of success so far. Perhaps I might give this by way of general indication as to how it appears to me at the moment: quite clearly, Gardner and its re-workings are, as everybody agrees, central to the case, but Canadian and other studies are not. While of course I shall give latitude to any problems that arise regarding Gardner and his re-working, it seems to me that the other studies really fall into what one might loosely term the Bradford/Hill area of the case, consistency and that sort of area. I trust it will not be thought necessary to examine those in anything like the detail with which it is necessary to examine the Gardner papers.

MR. ROKISON: May I jut reserve our position in relation to that? I would just say, with respect, that we are at an early stage in the epidemiological evidence and for our part we would, with respect, not accept your Lordship's preliminary analysis of suggesting that the Canadian study is not important.

MR. JUSTICE FRENCH: I am not saying it is not important.

MR. ROKISON: We will say - and your Lordship will see the way in which it will be developed - that it goes rather further than simply being relevant to the Bradford/Hill test of consistency, because it is one of the very few studies which have specifically examined the Gardner hypothesis, albeit in a different context.

MR. JUSTICE FRENCH: With different people and different doses presumably?

MR. ROKISON: I do not think, with respect, one should anticipate the extent to which there may be arguments about a linear dose response relationship and the extent to which it is legitimate to look at more than one study together. These are matters which will perhaps be in issue before your Lordship.

MR. JUSTICE FRENCH: Very well, I will wait and see about that.

MR. ROKISON: Thank you very much.

MR. JUSTICE FRENCH: There is one other matter. As has been most pertinently observed, matters are best dealt with and nettles best grasped while the facts are recently before the Court and fresh in one's mind. I would invite you to consider, both of you, whether it would be helpful to you, as it certainly would be helpful to me, if short preliminary submissions were made at the end of each witness by both sides.

MR. ROKISON: I think your Lordship did indicate that possibility to us when the matter was discussed with your Lordship at an early stage. My recollection was

that both sides were obviously prepared to do whatever we could to assist your Lordship in your Lordship's task in judging this case. When we discussed that, I do not think anybody contemplated that we would be able to produce a document summarising what we say are the crucial or relevant parts of a witness's evidence straight away. However, I think we do have in mind your Lordship's request that we would, at least possibly in summary form, simply draw your Lordship's attention to those aspects of a witness's evidence which we would regard as being of particular significance.

MR. JUSTICE FRENCH: It is the latter that I have in mind.

MR. ROKISON: No doubt that can be done, if I may say so, in due course within a few days of the witness completing his evidence.

MR. JUSTICE FRENCH: "In due course within a few days ..." is a very elastic concept.

MR. ROKISON: I think it must be, with respect, my Lord, because ---

MR. JUSTICE FRENCH: Steady on, Mr. Rokison. I am just wondering whether a reasonable length of time can be taken, because working to a timetable always concentrates minds, and I was going to canvass with you what might be a reasonable time. A week?

MR. ROKISON: I would think so, yes. We would obviously try to do it quicker than that, but I would have thought that a week would be a reasonable time.

MR. JUSTICE FRENCH: You see, if we have a defined interval, then when the defined interval has gone by one can start making rather more pointed enquiries.

MR. ROKISON: Yes. With respect, in principle it seems to me that that is the sort of period I had in mind. I would have thought that within about a week would be the right sort of period when it could be achieved. It must depend to some extent, I suppose, simply on the immediate burden of those involved in the case as to whether it is something which can be done within that time.

MR. JUSTICE FRENCH: I understand the problem very well. If we proceed on that basis and if the points can be produced within a week, let them be so, and if they are not produced after seven days have gone by, then I shall start making enquiries.

MR. ROKISON: My Lord, we will expect that.

MR. JUSTICE FRENCH: Does that course commend itself to you, Mr. Langstaff?

MR. LANGSTAFF: My Lord, it does indeed. A week seems to us to be the ideal period because it necessarily comprehends both the weekend and the day not in Court. It should be possible within that time-scale to produce something which will necessarily be in skeleton form.

MR. JUSTICE FRENCH: Indeed. It is easier to recollect what matters than to go hunting about for it in two or three months' time. What matters in the minds of the advocates a few days after the conclusion of a witness's evidence is much more likely to have a true bearing on the case than what one digs up after going through the transcripts and so on.

MR. LANGSTAFF: Very much so, my Lord.

MR. JUSTICE FRENCH: So skeleton certainly, not developed.

MR. LANGSTAFF: Your Lordship has not said so on this occasion, but I think your Lordship anticipates written submissions only, by what your Lordship is saying, unless your Lordship needs assistance having looked at both side's written submissions.

MR. JUSTICE FRENCH: I think that puts your finger exactly on it, Mr. Langstaff. I think in the ordinary circumstances I would expect written submissions, and only if there were something in them that I did not understand would I invite oral amplification. I am grateful for that.

Can I say one further thing before Mr. Rokison continues his cross-examination? I do not know whether the right person to help me on this is Prof. Evans or whether somebody else perhaps has done it already in some document that I have either not yet read or not yet given sufficient attention to. The point is this: that confidence limits or intervals, however one expresses them, is a concept which, in very broad and I fear rather woolly terms, I think I understand but I am not by any means sure. At some stage I would like to be told precisely what they are, precisely how they work, whether they are, as I believe they are, purely a mathematical product and have nothing to do with confidence as a subjective emotion, and matters of that sort.

MR. LANGSTAFF: My Lord, yes.

MR. JUSTICE FRENCH: I do not invite Prof. Evans to expound at this moment. I just ask that you both bear it in mind and see that I have a plain man's guide at some stage.

MR. LANGSTAFF: Indeed, my Lord.

MR. JUSTICE FRENCH: Thank you. Yes, Mr. Rokison?

MR. ROKISON: We can look it up. I think your Lordship might find the concept explained in layman's language in Dr. MacRae's first report, and it may be that we can point to a passage there which is not controversial which explains it.

MR. JUSTICE FRENCH: Or it may be you can say "MacRae as amplified by Howe", or whoever it may be.

MR. ROKISON: Yes, we will do that.

MR. JUSTICE FRENCH: Thank you.

MR. ROKISON: May I just make one reservation in relation to the point that your Lordship made about making submissions about the evidence of the witnesses? Although it may very well as a general rule be right that those matters which are emphasised shortly after the witness has given evidence may in most cases be those points which are the most important, it may of course emerge in a case such as this that the evidence may develop in a way where something that a witness said which may not have seemed terribly important at the time may become very important.

MR. JUSTICE FRENCH: Of course, Mr. Rokison, that is very clearly understood.

MR. ROKISON: And we are not obviously to be precluded from relying on matters which we do not emphasise to your Lordship at this stage.

MR. JUSTICE FRENCH: Certainly not. I think we can leave it there for the moment.

MR. ROKISON: Thank you.

MR. LANGSTAFF: My Lord, I am sorry to interrupt. I have been shown that passage of the opening which deals with confidence intervals, albeit in rather shorter terms than the terms used by Dr. MacRae in his very helpful examination of confidence intervals. In the opening between pages 156 and 158 there is our lawyers' understanding of that which we have distilled as the essence of confidence intervals.

MR. JUSTICE FRENCH: Yes, I am grateful. Yes, Mr. Rokison.

STEPHEN JAMES EVANS Recalled

Cross-Examined by MR. ROKISON (Continued):

Q. Prof. Evans, good morning.
A. Good morning.

Q. May I just pick up two matters which arose from your evidence yesterday, if I may? Do you have a copy of the transcript of yesterday's evidence?

A. I do.

A Q. There is just one small point that I wanted to pursue which I am afraid I had not picked up at the time when you gave a certain answer. Could you please turn to page 19, at the top?

MR. JUSTICE FRENCH: Sorry, would you pause for just a moment? This is the transcript of day 15?

B MR. ROKISON: Yes, page 19, my Lord, at A.

MR. JUSTICE FRENCH: Thank you.

Q. MR. ROKISON: I was asking you about the A-bomb data and the extent to which it demonstrated a very different dose response relationship from the Gardner hypothesis, you will recall?

C A. Yes.

Q. You said at the top of page 19, "In the A-bomb data the spectrum of radiation they received was entirely different", and I wondered where you had got that from. I do not think it is a point that is made by any of those who in a sense applied their minds specifically to the A-bomb data for the purposes of this case?

D A. I think that with those who apply the evidence to the A-bomb data, the different types of radiation, as I believe you will have dealt with in Court previously, have different biological effects.

Q. When you are talking about different types of radiation, are you talking about, for example, gamma radiation, low LET, as opposed to alpha high LET?

E A. I am talking about alpha, beta, gamma, and obviously things like neutrons.

Q. The position as far as the survivors of the A-bombs are concerned is that the dose which they received was substantially whole body gamma doses, and so far as the occupational exposure of Sellafield workers are concerned

F again it is primarily the whole body gamma dose which is taken into account and is the basis for the original Gardner study, is it not?

A. Yes.

Q. But you were not thinking of anything, perhaps if I might say so, a little more technical than that, of actually looking at the radiation spectrum other than in general

G terms?

A. Certainly the energy distribution of the gamma rays is likely to be different from an atomic bomb, the energy distribution itself of those. In addition, there are ---

Q. Just pausing there, is this a matter within your expertise upon which you are able to say to my Lord "You should make some allowance in relation to the application

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of the A-bomb data because the spectrum was different in such and such a respect and this would have a different effect"?

A. No.

Q. I mean this without any disrespect at all. Is it merely that from your general knowledge and from your discussion with others, you believe that the spectrum would have been different?

A. I would regard my knowledge as having been trained as a physicist and worked for the Atomic Energy Authority and worked at CERN as giving me a different view to general knowledge. However, I would have to agree that I am not an expert in this area. But there is no doubt that the exposure to alpha radiation would be entirely different in the atom bomb data. It could well be that with something that has much less biological effect in some respects, the beta radiation spectrum will be entirely different similarly.

Q. But you do not know?

A. We do know that in addition the lengths of time for which they are exposed, a few milliseconds probably or maybe a few seconds, in addition ---

Q. That, with respect, is a different point?

A. It is a different point, yes.

Q. That is a point which has been addressed by those who are specifically considering the A-bomb data as to the effect of the difference between acute radiation and chronic radiation?

A. I have to confess I would wish to try and make a very general point that the situation with those in Seascale was that they had not had an atom bomb dropped on them, fortunately, and that the effects that we generalise from dropping an atom bomb, which may be the best that we can do, I do not think to be the best way of generalising to the sort of exposure to a variety of different types of radiation that those who were in employment in Sellafield over a long time would have had, or indeed those living in Seascale.

Q. Would you agree that those who are involved nationally and internationally in considering risks to population and recommendations in relation to dose limits, and so on, at the very highest level take considerable account of the A-bomb data in reaching their conclusions and giving their advice?

A. Yes, they have no other data.

Q. Would you agree that those who are specialists in that field and are responsible for giving that advice and making recommendations will be likely to have studied the precise differences between the A-bomb experience and other exposures rather more closely than you have been able to do?

A. No doubt at all.

Q. I will leave that topic if I may. When we reached the end of yesterday afternoon's hearing, I think we had really completed asking you about that part of your evidence in which you had dealt generally with other studies in the United Kingdom of possible excesses round nuclear plants?
A. Yes.

Q. I think you had clarified in the course of your evidence yesterday morning - I do not know that it is necessary to refer to it; I can if you like - the reason why you had introduced that evidence, on the basis that although none of the studies that you were considering had considered the question of paternal pre-conception exposure to radiation, but nevertheless you felt that if the Gardner hypothesis was right, then it might be an explanation for excesses round other nuclear establishments?
A. Yes.

Q. You do not rely upon them, if I can put it the other way round, as assisting in answering the question "Is the Gardner hypothesis right?"?
A. I don't understand how that question differed from the previous one.

Q. Then let me make it clear. As I understand it, what you were saying yesterday morning was that these studies may be of interest because if the Gardner hypothesis is right, it might explain such excesses as had been identified by other nuclear establishments. But the fact, if it be a fact, that there may be excesses found round other nuclear establishments does not show that the Gardner hypothesis is right, does it?
A. No.

Q. That is the point?
A. That is the point, yes, I agree.

Q. Indeed, as we saw from the studies which we looked at culminating in the 1989 Cook-Mozaffari paper, one could summarise these as follows, could one not: that the increase in relative risks at other nuclear establishments generally - and I will come to the particular ones that you rely upon - is pretty small?
A. Yes.

Q. Secondly, that it appears to be limited to lymphoid leukaemia, if one looks at the picture generally?
A. When you say limited, statistically significant results were certainly seen in lymphoid leukaemia more than in ---

Q. I think lymphoid leukaemia only?
A. You could be right on that.

Q. There is no increase in either cancers generally or indeed in non-Hodgkin's lymphomas?

A. When you say "no increase", you mean no statistically significant increase?

Q. I mean, whether statistically significant or not, there is really no increase that is more than marginal. Indeed in non-Hodgkin's lymphoma there is no increase at all beyond what might be expected?
A. That may well be so.

Q. But as we noticed, the highest (albeit still comparatively small) increase in relative risk is in Hodgkin's lymphoma where you have a statistically significant result in relation to a disease which has no connection with radiation?
A. In one paper, yes.

Q. It is the final paper in which it is bringing together and trying to perhaps iron out some of the creases which were there in earlier studies?
A. I think that is relying on a single geographical study more than I would wish to do, but undoubtedly that Cook Mozaffari paper showed that, yes.

Q. I would suggest to you that if you take into account those factors which I have just outlined, far from these studies lending support to the Gardner hypothesis, they would tend to suggest that even if the Gardner hypothesis were the explanation for the Seascale cluster, it would be unlikely to be the explanation for the other excesses found in other establishments?
A. I don't quite agree with your general interpretation. May I be permitted to return to the paper you are referring to?

Q. Yes, certainly.
A. That is in Common Bundle C at number 44.

Q. That is Cook-Mozaffari 1989?
A. Yes, and Table 4 on page 480. As far as I can see, the relevant data there is that having adjusted for other factors, all leukaemias are increased, lymphoid leukaemias are increased a little more, so that for all types of leukaemias it is 1.15, for lymphoid leukaemia 1.21, for other leukaemia 1.07, for all lymphomas 1.10, and for Hodgkin's disease 1.24.

Q. And other lymphomas, being therefore non-Hodgkin's ---

Q. MR. JUSTICE FRENCH: I am sorry, I am obviously not reading the right column. I have got Table 4.
A. Table 4, page 480, and I am just reading the first column.

Q. The first column down, 1.12?
A. 1.12 is the unadjusted excess for all types of leukaemia, and 1.15 is the adjusted, that is adjusted for social class and for rural status, population size and so on.

Q. So you are reading down that column?
A. Yes. Each one of those relative risks has the uncertainty that his Lordship referred to, that we might express in a confidence interval. If I might interpose there, the confidence interval is undoubtedly just a mathematical calculation and does not involve any biological or scientific confidence in that sense at all.

Q. Nothing subjective about it?
A. Nothing subjective. The method by which you calculate it could be subjective because it may depend on what scale you have measured it on, but there it is a simple mathematical calculation.

Q. Somebody will no doubt go into more detail, but I am grateful for that.
A. So all of those have some uncertainty. In looking at the P values there, what we can say is that Hodgkin's disease adjusted has a confidence interval that will be between approximately 1 and 1.48.

Q. MR. ROKISON: Hodgkin's disease adjusted 1 and 1.48?
A. We can say that that will be approximately the case. Therefore, as you say, the evidence for any of the increases is small, and the increases in all of them, when we look at it in this geographical way, is really very small. But I think to say that Hodgkin's disease is different to the other malignancies cannot be said from that Table.

Q. If that is so, and if, for example, you cannot say that Hodgkin's disease is different from, for example, NHL, which has an adjusted point estimate relative risk of simply 1, then one asks oneself: what does one get from the table at all?
A. What one gets from the table very clearly is the major public health message that there is no major public health risk from these cancers around nuclear establishments.

Q. That is really all you get from it, is it?
A. That is all you will get in detail from that table, yes. You will also get some implication that there does appear to be an excess of the leukaemias, and possibly particularly the lymphoid leukaemias, but when you divide into small groups, if you divide it into a sufficiently small group you will find none of them to be statistically significant.

Q. Insofar as you do have your unadjusted and adjusted point estimates, and I appreciate entirely the point you make that depending on the number of cases there would within each of those categories, they have different confidence intervals and therefore the uncertainty will, although there is a degree of uncertainty here, it will differ from category to category. Nonetheless, it does appear

from the figures that the point estimate for Hodgkin's disease is actually the highest of all the categories, which would be inconsistent with a hypothesis which would depend upon radiation as being the cause.

A. Yes.

Q. Very well, I think we can leave it. I am wondering whether we can look at a paper which I thank you refer to in your statement, and you referred to, indeed, in your evidence in chief. That is the Cook Mozaffari, Darby and Doll paper relating to the similar excess which appeared to be demonstrated in the sites which were selected for future nuclear installations?

A. Yes.

Q. Do you recall that?

A. Yes.

Q. It is Cook Mozaffari, Darby and Doll, "The Lancet", the 11th of November, 1989. It is in C.43. If we can look at this fairly briefly. They refer to the fact that:

> "Mortality and census data for 400 districts of England and Wales were analysed with respect to existing sites of nuclear power stations and sites where the construction of such installations had been considered or had occurred at a later date (potential sites). Excess mortality due to leukaemia and Hodgkin's disease in young people who lived near potential sites was similar to that in young people who lived near existing sites. Areas near existing and potential sites might share unrecognised risk factors other than environmental radiation pollution."

Then they refer to the mortality data to which we have just looked at the beginning of the "Introduction" section. Is that right?

A. Yes.

Q. That is the data we have just looked at which is referred to in the first paragraph, isn't it?

A. Yes.

Q. They go on to say:

> "Since annual radiation doses received by children living near nuclear installations are much lower than those expected to cause any detectable increase in leukaemia, and many orders of magnitude so for installations other than Sellafield, we must ask whether the observed increases are due not to the presence of the nuclear installations but to another feature of the areas in which installations have been built that was not adequately taken into account."

That is the hypothesis which they want to look at?
A. Yes.

Q. So they say:

> "To investigate this hypothesis we have considered the two Central Electricity Generating Board nuclear power stations that were established in England and Wales after the end of the period to which our mortality data referred...and six other sites...were seriously considered for the construction of nuclear power stations."

They then set them out and they say:

> "These eight sites will be referred to as potential sites..."

Then they refer to their "Methods". I don't think anyone gets anything from Table 1, because it is simply setting out numbers?
A. That is right.

Q. Over the page Table 2 on page 1146, which is referred to under "Results" section of the paper:

> "Relative risks for existing sites are strikingly similar to those of potential sites, particularly for leukaemia of all types and for all malignancies at ages 0-24 years."

If one looks up the Table above, Table 2, one sees that for leukaemia that the two figures are here again unadjusted and adjusted?
A. I believe so.

Q. Therefore one is comparing for all leukaemias a relative risk of existing sites on adjusted figures of 1.16, as opposed to potential sites of 1.14?
A. Yes.

Q. You would agree that the larger numbers at the existing sites mean that the numbers may be statistically significant, whereas they are not for the potential sites?
A. Yes.

Q. Taking that into account, of course, that one must accept, but they are right in saying those figures are strikingly similar, aren't they?
A. I wouldn't use the word "strikingly", but they are certainly similar.

Q. Would you not? One gets a slightly increased relative risk which is on a point estimate only 0.02 apart. One would have thought that that is fairly strikingly similar?

A. Given the uncertainties in each of them, that isn't strikingly similar.

A

Q. So you would disagree with that.
A. But I would agree that they are similar.

Q. With lymphoid leukaemia there is only an elevated relative risk in potential sites to the extent of 0.9 as opposed to 0.2?
A. Yes, but I would not say they are strikingly different!

B

Q. No, you can't have your cake and eat it!
A. No.

Q. Can we agree on this, that if you look at that table of results they are similar?
A. They are similar.

C

Q. The "Discussion" they go on to at the bottom of that page where they say:

> "Our hypothesis that, with the possible exception of Sellafield, an increased risk for leukaemia is not associated with local environmental radiation pollution is strengthened by our new findings - namely, that the death rate from leukaemias in areas
D
> where there were no nuclear installations, but where the construction of such installations was considered, or actually occurred at a later date, was similar to that in areas near existing nuclear installations."

Just pausing there, would you agree that on the face of it this is a rather surprising result?
E
A. Yes.

Q. It is a surprising result because perhaps one's natural inclination would be to think, well, if there is an excess of leukaemias, albeit a small excess generally around nuclear sites, you would have expected it to be something to do with the installation and its operation? That is why it is surprising.
F
A. That is one of the possibilities, yes.

Q. That is why it would be surprising?
A. Yes.

Q. Nonetheless, there it is, and the authors of this study who included, as we see, not only Dr. Cook Mozaffari but
G
Sir Richard Doll, apply their minds to other possibilities. You see, and we will come on to what I might call the Kinlen hypothesis, but you see about half way down the right hand column they say:

> "Recent data from New Towns in Scotland and from England and Wales are compatible with an earlier suggesting that the increased incidence of leukaemia
H
> around Sellafield and Dounreay might be attributable

to a cluster of cases due to an infective agent associated with a large influx of people to these areas."

That is what I might call the Kinlen hypothesis, isn't it?

A. It is one of his hypothesis, yes.

Q. Well, it is the hypothesis which he develops in the papers which you reference in your report?

A. Yes.

Q. They consider that and express doubts about that on the basis of the sensus data for those sites which they have considered?

A. Yes.

Q. Then they consider exposure to radon and they conclude at the bottom of the page that that alone could not explain their findings. Then their last paragraph on page 117, they say:

> "We had previously believed it to be unlikely that districts close to existing nuclear installations might differ from those elsewhere in another characteristic relevant to the development of childhood leukaemia. This was because the adjustment for geographical variation in the socioeconomic and demographic factors that influence mortality from cancer had brought close to unity the relative risks of death at ages 25-64 from most malignancies studied, including lung cancer, and also from all non-malignant diseases. By contrast, our new findings point to systematic differences between districts near existing or potential installations and other districts with respect to some important, unrecognised risk factors."

Would you agree with that?

A. I think that that is slightly over stating it. I have slight reservations about the paper. As they point out, three of the sites are where there is increased radon exposure and they haven't adjusted for radon exposure. I think that the choice of where are the potential sites is also open to bias. I am not suggesting that it is, but I find their description of it and simply thanking CEGB for making available the grid references of the sites, there is no exhaustive description of whether these were all the potential sites and whether there were indeed other factors which were special to the potential sites.

Q. Nothing appears from...

A. But nothing appears from that and I would accept that it is evidence against the excess around installations being associated with the installations but I don't think it is strong evidence against it, but it is a contribution, yes.

Q. Well, that is very fair. In relation to the point you make about radon and the sites in Cornwall, it is true

that they don't adjust for that, but what they do is they separate the three Cornwalls from the others, and one finds that if one looks just at the others, that in fact the adjusted relative risk for leukaemia falls from 1.14 to 1.13, so it doesn't make very much difference if you take up the Cornish cases. That appears about five lines up from the bottom of the right hand column on 1146

A. Yes.

Q. MR. JUSTICE FRENCH: May we pause? One of your answers, Professor Evans, was that the study we are now looking at "contributes evidence of", and I don't want to write it down in case I get it inaccurately. It contributes evidence of what?

A. It contributes evidence against the excess near installations being caused by those installations.

Q. MR. ROKISON: You were referred in your evidence-in-chief to a passage in Prof. MacMahon's report, and I don't want to take you to that again. You said you thought he had over-stated the case a little, but that can be put to him. In his report Prof. MacMahon had referred to general studies of this kind in relation to installations in the United States and France and West Germany?

A. Yes.

Q. May I just ask you this: have you yourself looked at studies relating to excesses or absence of excesses of leukaemias or cancers round sites in the US, France or West Germany?

A. I recall that there is one around sites in France which was published in The Lancet, which I have certainly read.

Q. Are you thinking of the Hill and La Plage study?

A. I think so.

Q. Dr. Wakeford thinks that that one is in fact in "Nature"?

A. That could be.

Q. It is H.113. Is that the one you were thinking of?

A. Yes, I think that is the one I am thinking of. I certainly have read this paper.

Q. I don't want to take you at length through it, because it is something you have read and you haven't referred to particularly in your report.

A. No.

Q. Perhaps we can restrict ourselves at the moment just to looking at what the lawyer would call the headnote, but it might be called the abstract at the beginning:

> "Higher than expected mortality from leukaemia has been observed in the population under age 25 living around Sellafield and Dounreay nuclear reprocessing plants in the United Kingdom. We report the

results of a similar study for the population residing around nuclear sites in France. The number of leukaemia deaths was 58, comparable to the 62 in control areas, and slightly less than the 67 expected from national mortality statistics. Twelve deaths due to Hodgkin's disease were observed around nuclear sites; this is about twice the number of Hodgkin's deaths observed in control areas and twice the number expected from national mortality statistics. This observation must, however, be interpreted in light of the fact that several causes of deaths were studied, increasing the place of chance."

That is the point we were discussing yesterday?

A. Yes.

Q. They refer to the nature of the study. There were six sites selected; four geographic zones around each of those sites. It is really in table 1 where one sees the ratio in, on the one hand, the exposed cases as opposed to the controls, for all malignancies and the other categories. As far as lymphoid leukaemias at the bottom of that table is concerned, one finds there is in fact a noticeable - I don't use the word "significant" - but a noticeable deficit in lymphoid leukaemia cases. There is also a deficit in leukaemias. One finds that the significant excess that is only in Hodgkin's disease and a significant deficit in brain tumours. The only significant excess is in Hodgkin's disease, which, as we have agreed on a number of occasions, is something which is generally recognised as having no connection with radiation?

A. Yes.

Q. They say in the penultimate paragraph:

> "Our results confirm Viel and Richardson's study of leukaemia mortality around La Hague, which used geographical units with populations seven times larger than in our study."

I don't want to take you to it, it was not a paper you looked at.

A. Well, I think I have read that one. It was in the BMJ.

Q. That is correct. They say:

> "The excess leukaemia observed around nuclear sites in the United Kingdom is not observed around French nuclear sites, although the same methodology was used as in reference 4."

Reference 4 is the Forman document, so that is the summary of the large Cook Mozaffari study?

A. Yes.

Q. Now I think you said in your evidence in relation to studies of nuclear installations abroad that you would need to find sites which had similar releases, similar

occupational exposure and good quality data?
A. Yes.

Q. Would you agree with this, that if you were considering a possible environmental irradiation pathway, considering that hypothesis, then you would need either comparable releases or at least releases which would be sufficient to cause some observable excess?
A. Yes, and the other factors I mentioned as well.

Q. Well, you say the other factors you mentioned as well. If that was the hypothesis...
A. No, sorry, what I meant was, for example, quality of data.

Q. Well, we will come on to quality of data in a moment, and just trying to divide up the other two factors you mention - I mean, it depends which hypothesis you are looking at. If you are looking at an occupational exposure hypothesis then you will need to find an installation or group of installations where the occupational exposure was at least sufficient to give you an observable excess on a comparable basis?
A. Yes.

Q. However, that doesn't mean to say that the exposure would have to be as high?
A. Not as high, no.

Q. So far as the French sites are concerned, we know there are a considerable number of nuclear installations in France. It forms a large part of their power industry and it has reprocessing plants as well.
A. Yes.

Q. We also know that a number - as one would expect - a number of studies have been carried out in the United States where again there are quite a number of nuclear installations of different types, both weapons production as well as nuclear installations for the purposes of producing power?
A. Yes.

Q. Those studies have been reviewed in other papers and I don't want to take you to them. However, are you saying, and I want to try and get this clear, that the studies which have been carried out in France and the studies which have been carried out in the United States are of no assistance because there were shortcomings in the data available, or are you merely saying that you believe it may be more difficult to get hold of data in France or, on a national basis, in the United States and that is something that should be taken into account?
A. I think if you look at the paper you have just referred to on page 756, they say that the precise commune of residents corresponding to each death was not made available to them, and so on. That geographical studies in France are limited to ones which have to have large

areas. We know in regard to studies around nuclear installations that choosing larger areas will tend to minimise any apparent risk we see. Certainly if we look around Seascale, if we start increasing the area around Seascale sufficiently, then we find the excess there disappears entirely. Even if we include Seascale we don't find that Cumbria as a whole has an excess. If we look at large areas in France then we will find the same effect.

I think it does contribute some evidence, but not very strong evidence.

Q. If you look at the bottom of 755, one does find that they do say that they define four geographical zones around each installation according to the distance from the installation, and the smallest of those zones was less than 5 km?

A. Yes.

Q. So it would appear they were able to obtain sufficient data to be able to divide up their population, both their cases and their controls, into those areas?

A. I think it is very difficult in France from the knowledge that I have in studying infant mortality in France as a whole, to be able to do precise work of the sort that one can do in the UK. I think the description there is not sufficiently clear.

Q. What they are saying is, as I understand it, that the precise commune of residents was not made available but it appears in the passage which you mention on page 756, it appears they were able to allocate their cases and their controls into the geographical areas which they set out in their study?

A. Well, they have some method of doing so, but it isn't defined terribly clearly because they have talked about exposed communes, but they don't know about the deaths within those communes.

Q. MR. JUSTICE FRENCH: You would like to know more about their method?

A. I would like to know rather more about their method and from my independent knowledge of the way that one has to do studies of this kind in France, it is really quite difficult to do so. While I would agree that it is some evidence, I think the strength of the evidence is over-stated, and that is where we began this discussion.

Q. MR. ROKISON: Yes. I am not sure who is over-stating it, but I think we can leave it here that the French studies and the United States studies to the extent to which they appear to show that not only is there no excess of leukaemias around nuclear installations, but on the contrary, if one looks at the French study it would appear that there is a...

A. That there is a deficit - radiation prevents leukaemia!

Q. No, simply that... Well, you say that. No doubt if there had been an excess you would say radiation causes leukaemia. If there is a deficit you say radiation protects you against leukaemia. We are not suggesting that, but...

A. We are saying in the French sites that there is an apparent deficit in the areas where they build their sites.

Q. Yes, because one will find that because the causes of leukaemia are largely unknown, that it may very well be that there will be a deficit in a place where a nuclear installations happens to be built? No-one is suggesting...

A. Forgive me, Mr. Rokison. What I am saying is that I don't think there is evidence for a genuine deficit.

Q. Maybe.

MR. JUSTICE FRENCH: I think it was expressed in the form of a little joke, rather than a serious observation.

MR. ROKISON: Indeed. I wasn't suggesting that it was protective but I was merely...

MR. JUSTICE FRENCH: I don't think Prof. Evans was either.

MR. ROKISON: No, I don't think he was either:

Q. May I just take you to the La Hague study, just on this question of information, which is in V.252. It is Viel and Richardson, and it is an interesting study because it is concerned with the La Hague reprocessing plant. You see that the authors say:

> "The incidence of childhood leukaemia around nuclear facilities has been a topic of much public attention and epidemiological investigation. The Sellafield and Dounreay nuclear waste reprocessing plants have been particularly investigated. A similar reprocessing plant has been operating in La Hague, Normandy, since 1966, several years after Sellafield was commissioned."

Nonetheless, for several years by the time this study was carried out?

A. Yes.

Q. "The amount of radioactive effluent discharged has been much lower than that from Sellafield..."

The main radionuclides they there set out:

> "Discharges from Sellafield and La Hague differ in their isotope contents..."

They refer to those:

> "We report preliminary results on mortality from childhood leukaemia observed around the plant at La Hague."

Then they refer to mortality for the two period which they study, having been provided by the Institute National de la Sante et de la Recherche Medicale:

> "...which records all the medical causes of deaths in France centrally."

They took three age groups, studied all electoral wards which had half or more of their area within a specified radius. Again they took 10 km, 20 km and 35 km. It is a similar methodology to that which has been used in the UK studies we have looked at in selecting your areas?

A. Similar, yes.

Q. Well, selecting political areas which have a percentage of their population within certain radii?

A. Yes.

Q. They say:

> "The expected number of cases of leukaemia were estimated by applying the age specific rates for the Department de la Manche...Only one death occurred in the area closest to the nuclear installation between 1968-86. Only one standardised mortality ratio was significantly different from one: the ratio for the age group 5-14 living 10-20 km from the plant during 1968-78 showed a decreased risk."

If one simply looks at the Table one finds that there is no statistically significant increase in any of the areas for any of the age periods studied?

A. No. Of course they are looking at deaths and not at deaths and not at incidents of leukaemia because the quality of their data on incidents of leukaemia is not terribly high in France.

Q. Of course there were problems and we have looked at some of the papers involved in relation to registration data in the UK?

A. Yes, on a national basis, though as it happens in northern parts it is really very good.

Q. The point was made in relation to the earlier Cook Mozaffari study where they were comparing the installation with a control area that they found the control area had very low cancer incidence, which they attributed in part to registration problems, and for that reason suggested that mortality data might be a better way of doing it and they used mortality data for the purposes of the later study?

A. If you are doing it on a national basis for certain time periods, then registration data is poor, and certainly for very early periods around the fifties and so on, for periods prior to this study, then the mortality rate from leukaemia was so high that its incidence and mortality would be equivalent. We are talking about a period in which survival from leukaemia is really quite good and so this study has some evidence but it is very weak evidence, I'm afraid.

Q. Because it uses mortality data?
A. Because it uses mortality data for a late period, and the actual numbers you are studying of deaths are really very, very small indeed, and so the confidence intervals are very wide.

Q. I appreciate that. It is a study based on small numbers. We can leave France, I think. Can we come now to the specific UK sites which you rely upon? I think they are Dounreay, Aldermaston, Burfield and Harwell and Hinkley Point?
A. Yes.

Q. Why do you pick out these three particularly? We have looked at the general UK installations study. what more do we get from looking at these?
A. Well, obviously COMARE's third report, I would regard as really very relevant and I have picked out the papers they have referred to and COMARE III itself.

Q. Because that deals with Aldermaston, Burfield and Harwell?
A. Yes.

Q. Let's look at these quite briefly. As far as Dounreay is concerned, the story perhaps starts... If you could look at a paper or letter written by Heasman et al to "The Lancet" in 1986, which is H.107. Did you read that?
A. Yes.

Q. Just looking at it briefly. It was produced in anticipation of the Public Inquiry, for the planning application for a nuclear processing plant at Dounreay and they say:

> "Our primary concern here is to report findings for lymphatic and haematopoietic neoplasms for people aged 0-24 during the period 1968-84. The places of residence of the patients at the time of registration have all been postcoded and grouped into five groups: mainland enumeration districts whose centroids fall within 12.5 km or within 25 km of the existing Dounreay installations and the remaining mainland districts falling within the KW postal area."

That is Kirkwall and Orkney:

"and within 25-75 km of Dounreay...and Orkney and Shetland."

A They compare with expected rates based on all Scotland cancer registration. They refer to the table and the table appears up on the top right and they say:

"The cell in this table which gives cause for concern is that for leukaemia between 1979 and 1984..."

B From that, that stands out, that there one has 5 cases as opposed to an expected of 0.513?

A. Yes.

Q. That is, as one sees, restricted to that period of 1979 to 1984 with no cases in 1968-73, or 1974-78?

A. Yes.

C Q. And is restricted, the excess, to the 12.5 km from Dounreay, as opposed to the 12.5-25 km from Dounreay?

A. Yes.

Q. They make the point in the paragraph we have just left that it is less than 12.5 km from Dounreay:

D "In this group 4 of the 5 patients with leukaemia lived in Thurso, the only significant centre of population within 12.5 km of Dounreay, and the fifth lived about 3 km from Dounreay."

The point which arose from that is if you draw your 12.5 km circle round Dounreay that cuts across Thurso, so that if you had taken 10 km you would only have had one case, if you take your 12.5 you take in part of Thurso. E If you widen out your circle so as to take in the whole of Thurso your excess then disappears. I think this is an example...

A. This is an example of the difficulties.

Q. What you were saying yesterday, or the day before, or both, namely, that there are lots of ways of looking to F see if there is an excess and it may depend on where you happen to draw your boundary?

A. Yes. I imagine, knowing this was J. D. Urquhart and Heasman, that they did those before they looked at their data, as opposed to the media who are inclined to do it after they have looked at the data.

G Q. But they do say:

The importance of this finding is difficult to evaluate. The choice of radii and time periods is arbitrary and, although there is an excess of cases over the whole period 1968-84, no cases at all were registered within 25 km in the period 1968-78. On the other hand, the facts that all reported cases

H

within 25 km occurred within a 5-year period, 5 were in children under 15, and 5 occurred within 12.5 km of Dounreay, may increase its potential importance."

A

because you have an apparent cluster both in time and space?

A. Yes. It is some evidence but it is not entirely convincing evidence of the problem.

Q. I do not think there is any issue between us on this. They go on to say:

B

> "Preliminary examination of all other cancers in childhood and of leukaemia and selected sites of cancer in adults, and of the occurrence of congenital malformations, showed no significantly raised figures in the area around Dounreay."

Just pausing there, is that a matter which you considered of some importance?

C

A. Again, I think that the evidence is likely to be weak in either direction on that but what is clear here is that there is no major public health problem in terms of raised cancer rates that are dramatic.

Q. Yes, we would agree with that but would you not agree that if - of course, this is pre the Gardner Study - one were to seek to apply to this apparent cluster the Gardner hypothesis of preconception irradiation, some theory of damaging the germ-line, that one would perhaps expect to find an excess of congenital malformations in the population as well as an excess of leukaemia?

D

A. Yes, provided the numbers were large enough.

Q. Indeed.

E

Q. MR. JUSTICE FRENCH: Is the answer this, Prof. Evans? If radiation were responsible for the 1979-84 increase one would expect to find excess of, e.g., other cancers and malformations as well?

A. Yes. I think you are pushing me beyond the bounds of my expertise to say you would definitely expect it but I would expect it, yes. I think that the general expectation is that you would see congenital malformations and other cancers.

F

Q. The general expectation would be that one would find excess of, e.g., other cancers and malformations?

A. And congenital malformations, provided ....

G

Q. Pause please. Provided ...?

A. ... that the sample size, population size, looked at was large enough.

Q. Shall I call it sample size?

A. Yes.

H

Q. The sample size were large enough.
A. For the effect to be noticed.

A

Q. So that in the end what is your view as to the significance in a non-technical sense of the paper?
A. Well, of that letter alone, that it gives a hint of a possible problem.

Q. MR. ROKISON: I think that in answer to my question particularly, I think my Lord put the question on the basis that if it was caused by radiation, but I was
B suggesting that if the Gardner hypothesis, namely paternal preconception irradiation damaging the DNA in some way, were to be the cause, one would certainly expect, would one not, an excess of congenital malformation?
A. In paragraph 62 of my report I make it clear, in the sentence that is the very last one on page 22:

C

> "The overall hypothesis given is plausible but the mechanisms involved are outside the competence of both Martin Gardner and myself to assess."

Q. Indeed, I am not asking you about --- what I am saying is if the mechanism which is suggested, which is the mechanism which is suggested by Gardner, namely the
D possibility of some radiation damage to the germ line of the fathers, in those circumstances would you agree that you would expect to find an excess of congenital malformation, if one found an excess of leukaemia?
A. Yes, but again that is not my expert knowledge.

Q. I appreciate that and we can ask those who are perhaps more particularly qualified in that field.

E

MR. JUSTICE FRENCH: Mr. Rokison, if I pop in the word "paternal", and put "irradiation"?

MR. ROKISON: Yes, particularly paternal irradiation and congenital malformation is the point I was putting.

F

MR. JUSTICE FRENCH: Yes, I follow.

Q. MR. ROKISON: I will take you to it if you like, but did you look at a subsequent paper written by Heasman in the Health Bulletin which widened the study to other areas, other than Dounreay?
A. I do not have an immediate remembrance of that one in my mind. I think I probably looked at what they presented
G to COMARE. I cannot remember whether what they presented to COMARE was only that letter; I thought that they had extended some of their work for COMARE.

Q. Can I just refer to it very briefly? It is only one point I want to point out - I am sorry that this is taking some time.
H A. I do not think I have referenced ....

Q. It is H108.
A. It is not one that I have referenced.

Q. No. If you look at the summary, it refers to this cluster which we have just been looking at. You see that they extend the study to cover Dounreay, Hunterston, Chapel Cross, Torness, and they also look at the naval installations at Rosyth and Holy Loch as being possible sources of irradiation?
A. Yes.

Q. What one finds, the only point that I wanted to make and to point out to you is that it appears from page 149 that they analysed, although I think it does not appear from a table, NHL separately?
A. Yes.

Q. And found, it appears, no excess around Dounreay?
A. Yes.

Q. That is the only point I wanted to make, thank you. Perhaps we can look now at COMARE II, 1988 - it is C39, and what they did was to define their circles more precisely, didn't they?
A. Yes.

Q. They in fact looked at eight concentric circles, which we see on page 21, on Table 2.7, and as they say in paragraph 2.38:

> "Thus the question of whether there is a statistically significant excess of cases near Dounreay is sensitive to the area chosen. Four of the 5 cases in the Inner Zone lived in Thurso very close to the 12.5 km boundary of the zone, which may have led to a high estimate of incidence using this particular boundary."

and they conclude that paragraph by suggesting that:

> "... it would be wise to regard the result for the original Inner Zone with caution."

A. Yes.

Q. At paragraph 2.47, page 24, they point out, as we have seen already, that:

> "... within the Full Dounreay Area, cases had occurred only in the most recent of the 3 periods analysed by ISD, that is the 6 year period of 1979-1984. This is not a finding that would have been anticipated on the basis of any previously stated hypothesis of increased leukaemia risk associated with proximity to a nuclear installation but has been discovered by inspection of the data. However, this marked excess in a short period is a potentially important observation which should be

fully examined. In this section we therefore present and discuss the data for the recent period in the same way that we dealt with the full period above."

and one finds the tables in relation to that recent period of 1979-84. One sees from page 25, Table 2.13, although the Observed over Expected is high in respect of the area within 9.375 km, that is just one case, isn't it?

A. Yes. I think it is two cases, no it is one, they are not additional.

Q. It is cumulative. What we see is that they are cumulative as we move out through the circles, so you start off within 3.125 km, you find you have no cases, even though statistically you might have expected to have a hundredth of a case. As you move out you find when you get within 6.25 km you have one case. Of course, by that stage you would only have expected to have 3/100ths of a case, so there is an excess of Observed over Expected, but it is only based on one case. So by the time you get out to nearly 10 km that Observed over Expected has come down because by that stage you would have expected to have 6/100ths of a case. What one does find is that it is in the next extension, out to 12.5 km, that the change occurs because you have a further four cases occurring within that quite narrow band, and that is the point, that they just happen to be in a particular part of Thurso.

MR. JUSTICE FRENCH: Well, I have that point and got it quite a while back.

MR. ROKISON: Yes, my Lord:

Q. What they say, having discussed this, if one goes to their conclusions on page 61:

> "Although the general population is exposed to radioactivity from the atmospheric and liquid discharges from Dounreay, the levels of dose are low and could not, using either conventional methods of risk assessment, or the more extreme assumptions described in Section 10, account for the observed excess rates of leukaemia in the area. However, there are uncertainties about the effects of radiation at very low dose levels of exposure and we cannot exclude the possibility that some novel exposure pathways exist or that different factors need to be taken account of in the dosimetric calculations."

So that so far as COMARE was concerned one had this cluster in time and space which could not be explained by environmental factors?

A. No.

Q. Is there anything else that one needs to look at in the COMARE paper?
A. No, and I think in some senses the paper by Draper suggested that the Dounreay cluster was not re-found by their methods anyway.

Q. That is right. You mention that and I was not going to take you back to it. You mention it in paragraph 36 of your report where you refer to the fact that the Draper paper, which we looked at, on page 115, did not confirm the existence of the Dounreay cluster?
A. Using that methodology or the variety of methodologies.

Q. Which again demonstrates, as you say, that:

> "... results can be extremely dependant on the geographical area and time period selected for examination."

Indeed, they can also depend on the ages which you choose for your study?
A. Yes.

Q. They can also depend upon the way in which you choose to divide up your diseases?
A. Entirely so.

Q. And as you point out in relation to the Gardner Study, which we will look at in a moment, they may depend on, if you are choosing categories of dose, it may depend on how you choose those categories?
A. Yes.

Q. Can I pick up one point before we leave your report on Dounreay. You say in paragraph 33

> "NHL and leukaemia were considered together because of the similarity between the biology of the malignancies ...."

A. Yes.

Q. If you look at the COMARE II study at page 16, C39, what you say there is rather an over-simplification, isn't it?
A. Yes.

Q. What they say at paragraphs 2.21-2.23:

> "The leukaemias are a group of malignant diseases characterised by uncontrolled proliferation of primitive cells within the bone marrow and, generally, increased numbers of white cells in the blood."

They refer to the four main types, ALL, the commonest in children, representing about 80% of cases in the age group 0-14; then acute myeloid leukaemia, AML, which are the remaining cases of childhood leukaemia; and then

chronic lymphatic and chronic myeloid. They then refer in 2.22 to the fact that the introduction of new diagnostic techniques improve the precision of diagnosis, and I do not think we need to go into the details of that, but at 2.23 they say:

> "There is a close association between each of the T and B subtypes of acute lymphoblastic leukaemia, and analogous forms of the non-Hodgkin's lymphomas of childhood .... The latter are a heterogeneous group of cancers whose primary cell of origin is in lymphoid tissue which tend to form solid tumours. The application of immunological typing to these diseases has demonstrated that these subtypes of NHL and ALL really represent two ends of a spectrum of the same disorder, and whether they are called leukaemia or lymphoma depends on the percentage of leukaemia cells in the bone marrow. It is now conventional to call such cases leukaemia if more than 25% of the bone marrow is occupied by leukaemic cells. It is therefore important that uniform criteria are used for the registered diagnosis ...."

and so on. So the position is that it depends on the type of leukaemia and the type of NHL one is talking about?

A. Yes.

Q. I think we can leave the Dounreay Study because you come back and deal with the subsequent Urquhart Study in relation to Dounreay when you are considering studies post-Gardner?

A. I am sorry, you will have to repeat that. I was so lost by what you had said the last time, I thought you were trying to get at my phrase on the "biology of the malignancies being similar", and I thought that the sentence you had read out stated that in rather different words, but I agree it was an over-simplification. Anyway, let's forget that. I lost your next question, I am sorry.

Q. I was going to say that I was going to leave the Dounreay Studies now because I was going to leave over the James Urquhart Study, which you deal with later on because it falls into the post-Gardner period of the testing of the Gardner hypothesis. Can we leave that and can we move now to the Aldermaston, Burghfield and Harwell cluster, if such there is? Can I just put this to you: having read the papers which relate to the Burghfield, Aldermaston and Harwell cluster or clusters, are you of the view that there was any cluster around any of those installations?

A. I think there is weak evidence, again a hint of a problem, shall we say, taken together with the other things, a hint of a problem.

Q. I think then I may be able to take it fairly briefly. Can I just try and get Harwell out of the way? It may be

that you cannot, and I do not expect you to carry all the information in your head as you have other things to think about as well, but which is the study that you rely upon in order to suggest that there was a cluster round Harwell?

A. I do not think I said there was a cluster round Harwell.

Q. I think you do. You say:

> "COMARE's third report reviewed the research into childhood leukaemia and childhood cancer rates around Aldermaston, Burghfield and Harwell nuclear plants by Roman et al, Cook-Mozaffari et al and the Childhood Cancer Research Group."

A. That says it "reviewed the research into", and then I said:

> "The Committee concluded there was a small ... within 10 km of Aldermaston and Burghfield nuclear plants between 1972-85 ...."

Q. I see. I stand corrected, and when we looked yesterday afternoon at the 1989 Cook-Mozaffari paper you will remember Table 5, which is the one that sets out the various distance zones, the percentages of population within 10 miles?

A. Yes.

Q. That one found for Harwell, as set out in that, there was indeed an apparent deficit. Do you recall that?

A. I do not, to be honest, but I take your word, I am sure there was. I do not think I stated anything at all anywhere, and if I did it was clearly a mistake.

Q. I think I have misread this part of your report and I stand corrected. I think we can agree that there is no excess at or around Harwell. It is perhaps simply because your heading might suggest ....

A. Because it was what COMARE looked at.

Q. Very well. Can we look at that COMARE Report, which is COMARE III, C40? You will see that it has something in common with Sellafield insofar as it appears, from paragraph 1.3, that this exercise was also spawned by a Yorkshire Television programme, called "Inside Britain's Bomb", which alleged there was a raised incidence of leukaemia and lymphatic cancer in young people under 25 years of age around the Atomic Weapons Research Establishment at Aldermaston and the Royal Ordnance Factory at Burghfield in Berkshire. They refer to letters by Urquhart, Cutler and Burke, I think, and by Barton, and they make the point which you have already made, I think, that one has to look, therefore, with caution at any cluster which has been arrived at in this way?

A. Yes. I think that you should note that Dr. Barton raised it before the TV programme.

Q. Yes, you are quite right, she did, in the previous month. If one can look at Table A1.8, that is the table that deals with Harwell. If one looks at the table which is dealing with Aldermaston ....
A. A1.7 deals with Aldermaston.

Q. Yes, it is A1.5, A1.6 and A1.7.

MR. ROKISON: My Lord, I am sorry, I am slightly confused in my notes. It was a little time ago that I looked at that. May I ask you to keep this open and to look at the Roman paper which is being discussed here by COMARE? It is the paper by Roman et al in 1987, bundle R205.

MR. JUSTICE FRENCH: Would it be a good idea to check this over the mid-day adjournment, or have you got to come to it now?

MR. ROKISON: I won't be a moment in coming to it, my Lord; I can certainly pursue this part of the case in just a moment if your Lordship will bear with me. I seem to have lost the relevant note, that is all, my Lord.

Q. In the Roman paper you will see that what for the purposes of this paper the authors did was to take, as they say in the abstract, 143 electoral wards in two health authorities, that is West Berkshire and Basingstoke and North Hampshire, and out of the 143 electoral wards 50 of them either lay within or had at least half their area lying within a circle of radius 10 km round each of the establishments. One finds that, if one looks at Figure 2, you can see the map, either Figure 2 or Figure 3 on page 599 or 600 will show you your map, and it is right, isn't it, that Harwell is the one which is top left because it is actually outside the West Berkshire or North Hampshire area, and then the two that intersect the bottom left, being Aldermaston and the one to the right and slightly up from there being Burghfield?
A. Yes.

Q. One comes back, one sees that, in the 50 electoral wards - I am reading now from the Abstract - 41 children aged 0-14 were registered with leukaemia, 28.6 being expected, so that there was a raised relative risk, with a just significant P value?
A. Yes.

Q. And, as they point out, confined to children aged 0-4, where one has a more raised incidence ratio and a greater P value and, in the remaining 93, there was a small and non-significant increase, and no obvious trend over the 14 years.

If one goes over the page, please, you will see how, at the bottom of page 598 on the right, they worked out

their circles, their radii, and they discussed the results on page 599 and refer to the significant excesses, to which we just looked in the Abstract.

If you look at Table 2, at the bottom, that shows you, does it not, where the significant excess lies in the 0-4 year old group only?

A. Yes.

Q. If one looks at Table 6 on page 600, they there divide up the incidence ratios (observed/expected registrations) for electoral wards lying within 5 km and 10 km respectively of each of the establishments. Ignoring Harwell at the moment because there was only limited data because it was not within the relevant health authority, and we have put Harwell to one side, what one finds is that, with the circles round Aldermaston, there is no significant excess. Is that correct?

A. I am sure you must be right, but I have lost you. I thought you were looking at Table 4.

Q. No, Table 6, at the bottom on the right?

A. I am sorry. Yes.

Q. What one finds from that is that the only significant excess is an excess in relation to the circles round Burghfield, the 10 km circle round Burghfield, and not to either the 5 or 10 km circle round Aldermaston?

A. They are the only ones that are statistically significant.

Q. They are the only ones that are statistically significant and, if one looks back to Figure 2 and the incidence ratios and the distribution of leukaemia incidence ratios, one finds that the circle drawn round Burghfield, being the right-hand circle, embraces quite a lot of heavily shaded area and the black areas that one sees towards the outer rim on the right?

A. Yes.

Q. Which, geographically, is, is it not, the town of Reading?

A. Yes, certainly Reading is, I would have thought, about on that map where I see a 4. It is slightly to the north of...

MR. JUSTICE FRENCH: If we look on the opposite page, we can see Reading actually marked, can we not, so we get a fair idea where it is.

Q. MR. ROKISON: That is right?

A. I think the main black area is to the south of Reading.

Q. Just to the south of Reading, yes, I think that is absolutely right. If one looks at their discussion, which appears on page 601, they say:

> "These data confirm our preliminary finding of an increased incidence of leukaemia in children aged 0-4 in the West Berkshire District Health Authority."

They refer to the excess being restricted to 0-4 and so on. About just over half the way down that page:

> "Although the incidence ratios within the 10 km circle were significantly greater than those expected on the basis of rates in England and Wales, the incidence rates in children living within the 10 km boundary were not significantly different from those in children living outside the 10 km boundary. The statistical power of comparisons of leukaemia rates in two small areas is, however, low: data would have to be collected for more than 40 years before the almost twofold difference in incidence noted here at 0-4 between those inside and outside the 10 km boundary would have been significant at the 5% level...."

and they say:

> "That the pattern of childhood leukaemia in the two district health authorities is consistent with a random distribution partly reflects the low statistical power of such methods when applied to small populations."

They emphasise again, two sentences later:

> "That the overall pattern of leukaemia was compatible with random distribution might be thought to conflict with the observed excess incidence around the nuclear establishments, but the methods used to assess variation in the spatial incidence of disease were non-specific and not necessarily sensitive for the detection of a raised incidence around a defined point source."

601, on the right, they consider the possible causes, where, at the break, they say:

> "Could the finding of the increased incidence .... be explained by other factors?"

They refer to the characteristics of the children with leukaemia. No obvious differences in the characteristics of those who live close to or far from the nuclear establishments and so on. It is suggested that it might be increased in children whose parents are in the upper social classes, but suggests that the gradient is weak. Then they consider whether it could be due to chance and, at the bottom of the page, they say:

> "As the suspicion of an increased occurrence of childhood leukaemia in a village near the Sellafield

plant prompted many of the other investigations, the Sellafield data should be viewed as generating, rather than testing, the hypothesis that the incidence of childhood cancers is increased around nuclear establishments."

Then, just after the break:

> "The data reported so far do not cover all nuclear establishments...." and so on. "There were no cases at all in the two West Berkshire electoral wards with at least half of their areas included in the 10 km circle around Harwell....."

And they say:

> "The data described here are essentially confined to the area around Aldermaston and Burghfield, and all the excess registrations of childhood leukaemia described occurred there. The incidence ratios were similar around each of the two establishments, although significant only around the Royal Ordnance Factory," which is the Burghfield site, "which lies near areas of high population density. The reports so far may have selected establishments that happen to be located near areas with an increased rate of childhood leukaemia."

And the analyses they refer to for Dounreay, Holy Loch and so on, which we have looked at. Finally, they say:

> "Despite the significance of our findings the actual risk of leukaemia to children living in West Berkshire and Basingstoke and North Hampshire and within a 10 km radius is not great."

They say that about two of these children would normally be expected to develop leukaemia. Three have been registered, being one extra case of leukaemia amongst the 60,000 in each year.

Did you, in relation to this, read a letter from Dr. Wakeford in relation to the Aldermaston and Burghfield excess?

A. I do not recall, to be honest.

Q. It is a letter which was written and is published in The Lancet in February of 1988?

A. It does not appear to be in the Common Bundle.

Q. No, no, we have ascertained that. We have looked at our index. Can we come back to that, if we can find it, after the break? I do not want to take up time looking at it now.

MR. ROKISON: My Lord, may I, I think, leave this for the moment and come back to it after the adjournment because, as I have indicated to your Lordship, for some

extraordinary reason, my notes appear to be in rather a jumble in relation to this and I think we would be wasting more time if I pursued it now;

Q. Can I come back to it after the adjournment, not for very long, I hope, Professor Evans? Can I move on to Hinkley Point and try to get rid of that before lunch, if we may? The first reference is E 77, which is Ewings et al and is a paper which was published in the BMJ on 29th July of 1989. You simply say in your report that:

> "The Ewings report on leukaemia and NHL incidence in young people in the vicinity of Hinkley Point showed a two-fold excess of leukaemia and NHL cases which was statistically significant over 1964-84 when Hinkley Point was operating."

If you look at the Ewings paper, you will see where Hinkley Point is in the map which is on page 289. You will see that they took incidence of leukaemia and NHL. They took young people under the age of 25, living in a pre-defined area around Hinkley Point. It was examined for the period 1959-86 and during the period since it began operations, which was 1964-86, 19 cases compared with 10.4 expected, which gives you 1.82 relative risk, with a confidence interval of 1.1-2.85. They say that the incidence in the rest of the Somerset was also high, being 1.18, with a confidence interval there set out, and the high rate around Hinkley Point may simply have been reflecting the high local incidence.

What they do then is to do a ratio test in order to test that possible relationship. Is that correct?

A. Yes.

Q. They go on to say that:

> "Analysis of predetermined five year periods showed that the excess in the Hinkley Point area was concentrated in the 10 years 1964-73 after commissioning of the station, at a time when rates in the rest of Somerset were close to national average. In particular the nine cases occurring in the five years 1969-73 were about four times the number expected from national rates.... Rates in the area after 1973 were fairly low, especially as compared with the rest of Somerset. In the five years 1959-63, before it was commissioned, rates throughout Somerset .... were higher than the national rates."

and they say that the findings should be interpreted with caution.

You would agree, I take it, with the comment at the bottom of 289 on the right, that clusters of cases can quite easily be established if geographical boundaries are selected after inspecting the data?

A. I would, indeed.

Q. We would all agree with that. Over on to page 290, one finds in Table 1, where they set out figures for leukaemia and NHL separately and then lump those figures together for the whole of 1964-86 period. Correct?
A. Yes.

Q. And it is when they are lumped together that one gets the excess of 19 compared with 10.42?
A. Yes.

Q. The point they make that it may simply reflect increased rates in the whole of Somerset is a point which they make at page 291 in the left-hand column?
A. Yes, they cannot make it for the period that they state. It could not be for the period after 1973 - sorry, no, the period 1969-73.

Q. That is the earlier period?
A. The 1969-73 period, the rates were not very different in Somerset.

Q. No, that is correct, and they say, I think, under the "Changing time periods" part on 291 on the right:

> "Table IV gives the results of the 12.5 km area for the conventional five year periods. Standardised registration ratios are given for this area and Somerset health district residue based on national rates.... The excess cases for the 12.5 area km area were concentrated in the 10 years after the commissioning of Hinkley Point in 1964-73...."

and one finds that, over the page, in Table 4.

MR. ROKISON: I hope your Lordship's is better than mine. Mine is awfully difficult to read.

MR. JUSTICE FRENCH: My Table 2 is really illegible and Table 4 is very, very barely legible.

MR. ROKISON: My Lord, if one can just highlight - and we will try and get your Lordship a better copy of that. Mine is similarly a rather poor copy, but one finds in Table 4 that there are set out, my Lord, the different time periods of 1959-63, 1964-68, 1969-73, 1974-78, and then 1980-83 and 1984-86. One finds that it is within the period of 1969-73, which is the third period down, your Lordship sees that there are 7 observed, and the standardised registration ratio there is 4.01, I think?

THE WITNESS: Yes.

Q. MR. ROKISON: That is 1.61 to 8.24 confidence levels. On page 292, just below that table, they make the point in that paragraph that the small numbers in the series can make just one error alter the significance of

the result considerably. Of course, if that is the case, if the significance of the study depends upon one case essentially, then that would be a matter which should cause one to look at the results with a degree of caution?

A. Yes.

Q. Would you agree that that would be so with any study? If you were, for example, to remove one extreme case, if that were to have a significant alteration on the results of the study, would you conclude that the results should be looked at with a degree of caution?

A. With a degree of caution.

Q. They say, at 292 on the right, near the bottom, that:

> "Whether viewed in isolation or in conjunction with results for other nuclear installations, there is no ready explanation for the results relating to Hinkley Point."

On 293:

> "The suggestion that there is an association between some nuclear establishments and a raised incidence of leukaemia in young people in their vicinity is not a suggestion of a cause and effect relation."

Do you have any comment about that?

A. What I think they meant was "is not a proof of a cause and effect relation". To say "suggestion of", that is clearly nonsense.

Q. The point I am making is that an association does not necessarily prove causation?

A. Entirely so, and I think "proof" would be the word they should have put there.

Q. They refer to the analyses of Sellafield and Dounreay and they conclude, in relation to Hinkley Point, that radioactive waste disposal from Hinkley Point contributes a small fraction of total exposure, and they say, though they have not undertaken a complete analysis, it would be likely that the release would have needed to have been thousands of times the annual reported releases in order for the environmental pathway to explain the excess?

A. Assuming that the dose response model is correct.

Q. Yes, indeed, and they say:

> "Though radiation is at present the only known cause of childhood leukaemia, it is almost certain there are other causes and, indeed, many factors may need to be present."

A. Yes.

Q. "Probably at least some types of leukaemia have a virus implicated in their aetiology. A recent study has given some credence to the theory that the high incidence of leukaemia in young people near some nuclear installations could be the result of a large workforce coming together to build the installations, or introducing viruses to a previously isolated community with particular susceptibility."

That again is a reference to Kinlen's work, is it not?
A. Yes. I would agree with that as a possibility.

Q. Quite, and I think you do in your report, and I am going to ask you to look at Kinlen with me this afternoon. They say:

"There are other possible causes for the pattern of cases of leukaemia found in this series - for example, exposure to some unknown toxin," and so on.

And they make the point that nuclear establishments have many things in common apart from radioactive emissions and that the observed association may not have a direct cause and effect basis. So it is a cautious study?
A. Yes.

Q. Which points to a small excess and draws no very definite conclusions from it?
A. No.

Q. Would you agree?
A. I would agree.

Q. Can I just refer you before my Lord rises for lunch to two letters that were subsequently written in relation to that study, which were published in the BMJ in August of 1989, the first by Alexander et al and the second by Taylor.

MR. ROKISON: My Lord, those are referenced in Prof. MacMahon's report, so they are not in the Common Bundle and they are MacMahon Reference 1.

MR. JUSTICE FRENCH: How does that piece of information you have kindly given help me? It is in MacMahon 1?

MR. ROKISON: My Lord, the bundles of references, one has a Common Bundle.

MR. JUSTICE FRENCH: I have got that.

MR. ROKISON: Which are those that are referenced in more than one report. Then, in addition, each of the witnesses has his own additional references, and this is the first reference of Prof. MacMahon.

MR. JUSTICE FRENCH: Thank you. Is there some way of enabling my note to reflect that we are not looking at a common bundle?

MR. ROKISON: The difficulty, so far as your Lordship's note is concerned, is that if, for example, one were to say MacMahon 1, your Lordship might think it was Prof. MacMahon's first report. One might call it MacMahon Reference 1, if that would help, my Lord.

MR. JUSTICE FRENCH: Let us try that. Thank you.

MR. ROKISON: The only difficulty I raise in relation to that is, of course, that when one gets to any expert's reports, they have listed at the back a number of references and they are....

MR. JUSTICE FRENCH: I think I can persuade myself not to look at the list of references at the back.

MR. ROKISON: That is all right then. It is MacMahon Reference 1. That is fine, because within the individual bundles there are spaces for their references which are in the Common Bundle, so it is MacMahon Reference 1.

MR. JUSTICE FRENCH: And the document we are going to is....?

MR. ROKISON: There are two letters which follow each other, my Lord, which were published in the BMJ on 26th August of 1989.

MR. JUSTICE FRENCH: How are they flagged or numbered, as the case may be?

MR. ROKISON: It is 1, my Lord. It is MacMahon 1. They should be there as Reference 1 of the MacMahon references.

MR. JUSTICE FRENCH: So it is the letter headed "Incidence of leukaemia"?

MR. ROKISON: Yes, "Incidence of leukaemia in the vicinity of Hinkley Point".

MR. JUSTICE FRENCH: Reference 1, item 1.

MR. ROKISON: Yes, and the first letter, my Lord, being the Alexander et al. You will see that the same names appear very frequently. This was Alexander, Cartwright, McKinney and Ricketts from the Leeds Leukaemia Research Fund Centre, and they wrote in response to the Ewings paper and expressed, as your Lordship sees at the first break - do you see that, Prof. Evans - unease about any implication that the phenomenon was associated with the nuclear power stations. The

point that they are making, which is a point which I draw to your attention because it is a point which one will refer to again in relation to Alexander:

Q. Can I ask you to look at the second column, the middle column, and the bottom paragraph, where they say this:

> "We would modify the authors' conclusions as follows: it is no longer a question of whether raised incidences of leukaemia occur in Somerset, but why? Their study has presented some intriguing but ultimately uninterpretable results. Some aspect of the Hinkley Point plant or the environment nearby may be causally involved, but the evidence for this is weak. The current results are consistent with a random distribution within Somerset of an excess number of cases, and so undue concern about Hinkley Point should not be aroused."

Would you disagree with that?

A. No.

Q. "The future plans of researchers from Somerset Health Authority are to be welcomed. Larger sets of data covering other areas will provide increased statistical power and the ability to identify meaningful geographical associations. Our analyses of the incidence over half of England and Wales show that areas of high socioeconomic state and those adjacent to estuaries have higher rates. We plan to perform further analyses...."

Are you familiar with Alexander's work in relation to high socioeconomic areas?

A. Yes, I think some of it is published in the Draper report that we referred to.

Q. Right. We can come back to that. Before we do, may I ask you to look at the second letter, which perhaps raises a more important point in relation to the relevance of the Hinkley Point excess, if there was one, to the current litigation, and this is the letter from Taylor of the Health and Safety Department of the Central Electricity Generating Board.

MR. ROKISON: It is the very next letter, my Lord. It follows on immediately from Alexander.

MR. JUSTICE FRENCH: On the same page?

MR. ROKISON: Yes, my Lord.

MR. JUSTICE FRENCH: "Sir, I would like...."

MR. ROKISON: Yes, indeed, and reads as follows:

Q. "I would like to draw to your attention several points regarding leukaemia in west of Somerset that

were not drawn out in the paper by Ewings and colleagues but emerge from analysis of the data in the report to Somerset Health Authority....

> Firstly, the authors point out that the incidence of leukaemia is high at all ages throughout Somerset and there is no evidence of clustering of the disease. The generally high incidence is observed for all types of leukaemia, including chronic lymphocytic leukaemia, which is not thought to be associated with exposure to radiation. Secondly, during 1964-86 there were 19 cases of leukaemia and non-Hodgkin's lymphoma in young people resident within 12.5 km of the power station compared with the 10.4 expected from national registration rates."

That is the excess to which we have looked?

A. Yes.

Q. "Data presented in the health authority's report indicate that of the 19 cases, nine occurred in children under the age of 15 and 10 in young adults aged 15 to 25. The exact population in these age groups residing within the 12.5 km circle are not available from the paper or report so exact values of the expected numbers of cases cannot be found. If we assume that the proportions in each age group reflect those within the whole county at the 1971 and 1981 censuses and if we use the proportions of registrations that occur in these age groups we obtain the following: at age 0-14 there were nine observed cases compared with 7.3 expected (not significant); at age 15-24 there were 10 observed compared with 3.1 expected," with that low P value.

"Thus the significant excess occurs in young adults, not children. It would be interesting to know the exact diagnoses."

Is that assumption a reasonable working assumption in order to consider this point?

A. I think this is a very dangerous approach because this is the inverse of the TV approach. This is the CEGB approach to try and show that it does not exist and, if you torture the data sufficiently, it will confess in one direction or another, depending on who the torturer is. So I think that selecting the data and saying that all the cases are there, I do not think this is sufficient really. It may be a point, it may be a very valid point, but I think that he has picked - and I take it that it is a he - he has picked 1964-86 and looked at the 19 cases. He could have picked some other different time period and looked at things in a slightly different way. So it may be that the important excess is occurring in young adults and not children. It may be important to know the exact ages.

Q. You say he has picked the '64-86, but it is the case, is it not - and that was Ewings' starting point. I mean, Ewings' starting point was that between 1964 and 1986 there were 19 as against 10.4 expected?
A. Yes.

Q. And he is merely taking that and demonstrating that, taking the assumption as to the proportions in each age group which there were within the county censuses for those two years, 1971 and 1981....?
A. But the point that I think is really much more important - I am sorry. Maybe I should not be doing your work for you - is, if it is so, that the 13 that occurred in patients born before 1964....

Q. I was coming on to that. Yes, you are not doing my....?
A. But I think the other point is rather weak.

Q. You say the point is rather weak. It may be weak, but it is a reasonable working assumption that the balance of the ages of the population would be comparable to those shown in those two censuses?
A. Yes.

Q. And, if that is so, whether it is a weak argument or not, what it does demonstrate is that it appears that that excess, which, after all, is the excess which you yourself refer to in paragraph 39 of your report as being statistically significant?
A. Yes.

Q. That that excess is an excess which appears to be an excess primarily within the 15-24 years age group rather than in the 0-14s?
A. Yes.

Q. But the second point, as you say, which, quite rightly, is perhaps the important point for the purposes of this litigation, is that, of those 19 cases, 13 occurred in patients born before 1964, the year that Hinkley Point started operations. They say:

> "This is important in view of speculation that a route involving exposure of the unborn child to radiation is responsible for the excesses of leukaemia observed at, for example, Seascale."

If that is correct, this is an example where you have, as you had on one view of things in Dounreay, an apparently statistically significant excess in the vicinity of a nuclear installation but, if this data is right, one that could not be explained by the Gardner hypothesis?
A. No, I am sorry, you are wrong there. Those 13 patients could have been Atomic Energy Authority employees or somebody to whom it was exposed. We do not have the data on it. They may have had radiation exposure, but it will

not necessarily have come from Hinkley Point. So I think that you may be right and the assumption might be entirely reasonable, but I do not think that we know from that particular letter.

Q. Yes, I follow that, that they may have had some exposure but it would not have been exposure from Hinkley Point?
A. Entirely correct.

MR. ROKISON: I think we can leave it there. My Lord, I am sorry that we overran.

MR. JUSTICE FRENCH: "13 cases born before 1964, before Hinkley Point started, may have had previously irradiated parents. We do not know." Do we know how many of the young adult sufferers were NHLs and how many were leukaemias?

MR. ROKISON: Yes, we do. My Lord, it is Table 1 on page 290.

MR. JUSTICE FRENCH: Page 290 of what?

MR. ROKISON: Page 290 of Ewings, my Lord, which was the paper that we first referred to, but it is not split into age groups.

THE WITNESS: That is E 77.

MR. ROKISON: And it is both in Table 1 and, indeed, on....

MR. JUSTICE FRENCH: I do not terribly want to go to the tables if somebody can tell me the answer. If you cannot tell me the answer, then, for the time being, at least, forget it.

MR. ROKISON: What is your Lordship's precise question?

MR. JUSTICE FRENCH: I would like to know, if the information is available, how many of the young adults, the 15-24, were NHLs and how many were leukaemias?

MR. ROKISON: That I do not think we can tell you. We can tell your Lordship that, of the 19 cases, that six were NHLs and 13 were leukaemias, but I do not think they are divided by reference to age.

MR. JUSTICE FRENCH: If you cannot do it, you cannot do it. Thank you.

MR. ROKISON: We will look to see if there is any other data from which we can get that information. My Lord, I am sorry I overran.

MR. JUSTICE FRENCH: It is not your fault. I could have stopped you at one o'clock had I felt it proper to do so.

MR. ROKISON: Having made a shambles of Aldermaston, I thought we could at least get through Hinkley Point, my Lord.

MR. JUSTICE FRENCH: Yes. Ten-past two.

(Luncheon Adjournment)

Q. MR. ROKISON: Prof. Evans, I think we had finished all I wanted to ask you about Hinkley Point. May I just come back briefly, and it will be briefly, to Aldermaston and Burghfield? I think I can put the points that I want to put to you really pretty shortly. It arises from your report where you say in paragraph 37 on page 15 that the Committee reviewing the studies you there set out concluded that there was a small but statistically significant excess within 10 kilometres of Aldermaston and Burghfield. What I really wanted to clarify with you, in so far as we can from the published literature, is what seems to be accounting for that statistically significant increase. May I ask you to just come back - and I think most of it can come from this, if not all - to the Roman paper, which is R250? Could you look particularly at page 600 and Table VI? I put to you this morning and you agreed that from that Table it appeared that in relation to Aldermaston the excess was not statistically significant but in relation to Burghfield it was?

A. Yes.

Q. We can see, and it perhaps arises from that, that in the number of cases in particular where you are dealing with the area between 5 and 10 kilometres, the bulk of the cases are found under the Burghfield column rather than the Aldermaston column?

A. Yes.

Q. One has 38 cases 0-14, including 27 0-4 within that area?

A. Yes.

Q. You will see that there is a note at the bottom which says that it includes data for nine electoral wards that lie within 10 km of both the establishments at Aldermaston and that at Burghfield where five registrations were observed at age 0-4, 1.08 expected, and five registrations 0-14, with 2.38 expected?

A. Yes.

Q. If you look up to the map in the top left, one sees the wards there set out, and effectively what they are referring to is the piece in the middle which is intersected by both the circles?

A. Yes.

Q. If one just does a little bit of arithmetic using that information, if one were to take Aldermaston alone, that is, if you look up at the map, what I might call the

Aldermaston crescent being the section which is not intersected by the Burghfield circle, so that it would appear to be, if you look at the map, in area terms, although that could be very misleading, to be about half of the Aldermaston circle?

A. Yes.

Q. But it is the western bit?

A. Yes.

Q. If you were to take Aldermaston and take 0-4, you have seven cases, 3.13 expected, right?

A. Yes.

Q. If you then subtract the 5 as against 1.08, which is common, there you get 2 to 2.05?

A. Yes.

Q. So that there it is virtually what you would expect, a relative risk of about one?

A. Yes.

Q. MR. JUSTICE FRENCH: I am sorry, what is the figure again?

MR. ROKISON: My Lord, it is 2 compared with 2.05, observed as against expected. What I am doing - and I hope I made it clear to your Lordship - was subtracting the section which is common to Aldermaston and Burghfield from the Aldermaston figures.

MR. JUSTICE FRENCH: Having subtracted those in the "common" or overlapping segment?

MR. ROKISON: Yes.

MR. JUSTICE FRENCH: Yes, I follow.

MR. ROKISON: So just to make the point, as I was seeking to do with Prof. Evans, if one were to look at the map which is up on the top left-hand corner, one is effectively looking at that rather broad crescent.

MR. JUSTICE FRENCH: Yes, I have got that point.

Q. MR. ROKISON: Similarly, if you do an exercise for 0-14 and you subtract five cases, 2.38 expected, from 8.41, you come up with three against 6.03, so you would have a deficit of cases in 0-14 within that crescent, would you agree?

A. Yes.

Q. If by contrast you take the Burghfield figures, 0-4 for Burghfield alone, which is what I might call the Burghfield crescent, has by similar process 22 as against 11.11?

A. Yes.

Q. 0-14 has 33 as against 21.48?
A. Yes, I think you are correct.

Q. I think I am. I have done the arithmetic.
A. Yes.

Q. What that shows you is that if one were to take Aldermaston alone, it would be odd if there were to be a significant (in a broad rather than a scientific sense) excess resulting from Aldermaston activities in the common area of where the circles intersect, bearing in mind the Aldermaston figures in respect of the wide crescent of Aldermaston?
A. Yes.

Q. It would be somewhat odd if there were an effect in a sense on one side of Aldermaston and not the other? It is possible but it ---
A. It depends on the mechanisms. If we are talking about environmental pollution, it may well depend on the prevailing wind and you may well expect to find it to the east of Aldermaston and not to the west. If it were dependent on paternal exposure, it would depend on where people lived who were employed at Aldermaston.

Q. I quite understand that.
A. Why you are making such a big point about a circle in this way in great detail, I don't quite see the point.

Q. I am not making a point about a circle. What I am simply drawing attention to is that what one can see from that figure is that one can isolate the observed as compared to the expected cases of 0-4 and 0-14 in the area which lies effectively to the west of Aldermaston and, as one sees, there are very few cases and there is no excess?
A. To the east of Aldermaston.

Q. To the west of Aldermaston?
A. To the west of Aldermaston, yes.

Q. The only area in this whole picture of the two 20 km circles where one finds a large number of cases is in the Burghfield crescent, that is the area which is peculiar to Burghfield and is not within 10 km of Aldermaston?
A. Yes.

Q. That Burghfield crescent, as one sees from the map, is a crescent which appears to embrace the town of Reading?
A. Yes.

Q. MR. JUSTICE FRENCH: Just a moment. The solely Burghfield crescent embraces part of Reading?

MR. ROKISON: I think it is the whole of Reading.

MR. JUSTICE FRENCH: I thought there was some doubt about that certainly expressed by Prof. Evans.

MR. ROKISON: I think you had observed that the small black area appeared to be to the south of Reading in relation to Fig. 2

Q. MR. JUSTICE FRENCH: Does it embrace the whole of Reading or not? It must be establishable.

MR. ROKISON: I think it does, my Lord. It certainly includes part or the whole of Reading. I am not sure whether it includes the whole of Reading.

MR. JUSTICE FRENCH: I will say "embraces part or all of Reading". There is the only independent excess, is that how you are putting it?

MR. ROKISON: My Lord, yes, that is where one finds the excess.

MR. JUSTICE FRENCH: There is where one finds the only excess.

Q. Do you agree with that proposition, Prof. Evans?
A. I do.

MR. JUSTICE FRENCH: Thank you.

Q. MR. ROKISON: It is merely this: it is not a criticism, but people do and have in the papers that we have looked at, and indeed in your report, talked about the Aldermaston and Burghfield excess, but it would really be more accurate to refer to it as the Burghfield excess, would it not?
A. That is putting a particular interpretation on it, and I don't know that I would wish to. I think the way the Roman paper has been written was not particularly looking at a crescent-shaped area around Burghfield. To have picked on that after you have seen the data is not a reasonable one. What counts as far as I am concerned is Table VI, in which, by their definitions, there was a raised rate of 2.8 less than 5 km from Aldermaston.

Q. MR. JUSTICE FRENCH: Wait a minute. There was a raised of 2.8?
A. A raised ratio in Table VI.

Q. Within 5 miles or kilometres?
A. Kilometres.

Q. Within 5 km of Aldermaston?
A. Yes.

Q. MR. ROKISON: But that is a raised ratio based on only three cases and is not statistically significant?
A. No.

Q. You agree?
A. Yes.

Q. May I come back again - and I apologise for being persistent on this - to this: you have referred to a statistically significant excess within 10 km of Aldermaston and Burghfield, and I suggest to you that that is not quite accurate, that there is a statistically significant excess within 10 km of Burghfield but not Aldermaston?
A. I agree.

Q. Thank you. You know, of course, that Burghfield is an ordnance factory, is it not?
A. Yes.

Q. So you are not going to get much in the way of environmental discharges from an ordnance factory, are you?
A. I don't know. I imagine not.

Q. You say that you do not know. May I just refer you to two short passages, one in the Cook-Mozaffari 1987 study, which is C42, where at page 2 to 3 ---

Q. MR. JUSTICE FRENCH: This is Arabic rather than Roman?

MR. ROKISON: Yes, my Lord. Your Lordship sees paragraph 1.4 "Nuclear Installations in England and Wales"?

MR. JUSTICE FRENCH: Yes.

MR. ROKISON: Your Lordship will observe that they are depicted in Fig. 1, if you turn over the page, and I think your Lordship has already had a copy of that coloured up.

MR. JUSTICE FRENCH: Yes.

Q. MR. ROKISON: What you can see, Prof. Evans, is that they are marked there, Harwell and Burghfield are marked there, but what they say on page 3 in relation to the ---
A. I am sorry, I must have the wrong document.

Q. Have you got C42?
A. I am at C44.

Q. C42 is a volume on its own.
A. Yes, indeed, I am sorry.

Q. It is very confusing.
A. Page 3?

Q. Page 3. Can you see on page 3 in the first full paragraph:

"A map showing the position of all the installations that have been mentioned is given in Figure 1. The

CEGB power stations at Hartlepool and Heysham are not included in the present study because they became operational after the latest year for which cancer data are considered. The UKAEA research establishments at Risley and Culcheth and the Royal Ordnance Factory at Burghfield are not included because discharges are negligible (several orders or magnitude below those from the CEGB nuclear power stations)."

A. Yes.

Q. I need not refer you to it, but just to give my Lord the reference, we are in the COMARE 3 paper, the reference being at page 16, where there is a comparative discussion of the discharges of Burghfield, Aldermaston and Harwell, from which indeed appears that the discharges from Burghfield are more than an order of magnitude less than those at Aldermaston. Do you want to check that, Prof. Evans? It is the COMARE 3 paper, page 16.

A. Yes.

Q. Do you have it?

A. Yes.

Q. Can you see the reference on page 16 to the discharges from those three installations?

Q. MR. JUSTICE FRENCH: Which paragraph of COMARE?

A. 2.30.

MR. JUSTICE FRENCH: Thank you.

Q. MR. ROKISON: Where it says this:

"The results are described in detail in the NRPB report and are summarised in Annex 2. They show that the estimated peak annual doses to adults living 5 km from Aldermaston, Burghfield and Harwell were 0.0017%, 0.0000003% and 0.03% of natural background radiation, respectively. The doses delivered to the foetus and infant were even lower".

So it does appear that as far as Burghfield is concerned, which is the area in relation to which there was a small statistically significant excess, the discharges to the environment were minuscule, and at an ordnance factory you would not have the sort of picture that you painted yesterday of the brave young scientists taking their badges off because they wanted to get on with their work and not be too much exposed, would you?

A. One would hope not at any of the places. May I draw your attention to the fact that my words there in the bit that you are grumbling to, that last sentence is almost exactly the words of paragraph 4.1 from COMARE 3.

Q. Sorry, which passage are you referring to?

A. You have taken a lot of time over my last sentence of paragraph 37 of my first report. Paragraph 4.1 of COMARE 3 on page 30 reads:

"There is a small but statistically significant increase in registration rates for childhood leukaemia in the age group 0-14 over the period 1972-1985, in the areas within 10 km of AWRE Aldermaston and ROF Burghfield, compared with both the national rates, and the regional rates for Oxford and Wessex".

Q. I agree that that is what they say, and that is why I took you to the study in order to demonstrate that although people often talk about the excess round Aldermaston and Burghfield, upon analysis it is restricted to Burghfield?
A. But COMARE 3 looked at the data in rather more detail than did Roman in the paper you have drawn our attention to.

Q. Did they?
A. Yes, they looked at the data with different geographical spread and they used OPCS data.

Q. MR. JUSTICE FRENCH: Can we pause for a moment for me to make a note? This is 4.1 at page 30 of COMARE 2 or 3, I forget?
A. This one is 3, my Lord.

Q. You say that COMARE looked at the data more closely than normal?
A. Yes, in a slightly different way, was my understanding, in that they used the Oxford data base, paragraph 2.8.

Q. MR. ROKISON: They looked at both, did they not?
A. They looked at both combined.

Q. If you look at page 27, paragraph 3.45, where they are dealing with relationship to other investigations, they say:

"We have noted that the Roman et al and the CCRG data show that the incidence of childhood leukaemia and other childhood cancers is raised in the ten kilometre circles around Aldermaston and Burghfield in children aged 0-14".

So they are not, in effect, doing the analysis that we have done by looking at the data and looking at the circles and discovering that the true excess is not round Aldermaston at all but is round Burghfield, and that is why I took you to it; that they have fallen into - maybe it did not matter for the purposes of their study - the same error of looking at Aldermaston and Burghfield together simply because their circles intersect, is that not right?
A. That's possible.

Q. In your paragraph 38, you point out that ---

MR. JUSTICE FRENCH: How many volumes can I clear away now?

MR. ROKISON: My Lord, you can clear them all away now.

MR. JUSTICE FRENCH: Thank you. I hesitate to use again the expression "put away"!

MR. ROKISON: Yes, my Lord.

Q. You again raise the point in paragraph 38 of your report that although it is said that the doses into the environment would appear to be too low to account for the childhood cancer excesses, you refer back to paragraph 21 which is where you said it all depends on the accurate measurements of doses in the environment and the dose response model that you are using?
A. Yes.

Q. But you are not suggesting that either of those could be so wildly out that the discharges from Burghfield could account for the excess, are you?
A. No. I simply don't know.

Q. They would have to be massively inaccurate, would they not, in order to account for the excess, bearing in mind the figures we have just looked at in COMARE 3?
A. They would.

Q. MR. JUSTICE FRENCH: Do we in fact know how many of the cases, whether they be excesses or not, had "worker parents"?
A. No.

Q. We do not?

MR. ROKISON: No, my Lord, we do not.

Q. MR. JUSTICE FRENCH: "I do not know whether the discharges from Burghfield could possibly account for the excesses. Those to the environment are minuscule"?
A. Yes.

MR. ROKISON: I think Prof. Evans did agree that he was not suggesting that the doses measured or the dose response could be so massively out as to account for the Burghfield excess.

MR. JUSTICE FRENCH: I do not think I need know that.

MR. ROKISON: Very well. It will be in the transcript anyway.

MR. JUSTICE FRENCH: I would regard that as a legitimate question but an obvious proposition.

MR. ROKISON: I am relieved at that, my Lord, thank you.

Q. We can leave Aldermaston and Burghfield. We have dealt with Hinkley Point, and you now draw your conclusions concerning the geographical studies. We can therefore step back and try to look at them all. We reviewed first thing this morning the general studies around nuclear installations. So far as the particular ones which you have highlighted in your report are concerned, the position is that the Dounreay excess may or may not exist depending on where you draw your parameters?
A. Yes.

MR. JUSTICE FRENCH: Shall we pause after each answer, because this is really summing-up a large slab of the cross-examination?

MR. ROKISON: Yes, my Lord.

Q. MR. JUSTICE FRENCH: Dounreay may or may not show an excess. It depends on where you draw your boundaries.

Q. MR. ROKISON: So far as Hinkley Point is concerned, there is, as you say, a small statistically significant excess over a limited period of years?
A. Yes.

Q. But if the information in the Taylor letter is correct, then paternal irradiation exposure at Hinkley Point cannot explain the excess?
A. Yes.

Q. So far as Aldermaston, Burghfield and Harwell are concerned, you do not suggest that there was an excess at Harwell, is that correct?
A. I suggest that COMARE said that they didn't look sufficiently at the data there. That is my recollection. Certainly Roman didn't look at the data north of Harwell. I don't think there is any excess at Harwell, no.

Q. The Cook-Mozaffari study that we mentioned this morning, Table 5, shows that there was not?
A. There was no, not.

Q. There was a deficit?
A. Yes.

Q. So there is no excess at Harwell; there is no significant excess round Aldermaston?

Q. MR. JUSTICE FRENCH: Agreed or not agreed?
A. That depends on where you draw your boundaries.

Q. MR. ROKISON: The only boundary that has been drawn is the 10 km circle?
A. Yes, there is no significant excess at Aldermaston.

Q. MR. JUSTICE FRENCH: No significant excess at Aldermaston?
A. Statistically significant, that is.

Q. MR. ROKISON: There is a significant excess ---

A Q. MR. JUSTICE FRENCH: Again statistically?

Q. MR. ROKISON: There is a small but statistically significant excess ---

MR. JUSTICE FRENCH: I think maybe in this field we must use "statistically" whenever it is appropriate. I agree that shorthand is happier in most instances, but it
B is rather important in this field.

Q. MR. ROKISON: It is a small but statistically significant excess in the Burghfield crescent, as I called it, which embraces Reading?
A. Yes.

Q. Is that fair?
C A. Yes.

Q. Which could not be accounted for by environmental discharges from Burghfield?

Q. MR. JUSTICE FRENCH: Agreed?
A. By known environmental discharges from Burghfield.

D Q. MR. ROKISON: As you said yourself, being a Royal Ordnance Factory as opposed to an installation which either generates power or makes weapons or whatever, you would not expect that the environmental discharges would be substantial?
A. You would certainly hope not.

E Q. You would not expect that they would be substantial? Of course you would hope not, but you would not expect that they would?
A. I don't know what you mean by "expect" in those circumstances.

Q. MR. JUSTICE FRENCH: Do you know about this?
A. I certainly don't know about any, no.

F

MR. JUSTICE FRENCH: I think he is simply saying "I prefer not to answer".

MR. ROKISON: I think that summarises the main points, my Lord.

G Q. I would respectfully suggest that the conclusions that you reach in your report are perhaps rather too broad and perhaps not too generalised and not very accurate - perhaps I can put it that way - would you agree, with your paragraph 40?
A. No, I wouldn't agree, and I think that others have summarised things in that way, certainly in a general way.

H

Q. MR. JUSTICE FRENCH: Anyway you stand by your para. 40?
A. I stand by para. 40.

Q. MR. ROKISON: I suggest that having looked at the studies, one really cannot generalise and that there is a wide diversity and no general excess. Would you disagree?
A. I am not aware that anyone has attempted to put all of the studies together.

Q. You have in your report?
A. Yes, but I meant in a formal way and in a way that, as I say, COMARE 3, I have taken their conclusions from their paragraph 4.1, you have disputed them very clearly, but I think that there are others who would say that that's where they would begin from, that there is some evidence - not entirely convincing evidence - of excesses in several of these places, and that that is why there has been expressed concern within COMARE and why people have gone on doing these studies.

Q. I would not disagree with that at all, Prof. Evans. I agree that that may be your starting point. I agree that there is some evidence that there may be excesses in some of these places. What I was merely suggesting is that having gone through the exercise, which we have at some length, of looking at the available material and all the material which you have referenced in relation to the UK installations, the conclusion one should reach is that one cannot generalise and that there is no general excess but there is a wide diversity?
A. No, I didn't say that there was a general excess. I said "excesses of ..." I think the way you have approached it, because that is clearly part of your job, to dissect each individual study and to pick on areas that are crescent-shaped and so on, is the inverse of the media's approach to it, and I think if you look at any particular study you can find difficulties and problems with it. I think my view would be that the general view is that there is some evidence for excesses, but I think that trying to say that in every specific instance there is an excess is certainly something which, when you dissect each individual study, leads to some questions that could arise on a particular study; but as a scientist you try and take a broader view as a whole, and that is what I was attempting to do in the section f4 and those paragraphs.

Q. I would suggest as a scientist you should attempt to be as precise as you can?
A. I would agree.

Q. I would suggest that although it may be that if, for example, a television news programme looking for news finds what appears to be a cluster, then they may draw their parameters round the cluster?
A. Exactly.

Q. It is not the same, is it, to look and see what the data show, and to draw conclusions from that, such as the conclusion which I invited you to draw and which you eventually agreed with, namely, that the often talked about excess around Aldermaston and Burfield, on analysis, is not around Aldermaston at all? That is a perfectly legitimate exercise, isn't it?

A. What you have said is the statistically significant bit isn't around Aldermaston. There are relatively few people there. So the power of any study to detect it will be relatively small, but having defined your boundaries beforehand there was an excess, a relative ratio of more than 2. The fact it is not statistically significant is something that you wish to emphasise greatly and I think that my rather more broad brush approach in looking at it in that way, says that there are excesses.

Q. I think it will not be profitable for us to continue this...

MR. JUSTICE FRENCH: I think the battle lines are plain enough, Mr. Rokison.

MR. ROKISON: Indeed, and I was just about to say the same thing:

Q. May go on to the next part of your evidence? At last we come back to Sellafield and Seascale. First of all, in your paragraph 44 onwards you discuss briefly the birth and schools cohort studies. Now may we look first of all, as you did I think, at the schools cohort study, which I think you may find...

A. G.88.

Q. Thank you. As you point out this was a study which was one of those of which the Black Committee had recommended should be carried out?

A. Yes.

Q. It is recommendation 3, page 93, in Black:

> "A study should be considered of the records of school children who have attended schools in the area."

For this purpose they took the period 1950-1986?

A. Yes.

Q. It appears from page 820, second paragraph:

> "Children who had attended the other three schools, and who were born since 1 January, 1950 as in the Seascale birth cohort, were included in the study."

The result of that was that there was no excess of deaths in that study population. Indeed, there was a deficit of 0.79. There was no excess of cancer deaths.

Indeed, there was a deficit - a ratio of 0.49?
A. Yes.

Q. There was no case of leukaemia deaths?
A. Yes.

Q. The discussion - of course, this was published at the same time as the birth cohort study, they followed one another in the BMJ, and it largely discussed the comparison between the schools and the birth cohorts. One does find at page 821 on the right at the bottom, they say:

> "Although in the schools cohort overall the expected number of deaths from leukaemia was similar to that in the birth cohort, no deaths from leukaemia were reported compared with five among the birth cohort. The combined figure is five deaths observed compared with 1.07 expected - a ratio of 4.67. The apparent limitation of the high leukaemia death rate to children born in Seascale is notable, although the lower 95% confidence limit for the ratio of the rates in the birth cohort to the schools cohort is 0.93."

Is this similar? It is not the same exercise, but similar to that which you were doing in relation to the McLaughlin study of considering to what extent the results of one's study are compatible with the other?
A. Yes.

Q. So that although one gets this apparently marked difference in the results between the birth cohort and the schools cohort, no doubt because the numbers are so small, that they are actually, statistically, compatible with one another?
A. Yes.

Q. MR. JUSTICE FRENCH: Because the numbers are so small, the results of the schools and birth cohort studies are compatible?
A. Yes. It is not terribly clear what that 0.93 is referring to there, as to which rate it is, I have to confess.

Q. Does the proposition still stand that I read out to you?
A. Yes, but I am not entirely sure whether it is referring to leukaemia or all childhood cancers at that point. I think it is leukaemia.

Q. MR. ROKISON: I think it is talking about leukaemia because the discussion immediately before it where they set out the figures for the birth and schools cohort respectively, are leukaemia death figures.
A. Yes.

Q. So it would appear they are talking about leukaemias there?
A. That is what I believe.

Q. MR. JUSTICE FRENCH: So if I put the complete answer as this: because the numbers are so small the result of the school and birth cohort studies are compatible as regards leukaemia?
A. Yes.

Q. MR. ROKISON: That is despite the fact, is it not, that although Black had recommended there simply be a study of the records of school children who have attended schools in the area, because of the way the parameters were drawn, two of the cases, the Seascale cases listed in Black, were excluded from the schools cohort study?
A. Yes.

MR. JUSTICE FRENCH: From the schools cohort study?

MR. ROKISON: Yes, my Lord:

Q. You may have it in mind, Prof. Evans, but table 2.1, which is where the seven cases are set out, it is cases 1 and 2 - in the Black Report this is...

MR. ROKISON: I don't know whether your Lordship wants to look at that, or I simply give your Lordship the reference. It is the Black Report, page 13, Table 2.1 which sets out the Seascale cases. The first case, case 1, is excluded because the year of birth was 1947 and the study was limited to births after 1950. The second was case number 2, excluded because that leukaemia survived:

Q. Would you agree that if one is doing a schools cohort study, as recommended by Black, and to compare it with the birth cohort study, that it is perhaps unfortunate that two out of the cluster of seven cases who would otherwise have been included within the schools study, were not included because of the way the parameters were drawn?
A. No, I don't think that is necessarily true at all. I don't think to say that it was unreasonable to begin from 1950 onwards when the schools' records were complete, as far as I can tell, that that is an unreasonable thing to do.

Q. What I said was it was unfortunate?
A. No, I don't think it is even unfortunate.

Q. Because this whole exercise, the Black Report, the discussions in the Black Report, the study of the relevant data for the geographic area and so on, the recommendation of further studies, is designed to look at the question as to whether there is an excess and why?
A. Yes.

Q. Given that an excess has been observed and reported and, indeed, listed by Black, I suggest to you that if you are doing a birth and schools cohort study in order to see generally whether it is a factor that might be associated with living in Seascale as a child as opposed to being

born to someone resident in Seascale, it is very unfortunate if you exclude two of the seven cases where they weren't born in Seascale?

A. I don't think that the issue is a question of saying that it is unfortunate you have excluded them deliberately because you have defined your parameters in a different and objective way.

Q. You define your parameters, of course... This is not necessarily a criticism, although it may be it is a criticism of drawing too much from the comparison of the birth and schools cohort so as to thereafter concentrate one's attention on factors surrounding birth. However, of course, these cases were known and the list would have been appreciated at the time when the birth and schools cohort studies were set up?

A. Yes. I imagine in some senses it is a chance finding that case 2, which is in the list there, is the only one alive, and because that paper looked at mortality, then that case would automatically be excluded.

Q. It is not the only way. There is also case 7 which survived but that was Seascale born.

A. Yes.

Q. All I was pointing to was that if you are concerned to look by way of these preliminary cohort studies to see whether it looks like a factor connected with living in Seascale as a child and going to school there, or does it look as though it may be a factor more likely to be connected with birth, that if you are doing that your study may be somewhat distorted if you exclude, because of the parameters you draw, the only two cases who went to school in Seascale but weren't born there? That must be right, mustn't it?

A. Well, I am trying to understand what the meaning of your word "unfortunate" is.

Q. All I am saying is this, and there is no question of saying anybody did something wrong or they were at fault or they were deliberately trying to mislead or anything of the kind, all I am simply saying is if you are setting up two studies as recommended, the cohort studies, in order to give you perhaps a preliminary view to see whether this is likely to be something associated with being a child in Seascale as opposed to being born to somebody resident in Seascale, that you will distort the picture if in your schools cohort you exclude the only two cases who went to school there but who weren't born there?

A. Yes, I think there are some difficulties with that, but recommendation 2 specifically mentioned "born since 1950", and so the whole study is making a recommendation there that you are looking from 1950 onwards and not from 1947. If you start including them you would be open to the accusation that you have been driven in your study by Yorkshire television.

Q. No, we are looking at recommendation 3...

MR. JUSTICE FRENCH: Can I make a suggestion which I hope may be helpful? If one substitutes for the word "unfortunate" the phrase "excluding relevant data", would Prof. Evans find it easier to answer the question?

MR. ROKISON: I don't know, my Lord. I certainly didn't mean anything pejorative by using the word "unfortunate".

MR. JUSTICE FRENCH: Of course not. I think what you are putting to him is: "You are factually, if you do the exercise that you complain of, excluding relevant data." Is that not it?

Q. MR. ROKISON: Is that not true, Prof. Evans?
A. You are excluding data that is in the Black Report, clearly, yes.

Q. That is not quite good enough. If you are carrying out your preliminary cohort studies in order to examine... I put the question again: to get some sort of general view as to whether it looks like a factor which is connected with living in Seascale and going to school there as a young child as opposed to being born there or born to someone resident there. If you exclude from your studies the only 2 cases - and it is 2 out of 7, it is not as if it is 2 out of 700 - you are excluding 2 cases and the only 2 cases who went to school in Seascale but weren't born there, then you are excluding, as my Lord puts it, relevant data for the purpose of the exercise you are carrying out?
A. Yes.

Q. I suggest to you that...

MR. JUSTICE FRENCH: I am bound myself to have a reservation that one of them was outside the study period.

Q. MR. ROKISON: My Lord says one of them was outside the study period.

MR. JUSTICE FRENCH: Is that right or wrong?

MR. ROKISON: As the parameters were drawn for that study, yes.

MR. JUSTICE FRENCH: By Black?

MR. ROKISON: No, my Lord:

Q. If you look at recommendation 3, with respect, Prof. Evans, I would suggest you are wrong?

MR. JUSTICE FRENCH: I shouldn't have said Black. I should have said COMARE. No, it was Black.

MR. ROKISON: But Black's recommendation 3 did not lay down the parameters for the schools cohort study.

MR. JUSTICE FRENCH: Who did?

MR. ROKISON: Those who carried out the study, I presume.

MR. LANGSTAFF: My Lord, it is page 93 of Black.

MR. JUSTICE FRENCH: Would somebody read to me page 93 and then I will decide...

MR. ROKISON: Yes, my Lord, it says this:

"1. **Epidemiological**

Recommendation 1

A study should be carried out on the records of those cases of leukaemia and lymphoma which have been diagnosed among young people up to the age of 25, resident in Cumbria. These cases should be compared with suitable controls in respect of factors that could be relevant to the development of leukaemia and lymphoma.

Recommendation 2

A study should be carried out of the records on all children born since 1950 to mothers resident in Seascale at the time of birth. Its main purpose would be to examine cancer incidence and mortality among those children, including cases which might have occurred after moving from Seascale.

Recommendation 3

A study should be considered of the records of school children who have attended schools in the area."

Then it goes on to other recommendations.

MR. JUSTICE FRENCH: The date 1950 only occurs in recommendation 2?

MR. ROKISON: In relation to the birth study.

MR. JUSTICE FRENCH: Well, somebody born before 1950 ought they really to be included?

MR. ROKISON: My Lord, yes, because what you are considering are cases which have occurred in Seascale of leukaemia. I don't want to argue the case now, but the position is, my Lord, that if it is something to do with birth and if you are looking to see whether it might have anything to do with the Sellafield site...

MR. JUSTICE FRENCH: I have got your point.

MR. ROKISON: ...it makes sense to limit the birth to 1950, but if you are considering people who went to school there it may make sense to limit the period when they went to school from 1950, but not to cut out somebody simply because they happened to be born before 1950.

MR. JUSTICE FRENCH: Well, that may or may not be right. I have got the point and one can argue the toss to and fro all day.

MR. ROKISON: Certainly. I only wanted your Lordship to have the point.

MR. JUSTICE FRENCH: I think I had it quite a while back. I was just trying to reformulate the question in a way that Prof. Evans might find it easier to answer, but I was unsuccessful.

MR. ROKISON: Your Lordship was successful.

THE WITNESS: I answered the question, "Yes" to your...

MR. ROKISON: Your Lordship was successful in assisting me in reformulating the question.

MR. JUSTICE FRENCH: All right, I was successful, but the argument still remains.

MR. ROKISON: Indeed, and I don't want to argue the toss with Prof. Evans on this. It is not appropriate. I merely wanted to put the question.

MR. LANGSTAFF: My Lord, I hesitate to interpose in my learned friend's cross-examination, but my Lord I hope my learned friend will make it clear that he is inviting Prof. Evans really to comment upon an hypothesis rather than an established fact. So far as I know there is no evidence, unless my learned friend has some, that these two children actually went to school in Seascale. They may well have done. One was the son of a draughtsman, one was the daughter of a chemical engineer. My Lord, that is all the information I have about their parentage.

MR. JUSTICE FRENCH: You don't even know they went to school in Seascale?

MR. LANGSTAFF: We don't know. It may be the case, it may not be the case, but it is a hypothesis that Prof. Evans has been asked to comment on.

MR. JUSTICE FRENCH: Yes, I am bound to say that had escaped me. Mr. Rokison will no doubt grapple with it.

MR. ROKISON: Well, we do know that case 2 went to school in Seascale because it says so in this paper. If your Lordship will look, and Prof. Evans, if you look at page 821, half way down on the right, it is said:

"One further case of cancer, diagnosed as acute lymphocytic leukaemia while the patient was resident in Seascale, is known to have occurred among these school children and was included in the Black report (case 2 of table 2.1."

MR. JUSTICE FRENCH: Well, that is one of your points gone for a burton.

MR. LANGSTAFF: My Lord, I am happy to accept that. That had escaped me and I am happy to stand corrected.

MR. ROKISON: What we do know, my Lord, is that as far as case 1 is concerned, that the child concerned was born outside Millom in 1947, was diagnosed as having ALL in 1955 and died in 1956. Therefore, at the age of 9 died at Seascale and one would assume that that child had gone to school...

MR. JUSTICE FRENCH: Well, one may perhaps infer. Well, there it is. If you add 2 extra cases, the study looks very different from if you don't.

Q. MR. ROKISON: The position is, Prof. Evans...

MR. JUSTICE FRENCH: Conversely, if you take out these two cases the study looks very different than if you leave them in.

MR. ROKISON: Well, particularly where you are dealing with a study here where so far as the schools cohort study is concerned you had no deaths from leukaemia, so you were comparing a nil with a 5, whereas had these cases been included it would be 2 to 5.

MR. JUSTICE FRENCH: Yes, I follow.

MR. ROKISON: I think we can move on, my Lord:

Q. I think we can leave the schools cohort study...

MR. JUSTICE FRENCH: Forgive me, before we leave it, to make sure that by my intervention I am not inhibiting the pursuit of something important. Granted that the figures and the shape of the schools cohort study would look very different had those two cases been included, what is the conclusion that you wish to put to Prof. Evans as to the assistance given by the cohorts?

MR. ROKISON: Well, I didn't really particularly want to put any point at this stage, but I will put a question if I may, and it is this:

Q. In the design of the case control study, which is the main study, in a sense, which followed these school and birth studies, that in the design of that study the authors as their first exercise, and that is as far as they have so far got, limited the study to the children of mothers who were resident in Seascale at the time of their birth?
A. Yes.

Q. They did not include within the case control study those children, all children, who had been diagnosed as having suffered from relevant diseases in Seascale?
A. Yes.

MR. JUSTICE FRENCH: I am sorry, Mr. Rokison, to intervene again. I want to be quite clear, we are now talking about Gardner case control study 1?

MR. ROKISON: Indeed we are. We are talking about Gardner case control study 1:

Q. In Gardner case control study 1 the authors of that study started by carrying out a case control study limited to children effectively born in Seascale - or born in West Cumbria. It was, "born to mothers who were resident in West Cumbria."?
A. Yes.

Q. They did not, as a first stage, extend that study to all young people up to the age of 25 who had a relevant disease diagnosed while resident in West Cumbria?
A. No.

Q. As I read to my Lord a moment ago, recommendation 1, which was the recommendation that a case control study should be carried out, did not limit the case control study in the way in which the first Gardner study limited it? That is, by reference to West Cumbrian born children?
A. No. Of course, Prof. Gardner being a member of Black - he was Dr. Gardner at the time - no doubt phrased the recommendation and designed the study.

Q. He may have been in part responsible, but we have seen from further evidence that they say they intend to carry out a wider study in due course?
A. Yes.

Q. However, recommendation 1 was a recommendation that a case control study should be carried out in relation to all young people who had had a relevant disease diagnosed up to the age of 25 while resident in West Cumbria?
A. Yes.

Q. Limiting the study in the first instance to those born in West Cumbria was, it appears, justified, and I don't use that word pejoratively either, but it was justified, it appears, on two bases. One is that the schools cohort

and births cohorts studies had suggested it was more likely to be a factor connected with birth rather than living in the relevant area?

A. Yes.

MR. JUSTICE FRENCH: Cohort studies had suggested the diseases might be connected with birth as opposed to living in West Cumbria?

MR. ROKISON: Yes, my Lord:

Q. The second being that it would be a more difficult exercise and will be a more difficult exercise because it is more difficult to get preconception data from outside West Cumbria?

A. Yes.

Q. What I am suggesting is that so far as the first of those considerations are concerned, that the conclusion it was more likely to be connected with birth rather than living in the area must have been influenced, to some extent, by the parameters chosen for the schools cohort study, which excluded relevant data?

A. No. If I look at table 2.1 and I see that 5 cases are born in Seascale and 2 are outside, then that is sufficient to say that it is more likely that.

Q. Well, one can see that if you look at 2 against 5 you can say it is more likely, but it could very well be that they all have a common factor, namely, something in life in Seascale?

A. It could indeed be, but it is still more likely.

Q. If you look and compare 0 with 5, it looks very much stronger, doesn't it?

A. Yes.

Q. Of course, 0 and 5 was the comparison which emerged from the studies because of the exclusion of relevant data from the schools study?

A. Yes, and noted to be not statistically significant in spite of that.

Q. Indeed and even though they were compatible the one with the other, despite that?

A. Yes.

MR. ROKISON: That is where it was leading to, my Lord.

MR. JUSTICE FRENCH: Thank you.

Q. MR. ROKISON: Of course, it would follow, wouldn't it, Prof. Evans, that if one were to look at all cases of leukaemia and NHL diagnosed in those resident in West Cumbria, whether they were born there or not, it would very much widen the study?

A. I do not know how much but it would widen the study.

Q. Yes, and it would be a matter of speculation how that might affect the study's results?
A. Some speculation, yes.

Q. Let me just take as an example, and it is a hypothetical example, but suppose that cases 1 and 2, who were excluded from the schools study and were excluded from the case control study, but supposing it happened that their fathers had nil or very low doses, but some of their controls, local or area, had very high doses, then it could significantly affect the results of the study?
A. I find that very interesting and that is a point that Dr. MacRae made. I think in regard to case 1 that demonstrates a misunderstanding of the nature of the design and analysis of the case-control study. Case 1, born in 1947, would have how much occupational exposure as a result of Sellafield?

Q. Or indeed anywhere?
A. Or indeed anywhere, would have none, and hence cases also born at that time would also have none and they would be entirely excluded from the analysis, and would make no difference whatsoever to the result. I think that perhaps may even be a misunderstanding of my friend and colleague, Dr. MacRae.

Q. What do you say about case 2?
A. Case 2 is different, obviously. For case 2, I do not know instantly whether that person does have occupational exposure, but undoubtedly it is possible for them to have and for their controls to have.

Q. Either or both?
A. Either or both, and very clearly if they did have occupational exposure and their controls did not, that would increase the findings of Gardner quite noticeably.

Q. And indeed if they did not have occupational exposure and their controls did that could also affect ...?
A. It would decrease it a little as well.

Q. You say it would affect it very considerably if they did but it would decrease it a little if they did not. Is that what you are saying?
A. Yes, because we have relatively few cases who are exposed but we have a large number of control cases who are unexposed and so I can just demonstrate to you by the arithmetic, and if you like, if you are driven to it, feed into the analysis such a hypothetical person. I can demonstrate that if case 2 had parental exposure and the controls did not that would increase the significance of Gardner's findings quite a lot, but if the case in common with lots of others had no exposure, and the controls did have exposure, it would decrease the findings of Gardner, but only slightly because there are lots of cases, lots of case-control sets like that. This is a statistical issue.

MR. JUSTICE FRENCH: Can we pause? I am trying to note this exchange and then check its accuracy.

Q. What I have written is this, for correction please: if case 2 had parental exposure and control did not, that would increase Gardner probability considerably?
A. It would increase the statistical significance. The P value would actually fall a little, fall somewhat.

Q. That would increase Gardner's statistical significance considerably?
A. If the dose was particularly high.

Q. Depending on dose?
A. Depending on dose.

Q. The P value would fall slightly?
A. May fall quite a bit.

Q. The P value may fall quite a bit. If case 2 had no parental exposure that would diminish Gardner's statistical significance a little?
A. If at least one of the controls did have a dose?

Q. If case 2 had no parental exposure but at least one of the controls did it would diminish Gardner a little?
A. Yes, but not by as much as the exact inverse would increase it, so it is an asymmetric situation which Mr. Rokison, I think, did not believe me ....

Q. MR. ROKISON: I take the point you are making and I accept it.

Q. MR. JUSTICE FRENCH: But not by as much as the inverse would increase it?
A. Yes, and case 1 makes no difference whatsoever.

Q. Yes, I have got that.

Q. MR. ROKISON: This reflects the fact, doesn't it, that where you have a case control study which depends on a small number of cases, that either putting in or taking out of the study one case with a substantial dose may have a significant effect upon it?
A. It may do, yes.

MR. JUSTICE FRENCH: I am not noting that answer because I think it is a concept I have got very firmly in mind already.

MR. ROKISON: That is what this reflects and of course it may be very important when we are considering other matters.

MR. JUSTICE FRENCH: I am not denying that, or questioning that.

Q. MR. ROKISON: I think having discussed with you those points I am happy to leave the schools cohort and look briefly at the birth cohort, if we may, which is G87. My Lord has seen the main conclusions which are recited in the abstract. At the bottom of page 824 there is a reference to Table III, which is set out on the left-hand column of 824, and that is a comparison of "Live births by sex and stillbirths during 1950-83 to mothers who were resident in Seascale Civil Parish", and one finds the number of live births set out, males, females and totals, year by year, the male/female sex ratio and stillbirths, the total of stillbirths being nine. The comment on the stillbirths in the lower part of that column is:

> "The nine stillbirths occurred mainly in the earlier years, as would be expected from national trends in stillbirth rates, which declined from roughly 25 per 1,000 births in the early 1950s to about six per 1,000 in the early 1980s. Overall the stillbirth rate - 9 of 1077 or 8.4 per 1000 births - is low compared with the period average of 15 per 1000 for England and Wales."

and they go on to make the point that it is even low when one takes into account the high socio-economic class, which is a point they make at page 825, about three-quarters of the way down on the right-hand side. Is the low rate of stillbirths a matter which you considered to be of importance when considering the Gardner hypothesis?

A. I think that it is difficult to say what is the right comparison. You argued a little while ago that the right comparison around Hinkley Point was not England and Wales but Somerset. Similarly you may find that it is better to look at the stillbirth rate in Cumbria as a whole or in similar places. I think that the stillbirth rate is not significantly low at that point so my answer is that I do not think that the stillbirth rate affects my conclusions one way or another, and has very little bearing.

Q. I think you are wrong there, with respect. If you look at page 825, at the passage I was referring to about three-quarters of the way down on the right, it says:

> "There is no suggestion that rates are low in general in the locality. Thus for a comparable period of years, 1969-73, infant mortality ... was slightly greater than that ... in England and Wales."

MR. JUSTICE FRENCH: Where are you reading?

MR. ROKISON: My Lord, I was looking at infant mortality; it is an associated but different point and I apologise.

Q. Where do we find the reference to stillbirth rate being low in West Cumbria?
A. We do not, what I am saying is that I have not looked at the data on that and you might argue that the right thing to do is to compare it with West Cumbria.

Q. MR. JUSTICE FRENCH: Do we have a West Cumbria ...?
A. We do not in this paper but undoubtedly I did not consider the apparent, what you have regarded as a low stillbirth rate, as being particularly relevant.

Q. MR. ROKISON: It is not what I have regarded as a low stillbirth rate. I am not really in a position to judge. It is what is stated in the study to be a low stillbirth rate.
A. Yes. Statistics on stillbirths, and this is an area where I am sure I am an expert, is really very, very difficult.

Q. You agreed with me, I think, in relation to another study, earlier in the day, that if the Gardner hypothesis were to apply to any particular population you would expect an increase in congenital malformations. Would you also agree that if the Gardner hypothesis of a paternal exposure leading to some form of damage to the DNA and the germ-cells were to apply, one would expect to see an increase in the stillbirth rate rather than a population which has a low stillbirth rate?
A. You might do because if you had sufficiently large numbers then the congenital malformations that form part of the cause of stillbirths, but only part of, would be increased. It would not be expected to have an effect on other causes of stillbirth and it may also by the mechanism have an effect on spontaneous abortion, which is not able to be measured.

Q. I quite understand about spontaneous abortion but one would expect that there would be an increase in the stillbirth rate?
A. As I say, if the numbers were sufficiently large, yes.

Q. I think it boils down to this, doesn't it, that you would expect there to be an increase but whether it would be a noticeable excess or not would depend on the numbers?
A. Yes.

Q. But it is not a point that you particularly noticed in this paper?
A. No. I did not think it was of great relevance.

Q. The second point which I had taken you to a little early was the infant mortality figures which are referred to on page 825, and set out in Table VI?
A. Yes.

Q. As far as infant mortality is concerned, the deaths in the first year of life were substantially, indeed significantly, reduced in Seascale?
A. Yes.

Q. And in that regard it is stated specifically in the study:

> "There is no suggestion that rates are low in general in the locality."

and it is suggested that the socio-economic class may partly explain the low rate?
A. Yes.

Q. Again, is that something which you noticed or regarded as being of interest in relation to the Gardner hypothesis?
A. No, I do not think it has any great bearing on it.

Q. It is Table V which reflects the excess of child leukaemia deaths, isn't it?
A. Yes, and if you were to compare it, if you like, with other deaths and you were to use non-leukaemia deaths as your standard, the infant mortality and childhood rates would be dramatically low in Seascale, so you might use that as your standard.

Q. That is very clever, Professor.
A. That is a post hoc argument that I do not regard to be ....

Q. It is simply that if what one is looking at is some radiation damage to the DNA, and I appreciate you are not a geneticist but if one is looking at radiation damage to the DNA that may result, according to the theory, in the development of a leukaemia at some stage, but it may give rise to other defects of different kinds, malformations and so on, and that one would have imagined that there would be a rather greater infant morality during the early years of life in those circumstances than in a population which was not affected in that way, that is the point that I was putting to you before. Now, that is looking at ....
A. Provided the numbers are large enough, yes.

Q. But that is the infant mortality point. The point we are talking about here is deaths from leukaemia over a period and age --- it is the young people up to 25, isn't it, that are there being considered?
A. I think it is to the children who were born between 1950 and 1983. I forget what age their upper limit for deaths concerned; it may not have been any upper limit.

Q. If you will just bear with me, perhaps that is clear from the study. I think it may be that there is no upper limit.
A. That is my thought.

Q. It is simply to those who were born there. I did not quite understand the point you were making about infant mortality in relation to that?
A. What we are doing here is we are seeing an increase among malignant neoplasms, and especially when we look at it

among leukaemias, to a degree among other cancers, but other causes there is a deficit, and so if you think that if there were some general effect, causing a deficit, that you should take that into account when assessing the importance of the leukaemia excess.

Q. That is one way of looking at it but another way of looking at it is that if you only have a leukaemia excess and do not have other ....
A. And possibly other cancers and possibly non-Hodgkin's lymphoma.

Q. Yes, one case.
A. Yes, possibly.

Q. I see the point you are making. So far as the non-Hodgkin's lymphoma is concerned, very wide confidence limits because only one case?
A. Absolutely so. You would not build a case on that.

Q. No indeed. Well you would not, anyway! Page 826, the discussion, they refer again in the discussion:

> "There is some evidence that childhood leukaemia rates highest in social class I ...."

They also refer to the movement out of Seascale, and that is a point that is emphasised in other places, isn't it, that here you are dealing with a highly mobile population of a high socio-economic class? Would that be fair?
A. Yes. Can I just point out that the social class gradient in regard to infant mortality that you noted went from 12 to 31, and so is a ratio of 2.5, whereas the social class gradient in regard to leukaemia goes from 44 to, in fact, 37 at class 1, and the lowest one if you were to use that as a standard is still only a ratio of 1.3, shall we say.

Q. MR. JUSTICE FRENCH: Can I ask, putting the question shortly but not offensively, I hope, so what?
A. The important thing is as to whether a social class difference might explain something in infant mortality, where there is a big difference, but would be unlikely to explain an excess in leukaemia where the gradient is very small, so even if we have a larger proportion of social class 1 it is less likely to explain an odd finding in leukaemia than it is in the infant mortality.

Q. MR. ROKISON: Anticipating what I want to ask you about in due course, which is the possible application to explain in whole or in part the excess by reference to the Kinlen hypothesis, that what this paper emphasises is not just that it was a high socio-economic class but it was a highly mobile population?
A. Yes.

Q. When you referred to --- we have looked at the NHL case, as far as other cancers are concerned I think you said that there is a small excess of other cancers, it is of marginal significance, I think, isn't it?

A. Not even significant.

Q. I do not know whether it is. I do not want to be unfair ....
A. No, I do not think it is statistically significant.

Q. So it is not a matter on which we need dwell?
A. No.

Q. I think that I can come back, if I may, to your report and your comments about those two studies, if I may. I think that takes us to paragraph 47 and your conclusions. First of all you say:

> "a. They suggested that the cause of the excess of childhood leukaemia at Seascale is not likely entirely to be due to environmental exposure as a child."

and I think we would agree with that, I don't think that is an issue between us.
A. Yes.

Q. You say:

> "b. They provided the first indication that very early, i.e. pre-conception and/or in utero exposures of the child could be important ...."

I think the way in which it was put by Gardner in the schools cohort study was that he referred to it as being either pre-natal or early in life. Would you disagree with that?
A. No.

Q. You limit yourself to pre-conception and/or in utero but it need not necessarily be that early?
A. That was not my intention.

Q. MR. JUSTICE FRENCH: If you added the words, "immediately post partum"?

Q. MR. ROKISON: It is a little bit more than that ....
A. Very early.

Q. Early in life. Again, there is behind many of my questions a purpose, my Lord, and it does relate to certain possible explanations.
A. If I could have advance warning of the ones for which there was no purpose that would help! (Laughter)

Q. I just want to read the relevant part of the abstract from the schools cohort in this context. It ends in this way:

> "There is an interesting difference between the results of this study and the results of the study

> of children born to mothers who were resident in Seascale. In the latter study there was an excess of leukaemia and of other cancers, but a similar finding is not apparent among children who spent some time at schools in Seascale but were born elsewhere. This raises the question of whether one or more aetiological factors in childhood cancer were acting on a locality specific basis before birth or early in life."

and that is the point. What do you understand by the reference to "acting on a locality specific basis"?

A. They were something that were special in Seascale.

Q. That is how I understood it, thank you. Coming back to your conclusion "c":

> "They indicated that the excesses of cancers in Seascale are not confined to leukaemia ...."

Pausing there, I think you have already said that you would not regard the other cancers as being a matter of any significance?

A. All I have said is there may well be. I would still stand by that.

Q. I agree, but we have just agreed - we have agreed on a number of things but one of the things that we have just agreed on, and you indeed made the point yourself, that any excess in other cancers was not of statistical significance and that we therefore in a sense put it to one side. Where you say, "... the excesses of cancers ... are not confined to leukaemia ....", it would be right to say, "may not be", would it?

A. No, they are not in that sense but in that there is an excess the relative risk was 3.45 or something of that kind, but I would have to agree that is not statistically significant and therefore I wrote the words "there may well be", rather than "there definitely is".

Q. Now we come to the Gardner Case-Control Study. We have looked at the Black recommendation and we have looked at the question of its limitations and the limitation which was imposed by those who carried out the study. The 0-25 parameter is a parameter which appears to flow from Black?

A. Yes. It is probably 0-24. I have probably written 0-25. It is 0 to just less than 25. It partly flows from Black and partly flows from the way that OCPS provide their data. You mean as opposed to 0-23 or 22 or as opposed to 0-15?

A. Indeed, as opposed to 0-15. The position is that Black did actually specify in recommendation 1 that it should be 0-25?

A. Yes.

Q. It was laid down by Black and as we have seen Black made the reservation statistically it would have been better to look at 0-15 but they were looking at an identified cluster which went beyond the age of 14?
A. Yes.

Q. I have nothing to ask you about paragraphs 48 and 49. In paragraph 50 you say:

> "Despite these problems, case-control studies have been used effectively to isolate risk factors and possible mechanisms for diseases. They were the first to indicate the link between smoking and lung cancer."

It is right, isn't it, that in recent years perhaps particularly there have been a lot of case-control studies which have thrown up a number of associations between possible cause and effect, some of which have turned out in the end to be real associations and some have been shown to be spurious?
A. Yes.

Q. I think there are a large number of examples of these?
A. Yes, though smoking and lung cancer, of course, was suggested to be spurious at the time it was first found.

Q. Certainly, and it may be that a case-control study throws up for the first time a statistical association which cannot be dismissed as being clearly implausible, and that will generally give rise to further studies which may or may not confirm it?
A. Absolutely.

Q. MR. JUSTICE FRENCH: There is a reference in the papers to a notorious study connecting some drug with breast cancer?
A. Reserpine and breast cancer.

Q. I was completely unaware of this notorious case. When did it happen?
A. It is notorious in scientific terms, not obviously in legal terms, but my recollection is that it was in the '60s and I think it came from the sort of exercise that I mentioned before about going through a large database of drugs and looking for side effects and then they lighted upon Reserpine early on and they then did the study there and found that Reserpine and breast cancer in some case control studies were associated.

Q. MR. ROKISON: Yes, I think my instructions are, and I do not know, but I am told by my experts that there were actually three case control studies in relation to Reserpine and breast cancer, which both showed a....?
A. I am sure Prof. MacMahon is the expert in that area.

Q. But it turned out that it was spurious?
A. As far as we know, yes.

Q. And there are other examples. Saccharin and bladder cancer, I think, is one which I have been told about. Is that one with which you are familiar?
A. Yes, I cannot remember the history of that one, but angler's bait and bladder cancer was one that turned out to be correct.

Q. Yes.
A. Sorry, anglers' bait and mouth cancer.

Q. Oh, I see. Would you agree that one should be very cautious about drawing any conclusion as to cause and effect from only one case control study?
A. From drawing any conclusion, no, but from drawing extraordinarily strong conclusions, yes.

Q. One can draw a conclusion, as you have as a statistician, to the effect that statistically there is an association, but if one is to go further and to reach a broad conclusion as to whether, indeed, there is or is not cause and effect, that is a matter that is rarely, if ever, I would suggest, drawn scientifically from one case control study?
A. I would agree, scientifically.

Q. MR. JUSTICE FRENCH: Are you speaking now as a scientist or as somebody who has to express a view on the balance of probabilities?
A. I am speaking as a scientist.

MR. ROKISON: And so was I, my Lord.

MR. JUSTICE FRENCH: Yes, but it is just that the line can become blurred and I just want to know what a particular answer's reference point is.

MR. ROKISON: Yes, but I think it right, and I would not ask this witness to express views on a balance of probabilities or whatever. That, with respect, is your Lordship's function at the end of the day.

MR. JUSTICE FRENCH: He has done so in the course of his evidence and I would not seek to inhibit him doing so, but bearing always in mind what you say is absolutely right. It is for me in the end.

MR. ROKISON: In the end, on the basis of the whole of the evidence:

Q. Merely what you confirmed to me is that, scientifically, rarely, if ever, would one draw a conclusion as to cause and effect from one case control study?
A. No.

Q. You agree?
A. I agree with you. You would not draw it from one on its own.

Q. You then deal with the selection of cases and controls and I think you have already agreed with me, Prof. Evans, that it is important to set your parameters in advance and it is important that, once you have set your parameters, you should stick to them?
A. Yes.

Q. You yourself make the point, in relation to the selection of controls, that the selection methods for controls used by Gardner and colleagues is reasonable and does not exclude or include children of controls unless they would also have been, and I take it you should say, excluded or included, as the case may be, as cases?
A. Yes.

Q. Would you regard that as being important, that you should treat your cases and controls in the same way?
A. That is the most important aspect of a case control study.

Q. Yes. You know, Prof. Evans, that we on our side of the courtroom criticise the inclusion of a particular case, 00106, the Bristol case?
A. Yes.

Q. We say, and those advising me say, and our evidence will be, in our opinion, that should have been excluded as a case because it would have been excluded if a control. Would you agree that, if it would have been excluded as a control, it should have been excluded as a case?
A. If all controls of that type were excluded, then you should, indeed, exclude all such cases.

Q. I wonder whether, in relation to that, we might just have a look at the Methods paper of the Gardner study?
A. Yes.

Q. MR. JUSTICE FRENCH: Is this right, Prof. Evans? "The Defendants say the Bristol case should have been excluded as a case. If it should have been excluded as a control, I agree"?
A. No, I said if all such controls were excluded....

Q. "The Bristol case should have been excluded as a case if all such cases were excluded as controls"?
A. All such similar individuals.

MR. ROKISON: Yes, not all such cases were excluded as controls, otherwise one gets frightfully confused.

MR. JUSTICE FRENCH: Yes, it is a confusion of terminology.

THE WITNESS: Otherwise similar individuals.

Q. MR. JUSTICE FRENCH: "If all such individuals were excluded as controls, I agree"?
A. Yes.

MR. ROKISON: The Methods paper, does your Lordship have that?

THE WITNESS: It is G 89.

MR. ROKISON: G 89. It is also the second document in P4, which was my learned friend's Evans bundle.

MR. JUSTICE FRENCH: Yes, perhaps that might be a better place to go as I have looked at it there already.

Q. MR. ROKISON: If you would be kind enough to look at page 430, first of all? It is page 8 of P4, if that is where you are looking, or page 430 of the study?

A. I have that. I am just putting away Black.

Q. It is the first full paragraph at the top left:

> "Part of the rationale behind the recommendations was that geographical studies of incidence of mortality include little or no direct information on individuals with or without the disease. Thus the Black report recommended that 'a study should be carried out on the records of those cases of leukaemia and lymphoma which have been diagnosed among young people up to the age of 25" - and we emphasise for the present purposes - "resident in West Cumbria."

Then, if you follow down to the right-hand column, about two-thirds of the way down, there is a small paragraph which says this:

> "For this report we included only cases in people born and diagnosed in West Cumbria and omitted six cases of leukaemia and eight of lymphoma in people born outside."

Then they say:

> "The residential addresses were determined from hospital records, questionnaires, death certificates, or the Cumbria Family Practitioner Committee."

Then one gets to controls, which are referred to on page 431, and they refer to area controls and local controls and describe what those are and the matching of controls by sex and date of birth. Then, the bottom paragraph on the left, they say - and I think it must be that there is a comma here which ought not to have been a comma, otherwise it does not make any sense whatever:

> "Additionally, to have the potential to be a case in this study at the appropriate time, controls had to have been resident in West Cumbria at the date of diagnosis of their associated case."

So what they are saying is, as well as the cases having to be resident in West Cumbria, so did the controls. Right?

A. Yes.

Q. "This information was determined initially at the National Health Service central register by reference to the family practitioner committee area of registration at the appropriate date."

That is the date of diagnosis of the case?

A. Yes.

Q. "In total 195 controls registered outside Cumbria were thus excluded."

A. Yes.

Q. "Residence particulars for controls with a Cumbrian registration or no registration were examined in questionnaires sent to parents and a further 13 excluded. Those left out from the questionnaire part of the study," etc. "were reviewed within the Family Practitioner Committee records...."

Would you agree that what they appear to have done in relation to controls is this: they go initially to the FPC registration?

A. Yes.

Q. If they find that the control at the relevant date, being the date of diagnosis of the case, is registered outside West Cumbria, then those controls are excluded. Do you agree?

A. Yes. I think you will find a clearer description of it in Snee's thesis, but still....

Q. But do you agree that....

Q. MR. JUSTICE FRENCH: We will find it where?

A. In Dr. Snee's MD thesis, which is part of the evidence.

Q. MR. ROKISON: If they are registered outside Cumbria, they are excluded. Right?

A. Yes.

Q. And it is only if they have a Cumbrian registration or no registration that they go further and ask questions through the questionnaire?

A. Yes, there is a little more subtle point than that, though that appears on the face of it in there. That is a simplified description of what actually happened.

Q. Perhaps you might refer us to it if it is of importance tomorrow. May I just put the very short point to you in relation to the Bristol case, that, trying to be fair and to paraphrase the statement of Prof. Gardner and the letter of Hazel Inskip, that what it appears that those

carrying out the study did was that, in relation to cases, they took a broader, more judgmental view and, if they formed the view that, for example, the case had spent virtually all its life in Seascale and had had parents who were there, etc., the fact that it so happened that at the date of diagnosis a case was registered with a Family Practitioner Committee outside West Cumbria would not necessarily exclude that case from the study?

A. They did the same to some of the controls.

Q. MR. JUSTICE FRENCH: Let us take it step by step. In relation to cases, did they take a broader, more judgmental view than in respect of controls?

A. No.

Q. MR. ROKISON: That is something which you derive, do you, from the Snee thesis?

A. Yes.

Q. It is quarter-past four and perhaps we can look at the passage to which you want to refer in the morning?

A. Do you want me to give a reference in my report to it?

Q. Yes, that would be very helpful?

A. It is at paragraph 49 (3) on page 19 of my third report. The last paragraph.

MR. ROKISON: We will look at that, if we may, in the morning and pursue it then, if that is a convenient time for your Lordship.

MR. JUSTICE FRENCH: Yes. Are you on course?

MR. ROKISON: May I just check, my Lord? I think that I am. I think that I am just about on course to finish those aspects which I do not wish to reserve, for the reasons that I outlined to your Lordship - I should finish those tomorrow just about. Certainly there will only be a very short overspill, if there is.

MR. JUSTICE FRENCH: Yes. We might perhaps have an eye to sitting a little later tomorrow, if that is going to make the difference.

MR. ROKISON: Shall we see how we go? May I revisit that at the short adjournment?

MR. JUSTICE FRENCH: Obviously we will see how we go, but I say we might have an eye to that.

MR. ROKISON: Certainly. That would be very helpful. Thank you, my Lord.

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