Wednesday, 7 October 2020

(10.00 am)

DR BRIAN COLVIN, continued

MS RICHARDS: Dr Colvin, we were looking at documents around the middle of 1983, about the developing response to HIV and we'd looked at the letter from Professor Bloom and Dr Rizza, June 1983.

A. Yes.

Q. I'm just going to ask for that to go back on screen.

Henry, it is BART0000844. That might be the wrong reference. Apologies. It's the right one, good.

So we looked at the recommendations, I'm not going to go back to those, but if we just look at the first paragraph, we can see what's being said there about, I suppose -- relevant to an assessment of risk:

"So far one possible case has been reported ... This patient ... conforms to the definition published by the CDC in Atlanta ... but cannot be considered ... a definite case."

Then if we just go further down the page, please, Henry, below the paragraphs 1 and 2 that we looked at yesterday, Dr Colvin, we can see that it says:

"It was agreed there is as yet insufficient

evidence to warrant restriction of the use of imported concentrates ..."

And reference is made to the FDA recommendations in March 1983.

Now, would you agree that there's really comparatively little information in here to enable clinicians such as yourself receiving this to make a reliable assessment of the risk?

A. Yes, I would. I agree.

Q. Henry, could we have up on screen next to this CBLA0000043_040. This is the Galbraith letter that you referred to yesterday, Dr Colvin.

A. Yes.

Q. I thought it might be instructive just to look at them side by side if we can.

In Dr Spence Galbraith's covering letter to the Department of Health and Social Security, and we can see it's a few weeks before Professor Bloom and Dr Rizza's letter, 9 May 1983, he says in the first paragraph:

"Last week whilst you were away in Geneva a case of [AIDS] in a haemophiliac in Cardiff who had received USA Factor VIII concentrate was reported. The case fits the recognised criteria for the diagnosis of AIDS."

So we can see there already there is more information than we have in the UKHCDO letter. You know it's a haemophiliac in Cardiff from this, not mentioned by Professor Bloom and Rizza, and you know it's someone who's received American Factor VIII concentrate?

A. Sure.

Q. Then we can see it goes on to refer to three cases in haemophiliacs in Spain having been reported in The Lancet who have also received USA Factor VIII concentrate and then, in the same issue:

"... the tally of 11 reported cases in haemophiliacs in the USA is recorded and a paper describes a case in a multiply-transfused child in the USA."

So would you agree there's a lot more information relevant to an assessment of risk in Dr Galbraith's letter?

A. Dr Galbraith certainly has information which I wasn't aware of.

Q. Then if we then go to the second page, Henry, of the Galbraith letter, please -- sorry, before we do that I should perhaps just clarify, Dr Rizza and Professor Bloom obviously were providing a perspective as haemophilia clinicians. Dr Spence Galbraith was

director of the CDSC, so he had an epidemiological and public health perspective to bring to bear?

A. Yes.

Q. We can see his recommendation at the top of the page. I'll come back to the recommendation in a moment, Dr Colvin, but we can see his recommendation is:

"The temporary withdrawal of all blood products imported from the [USA] made from blood donated after 1978 ... until the risk of transmission of ... (AIDS) becomes clarified."

Then I want to go through his reasons with you and invite you to comment on them. His first reason is that the AIDS epidemic is probably due to a transmissible agent, and you would agree with that as a statement of knowledge at the time?

A. Yes.

Q. His second is that it's an agent probably transmitted by blood and blood products, and he sets out more details of a number of cases. Again, I think it is clear from your evidence yesterday you'd agree with that?

A. Yes, of course.

Q. His third reason -- go to paragraph 3, Henry -- is that:

"Although this number of cases of AIDS

associated with the administration of factor VIII concentrate is very small in relation to the number of individuals receiving the product, this may NOT indicate that the risk is small ..."

Again, you'd agree with that?

A. I can understand that very well, yes.

Q. Yes, and obviously from an epidemiological public health perspective that's where his advice might be key?

A. Yes.

Q. Point 4 then, please, Henry.

"Factor VIII concentrate (and pooled products) would appear to have a high risk of being contaminated with AIDS ..."

And that's because of the donor pool, and he goes on to say:

"... possible that the AIDS agent may be present in blood of healthy persons for several months before onset of symptoms."

Again, you would agree with those observations.

A. Yes.

Q. Then paragraph 5:

"... apparently no ... means of ensuring that blood or blood products are free of the AIDS agent."

That was also true at that time?

A. Yes.

Q. Point 6 is: the mortality rate, again, whatever the precise figure, known to be very high.

A. I understand.

Q. So, in terms of the actual factual matters he there sets out, you would agree with all of those?

A. It's a very carefully ordered and organised letter.

Q. Now, I know you're aware of this letter now.

A. Yes.

Q. Was it a letter, as far as you know, that was ever shared with haemophilia clinicians at the time?

A. The first time I knew about Dr Galbraith's work was when the document was shown to me when I was preparing to give an interview for the Panorama programme in 2017.

Q. So many, many years after the event?

A. Yes.

Q. Could we go back to the top of the page of this document, Henry. So above numbered paragraphs 1 and 2. Next page.

So we'll go back then to his recommendation, the temporary withdrawal of all blood products imported from the US made from blood donated after 1978, is proposed.

Now, obviously, that kind of action is not an

action that was in the gift of an individual clinician such as yourself, but what, if any, observations do you have upon that proposal?

A. It was very bold and decisive.

Q. If it had been implemented for, as Dr Galbraith says, a temporary period whilst seeking to clarify the issues in relation to AIDS, the implications in terms of treatment would presumably have been, clinicians such as yourself would have had to adjust treatment programmes, greater use of cryoprecipitate would have probably been inevitable as a response?

A. Not necessarily but I'm happy to talk to you about it.

Q. Please do.

A. So I think there would be two different sets of consequences. The first consequence would be: what do we do next? And I think you can see that from my own practice around that time, perhaps not quite half my practice was using commercial concentrate, but for some people I think, although you'll know better than I do, more than half and maybe almost the whole of their practice might have involved the use of commercial products. Because I've explained to you, and I think it's clear even from the correspondence you produced for me this morning, that I was committed, rightly or wrongly, to the National Health

Service and what it was providing for our patients.

So let us assume for a moment that half the concentrate in the country would have disappeared. That's not a bad calculation, and in fact if you look at the 1983 statistics, you'll find, I think, that half the product in the country is being NHS and half is commercial. So we're going to need to continue entirely on National Health Service product.

I've already explained and we've discussed that if you have a patient having large quantities of cryoprecipitate, the chances are they will be at high risk of hepatitis C, and we know that the NHS at that time -- we thought that the NHS pool was not contaminated. Which of course we were wrong, it was contaminated. So I think that your options are to try to withdraw to cryoprecipitate as you were implying, which I think would have been exceptionally difficult because one would have had to somehow build up the supply of cryoprecipitate, which, though not impossible, would not have been possible within a few days or perhaps even a few weeks.

So I think that the most likely response of UKHCDO would have been to use as much cryoprecipitate as we had, perhaps increase the supply of cryoprecipitate, as you implied, but I don't think

we would have abandoned the use of NHS concentrate. I just don't think that would have been in any way practical, and it wouldn't necessarily -- particularly if it was the case that the NHS pool was not contaminated, it wouldn't have been harmful.

Now how would you manage a service that was suddenly cut in half? I think the answer is probably an abandonment of all surgery except absolutely life-saving surgery, and I think the abandonment of the home treatment programme and a reversion to on-demand treatment only.

Even with that, you might want to scale down the doses that you're using. Scaling down the doses perhaps for -- and this could be controversial ethically, I haven't really thought it through, but maybe you keep the doses for little children the same and maybe sort of reduce the doses for the adults. I mean, I'm just purely speculating now in a very free way and I'm not sure what I'm saying necessarily makes sense but I'm trying to think what one could have done.

Q. Yes.

A. If you look at the position in India, Dr Srivastava, in Vellore, for many years has done work on how to manage with a little bit of Factor VIII and I've

already explained to you or discussed with you the fact that if you give a little bit of Factor VIII you get a massive bang for your buck, if you like. So immediately, from nothing to a little bit, you get a fantastic benefit, and the benefit at the end is, well, significant but, you know, it's what makes the difference between somebody who is all right and fantastic, which is not the same as having somebody who's awful and surviving.

So I think that if you had abandoned all surgery except for the most life-saving and if you had abandoned the home treatment programme and if you had cut back on dosage and accepted a few deaths and some serious joint damage and some serious and very painful illness, you might have got away with it.

Q. Yes. I'm not suggesting it would necessarily have been without consequences, I just wanted to know how -- (overspeaking) --

A. No, but I just wanted to try to share with you -- just thinking about what you could conceivably do.

The next thing, before we move on, and the second part of what I was going to say, was I wonder what difference that would have actually made to the final result. It's very interesting to hear Mark Winter last week saying that he tested all his

patients that he was able to look at in I think the end of '84, because that's when the testing became possible, and they were all positive.

Now, I don't know whether you want to discuss with me what he said, but I would like to say is that he doesn't know, I don't think, because he said he only took the blood when the testing became available and there wasn't blood taken beforehand -- he doesn't know whether all those tests were positive on the day he tested them or whether they were all positive on the day he started his unlicensed heat-treated Factor VIII.

So what we do know is that probably by 1983 a very large number of those who had received large amounts, or even perhaps, moderate amounts, of commercial concentrate were already HIV positive and that process had started in 1980. So that the consequences of the temporary withdrawal of all blood products in the way that Dr Galbraith perfectly understandably recommended was a probable, I think, significant reduction in anti-HIV positivity and a significant, and probably very significant, reduction in the quality of haemophilia care.

Q. We've looked, in these two documents, at the UKHCDO perspective and Dr Galbraith's public health

perspective. Could we then just look at a communication from the Blood Transfusion Service at this time.

Henry, it's NHBT -- you can take both those documents down -- NHBT0020668.

Now this is an internal National Blood Transfusion Service letter. I'm not suggesting you would have seen this at the time. It's dated 6 July 1983 -- so within the same span of weeks of the material we have been looking at -- and it's enclosing a copy of a final form of an AIDS leaflet which is going to be used in transfusion centres to try and reduce the risk of donors who might be HIV positive donating blood.

Can we just turn to the next page please, Henry.

We can just see what's set out there in terms of question and answers. If we can go to the bottom of the page, you will see there there's a question:

"Can AIDS be transmitted by transfusion of blood and blood products?

"Almost certainly yes. There is only the most remote chance of this happening with ordinary blood transfusions given in hospitals. However, in the USA about 10 patients suffering from haemophilia, an illness in which the blood will not clot, have

developed AIDS. Haemophiliacs are more susceptible to AIDS because they need regular injections of a product called Factor VIII. This is made from plasma obtained from many donors. Should just one of the donors be suffering from AIDS, then the Factor VIII could transmit the disease."

Would you agree with me that the answer that's being given there again is a very decisive, firm, unequivocal answer about risk, being communicated -- or the intention is within a few weeks to donors?

A. I understand.

Q. A much clearer statement of risk or association between blood products, blood and AIDS than we see in the letter that was produced by Dr Rizza and Professor Bloom?

A. Yes.

Q. Could we then go on to look at some documents that you would have seen at the time, Dr Colvin. Could we please have BART0002284. We've already looked at this document in part. It's the document co-authored by you and Dr Kernoff in 1983, "Haemophilia services in the North East Thames region, 1983". If we look to the next page under the heading "Summary", we see there's a reference to:

"Major advances in the management of

particular complications of treatment and one is:

"Plasma product-transmitted disease, particularly hepatitis, and possibly the acquired immune deficiency syndrome. An important function of haemophilia centres is one of regular patient health surveillance, which includes a commitment to public heath. At present, this aspect of a haemophilia centre's work does not have an important impact on spending, although it does on workload."

Then there's a reference to the possibility of hepatitis-reduced concentrate. So there's a reference there to AIDS, but it's in fairly tentative terms; would you agree?

A. Yes, of course.

Q. Then I think that may be -- sorry. There is one further reference. If we go on, Henry. If you look at the pagination at the top of the page, Henry. It's page 7 in the internal pagination. It's probably page 8 of the document you have. Or page 9 possibly.

Then we can see a further reference to hepatitis and AIDS at the top of this page, paragraph 3.3:

"Impact of hepatitis and AIDS. There is likely to be an increased morbidity due to chronic liver disease (CLD) over the next decade. Whether this will result in significantly increased mortality will

haemophilia."

And then reference to local regional arrangements and the role of the regional haemophilia centres, and then in paragraph 4, an issue there in relation to cost and the cost of commercial blood products and the importance of achieving national self-sufficiency.

Then if we go to the next page, please -- sorry, two pages further on. My apologies, Henry. That's the one. So if we look at the third paragraph down where it says:

"A fundamental problem in the UK is that the NHS is not self-sufficient in blood products."

We can see there reference being made in this paper:

"Not only is this expensive, but there are long-standing concerns regarding possible health risks. At present, the benefits of therapy outweigh potential disadvantages."

There's no specific reference to AIDS there. I am just going to show you what references there are to risks and then ask you a question.

A. Yes.

Q. Then if we go to the bottom of this page, please, we can see at paragraph 1.4 (b), reference is made to two

probably not be known for several years. Clinical problems related to impaired immunity seem to be rare at present but conceivably could become a major clinical problem."

So that last sentence, I think, is the reference to AIDS?

A. Yes.

Q. Now, that is, I think, the only discussion in this paper of HIV and AIDS. Now, I'm conscious the paper was in part a paper looking at issues of funding for the future, but is there any reason you can think of why you and Dr Kernoff were not addressing more directly by August 1983 the AIDS risk?

A. Well, I think because we didn't appreciate what was going to happen next, and one can only read what we said and I believe that we've said it in good faith, but I can understand that you might think that more could have been said.

Q. Then if we move on to the autumn of 1983. It's PRSE0004440 please, Henry. So this is a meeting of the Haemophilia Centre Directors on 17 October 1983; the big group meeting, rather than the smaller meeting of Reference Centre Directors. We can see that you were in attendance at that meeting?

A. Sure, yes.

Q. Could we go to page 10, please, Henry. We can see in the second half of the page under the heading "Any other business":

"Dr Chisholm raises the problem of patients refusing to take up commercial Factor VIII because of the AIDS care. She wondered, in view of the worry of the patients, whether the directors could revert to using cryoprecipitate for home therapy."

Then we see Professor Bloom's response:

He felt there was no need for patients to stop using the commercial concentrates because, at present, there was no proof that the commercial concentrates were the cause of AIDS. Dr Chisholm pointed out that there was a further problem in her region because of problems in getting large amounts of concentrates, whereas she could get unlimited supplies of cryoprecipitate. Other directors reported they had the same problems."

Then:

"After discussion, it was agreed that patients should not be encouraged to go over to cryoprecipitate for home therapy but should continue to receive the NHS or commercial concentrates in their usual way."

Do you have any recollection of this discussion or Dr Chisholm's intervention?

A. Well, I knew Dr Chisholm very well, and I think I can just about remember her intervention in the sense that I remember she was there and she said something, but I don't remember any more about it than that.

Q. Do you recall whether you were in the category of directors who were in a similar position to her: she had problems getting commercial concentrates but could get unlimited supplies of cryoprecipitate?

A. I find it quite difficult to believe that she truly was able to get unlimited supplies of cryoprecipitate. I can't believe that anybody would be able to get unlimited supplies of cryoprecipitate. Getting large amounts of commercial concentrates -- I mean, my memory, if you were prepared to get the commercial concentrates in, they were available to you. So I don't quite understand that sentence really, any of it, because I don't understand the difficulty getting large amounts of commercial concentrate if she's prepared to find it.

Q. That may be a funding issue, of course?

A. Of course, it could be a funding issue, and I don't really understand the concept of unlimited supplies of cryoprecipitate because you've got to collect the blood for the cryoprecipitate and put it in the freezer, and it has to go somewhere, and to provide

unlimited supplies would be a logistic challenge for any transfusion centre.

It's important to remember that the cryoprecipitate is a transfusion centre product, whereas concentrates are an Elstree or commercial production facility.

Q. Yes. I mean, whatever one might precisely be meant by terms such as "unlimited", the clear thrust of what Dr Chisholm was saying was: I am in a position to use cryoprecipitate rather than commercial concentrates; what do others think of this suggestion?

A. Obviously, there's the issue of home treatment because I think she's mentioning home treatment here and, as we discussed yesterday, although I've done cryoprecipitate on home treatment, I wouldn't recommend it. I think it would have created huge difficulties for patients for the reasons we discussed yesterday and I think we also discussed yesterday the fact that some of us, including myself -- and this would have been around this time, I think -- were trying to use cryoprecipitate where we thought it was appropriate. But we also have to acknowledge, as we discussed yesterday, that cryoprecipitate is also capable of transmitting either hepatitis C or HIV, although you only have one unit at a time -- although

you might be using ten units for a single dose.

Q. Was Professor Bloom right, in your view, to say that at present there was no proof that commercial concentrates were the cause of AIDS?

A. I think this is -- is it October '83?

Q. October '83.

A. So this seems to me to be more wishful thinking, if I may say so.

Q. The agreement that's recorded at the bottom is essentially: no action. Don't encourage your patients to go over to cryoprecipitate. It's not even left as clinical freedom for the individual clinician to decide whether to encourage their patients to do that. Patients should continue to receive the NHS or commercial concentrates in their usual way.

Does that surprise you, that no slightly bolder recommendation was being made by that time?

A. I think, at my personal level, it was always my choice to use NHS concentrates where I was able to do, and I only bought commercial concentrates when I had to. I think that's in the papers that you have just handed me this morning.

As I've said, there were others who didn't care much for the NHS or its product, and I suspect there may have been places where commercial concentrate was

actually dominant and, therefore, I think that there may have been some concern that, were there to be a change in the recommendation, it would create enormous difficulties of the kind that we've just been discussing.

Q. But this is effectively just maintaining the status quo; no change whatsoever?

A. It is, isn't it? Yes.

Q. Which suggests to you is -- I'm conscious of your answering by reference to your own practice, and we'll come on to look at that, but as a general recommendation from UKHCDO, it is surprising to see no recommendation for change by October 1983. Would you agree?

A. Yes, and in fact, in retrospect, of course if it had been that Dr Galbraith's recommendation, whatever its difficulties had been implemented, there would have been a great deal of clarity, and we would have had a completely different problem to deal with.

Q. Yes.

A. We would have no doubt in some way dealt with it. But it would have created a whole range of new problems that we would have had to address.

Q. Then just looking again now at some documents local to you and the London hospital and your policies there,

could we have, first of all, Henry, IPSN0000584_003. Can we go to the first page of that, please. Thank you.

We can see here this is a report. It looks like it's a UKHCDO-authored report. We don't have a precise date to it, but it's looking at data between 1981 and 1983 and, if you take it from me, it's then got some incomplete data from 1984. So it looks most likely as though it was a document produced in 1984.

A. But it is about inhibitor patients.

Q. It is about inhibitor patients, absolutely.

A. Which is, in itself, extremely important. I haven't been sent this document, I think, but I think it is important to note that this is about inhibitor patients.

Q. Yes, absolutely. This is just one small part of it. I just wanted to ask you something about your treatment of inhibitor patients on the basis of this.

If we could go, please, to page 9, Henry, and the document looks at policies and approaches in different centres, and I just wanted to ask you about yours. So this is London Hospital:

"Dr Colvin has six inhibitor patients, three of whom are low responders, two are high responders, one has spontaneously acquired antibodies. His policy is

to give human Factor VIII to all the patients for all bleeds, and if that fails, to use porcine Factor VIII. He's treated two patients with Hyate:C to date: one congenital, one acquired."

The first question is: is that a correct statement of your policy at the time?

A. I think it must be, although I could comment on some unusual aspects of it, but I think it illustrates my commitment to NHS concentrate and to human concentrate rather, perhaps if I can avoid it, than FEIBA. But the difficulty with what I've said is that if you have a high-responding patient, the Factor VIII concentrate isn't going to work. So you can give as much human Factor VIII as you like to somebody who's got a high responding antibody, and it won't work. Because I said that I've got three low responders and two high responders, so for the low responders, it's possible that it actually will work. For the high responders, if the antibody against porcine Factor VIII is sufficiently weak, then I would certainly use porcine Factor VIII because I was able to get a measurable response in the laboratory. Because what happens is, if you have got an inhibitor that is a fairly moderate intensity, the intensity of the inhibitor will be lower against porcine because it's moderately high

against human. Then you give the porcine Factor VIII. You can measure the Factor VIII in the circulation, and you get a clinical response which is proportional to the laboratory response. So if you can use porcine Factor VIII, you've got a really measurable response which reflects clinical practice.

However, if you're going to use something like FEIBA or Autoplex, which I never used, this is an activated product which hasn't got any units associated with it that are actually meaningful. I mean, they gave quoted units, but those units were correctional units; they weren't real units. So you actually don't know what you are doing. So you give a very expensive and very potentially toxic product to somebody which you hope will stop the bleeding. Now, that's worth it if your patient's bleeding to death, but if you can get control with a product which does give you a measurable response which is proportional to the clinical effect, that's fantastic.

Q. Is there any particular reason why, by 1983, porcine Factor VIII would not have been the first port of call over human Factor VIII, in light of the AIDS risk?

A. Well, it was quite idiosyncratic as a treatment. Peter Kernoff was an expert in porcine Factor VIII, as was Christopher Ludlum, and it was particularly useful

for acquired haemophilia. Maybe we don't need to discuss acquired haemophilia, but acquired haemophilia occurred in old normal people who got an antibody to Factor VIII and then started bleeding, and porcine Factor VIII was particularly good. Nothing to do with haemophilia itself. They got acquired haemophilia. It was an autoimmune response in a normal person. Nothing to do with congenital haemophilia.

But I think I was -- well, I was always influenced by Peter Kernoff, and the fact that Peter was an expert in this area meant that I was also very interested in it, and I would have used porcine Factor VIII for the reasons that I've suggested.

However, it was very allergenic. It caused a lot of allergic reactions. It was very likely to cause a fall in the platelet counts -- thrombocytopenia, which could be dangerous -- and it may well be that some people thought that this was an idiosyncratic treatment that didn't warrant use.

Q. Could we then go on to BART0000517. So this is a document I know you've only seen this morning, Dr Colvin. It's a letter dated 23 March 1984, and it's authored by you. It's to The London Hospital's district supplies officer, and it's really just to flesh out some of the evidence you gave yesterday

about what products were being used when.

So we see there in paragraph 1, you are using NHS Factor IX. Paragraph 2, as you just said, not using Autoplex. You have referred to using FEIBA once, and then you say in paragraph 4:

"Our use of imported Factor VIII is entirely dependent on the supplies of NHS material. Our usage over the last few years has been ..."

Then we can see there the figures set out for 1981 and 1982 where we don't have the returns, and then 1983 we've already looked at because we had the 1983 return and the figures are consistent.

We can see that in '81 you were using small amount of Hyland, a larger amount of Armour, and a much larger amount of Immuno; in 1982, no Armour, again a large amount Immuno, and a reasonably large amount of Hyland (certainly an increase on the previous year); and then in 1983 there is a drop in the amount of commercial concentrates being used to the figures we looked at yesterday.

Can I just ask, first of all, can you recall what -- how you made the decision, or you and your colleagues at The London would have made the decision as to whether to purchase Factor VIII in a particular year from Immuno, rather than Armour, or Hyland rather

than Cutter?

A. I really can't. What I think we did agree, and this may have come out elsewhere, was that it was always a good idea not to have one supplier only. I don't think there was any evidence that any particular product at that time was superior to another technically, so I think price would have been a feature. But you can see that, although I said I would like to have two suppliers, you can see in '81 I clearly had three, and in '83 I had four, although very small amounts of Hyland and Armour.

So I don't know the answer to that question. Of course, some of it might have been that there were people visiting the centre who were using a commercial product and sent in a return for a commercial product -- might even conceivably have been supplied elsewhere. I don't think it's very likely, but I'm a bit puzzled by these levels of 4,000, 3,500 and 5,500. They are very small quantities. I mean, not much more than you'd use for a single bleed. So I really can't entirely explain that.

But if you ignore those -- although I am not suggesting you ignore all of them, but if you take them out of the equation, it does look as though, in those circumstances, I am using two suppliers, but

I don't know why.

Q. The decision as to which commercial products to purchase, was that a decision taken largely by you and Dr Kernoff for the region, or was it done by others within the region?

A. I would have thought that it would have been Peter and myself, generally speaking, and I would have expected for me to be the person who chose what to buy for The London Hospital. But I can't explain these figures really.

Q. Then if we just go further down that page, we see that you refer there to the reduction in commercial concentrate purchased in 1983, and the explanation for that is it was almost entirely due to an increase in the supply of NHS material and the annual consumption of all forms of Factor VIII is fairly steady at about 1.5 million units a year.

A. Sure.

Q. So it would seem that, in the course of 1983, you have been able to use less concentrate because you've got more NHS available?

A. And that would fit in with my overall philosophy.

Q. Then could we have BART0002310, please.

Again, Dr Colvin, I know you've only seen this letter this morning. 22 February 1984. It's a letter

from you to Dr Watters at The Haemophilia Society, and it's obviously part of a chain of correspondence with a document that you have been asked to comment on. I am not going to ask you about most of the detail of it, but there is just one comment I wanted to ask you about. If we go to the second page. You say at point 4 this:

"I agree [you are agreeing, I think, with what Mr Watters must have said or what The Haemophilia Society document must have said] that we know little about AIDS at present. In my opinion, there is no reason to spurn commercial concentrate, and we have to keep an open mind on the risk associated with NHS material."

Can I invite you to comment upon that because it seems to suggest that you don't think there's an increased risk from commercial material in relation to AIDS as at February '84?

A. I don't think that follows. I think what I'm saying is that I believe we know little about AIDS at present, and that seems a statement. The question is whether we spurn commercial concentrate and the advice from UKHCDO and from our discussions is that it is not appropriate at the moment not to use commercial concentrate. I'm also pointing out that we're not

entirely clear, and it's going to be very difficult to know, whether there's a risk associated with NHS material or not.

So I think it is what it is and the word "spurn" I suppose implies that I am not, on the evidence presented to me, recommending that we don't buy commercial concentrate at all. That's what it means.

Q. Then if we can go on in the course of 1984 to another North East Thames region meeting. It's BART0000677. This is a document you had already seen, Dr Colvin. So these are the minutes of the Haemophilia Working Party of the North East Thames Region Association of Haematologists. This is 9 May 1984, and you're present, as is Dr Kernoff.

Could we just go down to the second page, please, Henry, and we can see halfway down the page is an update on AIDS, and it says this:

"Dr J Harrison ..."

That's Dr Jean Harrison --

A. Jean Harrison, yes.

Q. -- who was the director of the Brentwood Regional Transfusion Centre?

A. Following Dr Jenkins' retirement.

Q. "... described the efforts made by the BTC Brentwood to exclude at-risk donors by information and leaflets

distributed to donor sessions. It was noted that commercial suppliers also now are applying similar criteria in their donor selection. Heat treating concentrate has been tried but reported to cause loss of potency."

So that's, I think, an update from Dr Harrison. Then it says this:

"It was suggested that, until a positive test for AIDS and/or a vaccine is developed, it should be policy to avoid use of blood products except for essential treatments and to use cryoprecipitate or plasma instead of Factor VIII concentrate whenever possible."

Now, it's not entirely clear from the minutes whether that's a suggestion from Dr Harrison or someone else.

A. I don't know.

Q. It's not clear whether that's a suggestion that is then agreed by others.

A. Sure.

Q. Did that become The London Hospital's -- your policy in May 1984?

A. I don't think it did specifically, no. I can't say that there was a specific policy in 1984 that that should be the case.

Q. Then the reference there to "essential treatments". Do you know what was being encompassed in that concept --

A. No, I don't, but obviously it doesn't have a specific meaning.

Q. So it may be that was a suggestion rather a policy that was adopted?

A. It seems -- I mean, again, it is what it is. I understand what it says.

Q. But you can't recall any particular change in your own practice at The London Hospital after May 1984?

A. No.

Q. Then if we move on to late 1984, to December 1984.

Henry, could we have HCDO0000394_117.

This is a meeting, not one at which you attended, Dr Colvin, but this is a meeting that we know took place on 10 December 1984 at Elstree.

It's recorded here as being a note of Haemophilia Reference Centre Directors meeting but we can see there are a number of others who attend, including representatives of BPL and the PHLS, Dr Tedder and others. I'm just showing it to you to show that we can see Dr Kernoff in attendance.

A. Yes.

Q. That's 10 December. Now that resulted in the

production of a document that you would have seen, and we'll look at that next, HCDO0000270_007.

This is a document called Haemophilia Centre Directors Organisation AIDS Advisory Document. It was dated 14 December 1984 and our understanding is it was sent out -- we're not sure of the precise date -- to haemophilia clinicians.

A. Sure.

Q. Do you recall receiving this?

A. I don't recall this particular document being received but I recall December '84 very, very well, indeed.

Q. If we just look at the second page of the document, just so that we can see what was being recommended or suggested.

We can see that -- if we go towards the bottom of the page -- under the heading "Options in probably decreasing order of safety from AIDS for Haemophilia A", we've got heated UK concentrate as being the preferred option. It's noted that it's still a risk of non-A, non-B hepatitis. Second option:

"Single donor cryo or FFP."

Third option:

"Heated imported [concentrate] (... still [a non-A, non-B] hepatitis risk).

Q. Yes, and we'll look at that in a moment. We will just go towards the bottom of the page and then the top of the next page.

So we can see then the recommendations:

"Concentrate is still needed; bleeding is the commonest cause of disability and death.

"2. Use DDAVP in mild Haemophilia A and [von Willebrand's disease] if possible."

Then we can then see the recommendations in relation to haemophilia A. It's:

"... cryo or [heat-treated] NHS Factor VIII (if available)."

And we will see the issue in relation to availability of that in a moment.

For children and virgin patients, those not previously exposed to concentrate.

And then:

"... [heated] NHS Factor VIII, if available or heat treated ... commercial."

For severe and moderate haemophiliacs who don't fall within the groups already identified.

Then haemophilia B, fresh frozen plasma or NHS Factor IX, essentially for mild, virgin, those not previously exposed. Then there's NHS Factor IX for those who had previously used it.

"4. Unheated UK [concentrate].

"5. Unheated imported concentrate ..."

And the observation there is:

"... almost certain to be contaminated."

Would you agree with me that's -- I think it's clear from the heading, that's a set of options where safety is now the fundamental criterion that's informing that?

A. So obviously it says "probable decreasing order" so we don't know for certain.

Q. No.

A. And I mean, I can remember this really as if it was yesterday, in December '84, when I sit in my office with my head in my hands talking to Peter Kernoff saying, "What on earth do we do next?" because I think we'd reached, really, rock bottom, in December '84, of our understanding and our power to deal with this crisis, and I think really from January '85 onwards we began to address the problem and begin to get it right. But you're going to show me some documents which show that there was uncertainty on the relationship between 3 and 4, that is heated imported concentrate and unheated UK concentrate --

Q. Yes.

A. -- and that was a big dilemma.

So those were the recommendations. But if we just look a moment further on, we can see it says:

"In individual patients there may need to be a choice. In general heated concentrate appears to be the recommendation of virologists consulted but individual Directors may wish to make up their own minds. This is particularly true of unheated NHS material."

So still being left to the judgment of individual clinicians exercising clinical freedom?

A. Well, I think one can put too much emphasis on the concept of clinical freedom.

My feeling here is that we are looking at a series of questions to which there are no answers. The success of heat treatment depends on: (1) the donor pool; (2) the size of the pool; (3) the method of viral activation. All of those are critical in what happens next.

Now, obviously it's possible later that the donor pool is affected by tests for HIV but at that time it would have just been the donor pool, with or without donor selection, and we know that the commercial people have been actually trying to improve their donor selection.

So those three issues will determine the safety

of that product. You can't just talk about heat treatment. There are lots of different types of viral activation which were being tried out at the time. As you know, there was anxiety that the effect of the heat would actually damage the Factor VIII molecule and might reduce yield, which it did. It was also feared that this treatment would damage the molecule in a way which might result in inhibitor formation.

Most importantly, there was anxiety that the heat treatment wouldn't work. Now, why would we think the heat treatment wouldn't work? Well, all the work that had been done so far on hepatitis C -- which of course wasn't known by hepatitis C, it was just non A, non B -- suggested it didn't necessarily work and, although we now know that HIV is more heat-sensitive than HCV, we also know that in 1986 and 1987 a number of people in Canada and in Birmingham, UK, were infected with HIV from a heat-treated concentrate.

So I think we were in an impossible situation and this list, which is perfectly understandable and logical, is just a list of what is thought by one or more individuals to be the case.

Certainly in my own mind I was very concerned that if I use heat-treated commercial concentrate and it wasn't virally inactivated, I would infect a large

number of my patients who were not yet infected. At some point I'd like to discuss what Dr Winter said in his evidence about the early heat-treated concentrates, because I know that many of my patients were not HIV positive, and if I had given them all a heated commercial concentrate which was actually transmitting HIV, I would have transmitted HIV to a very large number of my patients who were so far uninfected.

Q. Can we look then at what was locally with The London Hospital after this set of recommendations was produced.

So if we could have first, please, BART0000676.

So we can see this another meeting of the North East Thames Region Haemophilia Working Party. It's 13 December 1984. So it's three days after the meeting at which the recommendations have been agreed but it's before the document that we've just been looking at has actually been finally drawn up.

We see, again, you and Dr Kernoff are there, along with a number of others, including, on this occasion as well, from the Royal Free, Dr Tuddenham.

If we go to the second page, please, Henry, we see "Update on AIDS". The first paragraph there is:

"The American experience suggests that the risk

of AIDS in treated Haemophiliacs is 1:300. It appears to be less than 1:800 in treated patients in the UK."

Now, I am not clear what the basis for those statements was. Do you know?

A. No, I don't know.

Q. Do you agree it doesn't necessarily appear to be correct in terms of an analysis of risk?

A. Well, I think it illustrates a level of ignorance that we all suffered from.

SIR BRIAN LANGSTAFF: Is it perhaps equating incidence with risk?

A. It might well be. But as I fear, we all know that the words "incidence" and "prevalence" sound the same but they are very different. Incidence is when things happen; prevalence is what's in the cooking pot.

MS RICHARDS: Then we can see that the next paragraph suggests that:

"A ... survey of 1,000 treated Haemophiliacs in England and Wales shows that 30 per cent are ..."

It says "HCTV", I think it must be "HTLV-III":

"... antibody positive. (The percentage may be higher in some centres - exposure to Commercial Factor VIII concentrate in the last 2 years may be a significant factor)."

Do you know what the source of that data was?

A. No, I don't. I mean, the chances are -- which year are we in now? We're in --

Q. This is December '84.

A. -- December '84, aren't we? Well, of course, the testing has only really just started. This almost certainly comes from Richard Tedder and, interestingly enough, it might come mainly from his centre because the Middlesex has a number of people with haemophilia in it, it's not a big centre but it may well be that the -- and this is pure speculation now, but it might be that the people in the Middlesex centre might be tested quite quickly by Dr Tedder, because he is in that area and, if that was the case, then it wouldn't perhaps be surprising that not so many of them are anti-HTLV-III positive because they are not big treaters.

Q. It may be worth just looking at the previous page and see who was there, and perhaps you just tell us where some of them were from, because you may recall the names.

Do you know where Dr Bolton Maggs was based at that time?

A. I think Paula was at the Royal Free at that time. She was doing work -- I mean, she was a clinician, of course. She had been -- had a very distinguished

career since, but I think she was doing work on Factor XI deficiency, which is particularly prevalent in the North London Royal Free community.

Q. Dr Boots, I think was from --

A. Colchester.

Q. Dr Colvin is yourself. Dr Edwards?

A. I don't know. Dr Edwards doesn't ring a bell.

Q. Dr Harrison is Dr Jean Harrison from the Regional Transfusion Centre. Dr Mills?

A. Southend.

Q. Dr Oakey?

A. That's Orsett.

Q. So Essex?

A. Yes.

Q. Dr Oxley?

A. Harlow.

Q. And Dr Traub?

A. Southend.

Q. And then we've got Dr Tuddenham for the --

A. And Sister Tubridy is my domiciliary nursing sister, who was the person who looks after haemophilia throughout the region.

Q. If we go back to the second page -- thank you for that -- we can see it's said:

"The clinical significance to those found to

have HTLV-III antibodies is not yet known."

Then it is explained that:

"BPL hope to have heat-treated Factor VIII concentrate available from April 1985 and that heat-treated Factor IX should be available later in the year."

Then there's a reference to a particular concern about the effect of heat treatment on Factor IX concentrate and a further discussion in relation to Factor IX.

Then we go to the next paragraph. We can see what's being agreed as at 13 December:

"Emergency stocks of Factor VIII concentrate held by associate centres should be exchanged for a heat-treated product by the Royal Free."

So that's the smaller centres in the local area, and then 2:

"Heat-treated material should be used whenever possible with the exception of the NHS Factor IX concentrate."

We will look at the whole thing. (iii) is:

"All new patients, and mild haemophiliacs with injuries requiring maintenance of high levels of [Factor VIII] should be treated with Heat Treated NHS [Factor VIII] or small pool [Factor VIII] concentrate

pro tem if treatment with cryoprecipitate or DDAVP is not possible."

So just trying to unpick -- sorry, Henry, if we just keep the bottom of the previous page for the time being. So bottom of that page again. If we just try and unpick the recommendations there, the heat-treated as a matter of generality is the express preference "whenever possible". Is that a reference to supply?

A. Well, not necessarily. I mean, I think you're probably going to ask me later about the way we used NHS unheated Factor VIII during the small transition period and I've already addressed my concerns about the risk of NHS unheated versus commercial heated. So I think I can -- I mean, I've already dealt with that part of the discussion.

The small pool Factor VIII concentrate which is being described I think is the product that I used in the paper that I've submitted, which was Professor Machin was the first author and I looked after two patients. That shows that there were tiny amounts of an NHS small pool heat-treated concentrate available at around that time, probably the middle of '84, rather before this decision was made.

Of course the new patients and the mild patients obviously had a certain priority. But I'm not aware

that by December '84 there was really any heat-treated NHS Factor VIII available, apart from my knowledge of the small pool, rather gently heated concentrate that I knew about and had obtained for my two patients.

Q. So we can see there's a slight difference of emphasis between point 2 and point 3 here. New patients and mild haemophiliacs falling within the description there, it's cryoprecipitate or DDAVP and if cryo or DDAVP is not an option heat-treated NHS or the small pool factor concentrate. There's no "whenever possible" or aspiration or aim. It seems to be a clear statement that's what the policy should be for those categories; would you agree?

A. That's really what it says. I mean, I think that when people are drafting their minutes or when they're making their statements they don't always know that 40 years later every word they've used will be dissected.

Q. No, no, understood. It's just this is the best guide we have --

A. Yes, of course, and I'm not complaining, I'm just saying it's very difficult to interpret individual words at 40 years' distance.

Q. The position of children is not expressly addressed there. Can you --

A. Well, the chances are most of the new -- most of the children will be new patients, if you like. But I take your point that the children do have a very high priority and that's, I think, a very fair point.
Q. Then if we just go to the top of the next page, to complete the picture, there's a reference to family members not volunteering as blood donors and then elective surgery being delayed until the position is clearer and the efficacy of heat treatment proven.
A. Which I think is the first time that's been mentioned.
Q. It is. So is it fair to infer that that course, delaying elective surgery, for the reasons there given, was a new measure being adopted around this time?
A. I think it was and, in fact, I happen to have -- because of one of my rule 9 questions, I happened to trip over correspondence with my surgeons in the very, very late part of '84 where I postpone an operation because that was the recommendation -- not then said this recommendation, but clearly we've been talking about it and so I contacted my surgeon and said, "Let's wait".
The difficulty with that decision, by the way, is that when would one be able safely to perform a surgical procedure and I think the answer to that

question, if you'd like me to give it to you, is probably 1988. The reason I use that date is that that's the date when the 8Y study is published. Even that is not a proof that 8Y was safe. It's only a probability but it's at the level of probability around 90 per cent.
So I think that really from '88 onwards published work that is safer or reasonably safe to perform elective surgery.
Q. Then can I just ask for those who were on home treatment at the time, what was the process that was put into place at The London Hospital to try and get back any stocks of what may have been untreated commercial products?
A. I don't recall that, I'm afraid.
Q. Would you expect there to have been a process?
A. Well, it's likely. I think we saw yesterday that I was using some commercial product in home treatment but I don't know who it was for and why it was being given; so, yes, you would have thought so.
By the way, if we're talking about recalling product, I can remember the most dramatic event of recalling product was at the time of the 1986 World Federation of Haemophilia meeting in Milan when we got the message that a particular commercial heat-treated

product was transmitting HIV, and that really caused enormous alarm.
Q. It's slightly out of sequence but I might as well ask you about it now. I know you gave evidence to one of the other inquiries about it. As I understand it, you were at the conference in Milan and received information at the conference about the issue of transmission from heat-treated concentrate?
A. Yes.
Q. What steps were then taken?
A. Well, I phoned home immediately and said, "Get this product out of the fridges and out of the patients immediately".
MS RICHARDS: Sir, I note the time. Is that a convenient point at which to stop?
SIR BRIAN LANGSTAFF: Yes, it is. We'll meet again then at 11.40.
(11.01 am)
(A short break)
(11.44 am)
MS RICHARDS: Dr Colvin, we were in December 1984. Just a couple more documents or three more documents from late '94/early '95 to look at.
BART0000519.
This is a document you've seen only this

morning, Dr Colvin, since you wrote it obviously many years ago. 20 December 1984, you're writing to Dr Harrison at the Regional Transfusion Centre, and it says this:
"Dear Jean,
"At your request I am writing to confirm that I am willing to continue to use non heat-treated NHS concentrate for the time being. This decision is, of course, in accordance with the discussions which took place at the recent Regional Working Party In Haemophilia."
Then you say you:
"... look forward to receiving heat-treated NHS concentrate as soon as it is widely available."
We'll take it in stages as we go through the correspondence, but can you recall, first of all, why you were being asked by Dr Harrison to give this confirmation?
A. No, I can't.
Q. You're setting out there a willingness to continue to use non-heat-treated NHS concentrate in circumstances where we know that the recommendation now from UKHCDO is to use heat-treated concentrate in preference to unheated concentrate. Why was that?
A. I've already explained, I think, that I was not

confident that heat-treated concentrate would in fact prevent HIV infection, and that if it didn't, then all my patients who were HIV negative would become HIV positive. So, as I've explained, this was an absolutely impossible situation in which to -- to place ourselves. I believe that, from this letter, that the decisions that took place at the Regional Working Party allowed me to take that action, and I think Peter Kernoff and I worked together very closely to try to construct a rational policy.

I have also confirmed, as we know, that heat-treated commercial concentrate was still transmitting HIV in 1986/1987.

Q. Although you didn't know that, obviously, at the time?

A. No, of course I didn't, but I knew that HCV (or at least non-A, non-B) was not necessarily being eliminated by the heat treatment processes that were being used at the time. And I've also -- I'll confirm it again that the safety of a concentrate depends on the donor pool, the way it's been selected, the size of that donor pool, and the level of viral inactivation, and those changed quite a lot over the period of two years that led up to the final safety of products by 1988.

Q. Just on the issue of size of donor pool, do you recall

what you knew at the time about the relative difference in sizes between NHS and commercial concentrates?

A. No, I can't recall that detail.

Q. Then if we go, please, to BART0000524, we can see that this is not long after that last letter. It's a letter from Dr Harrison, "Dear Colleague", 2 January 85. It says:

"Following further discussions between myself and Drs Kernoff and Colvin, it has been decided that, in the light of recent findings reported in both the lay and medical press, non-heat treated NHS Factor VIII concentrate will not be used in the [North East] Thames region after current stocks are exhausted."

So is it right that there was further discussion after that last letter between you, Dr Kernoff and Dr Harrison?

A. Of course I have no recollection.

Q. That would seem a fair inference to draw from the correspondence?

A. It looks as though that's -- I mean, what is written is written.

Q. Do you know what the reference is to "findings reported in both the lay and medical press"?

A. No, I don't.

Q. Then we can see there the proposal now is you won't use NHS factor concentrate in the region after the current stocks are exhausted. So you will use up what you have of non-heat-treated NHS and then presumably move to heat-treated or others?

A. That's what it says.

Q. Then if we look at BART0000526, this is a letter from you to the district treasurer and the finance department at The London Hospital, 20 January 1985, and we can just look at the second paragraph. You said:

"As I indicated in a previous letter we have reasonable supplies of non heat treated National Health Service concentrate at the moment which should last us for another two or three months. As noted in Mr Knight's letter we will then have to buy commercial heat treated concentrate unless, by that time, heat treated [NHS] concentrate that begun to become available."

So it would look from this as though, in terms of the treatments you were using in the first few months of 1985, your proposal, your intention, was to use up the current stocks or supplies of NHS concentrate, not heat-treated concentrate, which might

take you to approximately April of 1985?

A. That's the implication.

Q. At that point you would either have product from Elstree that's heat-treated or you would buy commercial products?

A. That's the implication.

Q. As far as you recall, is that what happened?

A. I have no recollection.

Q. What consideration did you give or do you think you would have given at this point in time to the fact that, notwithstanding the concerns you've expressed, the clear recommendation by this stage from UKHCDO was to prefer heat-treated over non-heat-treated?

A. I can only assume that Peter and I didn't agree with it.

Q. Then if we go on, please, to BART0000675.

This is a meeting now in May of 1985 of the Haemophilia Working Party of the North East Thames Region, and we can see again that you and Dr Kernoff are there.

If we go to the second page, we pick it up on the bottom half of the page, under the heading "Factor VIII Concentrate: Heat Treatment and Supply", we can see the position there set out as at May of 1985:

"The requirement that all Factor VIII Concentrate should be heat treated means that more commercial product is being used than before with the attendant risk of Hepatitis B and Non A/Non B Hepatitis transmission."

Then there's reference to pilot samples of the NHS product being effective. There's a report, in the next paragraph, from trial users of the newer NHS dry heated product. Then it says this:

"When sufficient is available this product will be the first choice for all newly diagnosed patients, children and others not yet exposed to the HTLV-III virus and will replace cryoprecipitate as the preferred treatment in appropriate cases.

"The USA experience confirms that cryoprecipitate has caused less Hepatitis and HTLV-III antibody sero-conversion, related to the small pool donations and the fact that cryoprecipitate is prepared from the plasma of volunteer donors unlike the large pool Commercial Factor VIII concentrate which is chiefly from paid donations."

Then at the top of the next page it said:

"Dr Colvin proposes to replace the use of Cryoprecipitate by NHS [heat-treated Factor] VIII ... concentrate as soon as possible. In the meantime

patients requiring infrequent treatments should continue on DDAVP and Cryoprecipitate."

So it would appear from this that by May of 1985 you're using DDAVP, you're using cryoprecipitate, and you're using commercial heat-treated products, or the region at least is using commercial heat-treated products.

A. Sure.

Q. The intention is to start using the NHS heat-treated product subject to availability?

A. I can't remember when the 8Y study starts. It must have been around '84 or '85 -- we will have to look to see when the 8Y study started -- but I was committed, as ever, to NHS activity, and I contributed, as we will probably discuss later, to the 8Y study.

Probably by the time we get to this paragraph I'm personally convinced of the safety of 8Y, although it hasn't been published and I can't be sure, and the proposal to replace the use of cryoprecipitate by 8Y seems appropriate, to me, at that point, I think.

Q. Now, in your statement, Dr Colvin, you said, in relation to the question of reverting to cryoprecipitate, that you didn't believe that reversion to cryoprecipitate for all patients in all circumstances was ever a realistic choice. We've

obviously touched on this to some extent but can I invite you to just summarise why you say cryoprecipitate for all patients in all circumstances was not a realistic choice.

A. Okay. So I've already indicated that I do use or did use cryoprecipitate for small children and also for the selected patients that I discussed in the paper we looked at yesterday.

I also explained that cryoprecipitate is allergenic and I also explained that cryoprecipitate is a single dose preparation whose actual dose is not known and that if you used ten bags of cryoprecipitate for one treatment, that would be ten patient exposures; that supply was not unlimited, contrary to what has been said elsewhere; and that the material was difficult to make up; and that certainly at night it would be extremely difficult to deliver eight-hourly treatment using cryoprecipitate in that way.

So I think all this is rehearsing material we've dealt with before and it is the case that I never believed that cryoprecipitate could be used in all circumstances for all patients.

Q. If one moves back from all patients, all circumstances, what about reversion to

cryoprecipitate -- in response to the AIDS crisis from January '83 onwards, say, what about reversion to cryoprecipitate for a much larger cohort of patients than you did?

A. Well, there's nothing I can say really.

Q. Would you accept that would have been possible?

A. I don't know.

Q. What are the factors that bring in uncertainty? Supply?

A. How much cryoprecipitate was available, how much we could arrange for material to be delivered to us, how much difficulty there would be in training people to do something that was extremely old fashioned, how to persuade people to use very large syringes to deliver treatment that had been delivered in small syringes. I mean, a whole host of logistic aspects.

But I don't think it ever actually occurred to me to do such a thing.

Q. So those logistical difficulties -- that's I think one of the phrases your use in your statement, in fact -- were not explored at the time. You didn't ask the questions that you have just posed, about whether you could get the supplies, how you could to the training, because, as you have just said, it didn't occur to you as an option at the time?

A. I didn't think it was realistic.
Q. But did you explore the practical realities with Dr Harrison or others?
A. I don't recall.
Q. What about the option of offering cryoprecipitate on an individual basis to your patients, explaining the pros and cons, as you have done to the Inquiry, and leaving the choice with the patient? Did that occur as an option?
A. No.
Q. Do you think it should have done?
A. No.
Q. Why?
A. Because I don't think it was realistic. I really don't think that this was a realistic option. I don't think anybody that I knew ever thought that to go back to using cryoprecipitate for everybody in all circumstances in this position was realistic.
Q. Forgive me, Dr Colvin, I understand your answer to all patients, all circumstances. What I'm inviting you to consider now is you could have sat down with individual patients, explained to them the risk of HIV, explained to them your view of the disadvantages of cryoprecipitate but the advantage in terms of viral safety, and the disadvantages of concentrate in terms

of viral safety but its advantages in the other respects you've identified, and given them a choice. That wouldn't necessarily have led to every patient taking you up upon that.
A. Well, clearly, I did do that for some patients. I presented a paper (admittedly for only half a dozen patients) where that exactly happened. So it's a matter of what I could do at the time and what was realistic and what seemed sensible to me and, at this distance, I don't really think I can offer you any more detailed answer to the ones I've given.
Q. Is it fair to say that it didn't occur to you to do this exercise of an individual conversation with every patient at the time?
A. That's fair.
Q. In terms of the categories of patients who you were during 1983 and 1984 treating with cryoprecipitate -- leave aside the children who we've discussed -- you've referred to there being some other patients who were being treated with cryoprecipitate, and we can see that from a number of the documents and from the study you referred to.

Can you recall what the characteristics of those patients were? Other than children, how did you decide which patients to offer cryoprecipitate to?

A. Well, I think what happened was that there were a small number of patients with haemophilia or von Willebrand's disease that was not suitable for DDAVP who were likely to require quite significant amounts of product and had not been treated much in the past, where it seemed to me that cryoprecipitate was a realistic option to cover a particular procedure that would otherwise use a moderate but not very large amount of material.
Q. So it was based upon the particular individual circumstances of a limited number of patients?
A. Exactly.
Q. If any patient had come to you -- and I'm thinking particularly here of the 1983/1984 period, the concerns about AIDS. If any particular patient had come to you and requested to change their treatment from concentrate to cryoprecipitate, would you have acceded to that request?
A. I can't answer that question really. I would always try to meet my patients' requests if I was able to do so. I can see that if a patient wanted to have cryoprecipitate on a particular occasion, I would certainly have considered it. If they wanted to go back on to home treatment with cryoprecipitate, I'm not sure that would have been possible, and

I certainly wouldn't have recommended cryoprecipitate for major surgery.
Q. Just dealing with home treatment, why would it not have been possible on an individual patient basis?
A. Well, I said I think it might have been possible on an individual basis. Nobody ever suggested it to me, and I never suggested it to a patient.
Q. Can we then consider the question of what information was provided to patients about risks of HIV.

First of all, would you agree that, once we get to January 1983: the New England Journal of Medicine, we haven't looked, but there were reports in the national media in the early part of 1983, the meeting at London Airport with Immuno, once that was known, that there was at the very least a very real risk that blood products could transmit AIDS, would you agree that patients needed to be told that?
A. I would expect that to be the case, yes.
Q. The purpose of telling them, would you agree, would be so that they could exercise a degree of choice as to whether to continue with their treatment or not?
A. Clearly, patient autonomy is important, so I accept what you say.
Q. Because it would be their -- ultimately, it's their health if they don't have concentrates; potentially

their life --
A. Sure. Yes, it's a matter of concern. I agree.
Q. Doing the best you can, Dr Colvin, when do you think you began to tell patients about the risks of HIV?
A. Well, the answer is that I can't recall. Any comments that were made by me about this would be in the clinical notes of patients, and that's the only place really one could look for a contemporaneous account of what happened.
Q. I don't know whether you can answer this or not, Dr Colvin: do you think it more likely than not that you did tell your patients about the risks of HIV from early 1983 onwards?
A. I know that I spoke to some patients because I have recalled speaking to some patients, but I can't recall obviously every patient. But there was -- certainly, during '83/'84, I was talking to patients about the risk of HIV, but I couldn't possibly tell you about each individual patient or whether there was a general policy of doing such a thing.
Q. The example or an example you give in your statement is having conversations with patients who were undergoing surgery or about to undergo surgery.
A. Sure.
Q. In terms of the routine clinic appointments that you

would have, as I understand it, at least with your regularly treated patients, you'd have on a -- every few months, you would expect to be seeing patients.
A. Yes.
Q. On those appointments, not for surgery but to decide -- you know, to see how the patient is and plan their treatment for the next few months, do you think you would have told them then about the risks of HIV?
A. Well, it was in the press, and they would have had the opportunity to see that information, and I would have expected them to ask me and for me to speak to them about it. But whether I did or exactly when I did, it can only be a matter of record, and we're now looking at 35 years ago and I can't make that kind of recollection.
Q. Would you agree -- this is as a matter of principle, as it were, rather than asking you to remember things you have indicated you don't remember. Would you agree that one reason why it would have been important to share that information with patients is not only so that they could take their own decisions about treatment but so that they could think about the safety of their close family?
A. Yes. I can see that point of view.
Q. Do you recall having any conversations with -- again,

in 1983/'84; this is before we get to the testing of your patients for HIV -- any conversations with family members, spouses about risks of HIV?
A. I have no particular recollection of such conversation.
Q. Is this fair: if there had been -- as we've heard some descriptions in some centres of group meetings being organised. If there had been any kind of group meeting that you'd organised, would you expect to remember that?
A. The only group meeting I can remember was one that took place much later when we did have such a group meeting, but I don't remember any group meetings at that time. Which is not to say there wasn't one, but I don't remember such a group meeting.
Q. This is a hypothetical question, Dr Colvin, in some respects, but I understand from your earlier answer that the best guide to whether you did spell this out to your patients at the time will be in your handwritten notes.
A. Exactly.
Q. So if they are not in the notes, the inference -- the correct inference to draw is in relation to that patient you didn't spell it out?
A. I think that's an impossible conclusion to draw, but

I did try to take clear notes.
Q. Well, can I press you a little on that because it's really about what your note-keeping habits would have been at the time, and I think you said yesterday you regarded the keeping of accurate and comprehensive notes as important.
A. Indeed.
Q. If you were telling your patients something as significant as, you know, there is a risk that this treatment could, if you continue with it or if you start on it -- for a new patient -- could bring with it a risk of what is understood to be, albeit imperfectly, a fatal condition. That's the kind of material that you would have included in your notes?
A. I would have expected to, yes.
Q. Can I come on then to the process of the testing of your patients for HIV and then being informed of their diagnosis.

We've seen reference and, indeed, you've made reference to Dr Tedder and the tests that he performed at the Middlesex. I understand from your statement that you had access to testing facilities through Dr or Professor Collier --
A. Yes, that's true.
Q. -- who was a colleague at The London Hospital?

A. Yes.
Q. So what, as far as you can recall, were the arrangements that you made for the testing of your patients for HIV?
A. I think I must have looked to see who I thought was at risk. And then we know that the virology department had stored samples of serum, which I had not control -- I mean, they did that anyway; it was automatic. Professor Collier decided that we could look at those samples and I think he had a test of his own, actually. I think he then referred his results to Dr Tedder, probably for confirmation of what the results were, and then at that point I decided to -- once he was confident of the results, I decided to communicate that to my patients.
Q. So, as I understand it from your statement and indeed evidence you have given elsewhere, you did not tell your patients in advance that you were testing their sera for HIV?
A. That's correct.
Q. We will come on to why in a few minutes.
In terms of the stored sera, you referred to communications with Professor Collier. Whose idea was it, if I can put it that way, to use the stored sera rather than asking for patients to come in and give

fresh samples?
A. I imagine Professor Collier and I decided to do it together, but I don't remember whose idea it was.
Q. Do you know or have any idea -- I don't expect you to remember specifics of dates for individual patients -- but do you know when this stored sera would have been stored? Because if you've got someone and it's sera stored from two years ago, it's going to tell you if they were HTLV-III positive two years ago but not whether they have, subsequently, seroconverted?
A. Well, we did know. We did know that there were such samples, yes.
Q. These were recent samples?
A. They were -- it's every three months or so that they would be seen, and then they would automatically go down into the store. So I think that the point you are making, I think, is there may have been some samples that went down at the time of testing. But I think that the situation was different at this time to the time later (when, of course, there were still people who didn't know whether they were HIV positive or not) at a later time, then they would have, I think, been asked if they were happy for their samples to be tested. We're talking now about the crisis when there were probably 80 patients who

potentially might have been infected, and that was a problem that needed to be managed at that time.
Q. Just dealing, if I may, with the stored samples, so a sample was taken at every clinic appointment?
A. Yes.
Q. A spare sample, as it were, that was stored?
A. Well, I wouldn't say it was spare. I think we -- the virology department and the -- maybe the chemistry department -- I'm not sure who stored the samples where. But I think every time we did anything that produced serum, it got stored.
Q. But as --
A. They're not spare exactly.
Q. You took -- you would take bloods on a routine clinic appointment for a range of tests that were going to be done there and then, as it were -- ALT tests, the liver function tests. This is a sample that you were keeping, not to test at that point in time but in case it might need to be tested at some point in the future?
A. I guess so. I mean, I don't think I was involved in the process of keeping samples. I didn't say, "Please keep this sample", particularly. I mean, my memory -- it's a very long time ago -- was that this is what happened.

Q. Just -- I know you have told us yesterday about how when you first started out you could actually do quite a lot of the testing yourself, you had, as it were, the skills that you had learnt on the job in the 70s. By the time we get to 1983/1984, where, physically, did the blood get taken from the patient and who by?
A. Yes. Well, once we knew about the HIV risk and we had reacted to it -- and I'm not sure exactly when that was -- as you may come on later, there was a discussion about high-risk labelling and once that high-risk labelling came into play, then I arranged that all my samples would be labelled by me before I went down to the clinic, and then my nursing staff took all the samples themselves so that patients didn't have to go down to the phlebotomy department to have their blood taken.
But exactly when that happened I don't recall, but I do recall extremely clearly arranging all the requests would be written out by me and that the nursing staff would take the bloods.
Q. We'll come on to the question of what the applicable standard might have been at the time, but why didn't you tell patients that their blood was going to be tested for HTLV-III?
A. I can't answer that question really because I think

what we did was to recognise the need for the samples to be tested, that there were 80 samples, probably, that needed to be sorted out, which was a very large number for a very small service, and that -- I must have felt at the time that that was the only way to proceed.

It's important to appreciate that the mindset of a haematologist is, as Mark Winter was explaining, that one is both a physician and a pathologist, and that the purpose of being there at all is to address a clinical problem and to solve it.

The advent of testing meant that finally we had some kind of handle on what on earth was happening and at the time, although of course things changed very much in the later part of the 80s, it wouldn't necessarily, and I don't think it particularly did, occur to me that there was anything very special about the anti-HTLV-III test that was different from any other blood test.

Maybe that mindset was a wrong one but it was a view that I think was very widely held in the haemophilia treating community, and we felt that we needed to get on to do this work.

Let's say for a moment, at the end of '84 or early '85, I had brought up 80 patients one way or

another to discuss with them whether we were going to test their blood. Because it needed to be done quickly; it wasn't a question of waiting for the next three-month appointment. They would then have been brought up to see me and I would have said, "We have this test which we think is now valid, but we're not sure" -- because there were false positives and false negatives -- "We can arrange for your blood to be tested but it will take at least two or three weeks for the results to come through and, when the results do come through, we'll call you all up again to deliver the message."

I wouldn't have wanted the patients to have telephone me for the answer. I wouldn't have wanted to write to them with the answer. What would have happened would have been that, over quite a long period of time, 80 patients would have had to come to see me, had their blood tested, spent two weeks worrying about what was going to happen next, and then have to be brought all the way back again, often from far away, in Essex, to be given the answer.

Now, maybe that was the right thing for us to have done but I think it would have been extremely difficult.

Q. Because the converse position is, as has been

described to the Inquiry by some patients -- not necessarily patients of yours, Dr Colvin, I can't recall whose patients they might have been, but the converse, some have described the sense of shock at being told that they have HTLV-III in circumstances where they didn't even know they were being tested for it.

A. I can understand that. What I can say is that, whichever way you look at it, there was going to be a huge sense of shock. I mean, let's say we'd done what I just said, bring 80 people up, testing them, bringing the 80 people up again and then telling them. They still have terrible shock, and it would be extremely difficult to avoid giving information on the telephone or by letter before they came to see me in person.

The situation was terrible whichever way you looked at it. I'd also say, and I know this isn't, in a way, perhaps, evidence, but nobody ever said to me in that period that they wished that I had asked their consent before testing their blood and giving them the information. I believe I gave every patient the information personally and I don't believe that any patient of mine ever complained at the time that they hadn't been treated properly.

Q. In terms of the telling patients the test results, did you only tell patients whose test was positive, and leaving aside, I know, the uncertainties about exactly what the positive test meant, but did you only tell the patients who had tested positive or did you also communicate the test results of those that were negative to the patients?

A. I must have told the patients who were negative that they were negative. Surely. I mean, I don't remember but I must have done.

Q. Can we look at BART0000675, please. So this is the document that we had been looking at for a different purpose a moment ago, the meeting of 22 May 1985. If we go to the second page we see at the top of the page it says:

"Concern was expressed about the confidentiality of personal details of HTLV-III [antibody] positive patients. No lists are circulated to avoid the risk of misuse by unauthorised people."

Then this:

"All patients will be informed of the result of their HTLV-III antibody test on request ..."

Then it goes on to talk about being counselled on the implications of a positive result.

This doesn't appear to suggest the intention

within the region automatically to tell every patient. It was left to patients who didn't know they were being tested to request the result?

A. To the best of my knowledge and belief, I went through all the positive tests and informed all of the patients in due course that their result was positive. Now, that's not an easy task and it would have taken a period of time and I think you have got another letter, which you may want to show me, which fleshes out the bones of this discussion to some extent.

But it was never my intention to withhold the results of anti-HTLV-III tests from my patients, and I don't believe any patient ever said to me that they thought that I had done so.

Q. Are you able to explain why these minutes, of a meeting which -- it was a regional meeting, obviously, not exclusive to The London Hospital but a meeting you attended -- records that patients will be informed of the result on request and then it goes on to say -- I didn't read this bit -- that:

"... doctors and dentists responsible for the care of 'positive' patients should be informed of positive tests ..."

So there appears, in this, to be automatic provision for telling treating doctors and dentists of

the result but patients will only be told on request.

A. Well, I'm puzzled by this note myself, because I find it hard to believe that there was a policy of telling people their results on request, in the sense they had to come and ask me to deliver those results. It doesn't make sense to me.

Q. Because that would be impossible if they didn't know they were being tested in any event?

A. I agree that's possible. When I look back on the 40 or so patients who were positive under my care -- which would not actually be the number who seroconverted under my care, because I saw a lot of patients who had seroconverted elsewhere. So I don't believe that I ever heard of or saw a patient who said, "Dr Colvin, you didn't tell me about my HTLV-III positivity", or HIV positivity. It just didn't -- to the best of my knowledge and belief, it never happened.

Q. Can we look at the letter that you may have just been alluding to, Dr Colvin. It's BART0000535_001.

So this is a letter from 16 April 1985. If we go down we can see -- go down to the bottom. We can see it's from you. It's "Dear [colleague]", and it's attaching a set, I think, of recommendations.

In the last paragraph you say this:

"I now have anti-HTLV-III results from many of the home treatment patients ..."

So that may give us an idea of the timing. This is April 1985.

A. Yes.

Q. "... I am reluctant to give the list wide circulation because of doubts about the meaning of the test and the sensitive nature of the information."

Then you say this:

"By no means all the patients who are positive have been told the result of their test though I have not hidden the information from those who wish to know. If you need to talk to any of them about the AIDS problem perhaps you could give me a ring to discuss the latest position."

Can you explain that sentence, "By no means all the patients who are positive have been told the result"?

A. Well, if you go to the second paragraph:

"Secondly I have issued a set of guidelines for the management of haemophilia in general."

It goes on to say:

"This is also rather parochial but you will note that we are treating all samples from patients with haemophilia being 'High Risk' and are attempting to

deal with the difficult problem of providing adequate dental care for our patients."

Then the paragraph I think above -- sorry, I need -- can we go back to the main letter.

Q. Yes, of course.

A. Sorry I was choosing the wrong paragraph:

"Thirdly and perhaps most importantly ..."

Not "not importantly".

"... I have given all our home treatment programme patients a new set of home treatment rules together with some counselling on the AIDS problem and virtually all of them have now been spoken to."

So I think what this means is that I had a discussion with all the patients at risk, would have discussed whether they wanted to know about their situation and about the AIDS problem in general, and would have provided them with information.

I clearly -- autonomy would also dictate that if a person didn't want to know the result of the test, then they could refuse to have it. But I don't remember anybody ever saying such a thing. So I think you need to take these two paragraphs together.

Q. Let me see if I've understood that, Dr Colvin.

So the paragraph you have just drawn attention to is to say you think you have spoken to all the home

treatment patients or "virtually all of them", it says here, have been spoken to. It is clear from the next paragraph, the sentence I asked you about, that there must be some patients who have not been told the result of the positive test. Reading them together, it is your suggestion that that's because they may have not wanted to know?

A. I don't know. I mean, I've only seen this letter this morning.

Q. I know.

A. I am very surprised by it because my memory is, and I repeat it, that all the patients who were anti-HTLV-III positive as a result of the testing process were told of their results and were counselled and were involved in their care at a complete level, and I would never have dreamed of not telling patients about their results. It doesn't make sense to me.

Q. Okay. Can you recall if you communicated the results of the tests at specially arranged appointments? So for the 40 or so who were positive, did you bring forward their routine appointments to speak to them relatively urgently or was it at the next routine appointment?

A. I think there probably was a mixture, depending on whether -- perhaps when the appointment was coming up,

but I would have thought most of the communication would have been taking place outside the clinic. Because, I mean, the clinic itself is not a good place to have those sort of discussions.

So, I mean, I can recall one or two of the conversations but not many of them at a specific level. But I think that I would have -- I would have seen patients separately. But I can't confirm that and I again repeat that nobody under my care ever said to me, "You didn't do that properly."

Q. In terms of the counselling or information that was provided, what -- as far as you can recall at this stage, in 1985, what kind of information do you recall you were giving your patients who had tested positive?

A. That was obviously a long-term discussion. You can't just do the whole thing at once. And what I recall is that I assessed the situation, produced an information sheet on the subject and then gave them that information sheet, which -- it hasn't appeared in the papers, I don't have a copy myself, but there's no doubt there was such an information sheet and that was given to patients to explain to them their situation and the precautions they needed to take.

So there was no formal counselling at the time, it was down to me. But I did prepare a document.

Even my nursing staff -- there weren't that many nursing staff at that time, we were still very short of nursing staff. So, whilst I relied on my nursing staff, it was my job and I believe that I fulfilled my obligation by giving them the information and giving them the leaflet and then continuing to see them to the best of my ability with the counselling I had available to me.

Q. Did you advise them all as to the risk of sexual transmission?

A. Yes, of course.

Q. Were tests offered that stage or at a later stage to the partners of the patients?

A. Many, many partners were tested but exactly when I can't recall the detail, but there's no doubt that partner testing did take place at my centre. Although, as you know, I've said in my statement, that sadly one of my patient's partners was infected. But that wasn't -- mine was not the only centre where that happened, and only one -- I think one person was infected.

Q. One partner?

A. Yes.

Q. Then, in terms of the numbers who were infected with HIV, you have set those out in your statement. If we

could just look at that, it's paragraph 63. I'll just put it up on screen as well for ease of reference. It's WITN3343007, Henry, and it's page 21 of that document. We can see it's paragraph 63, bottom half of the page:

"As of September 1987 (source personal educational lecture series).

Severe haemophilia, 31; moderate/mild haemophilia A, 9; haemophilia B, 1; von Willebrand disease, 0."

Children is not there identified. How many -- I don't know if you can recall an exact number, but approximately how many child patients were infected with HIV?

A. Okay. I had three patients who had been to Lord Mayor Treloar College, so they were shared care, and I didn't have really any influence on the treatment they had at the Lord Mayor Treloar. I know you will be discussing that at a later time. So those three patients were all anti-HIV positive.

There were three other children I can immediately remember who -- two of whom eventually died in adolescence of AIDS, and one of whom died of a head injury in early adolescence. Those are the patients that I can remember.

Q. So six children, three of whom were Treloars children who were infected with HIV?

A. I think that's the case, but of course, one of the problems that I have identified for the Inquiry is that, first of all, there's the definition of a child, and then secondly, there's the definition of when the childhood begins and when it ends and when the infection takes place. So it's quite a complex question which I find very difficult to answer.

But I believe, as far as I'm aware, that there were six children under my care who were anti-HIV positive.

Q. Is that number subsumed within the overall total here which is 41?

A. Oh, yes. It's included within.

Q. And of those six children (and, again, without asking you obviously to deal with any individual by name or particular circumstances), do you recall whether those six children or any of those six children, were they all severe haemophiliacs or were some of them within the moderate or mild group?

A. At least five were very severely affected, and one of them -- the one who died of the head injury -- may have been moderate rather than severe. By that I would mean a level of, say, 1 or 2 per cent

Factor VIII, as opposed to less than 1 per cent Factor VIII.

So the answer is that all six were really severely affected.

Q. Then you go on to say in the statement that some of these were patients -- this is how I am reading it, looking at your statement. Please correct me if I'm wrong, Dr Colvin. Some of these were patients whose treatment was shared between you and an associate centre?

A. Yes, and some of them were patients who had come to me already infected -- because this is 1987 now -- so that some of them were patients who had, for some reason, arrived in Whitechapel already infected. So it might be that somebody came to Whitechapel for -- or at least to London for university. It might be that we had another immigrant community.

There was one patient -- I am not sure if it was '87 or not. But suddenly one day arrived in Whitechapel an African refugee with no English who turned out to have appalling haemophilia and was also HIV positive. Although, curiously, he wasn't hepatitis C positive. So his HIV positivity must have been transmitted either sexually or by blood transfusion because if he had concentrate, he would

have been HCV positive as well.

So how to manage a patient who arrives on my doorstep with no English who's got a terrible haemophilia and is HIV positive is, if you like, for you an example of how difficult our lives could be.

Q. Is it fair, however, to assume that the majority of the numbers listed here were patients who were being treated at The London Hospital and/or one of the associate centres within the region at the relevant time?

A. That's likely for the majority. The point I'm trying to make is that the 41 patients were not all mine exclusively by any means.

Q. If we just go over the page, please, to paragraph 64, what you have said there, and this was in answer to a question about what work was done to establish dates of seroconversion, you said:

"The timing of infection was studied using stored serum samples. The result was that it was possible to determine the probable location and even date of infection fairly accurately in a number of patients."

Then you talk about the patients often wanting to know the site and date infection, but given significant movement between centres, many of them

could have been infected at any centre they had attended.

I'm not clear from that, Dr Colvin, whether work was done at The London Hospital to -- at some point after you got your initial results in 1985 or thereabouts -- to do effectively what you've just told us was done at the Royal Free and to look at the -- go back and test?

A. Yes, we did. Yes, we did.

Q. Your statement doesn't tell us what the outcome of that was.

A. The outcome was that some patients, particularly perhaps the Lord Mayor Treloar patients, had been seroconverted very early.

Q. What about the bulk of your patients?

A. I don't recall.

Q. In what form, if you can answer this, was that information collated? In other words, if the Inquiry wanted to try and find that information, where would it have been?

A. Well, when these tests were done, I wrote out a carding system, which I've described in my statement, and I believe that in that carding system are records of most, if not all, of that look-back, if you would like to call it that, and when I retired

I gave that carding system to John Pasi or Dan Hart for safekeeping, and I believe they have kept it. But it would be very difficult, I think, to go through every single patient's notes to find out. I think that would be the best source.

Q. Okay, thank you.

Then -- I've put your statement away. There was something else I wanted to ask you about it.

You say, I think, somewhere in your statement about two patients being infected at other hospitals. So, as I understood, it was two patients under The London Hospital's care but whose infection was elsewhere.

A. I mean, there were a number of patients of mine who could have been infected elsewhere, and sometimes I knew they had been.

I think one of the points I wanted to make, and it may be relevant, is that patients understandably wanted to know where they'd been infected, and yet for me, it wasn't -- what was much more important was to look after them, having been infected, rather than just spend a lot of time working out exactly when and where they had been infected in a way that wasn't necessarily going to help their care.

So, for me, it was much more important to look

after my patients than to worry about exactly where they had been infected. Because many of them, or some of them, had been infected outside my care, but I didn't care for them any less because they had been infected away from my care.

Q. Were there any patients in respect of whom you were able to establish that their infection had been not at a haemophilia centre, yours or associate, but at another hospital; for example, receiving emergency treatment in an A&E department? Did you have any patients that fell within that category?

A. Yes.

Q. What, if anything, without revealing any identifying details about any individual patient -- what, if anything, can you recall about that?

A. Well, this is very difficult because you've just said that you don't want me to identify a patient, and of course I can comply with that to the best of my ability. But I will, if you wish, try to tell you a story that is de-identified. Would you like me to try to tell you a story that's de-identified?

Q. Yes. If it assists, Dr Colvin, what we are interested in is examining if there were, as it were, systemic failures or systemic problems which led to patients with haemophilia being treated in circumstances where

they wouldn't necessarily have received that treatment at their haemophilia centre. Dr Winter gave us two examples of that, and I'm just --

A. Do I have to reveal the centre at which they were treated?

Q. The hospital but nothing else for present purposes.

A. Well, I mean, if I identify the centre at which the person was treated, he can almost certainly be identified fairly easily by the Inquiry or by other people listening to this discussion.

Q. That would be possible to be identified simply from naming a hospital?

A. Yes. I think it would.

Q. In that case, what I may ask -- we can perhaps ask you to give that information in the form of a supplemental written statement on that issue later.

Are you able to tell us anything about the circumstances in which patients with bleeding disorders became infected not at haemophilia centres that doesn't identify anything?

A. Well, I think what I'm going to try to do is to, if I may, not mention the centre itself for the moment. I may mention it at a later time in private or in a witness statement, but I can give you an example of something that happened which is important.

So a patient comes to London for educational and professional purposes. He's a young man who's got relatively mild haemophilia. He has had an injury, a sports injury, and he's treated and develops HIV infection. He is, understandably, deeply upset by that, and I spend many, many, many hours with him and with my nursing staff trying to manage his distress and his clinical circumstances. Some years later, he becomes mortally ill, and my haemophilia nurse and I are told about this one morning, and we drive over 100 miles to try to reach him before his death, and he's unfortunately passed away when we arrive there.

I tell this story really to illustrate the terrible nature of what was happening and I tell it particularly to describe his resentment at the fact that he had been infected in a way which he thought was completely unnecessary.

Now, I'm not in a position to judge whether his infection was unnecessary or not, I wasn't there at the time when he was treated and infected, but I do know that he deeply resented what had happened and I can understand that.

But the point that I was making before was that I wasn't sure that that deep resentment necessarily helped him to manage his situation, and that my job

was to do my best to help somebody in a position which was, of course, intolerable and terrible, and which I and my nursing staff spent many, many hours trying to resolve. It cut to the heart of what we were trying to do.

Q. If we look at one further document, Dr Colvin, it's BART0000509.

This is a document that you've seen:

"... enclosing a confidential list of patients who were under our joint care whose anti-HTLV-III results are known to me."

Then appended to this are, by reference to different associate centres, lists of patients, dates of testing and the result. Is this the means by which you were communicating to the directors of the associate centres which of their patients were HTLV-III positive?

A. Obviously, I can't recall this letter. When I was sent it, I was very surprised because I didn't remember ever having seen it before. But when I looked at it I was able to see that each of these separate centres had been addressed and that, I think you're right, this was the means by which I communicated the anti-HTLV-III results to the associate centres. It relates a bit, as I've pointed

out in my personal statement, to the letter which -- or the minutes that you have are already referred to, and I put that in my personal statement.

Now, my belief is that by this time all these patients would have known of their status, and I believe it was my duty to share with my associate centre directors the information I had, for many reasons, including the reason that at the time we were regarding samples as hazardous, and therefore they needed to know, partly for their own information about how to treat these patients if they appeared in their hospitals and also partly to know how to deal with their samples.

Q. Just in terms of timing, is it right to infer from this, 27 August 1985, by this time or around this time you would have completed the process of the testing, at least of the patients that you had identified as being potentially at risk?

A. Obviously, I can't prove that but I believe so.

Q. Can we then just look at a couple of further documents on the issue of providing information to patients.

BART0000674, please.

If we go to the third page, we can see under the heading halfway down the page, "HTLV-III Antibody Positivity" -- sorry, I should have said this is

November 1985, 27 November 1985, and it's the North East Thames Region Haemophilia Working Party again.

You can see here what's being said:

"It was stressed that informed consent should be obtained before patients are tested for HTLV-III antibody and wherever possible counselling should be offered prior to testing."

This is regional rather than simply limited to The London Hospital, but this would seem to suggest that there were still, at least regionally, patients who had not yet been tested?

A. Of course it is possible. I can't be sure of this.

Q. Here it's being said that their informed consent should be obtained before testing.

A. Yes, and -- indeed, I mean, there were one or two patients who we realised, even in the late 80s, had been at risk that we didn't realise had been at risk, had been lost to follow-up, and somebody would come to the hospital and my nurses and I would say, "Good heavens, this patient is potentially anti-HTLV-III positive" and then we would, of course, get their consent before testing. Because the testing without consent was really confined to that period, I think, of crisis in the latter part of -- the very end

of '84/early '85.

Q. Can we just see the follow-up minutes to this. So this is saying informed consent should be obtained before patients are tested for HTLV-III antibody, and then if we go to BART0000673 -- I hope this is the right reference -- at the bottom -- so we can see the date is 25 June 1986. So this is the middle of the next year. Again, it's the same working party. You are present, Dr Kernoff is present, and others.

If we go down towards the bottom of the page, we can see it says:

"Amendments of Minutes of the last meeting - 27th November, 1985."

Then under "HTLV-III Antibody Positivity", it is suggesting that the minutes should be amended to read "informed consent should be obtained if it is possible or reasonable to do so before patients are tested".

So the rather more absolute position that was articulated in the minutes in November does not appear to represent, in fact, the policy of the region, which is you obtain informed consent if possible or reasonable.

A. Well, minutes are, of course, the only record we have of these discussions. They don't necessarily -- no minute necessarily gives an absolutely accurate view

what was decided. That is a sad fact of minutes in general. But there is quite an interesting example of where it's not quite clear what a minute perhaps meant. Even a very definite minute may not have meant exactly what it was said to mean in the minds of the people who were present at the meeting.

Q. What we can see is in the middle of 1986, so away from that crisis period that you've described, the position that's being articulated -- presumably on mature reflection, because you have taken the positive step of amending a previous set of minutes -- is to say -- it's not "always obtain informed consent", it's "obtain informed consent if possible or reasonable to do", with no guidance as to the circumstances in which it might be possible or reasonable to do that?

A. I can understand that. But, I mean, I think by the middle of '86 -- I mean, we're now well into that period -- I would expect that if I needed to consider an anti-HIV test or anti-HTLV-III test, I would have thought I would have obtained informed consent to get it.

Q. In your witness statement, Dr Colvin, if I can find the paragraph reference for you.

It's paragraph 56, Henry, it is WITN3343007.

If we go to page 19, we see, towards the bottom

of the page, in paragraph 56, you are addressing the issue that I've just been asking you about over the last 20 minutes or so, Dr Colvin, about the process of testing patients for HIV.

Then if we go to the next page, what you have said there in the third -- actually, before we get to that, in the first paragraph you refer to Professor Zuckerman being particularly interested in the search for the NANBH virus, which is not in itself a controversial statement but you say that in the context of a discussion about the retained samples. Were the samples of sera that were retained at The London made available to Professor Zuckerman?

A. Well, I think there was one particular occasion when one of my patients had a nasty attack of clinical non-A, non-B, when I must have discussed this with Professor Collier probably, and he said, "Well, Arie Zuckerman is particularly interested in looking for the source of non-A, non-B hepatitis, may we share this sample with Arie?"

And Arie wrote back to me initially saying that he thought he had made a big advance in the search for the virus, and in fact he later said that he hadn't.

Of course, it was during this time, and we just heard from the Nobel Prize-winning material recently,

that British physicians and pathologists were indeed searching very, very hard for the virological cause of non-A, non-B. I think I accepted with Professor Collier the idea that this particular acute attack of non-A, non-B in a patient of mine might by looked at by Arie Zuckerman.

Q. Would you have told the patient that?

A. I shouldn't think so, no.

Q. We go on to see you say -- you refer to:

"... an emerging discussion, which stimulated a considerable divergence of medical and medico legal opinion, as to whether further consent for anti-HIV testing was needed."

Then you go on to say this "culminated in the GMC guidance of 1988". I think you perhaps should just look at that because we haven't looked at it in the hearing so far.

Henry, it is NHBT0010410, please.

So the first page is a press notice, May 1988. If we go over the page we see the GMC's paper that it put out. There are a number of matters relating to HIV and ethical considerations, which I don't need to trouble you with, Dr Colvin.

If we could go to paragraph 12 please, Henry, which should be the bottom of the next page.

If we pick it up in paragraph 12, under the heading "Consent to investigation or treatment", what's there said is:

"It has long been accepted, and is well understood within the profession, that a doctor should treat a patient only on the basis of the patient's informed consent. Doctors are expected in all normal circumstances to be sure that their patients consent to the carrying out of investigative procedures involving the removal of samples or invasive techniques, whether those investigations are performed for the purposes of routine screening, for example in pregnancy or prior to surgery, or for the more specific purpose of differential diagnosis. A patient's consent may in certain circumstances be given implicitly, for example by agreement to provide a specimen of blood for multiple analysis. In other circumstances it needs to be given explicitly, for example before undergoing a specified operative procedure or providing a specimen of blood to be tested specifically for a named condition."

Then if we go over the page, at paragraph 13:

"Testing for HIV infection: the need to obtain consent:

"The Council believes that the above principle

should apply generally, but that it is particularly important in the case of testing for HIV infection, not because the condition is different in kind from other infections but because of the possible serious social and financial consequences which may ensue ..."

So, pausing there, Dr Colvin, you're right, I think, to point out at this is the first publication by the GMC on this specific issue, and it was in 1988. Would you accept that the GMC is not here saying anything new? It might be speaking for the first time on the issue but it's saying the long established principles of informed consent apply?

A. Of course the long established principles of consent apply, but I don't believe that it was ever the case that there was ever any other particular investigation which absolutely required specific patient consent. This was really the first time, in '88, that this had been suggested.

I referred earlier to the role of the haematologist. That is being both a physician and a pathologist. So if I was sent a patient for investigation, then I would decide which investigations to perform, and when I got the results of those investigations in my laboratory, I would then choose what further investigations were to be

performed to solve the problem.

Now, all that has a continual flow of information, of course, and it's my responsibility to choose the right tests in the right circumstances. However, there may be another instance where that chain of flow has been broken, and then it would be necessarily to go back to the patient to ask for consent to continue a separate investigation that now became apparent.

But the reality was -- I think this was partly the mindset of haematologists -- that we felt that we were physicians who were also pathologists who had the task of solving problems and we believed, perhaps incorrectly, that we did have the kind of consent that we required to undertake these investigations to solve an urgent problem.

Now I can see there may be those who disagree with that view but that was the view I think that we had.

Q. These were samples, as I understand it from your evidence earlier, that they weren't samples that had been taken back in 1979 and stored since then, they were samples -- because you were using them for the purpose of finding out whether the patient was currently infected, they were the samples that would

have been taken at the last appointment?

A. Yes.

Q. So for most of these patients they are likely to have been samples that would, in fact, have been taken in the course of 1984?

A. Possibly, yes.

Q. Possibly in 1983 but most likely 1984. At which time you may not have had, in June 1984, to pick a date, potentially, at random, you wouldn't that stage have been able to test for HIV but you would have known that testing for HIV was something that was being investigated.

A. Sure.

Q. How could it be said that a patient's giving informed consent to the use of their sample for testing for a virus that no-one's spoken to them about?

A. I mean, I understand what you're saying. I'm just telling you what I think was in our minds. What was in our minds wasn't necessarily appropriate. I mean, I personally don't regret what we did. Maybe I shouldn't say that but I really don't regret what happened.

But the position was that nobody explicitly said that HIV testing had to be done with informed consent until much later.

Q. You have previously made a number of observations at invitation about the lack of UKHCDO guidance on certain issues. Can you recall whether there was anything that emanated from UKHCDO at this time, either in the meetings or in any communications on the issue of how to go about the process of testing?

A. No, I can't. I mean, we had a look at something a while back, I think, which just said everybody ought to be tested for anti-HTLV. I mean, we had that discussion yesterday I think.

Q. Yes, but on the specific issue that you're talking about, about whether the patient's consent should be sought in advance --

A. I don't think, although I can't prove, but you are in a better position to judge than I am, I don't think that UKHCDO gave a view on that.

But I don't actually want to spend all my time blaming UKHCDO for the things that happened in our practice. I mean, I recognise that UKHCDO couldn't tell us exactly what to do about everything.

Q. Then just dealing with some issues of consent and information and testing more broadly, we looked yesterday at documents which showed the programmes of testing that would be undertaken at routine appointments in relation to liver function --

A. Mmm.
Q. -- with some tests being undertaken more frequently, depending upon the outcome of first sets of tests and so on.

Can I just ask you to assist in relation to the information provided to patients about that process. Again, we're talking here typically late 1970s, early 1980s, so I'm asking you now about before HIV was on the horizon in '82 and '83. What information do you recall giving or thinking would have given to patients about the reasons for taking samples of blood from them?
A. Well, I think we would have told them that the liver function tests that we had were not always normal, that the outcome of those abnormalities was unclear, and that we needed to continue to monitor their progress or lack of it.
Q. Given that the practice, as I understand it, of taking a sample that was then stored for potential future use was a regular occurrence, it wasn't something that you did new in 1984 --
A. No, sure.
Q. -- did you tell patients that you were doing that, that you were taking a sample that was not going to be specifically tested for liver function tests but would

actually be stored potentially for future use?
A. Not that I know of, no.
Q. Before we come to look at testing for HCV, which we can do after lunch, there's just one small point which will require us to look at two documents quickly that I wanted to ask you about, and that's about providing information about patients to third parties.

If we look at HCDO0000177_010. This is just an example -- there are lots of other examples from lots of centres about information that was provided to Oxford. This happens to be January 1984. We can see it's from you to Ms Spooner referring to the registered details and making a number of comments about specific named patients. We know that information was regularly provided by centres --
A. Yes, sure.
Q. -- to Oxford for the purpose of inclusion on a register. What did you tell patients about that process, if anything?
A. Nothing. The policy was very clear that, from the 1960s, the UKHCDO record had been a named patient basis, and the data that we had was transferred to Oxford. The result, of course, was that we had the best database, as I think I mentioned, in the world for knowing who was where, what they were being

treated with, what their details were, and what was happening to them.

Now, I've seen this -- obviously, you've given it to me -- and it doesn't surprise me. That's the way it was.
Q. I think we know, when we come on to it this afternoon, it was an issue that then reared its head again in the 1990s at various UKHCDO minutes. I'll come onto that.
A. Of course. At this time, it looked completely routine to us.
Q. Then just one further document before we break. BART 0000543_004.

So this is a letter, January 1986. This is you sending data to Dr Craske. If we just go to the second page, please. So we can see it's from you. If we go back to the first page. We've obviously concealed the names, but you are here sending Dr Craske at the Public Health Laboratory in Manchester information data about named individuals: their HTLV-III result and the date of it.

Again, I understand why that data was being collected by Dr Craske; we see it from the UKHCDO minutes. But did your patients know that their test results and names were being supplied to Dr Craske?
A. I don't believe so, no.

MS RICHARDS: I think that probably is a convenient moment, sir, to break for lunch.
SIR BRIAN LANGSTAFF: Yes. It looks like 2.00, then, that we will meet again. So 2.00, if you please.
(1.02 pm)
(Luncheon Adjournment)
(2.00 pm)
MS RICHARDS: Dr Colvin, I'm going to ask you in a moment about HCV testing. Before I do so, hepatitis B: your statement says that you had a comparatively small number of patients who had an ongoing hepatitis B infection; is that right?
A. I think we all did. We all had a very small number of hepatitis B infection -- patients who had ongoing infections, yes.
Q. You had a larger cohort of patients who had shown some signs of being infected in the past --
A. Yes, it was absolutely routine to test people for evidence of previous hepatitis B infection. To begin with that was just something that one thought of because of the risk of hepatitis B infection from blood products, but later of course, by '86, there was a recombinant hepatitis B vaccine, so it became really important to test people for hepatitis B or evidence of previous infection so that one could offer

vaccination to those who had not been infected, particularly, of course, small children, would then become routinely vaccinated against hepatitis B.

Q. So in terms of the hepatitis B vaccine for your patients, your recollection is that was the mid-1980s?

A. Yes. You see, the original hepatitis B vaccines were not recombinant so they would have been derived from human plasma. They were almost certainly non-infected because of the way they had been prepared and treated. But I think that by the 80s we had already become wary of blood products and the prospect in '86, I think it was, of a recombinant hepatitis B vaccine was very attractive.

Q. Had you used the hepatitis B vaccine the non-recombinant --

A. I'm not sure I had actually. I really can't be sure but I think as soon as the recombinant vaccine became available we all said: We've got to do this, particularly for children.

Q. Were you aware of the possibility of sexual transmission of hepatitis B?

A. Yes.

Q. Were your patients counselled about that, those who did have infection?

A. Yes, I mean, I had two or three patients that I --

I don't recall whether -- it would have been back in the 70s probably that I would have -- I don't think we had any seroconversions from probably 1975 onwards. So yes, in principle, but I don't recall the detail.

Q. Then can I move to the process for testing for hepatitis C. So this would have been 1990/1991, perhaps 1992, but the early 1990s.

My understanding from your statement, Dr Colvin, was that was also done from stored sera without seeking patient consent in advance?

A. Correct.

Q. Bearing in mind that this is now the early 90s, and you weren't in what you described as the sort of acute phase, from late '84/early '85, of trying to deal with the HIV emergency, why was it that patients' consent was not sought for the testing?

A. So you have already referred to hepatitis B, and nobody ever thought of -- I don't think -- requesting specific consent for hepatitis B testing in the past. Maybe people were requested -- maybe people did ask for permission but I don't remember that being done, and I think that hepatitis B testing in the past would have been done without consent largely. I think so. Hepatitis A testing, of course, also was done -- anti-HAV testing also done without consent.

So the next question is: was it appropriate to test for hepatitis C without consent when that test became available? I want to quote very briefly from the Penrose Inquiry report, if I may. May I do that?

Q. Yes.

A. So in 32.40 of the Penrose Inquiry Report it states, it's a little extract:

"Professor Nathanson ..."

Who at that time I think was head of ethics at the BMA:

"... recollected that in about 1986-87 the BMA had actually voted for a policy which allowed doctors to test without consent. As soon as the policy was agreed, the BMA took legal advice and they were advised that testing without consent was illegal. The policy was reversed the next year at the BMA's annual meeting of 600 representative doctors. In fact the BMA had received conflicting advice from two QCs on the topic. The rationale of the advice which was followed was that because the HIV test was not a standard test, the implied consent given was, therefore, required."

So the position as far as hepatitis C was concerned was that we'd known about hepatitis C for, I suppose, 20 years and the patients had known as

well, and so I think that the testing was part of the routine work of looking at liver function and the healthcare of people with non-A, non-B infection. It wasn't HIV and the GMC guidance was about HIV testing. So it didn't really fall within the HIV testing regulations, if you like to call them that, of the GMC. There was obviously debate in the BMA, if not in the GMC, about what was appropriate, and it wouldn't be until, I think, 1999 that there was any advice that was clear from the GMC on communicable diseases in general.

So I think in my mind whilst this was not what one might describe as a completely routine test, nevertheless it fell within the area of testing which was appropriate for the consent that had been given for the liver function test that had been performed over the previous 20 years.

Q. Again, were the samples that you were using recent samples or were they --

A. They would have been, I guess, contemporaneous samples, yes.

Q. So they would have been taken over the months preceding the test. It would, presumably, have been perfectly straightforward to tell the patient that one reason why the sample was being taken was because

there was, by that time, a realistic hope of having an HCV test?

A. Well, that may indeed have been the case. We may well have talked to patients about the possibility of an anti-HCV test. Also, it's entirely possible that, as the test became available, I may well have spoken to patients and they may well have given me consent for the test to be done. That's perfectly possible. I can't be sure it was.

But, again, I point out the logistic difficulties. We've got almost certainly over 100 patients now who might be anti-HCV positive, and these patients will be coming up over a period of up to a year. And clearly, you might say, "Well, get them all up and invite them to be tested" and my answer is that that would indeed itself have been a major logistic problem, and the view I took was that it wasn't essential and necessary for that to be done. Once I had the results, we then the problem of how to deliver them. You may want to talk to me about that.

But we're talking now about I'm sure over 100 tests because we have 80 positives.

Q. The Inquiry will be looking in some detail at the different parts of ethical guidance issued by BMA and to you, GMC, over the years in due course. Do you

recall whether, on this issue of testing -- and bearing in mind, as we've seen from the various documents we looked at from the mid-80s, it had become a topical issue in relation to HIV -- do you recall whether you discussed it with colleagues or sought any advice on the question of HCV testing?

A. No, I don't. I mean, I used to talk a lot to Len Doyle, who was our professor of medical ethics at Barts in London, but I don't recall discussing this with him particularly myself, no.

Q. Whilst you are obviously right to say HCV is not HIV, they're different viruses, perhaps with different stigmas associated, perhaps not, but they were both very serious conditions with potential for -- in terms of mortality, HCV is a condition that you would have known by the early 90s could, as with AIDS, lead to serious illness and death --

A. All the more reason to get on with testing patients and discussing it with them.

Q. I understand all the more reason to do the test and tell the result, but does that not distinguish the HCV testing process from a standard liver function test?

A. Well, it depends what you mean by "distinguish" and "standard". It isn't a routine test. It couldn't be because it had just come into action. But there was

a need to decide whether it was necessary to go back to the patients to ask for further consent and my view at the time was, rightly or wrongly, that it wasn't essential.

Q. Viewed with hindsight now, do you think that was the right decision, not to tell patients in advance and seek their consent to HCV testing?

A. I don't think it was essential.

Q. You don't think it was essential to seek their consent?

A. Yes.

Q. Why is that?

A. I think I have already explained that I felt that it was a natural progression from the liver function tests into the solving of a problem that everybody knew about, including the patients, that it would have been extremely difficult in a very short space of time to bring 100-plus patients to the hospital for pre-test counselling. But I'm not aware that any pre-test counselling went on -- it's possible it did -- in this field in haemophilia care and, finally, that, once again, nobody ever said to me that they shouldn't have been tested after being told of their results by me personally.

Q. Can you recall anything about how the testing was

organised? Was it, again, done in-house at The London Hospital or externally?

A. No, I don't know the answer to that question, but I think Professor Collier would have certainly been involved. He was a distinguished professor the virology and he would have advised me and the advice he gave me I think would have been taken.

Q. Then, in terms of the numbers who tested positive, I just want to take that --

A. They're just over 80.

Q. -- from your statement.

Yes. So we've got, I think, the precise figures in your statement. You said you believed approximately 84 patients were found to be anti-HCV positive at The London Hospital Haemophilia Centre, and you've broken that down as: haemophilia A, 62; haemophilia B, 17; von Willebrand disease, 3; and haemophilia carriers, 2?

A. Yes, that's the figure I had in my -- well, in my collection of lectures.

Q. Do you have any recollection of approximately when this was that this process of testing was undertaken, as in which calendar year?

A. Oh, it would have been 1990.

Q. How then did you go about the process of communicating

the test results to patients?
A. This I can't recall precisely but I think I've said in my statement that I believe that I would have given the patients' personal information about the results. I couldn't absolutely swear that none of them received the information by post but I'm reasonably to very confident that they would have had a personal interview with me.
Q. Again, I appreciate I'm asking you to recall events from some time ago, but do you think they were called in for a special appointment to be informed of the result or was it a question of at their next routine appointment they would be told the result?
A. It might well have been a mixture actually. Obviously, people come in to the hospital from time to time for advice about other things, people come in for their routine appointments, people might be concerning and ask for an early consultation, or I might find myself in a position where I had a particular consultation that I needed to undertaken and would then tell the patient -- I'm really not sure. But I don't think you can assume that it was just a matter of telling people at their routine appointment. Even if it had been that, it wouldn't have made an enormous amount of difference, although obviously there were

aspects of their known infection which needed to be dealt with and the communication needed to be done without delay.
Q. What information and advice in 1990/1991 do you think you were giving the patients when you told them their diagnosis and over the months that followed?
A. Well, obviously I can't recall that detail. But I think -- the Inquiry has provided me with a document, I think from 1996, which gives a list of things that I did for people with hepatitis C infection, just as I had a list of things that I asked people to know about HIV infection. So it's quite clear I did put together a list -- because an aide-memoire is very important for patients when you have been speaking to them, they don't necessarily remember exactly what you've said or might have interpreted what you said incorrectly. So an aide-memoire was important. The Inquiry asked me whether I remembered whether there was a previous aide-memoire and of course I don't know.
Q. In terms of the information that you think you would have been giving, whether with the assistance of an aide-memoire or not, at that initial stage of telling patients the diagnosis, you made the point that although it was relatively newly known as hepatitis C

and the ability to test was relatively new, it was a condition which had been known about for a number of years?
A. Yes. If you remember, the Mannucci paper where I introduced the idea that some people thought that this was still a benign infection in 1982, which I know was perhaps against the run of play, as it were -- I mean, against the run of advice. The paper from Charles Hay in '85 gave a really definitive view that this was a serious problem which would have then been discussed with patients up to 1990. And then it's not very long before there's the issue of interferon therapy. Once you get to the point of interferon therapy being available, then not only is it important to know your anti-HCV status from a point of view of why your liver function tests aren't perfect, but it's also terribly and important to decide whether it's appropriate for you to consider some form of treatment for this condition.
Q. In terms of lifestyle advice, what kind of lifestyle advice would you expect -- do you think you would have given in the early '90s?
A. Well, the main thing is, as it would be for whether there was an anti-HCV positive test or not for somebody with abnormal liver function test, one would

advise on limitation of alcohol consumption. That's probably the most important thing to consider.
Q. How had you gone about identifying the cohort of 100 or so patients to be tested for hepatitis C?
A. Well, I don't remember exactly how I did that, but I would probably have looked through our register -- we had a register -- and tried to remember who had or had not had blood products. I might have got that information from Oxford. I don't recall doing so. They had the power probably to tell me that.
But by the time we get to 1990, I have been director of the centre for up to, I don't know, 20 -- up to 15 years. So in here, it's quite a lot of information about a very large number of patients. Of course, that's not reliable, and it needs to be checked against the register or against the Oxford register. But I think that in those days, before computers were that much used, because we're probably talking about '88 or something, perhaps the middle of the mid-'90s. I'm not sure when computers become really important, but it's probably in the '90s. I'm really relying on Oxford to have the data if I ask them for it -- I don't remember recall doing so, but I might well have done -- or my memory, or my own register at the hospital, or a combination of the

three.
Q. So in terms of the criteria, would it have been every patient who received a commercial concentrate or an NHS concentrate prior to a certain date?
A. No. Anybody who had ever received blood products.
Q. So you would have been testing anyone who had received cryoprecipitate, NHS Factor VIII in any incarnation, and any form of commercial concentrate, and some others --
A. And/or -- Yes. I mean, obviously things like FEIBA or -- we didn't use Autoplex. But all the human-derived blood products were a risk for hepatitis C infection, and of course the blood transfusion.
Q. Can I ask you a little about the arrangements that were made at The London Hospital for the treatment of those, first of all, who had been found to be HIV positive, as you were a haematologist, not a physician specialising in the care of those with HIV.

Can you just outline for us, please, what the arrangements were in that second half of the '80s and up until the middle '90s where I think you then had some specialist assistance.
A. Well, I had always taken a view in the haemophilia centre that patients who might be coming 60 miles from

their home on the south-east coast to see me should have what I might describe as a one-stop shop, so they could come to the hospital, get everything dealt with and go home. So what I did was to link my Wednesday morning haemophilia clinic, which was also a general clinic actually, to see the people with haemophilia at the end of that clinic, and then a rheumatologist would come to see the patients that I had selected who needed a joint doctor on that day for their joints.

For the HIV clinic, to begin with, that was done on a different day from the rheumatology clinic.

The Trust had appointed a counsellor, Nigel Harvey, to assist me and what then happened was the patients who were anti-HIV positive (although we wouldn't advertise that fact; they happened to be there, and we would make sure they were there when the counsellor was available) would come and and their HIV infection would be discussed with me in my routine clinic with Nigel present. Then if there was a need to do so, which there very well might be, then Nigel would see them separately. Indeed, there's evidence he went home to see patients. Some patients were visited in their homes.
Q. So, in terms of the treatment of HIV-related infections and/or AIDS, you were the prescribing

clinician?
A. I'm just coming on to that because to begin with, of course, in 1985/'86, there's no treatment. All we can do -- and it's a lot to do -- is to find out for ourselves more about the natural history of the condition, to discuss with our patients the natural history of the condition, give them their list of things they should and shouldn't do and the risks of sexual transmission, all these things, with their leaflet. And then a year or so has passed, everybody I think has been informed, and I've completed my initial counselling with Nigel's help, and then AZT arrives -- so Zidovudine arrives.

At this point, I began to talk to my patients about the possibility of zidovudine treatment and, indeed, we treat patients who wished to be treated. Because since Zidovudine is not that effective, and since it's not entirely clear what the indications are patients do, of course, have a significant role to play in deciding whether they are going to be treated or not and some patients, I know, decided they didn't want to be treated.

But once we get slightly more effective treatment, it becomes apparent that, as Mark explained, there is a need for a specialist input. Of

course, in his case, it's him. But for me, I knew that I now needed specialist advice about therapy beyond just zidovudine. I think I could manage zidovudine by myself.

So what I did was to go to the HIV specialist, and then we made a decision which might be regarded as controversial to see my HIV positive patients in the Grahame Hayton Unit on a Wednesday afternoon. So what I then did was do my clinic, and then instead of having the HIV clinic at the end of my morning clinic, I'd go off and have a sandwich, and then I'd go to the Grahame Hayton Unit, which is the sexually transmitted disease block, and separately from everything else we would run an HIV-related haemophilia clinic.

Now, you might say, "What are you doing asking your patients with haemophilia to go to the sexually transmitted disease clinic?" and the answer to that is, that's where the specialty is and that's where the specialist knowledge is. I explained to my patients what I recommend, and they all agreed because they came to the clinic, and they were happy, as far as I was aware, with the arrangements. The other advantage of that, if you look at Mark's evidence, was that, of course, they immediately got access to the treatment without cost because, for all the

authorities knew, they were part of the HIV process.

I'd just like to emphasise that, in my mind, there wasn't any stigma for being HIV infected and, from my patients' point of view, although it's true they were attending the clinic which otherwise would deal with HIV for the other reasons it could be transmitted, they were in a separate clinic where they were looked after carefully by the HIV specialist, and I was always there.

Q. And that was, as I understand it from your statement, roughly the mid-'90s?

A. Yes.

Q. So the first decade --

A. Probably earlier than the mid-'90s because, I mean, '96, we get triple therapy. Now, we were certainly doing this HIV clinic in the Grahame Hayton Unit before '96 because I can recall -- I think I mentioned this in another context -- that some patients, by '96, were desperately ill and that the introduction of triple therapy changed everything. Mark referred to this in his evidence, that it was extremely difficult for patients who were actually dying in front of our eyes, who were prepared for death, who suddenly got better and, although you might think, well, it's wonderful to get better when you have been so ill,

actually it's a very confusing time, and patients had great difficulty in coping with it.

Q. The counsellor to whom you have referred, that was someone from a social work background?

A. Yes, he was a social worker. I think he was a trained counsellor as well as a social worker.

Q. Was that a post that was filled at the centre for the rest of the time that you were there --

A. No.

Q. -- or was it something that was just for a few years?

A. I think for a few years. Nigel -- I can't remember where Nigel went, but he eventually went.

I think that the counselling aspects were, to some extent, taken on, apart from by me, by the nursing staff who were haemophilia-trained nursing staff. But I think during the critical period, when we were trying to sort of get used to what was happening to everybody, Nigel was there for the counselling service. But I can't remember the exact date that he left.

Q. In terms of care of liver disease, your statement says that you set up a joint clinic with a hepatologist.

A. This is another of my late Wednesday morning clinics. So I know there are only a certain number of Wednesday mornings in the week, in the month, but we eventually

settled down to probably one or two hepatitis clinics a month, and one or two rheumatology clinics a month.

So the emphasis was always on my trying to make sure that, for every clinic appearance for my patients, they saw me with the specialist, rather than going off to see a specialist separately. Partly because I wanted to know what was going on with them, and partly because I wanted to make sure they knew that I was there for them.

This can be extremely important in clinical practice in haemophilia care. For instance, for liver infection, it might well be that the hepatologist wants to do a liver biopsy. Well, it might be that I don't think a liver biopsy is a terribly good idea. I then need to discuss with him or her if we are going to do a liver biopsy, under what circumstances, and with what cover and when. So, for instance, doing a liver biopsy on a Friday afternoon wouldn't be something that I would be very keen on. Not that I was keen on doing liver biopsies anyway. But this relationship between a physician and a specialist I think is absolutely critical for haemophilia care.

Q. I think we've got a couple of documents that give a snapshot in relation to hepatitis C provision.

Henry, it's BART0000567_001, please. This is

a document completed by you in October of 1991, and it's to send to Professor Preston in Sheffield. We can just see:

"Have you performed anti-HCV testing on your patients? Yes. First generation and second generation. What categories of patient do you test?"

We can see you have tested:

"Those who who've received inactivated or treated materials, those who'd received non-virally inactivated products, and those with abnormal ALTs."

You're asked if you measure ALT and AST levels in all patients. You answer: yes.

ALT and AST testing is four times a year.

Then you are asked about patients with clinical features of chronic liver disease. You identified there that you have two.

You're identifying, in 6, in terms of the stage of liver progression where you've got to.

"Are you treating any of your patients with chronic hepatitis C with interferon? No, but we're about to enter patients into a trial."

What, if anything, can you recall about the interferon trial?

A. Well, the position then was we had all these patients who were not actually themselves ill at this point.

And, once again, I have a close relationship with the Royal Free and Paul Telfer (who is now a consultant at The London, actually, dealing with sickle cell disease) is at that time at the Royal Free working with Christine Lee, and he has a trial of interferon available. And it seemed to me that this would be a very good opportunity to attain knowledge about the success of interferon therapy at the same time as offering interferon therapy to some of my patients who were regarded as requiring it.

Now, I don't remember the exact details of the trial, but it was interferon alone, and it would have had entry criteria that meant you could know who was eligible and worth trying to treat with this drug. So probably through Christine Lee, or maybe I just knew Paul anyway, I said to him, or he said to me, "What about this possibility?" It seemed to be an ideal opportunity to offer my patients interferon therapy.

Q. What was your experience, as the clinician, of your patients' experiences on interferon, both under the trial and then latterly being treated?

A. It's horrible. The problem is that it gives you a flu-like illness. You feel absolutely dreadful. And certainly with interferon itself, you feel so ill while you are taking it that you almost wish you

hadn't started. It's extremely difficult. It was extremely challenging to complete a full course of interferon therapy, or indeed the later dual agent therapy, and I felt very sorry for my patients who had to go through this awful experience when it actually wasn't terribly successful.

Q. Then we can see, just continuing with the questionnaire, if we go to the next page, question 8 is not answered, but question 9:

"Are haemophiliacs with chronic liver disease seen jointly with your local gastroenterology or hepatology specialist? Routinely.

Do you discuss with your patients abnormal LFTs in hepatitis C?"

You have said yes.

"Are you testing contacts of patients with hepatitis C for antibody to hepatitis C?"

You said yes.

Can you recall what the outcome was in relation to the testing of spouses and sexual contacts --

A. I'm not aware of any transmission of hepatitis C in that way and, certainly, my review of the literature suggests that it is extremely unusual, if not -- I wouldn't say not impossible, but extremely unusual for that kind of transmission to take place; no doubt

it could happen. But this raises the question of how much information is appropriate to give to people for a risk which is exceptionally low.

It's clear that I did discuss it with patients, but you'll also see, from the information sheet I provided, that there's quite a bit of reassurance in that sheet. But if a patient wanted to have a test, or their spouse wanted to have a test, I mean -- their personal contact wanted to have a test -- then, of course, they would be offered a test. But I'm not aware of any positives at all.

Q. Then:

"How many of your patients had died in the past five years? 14."

And then you have identified two in which liver disease was an important contributory factor.

"How many had developed hepatocellular carcinoma at that stage? Zero.

Autopsies: zero."

And that effectively completes the form.

There is a further snapshot from '94. I'll just look at it in terms of getting some statistics into the evidence. BART0002322_001.

A. Before you refer to that which is just coming up, I think it's important to note that I haven't filled

in this form correctly. I don't know whether you noticed this. If this is the one -- well, let's see what Henry produces, but I think what he is going to produce is evidence of my having mis-filled the form in.

Q. Perhaps you can assist us with how because what I was going to just to draw attention to was: we've got the numbers of patients here:

"How many anti-HCV positive patients do you have alive? 83. 17 of those are co-infected with HIV."

And then there are further questions which you answer. Which bit is filled in incorrectly?

A. If we can move on, Henry, I think there's a point where I've said:

"Do you routinely carry out additional investigations in patients with intermittently elevated liver enzymes?"

I've said "no". And then:

"Do you routinely carry out additional investigations in patients who are HCV antibody positive with persistently abnormal liver enzymes?"

And I've again said "no" which doesn't seem very logical and must be an error.

Q. In fact, you have gone on to answer in point 3 --

A. Yes, exactly.

Q. -- "which of the investigations do you perform?" and you have identified them.

A. Obviously, I didn't have the chance to tell the Inquiry this because I only saw the documents a couple of days ago. But you will see that I have left liver biopsy out as one of the things that we, in collaboration with the Royal Free, by and large didn't do.

Q. Could we just have on screen, please, Henry, BART0000660, please. I'm not sure that's the right reference, Henry. BART0000666, my apologies.

We can just see this is now October 1990. It's another meeting of the Haemophilia Working Party of the North East Thames Region Association, and you and Dr Kernoff and others are there. If we go to the second page, there's a discussion about treatments.

We can see, I don't need to go through it in detail but we can see (i) and (ii) are talking and (iii) I think are talking about potential treatments for HIV, and then (iv) and (v) are talking about treatments and other matters relevant to hepatitis C.

It looks as though, from the attendees on the first page, at this stage at least these are matters that are being considered without specialist hepatology or HIV physician advice, at least at this

meeting?

A. Okay.

Q. If you just look at the first page you can just assist me, do we have in that list of names anyone with a hepatology or HIV speciality?

A. Oh, I see what you mean. Well, no -- Peter Kernoff who was the Chairman just before his death probably, or at least before his serious illness, but Christine Lee is a very important authority on haemophilia and hepatitis at the Royal Free. Of course she would have worked with the people at the Royal Free. But she's a pretty authoritative view on liver disease in haemophilia, and I would have thought that she was capable of giving very sound advice. But I guess you'll be asking her anyway.

Q. I can indeed.

Then if we could just please have -- again from the same time, 1990 -- BART0000573_004.

We can see this is a letter being written by Dr Kernoff copied to you. It's to someone described as the "Regional HIV and AIDS Coordinator" in the North East Thames Regional Health Authority, 8 October 1990. The concern is being raised by Dr Kernoff about funding for HIV care.

If we look at the third paragraph, Dr Kernoff is

saying, by reference to the "District", I don't know whether he's talking there just Royal Free or the region, about the large number of patients and inadequacy of existing facilities and wanting to be able to use earmarked HIV/AIDS funding.

What, if anything, can you recall about the issue at the time of trying to ensure sufficient funding for HIV care?

A. I think that this is probably dealt with by my transferring all our HIV care to the sexually transmitted disease unit, the Grahame Hayton Unit, within the hospital, because I think the people with haemophilia just got lost, financially, in those arrangements.

Now, when exactly when I transferred to the Grahame Hayton Unit I don't know, and I don't think I was necessarily thinking I was clever to bypass the financial arrangements, but I really don't recall any financial difficulty either with the haemophilia HIV practice in general or the drugs in particular.

Now it would have been helpful, I guess, to have more counselling than was provided by the Trust on -- I think on a secondment basis, but that's what we had.

The other great advantage of the Grahame Hayton Unit was -- I've mentioned this in my

submission -- that Colin Murray Parkes, a distinguished psychiatrist, internationally distinguished psychiatrist, was running a service, not related particularly to the patients but for the staff within the Grahame Hayton Unit, and our physicians and nurses joined in that counselling service for themselves. And I also was able to secure the services of Colin Murray Parkes to look after my own patients' psychiatric needs, which were considerable.

Q. Can I then, on a slightly different topic, ask you to look at BART0000873_001.

This is a letter from Professor Christine Lee to you, 14 December 1993. It's in the context of a particular piece of research funded by the Medical Research Council, and I don't need to ask you about that. We have documentation relating to that. But she says this:

"I find it quite ridiculous that all this work and effort is being put into getting information from death certificates. I think the only valid information from death certificates is actually the date of death."

Then she goes on to talk further about the authorship of a particular paper.

What was the approach you took at The London

Hospital on the issue of death certification and what to record and what not to record and did your views accord with Professor Lee's?

A. First of all, I haven't a very secure memory of what I put on death certificates because I don't remember signing very many. I'm not sure why I might not have signed very many. But I do remember on one occasion, I think, when a patient -- or a relative asked me not to put HIV on the death certificate and my declining their request. Because my view was that this was a matter of fact which was my responsibility to put on the death certificate. So, insofar as I have a memory, I would have put "HIV infection" on the death certificates.

As far as the research was concerned, I mean, I think, as I recall, Sarah Darby was the leading researcher in this area, which is mentioned in this letter, and she was working with UKHCDO to look at the pattern of deaths amongst the people with haemophilia and HIV in the United Kingdom. As far as authorship is concerned, I'm not sure that I would have had a particular view on authorship.

Q. I don't need to ask you about that. There is quite a lot of documentation about it and we have the final study in any event.

Just, again, on a variety of different topics, porcine Factor VIII, you say in your statement that that was withdrawn due to concern about the possible transmission of porcine parvovirus. Do you know when that was approximately?

A. I don't know the exact date, but I mean, Ipsen took over from Speywood and there must be -- there must be a date when the porcine Factor VIII was withdrawn.

It was always a rather sort of niche product, in the sense that it only applied to a very small number of patients. It's a pity it was withdrawn because it was rather useful. Later it was possible to produce recombinant porcine Factor VIII, although this was after my retirement. I think it's mindbogglingly expensive. I'm not quite sure what the rules are these days on the use of porcine Factor VIII. But all this was at the time when everybody was getting extremely -- quite rightly -- nervous about any kind of virus infection, and I think the manufacturers of porcine Factor VIII, that's Speywood, must have realised that there was a potential risk of parvovirus infection. Whether porcine parvovirus is actually capable of causing infection in humans I'm really not sure, and of course parvovirus itself doesn't produce a particularly worrying infection in most humans, but

it's also a virus that's very difficult to virally inactivate. And so I suspect that, with it being a niche product and the need for some kind of recognition that it was a risk, and knowing that viral activation was either not possible or extremely difficult, the view was that it was best to withdraw it.

Q. Can I next ask you about the interactions you had with pharmaceutical companies, and I'm really thinking here of the 70s, 80s, 90s. You have given some information in your statement about consultancy roles of the like that you took up after your retirement with pharmaceutical companies, but I'm talking about when you were still a practising clinician.

First of all, in terms of the decision-making as to which commercial products to use and to purchase, you have told us, I think in answer to an earlier question from me today, that that was ultimately your call. Other than issues of cost, as we've seen teased out in some of the correspondence, and leaving aside your stated preference for NHS over commercial, did anyone else seek to influence you in terms of which particular commercial products you would purchase?

A. Well, I think that my stated preference for NHS factor concentrate is actually extremely relevant. I was

always an enthusiast and a true NHS supporter.

I, like many others, had commercial representatives knocking on my door, speaking to me, trying to persuade me to use their products. I did see them and I hope they thought I was very difficult to convince, because they had all sorts of arguments for telling me that their products were better than anybody else's.

Paradoxically, when we eventually got to 8Y, I think that it was actually the best product available, for a range of reasons that I can discuss with you should you wish to discuss it, but by this time the commercial manufacturers were making lots of products which they believed, or liked to believe, had advantages, and of course were more expensive. But as it happened, I think that the NHS Factor VIII Y Volkswagen got you from A to B as reliably and more safely than some of the Commercial Department Ferraris.

Q. So the purpose of pharmaceutical companies visiting clinicians such as yourself is, as I think you've recognised, to try to persuade the clinician to use their product. That's why the pharmaceutical reps are attending.

Do you have any recollection of what the

pharmaceutical representatives may have said to you from time to time, particularly in the late 70s/early 80s about the viral safety of their products?

A. No. In fact, it was very interesting to hear that Mark Winter had access to a commercial heat-treated product in the spring of '84 when I was unaware that there was such a product available and, indeed, it wasn't available to me and it wasn't available to the Royal Free either.

Q. Were there offers of funding, of training, products for patients? Were there financial incentives offered by pharmaceutical companies to you?

A. I can't remember whether they invited me to take these kind of advantages, if you like to call them that, but if they did, I rejected them.

Q. We've seen from the correspondence and returns that we looked at that you did change pharmaceutical product from time to time.

A. Yes.

Q. You already explained that your preference was to have more than one so that you weren't solely reliant upon one.

A. Yes.

Q. Again, thinking about the period really from '77, when you took over as consultant, through to 1984, what

factors, what reasons would lead you to change product?

A. I really don't remember, and I think they were, in fact, equivalent -- all these products were effectively equivalent up to the time of the introduction of high purity concentrates, which we can discuss later if you like, but I'm not aware of any particular reason that I would use any particular commercial product that wasn't related to price, because I regarded them as, frankly, equivalent in both efficacy and safety.

Q. Let me move on to ask you about variant CJD --

A. Yes.

Q. -- and the information that was then provided by you to your patients.

There are two documents I'm going to ask you to look at. One is BART0002084_011.

So, this is a letter, November 1997. It looks like a standard form letter from you to patients:

"In recent weeks there has been growing concern about the remote possibility that new [VCJD] ... might be passed on by blood transfusion or even by the transfusion of blood products. Some batches of NHS factor concentrate which were issued a year or two ago have recently been withdrawn because of this concern,

although the concentrate has already been used up.

"There is, in fact, no evidence that new variant CJD can be passed on by factor concentrates and it is very unlikely that this will occur.

"Nevertheless, I feel I must inform you that you were one of those who received one of the withdrawn batches of concentrate so that we can offer you a discussion about what this means. Please be assured that I do not believe that your health will be affected in any way."

Was that -- we've seen various standard form documents in patient records, really, across the country.

A. Yes.

Q. This appears to be a document that you have drawn up yourself.

A. Yes.

Q. Did you send out the standard form notifications as well as far as you recall?

A. I believe I did and I think this letter in itself I would regard as necessary but not sufficient.

You'll see that I've included my bleep number, which is an important point, because if you tried to phone the hospital switchboard you could wait for quite a long time (not perhaps quite as long as

getting through to Virgin Media, but you did have to sometimes wait) and giving them the bleep number of me and the nursing staff was designed to make sure that patients could get me if they were worried.

In addition, I think that the number of patients we're talking about here was quite small, and I would hope and believe that we had a number of conversations with these patients that were personal, and that's why I say the letter was necessary but not sufficient.

Q. So this is one notification you would have sent out because of the particular withdrawn batches that are in issue here?

A. Yes.

Q. But as far as you can recall would you also have sent out the broader notifications?

A. Yes, I think so, yes, indeed.

Q. Then we can see a little later on -- this was '97 -- a further notification.

HSOC0004260.

We can see again this is a letter from you. This is January 2001:

"We have been notified that another blood donor has developed variant CJD ...

"Our records show that this donor's plasma was used to make clotting factor concentrates and this

material has now been used up so there is no immediate action that can be taken."

Do you mean by that it can't be recalled?

A. Yes.

Q. "We know that on this occasion you/your son have/has received plasma concentrate prepared from the affected donor and this will be recorded ..." and then you emphasise:

"... there is no evidence that [VCJD] is spread by blood products and no one in the haemophilia community has developed [it]. A fact sheet is enclosed."

Again, you provided contact details.

What, if anything, can you recall about how patients who received notifications such as this responded?

A. Well, this was a perfectly beastly time, as you can imagine, when we had been through all the hepatitis C and HIV tragedy and then here we are with another example of uncertainty and the likelihood or possibility -- not the likelihood but the possibility -- that might be a transmission of an agent via a blood product.

So my recollection is that I had a number of extremely difficult conversations with patients,

particularly parents of patients.

What's the date of this letter? Have you got it?

Q. This is January 2001.

A. Okay. Well, this is a particularly difficult time, because by this time, in 1997 -- I don't know if we're going to talk about recombinants --

Q. We will, yes.

A. But by 1997, I have gone to my medical director and said, "I can't do this anymore, I've got to have recombinant product for my children" -- maybe I should have said for these adults as well, but -- but, "I've got to have it for the children."

The medical director accepted the situation, and by 1998 Frank Dobson had said yes. But I couldn't tolerate anymore the delivery of plasma-derived concentrates, at least to my children, and then of course the recombinant for all came in some time later. I can't remember the exact date the recombinant for all came in. But just at that time one of the factories went down in the United States so that we hadn't got enough recombinant to go round.

Now I can't remember whether this is exactly contemporaneous or not but the result was that things got extremely confused, and we found ourselves, for

adults, having to put patients back on plasma-derived products that -- when they'd previously had some recombinant products. Even some young children -- not the very youngest children, I think some sort of adolescents may have had to go back on to plasma-derived products.

So this was a period of extreme difficulty and distress not caused by a problem that became a major clinical problem but caused by a problem which might have become such a clinical problem. But it was extremely uncomfortable and difficult to deal with.

Q. I'm not, I think, going to ask you any more about recombinant, not least because you have dealt with it in your witness statement and we have a lot of documents from UKHCDO and the Department of Health which tell the recombinant story that the Inquiry can scrutinise in due course.

Ultimately, however, it was a question of funding, was it not? Leaving aside the shortage that you refer to because of the particular factory problems, this came down to the Department of Health not making available sufficient funding and effectively saying it had to be sorted out on a local basis?

A. Obviously, a little bit more complicated than that

because the recombinant concentrates -- when Factor VIII -- recombinant Factor VIII was first introduced it contained human albumin. Now the human albumin I think was safe but it was a biological product, and therefore I think the Department of Health perhaps took the view that it was still biological and, therefore, what was the point of paying all that money for the recombinant. There was some credence for that view although I think it was in many ways misguided.

The other problem was that it was, of course, extremely expensive and it was thought not to be affordable. But it soon became apparent that it was necessary to purchase it.

Q. I wanted to ask you a couple of questions relating to your testimony to the Lindsay Inquiry on the issue of research. I'm not going to ask you about the specific papers that you've referred to or that we have because we have those in documentary form but if we could have, please, Henry, LIND0000314, please.

Could we go to page 7. I just wanted to ask you about some evidence you give in that top paragraph. You set out there some studies that you had had some involvement with, and then towards the last five lines of that answer you say this:

"... I guess I was doing some scientific work, particularly early scientific work in '84, certainly; I'm not sure about '83, but certainly in '84 we were looking at previously untreated patients, particularly -- perhaps mildly affected patients who had not ever been treated before and then suddenly did a treatment, partly for scientific reasons."

I wanted to ask you about what you meant there by saying "partly for scientific reasons", because treatment decisions for an individual should be presumably only ever be driven by clinical considerations?

A. I think these are the cryoprecipitate paper and the 8CRV paper. Now, the answer to that -- I've already given the answer I think -- is that when a crisis arose I -- with Professor Sam Machin, in that paper -- I gave two patients a heat -- an early heat-treated low pool concentrate.

Now, this was not something that could be planned for. As you are -- you are quite right in saying, of course, that this had to be a specific clinical indication, and in one case it was a young woman who was truly a haemophiliac because her level of Factor IX was very low, resulting -- because of her genetic status (I can discuss it if you like in

detail) but she had a very low level of Factor VIII, unlike most carriers, and had a major crisis during pregnancy, and I was able to give her the 8CRV product from Elstree, which was a low pool, early heat-treated Factor VIII concentrate. I obtained consent -- I think from her husband -- to do that, and then we followed her over a period of six months to a year to make sure that she had not been infected.

The same thing applied to the paper we looked at yesterday, which was to do with cryoprecipitate, and for all those patients, fortunately, they avoided hepatitis C and HIV infection.

Now, the only science involved was the monitoring of a treatment that I decided, for clinical reasons, with the patient's consent, to confirm the lack of any infection as a result and that's what that means.

Q. Would you accept, as a matter of general principle, that in relation to previously untreated patients, or indeed any patients, it would be wrong as a matter of principle to give a treatment that is anything other than the treatment you believe is the best treatment that's clinically indicated for that patient?

A. Of course, and that was exactly the situation with the use of cryoprecipitate in those patients we discussed

yesterday and the quite awful circumstances of this young woman who, if she had been treated in any other way, I think would have definitely been exposed to HCV infection, quite possibly HIV. So my view was that this was absolutely the best possible treatment for her on the day.

Q. If we could just go to a further reference in your evidence -- which is page 26, Henry, it's the top half of the page -- the first part is you are answering a question about not having a specific regime in place for previously untreated patients. I have already asked you about that so I don't need to ask you again, but you then go on to say:

"What we were very aware of was that previously untreated patients obviously must be given the best available treatment; that goes without saying. That's the whole basis of modern clinical ethics. But that if we don't know what was the best treatment, then the previously untreated patients were very suitable for inclusion in scientific studies to establish what was the best treatment."

You say that, of course, was an important part of medical progress when you don't know the difference between one treatment and another, and you believe that both are potentially the best treatment, then

it's permissible, with research ethics approval, to study such a patient.

Can I just ask you to explain a little more what you mean when you talk about not knowing the best treatment, and then the particular suitability of previously untreated patients for inclusion in scientific studies.

A. Well, I think this must relate to the 8Y study. So we get to the point where we're giving -- we're going to be treating either previously untreated patients or patients who had hardly any treatment in the past, and the question is: when we get to 1984/'85 -- '85 really -- we have to decide what treatment to give a new patient.

I think the answer is that we don't know what is the safest treatment. We don't have any -- we know that unheated product probably is capable of transmitting a virus. We know that -- certainly by 1986, we know -- or before that we suspect that heat-treated commercial products are capable of transmitting HIV, and we believe and hope that we believe that the new NHS superheated 80 degrees Celsius 8Y will prove to be the best treatment for previously untreated patients.

So in those circumstances, I think it was

justifiable to mount the 8Y study and to contribute to it. Now, of course, it is true that I was the person who contributed more patients than anybody else to the 8Y study, but it was a fully -- I think -- ethical and well put together study from Oxford in which other centres participated. In many ways, in terms of any kind of investigational work that I ever did, I think it was the best thing that I ever did.

You'll be aware from my CV that I'm not an academic. I mean, it's true that I have an honorary professorship at Queen Mary for other reasons. It's true that I did some investigational work. But, generally speaking, I was working to help other people with their work I hope in an ethical way. But I wasn't an original researcher and I was doing my best for my patients, bearing in mind what I believed was the best available treatment. And this is a very good example of where my belief, as it happens correctly, was that the best treatment for my patient was to use 8Y as soon as possible and I relied on, admittedly, my hope that this was going to be the answer, which I think it was.

Q. Would you agree, as a matter of principle, that if you're in the situation you describe where you don't know which is the best available treatment, you want

to find out, you think that the treatment in the trial is a contender for that, that that is something that needs to be spelt out to the patients who are being invited to participate in the trial? They would need to be told: we don't know which is the best. We want, through this trial, to do it, and here are the pros and cons of different options.

A. Well, of course, ideally, you'd do what is known as a double-blind sort of trial where you take your best treatment, you take your hopeful treatment, and you compare the two using a randomised process. Well, I mean, this is unthinkable in terms of investigation of previously untreated patients because there are hardly any anyway, and to compare one of the available treatments that I just described with you with the 8Y I think would have been quite wrong because I suspected that any of the other treatments might transmit a virus, so I couldn't ethically do that. So the only thing I could do was to offer the 8Y in a clinical trial.

As far as the information sheet is concerned and the communication, I don't have a copy of the information sheet; maybe you do. But the information sheet and the communication to put such a patient into a study would have been along the lines that: things

have been extremely difficult, we're trying to make the safest treatment possible, we believe this is the safest treatment, are you willing to go into this controlled trial with proper consent and to be able to contribute the necessary samples to get the answer?

So it's a little bit like the cryoprecipitate patients or the HCRV couple of patients, except that instead of being a response to -- a personal response to a critical situation where I'm doing my best for my patient then looking at them to make sure it's been all right, here we're doing a proper trial with informed consent, but it's obviously important that that consent is properly informed.

One of the things that I think was mentioned in a previous discussion was, you know, what is proper consent? It's quite clear that just signing a form isn't enough. I was involved, as I think I mentioned, with creating the patient consent form for operation at The London Hospital, and it was quite a long form. We tried to make it as short as possible, and I worked with the professor of medical ethics to make it as suitable as possible. When it came to having my own knee arthroscopy, when my knee went wrong, I signed the form without actually reading it. As I signed it, I thought, "Oh." Quite clearly, all my work on this

consent form hasn't been perhaps terribly useful since, as a receiver of the treatment, I'm not actually reading it properly. Bit of an eye-opener for me.

But, of course, in terms of this study, the 8Y study, essential for proper information and a proper information sheet and proper communication before entering a patient into the study. But I think it was ethical.

Q. Just perhaps one more question before the break. Picking up on the issue that you just mentioned about consent forms -- I'm not going to take you through all the various sets of UKHCDO minutes because we have them as a matter of record.

A. Yes.

Q. But is this right that, in the course of the 1990s and during part of the time when you were Chair of UKHCDO, there were two particular consent issues that were being considered by UKHCDO?

A. Yes.

Q. One was the issue of consent forms and whether there should be a written consent form for first treatment and changes of treatment, as to which my understanding from the documents is that it wasn't ever really resolved in that period.

A. Not in my time. I think you know that I was in favour of a signed consent form for first use and for change of use. But as Chairman, I wasn't able to take the group with me, not least because I think there was also anxiety in the National Blood Transfusion Service because it had never been traditional to sign consent forms for blood transfusion, for reasons that we don't need to discuss but are self-evident.

Q. Being the potentially emergency nature of the transfusion?

A. Yes.

Q. The second issue in relation to consent that emerged during -- in UKHCDO discussions at around this time is the issue of the consent to data being held upon the UKHCDO database.

A. Which we've referred to earlier.

Q. And I can ask Professor Hay about that in due course.

A. Yes.

Q. But I think, as part of that, advice was sought by UKHCDO from Professor Doyle.

A. I'm not sure he ever really gave it. I was talking to Charlie a while back -- sorry, to Professor Hay a while back, and he couldn't -- there's reference in the Information Technology Working Party minutes that UKHCDO have that we arranged for Len to, for a fee,

give us formal advice, but I don't think he necessarily ever did.

But the position was that when I very unexpectedly became Chairman of UKHCDO, my first duty I think was to sort out the Reference Centre Directors and really the constitution of UKHCDO, also to regulate the comprehensive care centres and to begin the process of national audit. And, of course, it was necessary for me to begin the process of putting right UKHCDO from Data Protection Act point of view, and also, as far as I could but I was unsuccessful, to look at the position of informed consent for treatment.

I'm not sure we have time for this, but just to give you an example of the difficulty of informed consent to treatment. In this period of perhaps around 2000, perhaps a little bit later actually, I'm called to the Emergency Department on a Friday evening at 7.00. And there, sitting in front of me, are a mother and father with identical twins with severe haemophilia, we find, at the age of 18 months maybe. They've both got severe haemophilia, and my job then is to look at the injuries or the bruises or the haemarthrosis the child has got and then to introduce to the family, who have got no family history of

haemophilia, the fact they have now got two boys with severe haemophilia, probably need treatment, and have to have explained to them the whole sort of panoply of what it is to have haemophilia and how to manage it.

Now, I can't remember whether the boy needed treatment that night or not, but the task for a physician, even as experienced as I was at that point, of explaining to patients what on earth this is all about -- especially when it's quite likely they have already been accused of non-accidental injury by their local GP or Social Services -- this is a monumental task which can't be completed in a month, let alone an hour. And to get informed consent, valid consent, to treatment for that sort of situation's extremely difficult.

Now, fortunately, by then we were using recombinant, I think, so it was a bit easier. But it would have been a quite appalling task in any other circumstances and to get a signed consent form within an hour for an injection would have been quite impossible.

Q. I note the time, sir. Just before we break, would you agree, just picking up on your last answer then, that what lies at the heart of informed consent is the quality and extent of the information that's provided

to the patient in circumstances which ensures that the patient understands that. Of course, there may be a huge variety of different situations but, as a matter of general principle, would you agree with that?

A. So I prefer the word "valid" consent to "written" consent or "informed" consent. Now, of course what is or is not valid consent depends very much on all the circumstances of the case and, of course, of the notes that are taken at the time. The answer to your question therefore is that signed consent is not necessarily enough. Informed consent implies a one-way traffic of information, whereas the concept of valid consent implies a two-way traffic.

MS RICHARDS: Thank you.

Sir, is that a convenient time at which to break? I've got a number of questions on a range of issues that have been requested by Core Participants to come on to.

SIR BRIAN LANGSTAFF: Well, I am sure there may yet be more, so we will take a break. Is half-an-hour long enough for you, do you think?

MS RICHARDS: Yes, it is.

SIR BRIAN LANGSTAFF: Very well. We will meet again at 3.45.

(3.15 pm)

(A short break)

(3.45 pm)

MS RICHARDS: Dr Colvin, I have a number of questions suggested by others, and so they are going to be somewhat miscellaneous in that they don't necessarily follow a chronological sequence.

A. I understand.

Q. You gave evidence that there were clinicians who, in contrast to your own preference, didn't care for NHS products and preferred to use commercial concentrates. Do you know which centres in particular that was true of and why?

A. I think it might be unwise for me to comment, in the sense that the work of the Inquiry is extremely meticulous and I think you will find out for yourselves if what I've said is true. I think it's true but I think for me to make a specific allegation might be unwise and I think it must come out, if it's true, from your investigations.

Why, I think I can answer. Would that be acceptable?

Q. Ultimately a matter for the chair --

SIR BRIAN LANGSTAFF: Yes.

MS RICHARDS: Certainly the why may be particularly

important.

A. I think that there were people, going back to the 70s, who thought that the NHS response to the demand for Factor VIII and IX was inadequate, both of volume and of technical level.

David Owen mentioned this, in the sense that he wanted to allocate -- well, he did allocate £500,000 to making things better, but Peter Jones, in his statement on camera said he wanted 20 million and Lord Owen said, "Well, you were never going to get 20 million", and I think that the -- it was felt by some physicians that the response of the Department of Health and the fractionation service was inadequate not only in quantity but in quality, and that feeling continued throughout the period that I was a physician in this field.

Let's take the period after the introduction of heat-treated concentrates, when people started to fractionate at a much higher level using monoclonal antibodies to produce much purer concentrates. The word "purer" I think was used by Mark Winter.

One of the problems with the word "pure" is it has quite a lot of emotional connotations, and I mentioned this afternoon that there were those of us who thought that the Volkswagen was good as or better

than the Ferrari and maybe that's -- it shouldn't be a joke because it's not a joke, it's a comparison to try and produce colour to my speech. But I think there were people who, understandably, felt -- and I mentioned this before in my submission -- that there was no limit to what we should do to promote the healthcare of the haemophilia people, which I agreed with, except that I wasn't always convinced that just throwing more and more money at the problem would solve the problem. It might even, sometimes, be counter-productive.

Of course you have pointed out that, I think repeatedly, that why didn't we go back to cryoprecipitate, which was fundamentally a primitive way of treating haemophilia.

So there were these positions. I think Mark himself said that he believed there were factions within UKHCDO. Now I don't know whether that was true or not, because I wasn't the chairman of UKHCDO until '93, and I wasn't even a member of the reference committee, but you might reflect: why was Dr Colvin invited to chair UKHCDO Reference Centres, or the organisation, when he had never been a member? Now, I'm not sure I know the answer to that question but somebody must, and it does imply that there wasn't

always a consensus in UKHCDO.

Which, having been said, I do think that UKHCDO made a magnificent contribution to the care of people with haemophilia in the United Kingdom and the world recognition of what haemophilia was and how it should be treated. And if I had been critical in any way of UKHCDO in the last two days, I do want to record that I think that it was a world-leading organisation on a voluntary basis.

I'm not sure whether you are going to ask me about my time as -- further time as Chairman of UKHCDO --

Q. I am not proposing to.

A. -- but the organisation was purely voluntary, and when we got to '93/'96, it became apparent that it had to be more professional.

This occurred with many organisations, I think at this kind of time, where the old days of an amateur effort had to be replaced by professionalism. I mean, if I may make a further trivial comparison, I mean, rugby football used to be entirely voluntary, athletics used to be entirely voluntary, and then after the Second World War, as we moved in towards the millennium, everything became very professional and everything got money associated with it.

The whole, perhaps, emphasis of Thatcherism was about money and about business-like activity and I think it was the case by the time I became Chairman that the days of that kind of amateur approach were over and we needed to try and build something professional, which of course Professor Hay -- and you will be seeing him -- has done in the most amazing way, and it's now a significantly professional organisation.

So I think that, to come back to your earlier question, it may well have been that there were differences of opinion and you know that I tried to align myself to the Royal Free, with whom I had great relationships and much confidence, which included, therefore, Professor Pasi, who eventually came down to work at The London. But there were other centres, and I don't really want to name them, I think it will become apparent where that might not have been quite the same approach.

SIR BRIAN LANGSTAFF: I think you were asked why it was that some clinicians would have preferred not to have NHS concentrate.

I think that was the question, was it not?

MS RICHARDS: Yes.

SIR BRIAN LANGSTAFF: The answer you have given is for two

reasons: (1) because you couldn't get hold of it, and second was because it was not as good.

A. Yes, sir, that's absolutely --

SIR BRIAN LANGSTAFF: In what respects wasn't it as good? Because I don't think you have answered that part.

A. I tried to answer but I am going to try and answer it again, in my discussion of the word "purity".

I think that there were those who thought that the only way forward was to have Factor VIII and only Factor VIII in the preparation, and that that was a very important part of the work and would lead to greater chemical purity, also mentioned by Mark. But whether that was better than something that was less sophisticated was not quite so clear.

It's important to remember that, as you were explaining earlier this afternoon, the pharmaceutical industry is there to make profits for their shareholders. Of course you know now, because I've told you, that I spent five or six years in the pharmaceutical industry after my retirement, and I am proud to have done that, but I became aware that they liked to think of themselves almost as part of the NHS, when they clearly were not. They were very heavily regulated by the time I became associated with them, but they always had to look for and find added

value for promoting their particular product over another product.

Of course, if all these products are actually really equivalent, then things like chemical purity become important. And so I think that it may be that there were those who not only couldn't get the volume but thought that the chemical purity at, perhaps, almost any price was worth paying for.

Now, I'm not saying they were wrong. I cannot possibly say that. But it is, I think, the answer to your question, that there were those who saw the technological advances as really having precedence over any thought that a more modest approach, producing adequate volume and adequate quality, would compare.

Does that answer your question, sir?

SIR BRIAN LANGSTAFF: Well, it does and it doesn't, because I'm not sure I understand the reference to "chemical" purity. Let me -- I'll tell you why.

A. Yes.

SIR BRIAN LANGSTAFF: Would "biological" purity perhaps be a better word to use? Because my understanding so far -- and tell me if I'm wrong -- of purity is that you can, in the fractionation process, end up with quite a number of different proteins in the same

fraction.

A. Yes.

SIR BRIAN LANGSTAFF: Factor VIII is one protein, so also is fibrinogen, and perhaps one or two other stray proteins as well.

A. Yes.

SIR BRIAN LANGSTAFF: In order to have a pure product, you wanted to have just the Factor VIII protein and not fibrinogen and not the others.

A. Yes.

SIR BRIAN LANGSTAFF: So that's what I mean by "biological purity".

A. Yes.

SIR BRIAN LANGSTAFF: That's the same thing as your "chemical" purity, is it, or not?

A. That's what I was driving at. I think I used the word "chemical" purity because Factor VIII is a huge molecule but there's very little of it in the blood, and so, as Professor Tuddenham discovered in 1984 when he tried to make Factor VIII from plasma, you start with a lot of plasma and you finish up with very little Factor VIII because there's hardly any of it. In that sense, by the time you've got down just to Factor VIII, you've almost got chemical purity without biological presence. Of course, when --

SIR BRIAN LANGSTAFF: We are talking organic chemistry?
A. Yes.
SIR BRIAN LANGSTAFF: That's all that I wanted to clarify. Thanks.
A. I think organic chemistry was never my strongest point.
MS RICHARDS: Next, in a completely different topic, in relation to the transmission of hepatitis C, could we look at BART0002065, please. This should be the information sheet on hepatitis C.

So this is the March 1995 information sheet that you referred to in your evidence earlier, Dr Colvin. If we go down to -- if you keep going down. Yes. So we can see there that you mentioned -- it says:

"Dr Colvin will have mentioned the low risk of sexual transmission of the virus to any partner. He will have reassured you that ordinary, everyday household contact will not transmit the virus, so your family do not need to worry."

Can you recall whether, other than what's set out here, any advice was given to patients about the particular risks of hepatitis C that may be associated with family members dealing with patients who bleed?
A. I don't. I don't recall that, but I think that the evidence is that it's really quite difficult to

transmit this disease sexually, as I've said.

I think that if there were to be cuts with bleeding and there were to be cuts in a person who was also next to the blood of person with hepatitis C, of course I appreciate that that is a possible means of transmission. But the words "everyday household contact" doesn't, I think, imply the exchange of blood products in everyday life.
Q. No. As I understand -- well, a reading of this might be that sharing plates, sharing cups, ordinary physical contact would not transmit the virus.
A. I don't think there's really evidence that that happens.
Q. No. So the question I have been asked to ask you, Dr Colvin, is: do you recall whether you gave any specific advice about what might be regarded as the additional risks that could arise if you are someone with a bleeding disorder and you bleed; the risk which isn't necessarily ordinary, everyday household contact that a family member might have hepatitis C transmitted through that route?
A. I can't recall, but if I had a conversation with a patient, and if we handed out this form after that conversation, if they had a question to ask me about that issue, I would try to respond.

Q. The next question, Dr Colvin, concerns the consideration that you gave second half of the 1970s, perhaps '77 onwards, really through into 1984/1985, to public health considerations.

So we've talked in some considerable detail about the issue of risk to patients and what information should or shouldn't have been provided to patients in that regard.
A. Yes.
Q. What about wider public health implications? How did the consequences of potentially having people who may be infected with hepatitis or HIV, and how that might impact upon others and the risks they would pose of transmitting it to others, how did that factor into your general decision-making process about what information to give?
A. I think during the '70s, for non-A, non-B, we had really no information that we could have imparted. We didn't know enough about it to give useful advice, I don't think.

As far as HIV was concerned, I think that there was a desire to make sure that the known routes of infection were covered, but there was a lot of doubt about what level of protection there should be offered to hospital staff or to the isolation of patients

within the hospital, or to advice on what might be described as public behaviour -- you know, the way to behave the public.

I think that the contrast or the dilemma was between trying to reduce the risk of infection where there was a high risk and the desire to make sure that people who were HIV positive could lead as normal a life as possible within the limits of risk and I think that, in general, society tended to label people as being high risk when they weren't. And, of course, this was the great work of the Princess of Wales who actually made it quite clear that she was determined that people with HIV infection should be treated as people.

I think that we in the hospital service tended to err on the side of safety with our high-risk policies (which I'm sure patients didn't like for very obvious reasons) and yet, at least to begin with, we felt obliged to invoke such procedures. But in public life, I think whether she knew it or not, the Princess of Wales was quite right.
Q. I understand how the risks of infecting others was something that may have formed part of the information you gave to your patients once you were telling them that they were positive for HIV or hepatitis C. But

when you were taking your decisions about what treatment to offer, in the period before patients were tested and diagnosed, did the risk that patients might unwittingly be infecting others, did the broader public health considerations come into play at all in your decision-making process about what treatments to use?

A. I'm not sure I still really fully understand the concept of a "broader public health implication".

Q. When you were deciding what treatments to provide to patients --

A. Yes.

Q. -- did you consider -- and indeed what information to provide to patients about risks before you knew that anyone had tested positive -- did you factor into your decision-making process the possibility that not only might your patient be infected or be at risk, but that others might be at risk?

A. I see what you mean. I think my answer to the question is: I don't know. Not least because I didn't know what on earth was going on, if that's any kind of satisfactory answer.

I think there was an enormous amount of ignorance of where we were and where we were going. And, of course, throughout this whole Inquiry, we

obviously -- I know this is implicit in any inquiry, is that one has to somehow try to separate what we now know from what we knew then. And I think in this period before we knew what was happening, it was perfectly obvious, from what you've shown me from UKHCDO, that we or my colleagues didn't -- and probably both of us didn't fully understand what was happening.

Q. The next question is about the structure of UKHCDO. You've told us how you were invited to become the chair.

A. Yes.

Q. Do you have any knowledge as to how in the years prior to that the Chairmanship of UKHCDO was determined?

A. No, I don't.

Q. If you could think for a moment about the position of mild haemophiliacs, Dr Colvin -- because that's the subject of the next question and, indeed, a number of later questions -- to what extent do you consider that lifestyle advice, advice about what activities to do or not to do in the management of haemophilia, in particular for mild haemophiliacs, could have been used as a means of avoiding bleeds of a severe nature and thus the need for treatment?

A. Yes. So I think that, during the early part of my

experience of haemophilia care, we were trying to identify people with mild haemophilia, and I've already explained to you that many of them had no symptoms at all. I've also explained to you that a person with mild haemophilia is not, in some ways, a mild haemophiliac because if they're injured or get a bleed, they will go on bleeding until the bleeding is treated.

It's also the case that the person with mild haemophilia is different, depending on what activity they have. So if you have a young person who's a bit of a nerd and spends the whole of their life sitting in front of their computer screen, the chances of them having a bleed are quite small. So they can quite happily live a normal life in front of their computer screen, although they should get up from time to time to exercise.

If you have got a tearaway child, particularly somebody in difficult social circumstances who is hard to control, then, obviously you will give them advice about their activity. Whether it will be taken or not is another matter.

I recall another example where there was a child who was brought to my care who actually had extremely mild haemophilia, but generations of his family in the

past had been generals in the British Army. And the diagnosis of mild haemophilia in that particular case (where the mother was of course not a member of that family of generals but was a carrier of haemophilia) meant that the occupation of service in the British Army was not going to be possible. So the advice that one gave to patients determined, to some extent, their behaviour but didn't always determine what they did.

I had one patient, actually, who became a very distinguished person in one of the potentially combative -- not combative, but potentially high-risk services -- I had probably better not name which one it was. And he came to see me and said, "Dr Colvin, I want to join the Royal Marines" and I said, "Well, you know, you can't join the Royal Marines". He said, "Well, what about the police force?" So I said, "Well, I don't think the police force is going to take you". So he said, "But I want combat". So I said, "Well, it's very difficult".

He eventually went off and joined this other service without telling them that he had haemophilia. The first thing he did was fall off the back of a lorry and break his ankle and finished up in hospital under my care with a broken ankle and mild

haemophilia. We put him right -- and this was after the great HIV and hepatitis C crisis. He went back to work and became a very distinguished person, but he had decided not to reveal to his employers that he had this condition.

Now, the only things that I would say always I advised against were rugby, football and boxing. But once we got on to prophylactic care later, one of my severely affected patients became a world champion cyclist. So I think -- and the reason he did that was because I'd told his father it would be best for him to go swimming. And he went swimming and then, when he became a very fine swimmer, he got on a bike and decided he was an even greater cyclist.

So, I'm sorry, the answer to your question, I think, is that for people with mild haemophilia, one would try to give them advice about what was the most sensible thing to do, but what was sensible depended on all the social circumstances that they were in, and what would be suitable for one family might not be suitable for another.

I also, finally, was invited many years ago -- I think I mentioned this earlier -- to give a paper to The Haemophilia Society saying, "I can go skiing." Now, whether it's really sensible for somebody with

haemophilia to go skiing or not is a matter for debate. People might say, well, there's no point in a person going -- with haemophilia going skiing because they're going to get injured, and then it's going to cost a lot of money to put them right. My argument against that was that a lot of my patients with either mild or severe haemophilia would go out on a Friday night and get less than totally so sober and get injured, and when they came to the hospital, I treated them. I might say, "I really wish you hadn't spent the evening getting drunk and getting into a fight," but of course you still get treated. It seemed unreasonable to say to a patient, "I won't treat you when you have got into a fight." That's obviously unreasonable. Equally, therefore, it seemed unreasonable to say: you mustn't go skiing.

Q. But lifestyle advice could be one part of the jigsaw of trying to avoid bleeds.

A. Yes.

Q. We touched on -- self-sufficiency. You have dealt with it in a number of parts during your evidence, so I don't need to ask you in any detail about it.

Can I just ask this: in the mid-1970s when Lord Owen made the pledge -- I think his term -- in Parliament for self-sufficiency, and I'm aware that

you were not yet the consultant and director at the Royal Free. But what was your understanding of the scope of self-sufficiency? In other words, was it based upon hospital treatment, home treatment, or prophylactic treatment as well?

A. Well, I think we'll never know. If I may -- and perhaps this is unwise -- but I'd like to refer you to Rudyard Kipling's Just So stories. The butterfly that stamped is a story about a king who decides that he is going to feed all the animals in the world for lunch, and he gets lunch for all the animals of the world and puts it on the quayside, and he invites all the animals of the world to come for lunch, and just as they are about to start their lunch, an enormous animal comes out of the sea, eats everything, and says, "What's for afters?"

Now, I don't mean this to be a trivial story, it's a story that Rudyard Kipling wrote, but I think that at a serious level there's an element of this in the self-sufficiency argument. That Lord Owen for very good reasons and with great sincerity said that we will become self-sufficient, but did he really have a clear idea, bearing in mind that Dr Jones wanted 20 million and Lord Owen was offering 500,000 -- did he have a really clear idea of

what the real demand would be?

I think the answer is that nobody could really and it is not unfamiliar for desire to reach an objective to fail to quite appreciate what the real achievement of the objective might entail.

Q. As far as you are aware, however, did the haemophilia clinician community in the second half of the 70s understand that, according to Lord Owen in any event, what was meant by self-sufficiency from the Government's perspective was home treatment but not prophylactic treatment? Was that understood by clinicians?

A. I don't think -- despite Inga Marie Nilsson's work in Sweden, I don't think that we had really appreciated or got our heads round the idea that prophylaxis was the answer, and I don't believe when David Owen recommended and offered to create self-sufficiency for the United Kingdom that he'd really thought of what it would cost to offer prophylaxis because I don't think the haemophilia treaters themselves had addressed that idea either. Yet it was the correct answer to the problem of haemophilia.

We now find ourselves in the United Kingdom with patients who don't know what it's like to have severe haemophilia because they don't suffer the same

bleeding, and doctors who have got no idea what it's like to have haemophilia. At the end of my career, I was busy trying to train the doctors of the future with patients who weren't actually showing the signs of haemophilia, which is great, but it was the case that until we really grasped the reality or the possibility of prophylaxis and created the environment in which that could be achieved, haemophilia treatment was inadequate. And I don't think that in the 70s anybody, apart from possibly Inga Marie Nilsson, had really grasped the opportunity that was there to be taken if we had only had the resources available.

Q. You told us in your evidence earlier, and it's in your statement, that there was this small amount of heat-treated material available in 1985 which was then used for the patients that you described and the term you used was it was for certain special situations.

Other than the circumstances of those individual patients, what were the special situations for which this treatment was available?

A. Are we talking about the 8CRV material or the 8Y?

Q. It's the material that you talked about in 1985. We can look at your witness statement, perhaps for clarity.

SIR BRIAN LANGSTAFF: Wasn't it 1994 in the evidence you

mentioned --

A. The 8CRV material was '84 and was an attempt by Oxford to create what I understand was an extremely small volume of material from what I seem to remember Charles Rizza described as "trusty donors", that is people who had, in some way, been regarded as being particularly suitable for giving blood, in very small pools, with early heat treatment.

Now why Oxford did that I'm not entirely clear. Perhaps they were just trying to think of the ways to improve the situation. But I became aware -- and I think this is recorded in Terry Snape's notes, that I became aware, with others, of the availability of this very small amount of concentrate, which I think they would only have given to me for people who hardly ever been treated before and for people who didn't need much treatment.

So I think it was a very niche product, if that's the right word for it.

MS RICHARDS: You have referred in your evidence to 1988 being the year in which the safety of the 8Y product was established.

A. Mmm.

Q. I just wanted to ask you to look at a couple of documents to see what the position was prior to 1988,

when I think the final study was published.

Could we have CBLA0002224, please, Henry.

We can see this is a document from July 1985 from BPL.

A. Yes.

Q. And it's setting out some information as at July 1985 about this product.

A. Yes.

Q. With some information about what was known about it at that stage. Is it right to understand that clinicians such as yourself wouldn't have been putting patients forward for this trial if it wasn't thought that likely that this was going to be a safe product?

A. But thinking and believing and knowing are very different things. The point that I was making was, and I'm no statistician, is that when you look at the number of patient who are treated in a previously untreated patients study -- because it needs to be previously untreated patients. If you look at the number of patients treated and the carefulness of their follow-up and the analysis of their results, if you're a statistician you can come up with confidence limits of how likely it is that this result is a true result which gives you certainty.

I think you'll find either in the 8Y study or in

one of the commentaries on the 8Y study that it's pointed out that the confidence limits are not 100 per cent. And I think you might also find there's a paper, quite a late paper, by Professor Mannucci again, where he looks at all the different ways in which plasma can be virally inactivated and tries to assess how safe they are.

One of the points that Duncan Thomas used to make, who was one of the leaders in the National External Quality Assurance Scheme *(sic)* and the NIBSC, the Biological Standards and Control organisation, one of the points he made many years ago was there is nothing -- there is no such thing as a safe blood product.

So you can do your study and you can measure your measurements and you can publish your results but those results still have confidence limits.

And the point that I was really making was that whilst we all hoped and believed and thought that this would work, and it did, I don't think that before 1988 you could look a patient in the eyes and say, "I promise you that this is safe", because of the confidence limits statistical issue.

Q. I understand what you say about the ability to promise, the ability to say "conclusively established"

or words to that effect. I think it's right, and we can look at the documentation if necessary, but it is material that you saw in advance of your evidence that there was, as one would expect, an interim reporting by Dr Smith in September of 1986 which suggested that, thus far, the indications were all very favourable?

A. Yes. Absolutely. I don't deny that at all. I think it's an extremely important point. But if you look back -- perhaps maybe I may go back to the issue in May '84, when it was decided in one or two, three, perhaps four centres to go to a heat-treated product that was commercial, there was, I think it's fair to say, zero evidence that that product was safe. Completely different scale of evidence to the evidence that later became apparent for 8Y.

I'm not criticising the decision, I'm just saying that if you're looking at real evidence and you want to say to a patient, "I promise you that this will work" or "I nearly promise you this will work", you need to have evidence which has been assessed by a statistician.

Q. This is now moving to a completely different topic, Dr Colvin.

Could you just very briefly outline in the 1970s what the approach would have been, from your

perspective, to diagnosing haemophilia or von Willebrand's disease. Was there a standard model of diagnosis?

A. So this is a very complicated question.

The first thing to say is that people with haemophilia are usually male -- not always. In fact, Sir Frederick Treves had a patient under his care who was female and a true haemophiliac, and I looked after four of that family because the sons of that woman, or one of the other women who had true haemophilia, were all haemophiliacs, because both her X chromosomes were abnormal and, therefore, all her sons were bound to be haemophiliac. I looked after that family. So that family from the 19th century came down through to Whitechapel.

So, first of all, nearly all the patients with haemophilia are male.

Secondly, people with von Willebrand's disease do have low Factor VIII clotting activity, so they could be confused with haemophilia. But in the 70s, I think early 70s, the von Willebrand factor antigen and a material called von Willebrand factor were described, and once we had von Willebrand factor and von Willebrand factor antigen, if that was low as well as the Factor VIII level, it was likely that the

patient had von Willebrand's disease.

However, that is not absolute, because there are people who are -- look as though they've got haemophilia but actually have a form of von Willebrand's disease.

So it's a very complicated issue and I think that, to begin with, we were struggling very much to classify von Willebrand's disease. And I mentioned I had some involvement in a project that was trying to classify von Willebrand's disease in my community. It didn't eventually come to anything.

But as time went by we got much, much better at diagnosing von Willebrand's disease and distinguishing it from haemophilia. But in the early days, before 1970 or so, it was -- well, it was difficult to make that diagnosis. The difference between haemophilia and von Willebrand's disease at a clinical level was people with von Willebrand's disease tended to get soft tissue nosebleeds and capillary bleeding, whereas people with haemophilia tended only to bleed after injury or surgery, or spontaneously if they had severe haemophilia. So you could tell some difference between the two conditions clinically but until the 70s you couldn't distinguish in the laboratory, and even then, even up to the late period, it wasn't

always that straightforward.

Q. Next question is about records, record-keeping.

Do you agree that where you have a haemophiliac patient who has a life-long condition and may require a range of different blood products throughout their lives, it is very important that the details of those products should be recorded?

A. Yes.

Q. So one would expect -- and I'm talking generality here rather than The London Hospital. One would expect hospitals, haemophilia centres, in the 70s and 80s to be keeping very precise records of products used for patients?

A. I went to great lengths to keep records of all the products used at The London Hospital, and I also went to some lengths to try to make sure that people were treated with the same batch of material if it was available and those batches were recorded. So we took record-keeping very seriously and I believe the record-keeping should be taken very seriously.

Q. Then coming specifically to The London Hospital, my understanding is that in providing statements in response to some specific issues raised by individual patients, you have been told by St Barts that some of the -- the handwritten records that you would have

kept at the time can no longer be located; is that correct?

A. Well, I've been told by Barts NHS Health, who of course have taken over the -- now the London -- the hospital, I've been told that one patient's notes had been destroyed in 2008. I was also -- managed to, before lock-down, see the red envelope, the brown envelope of another patient, and I also saw his contemporary notes from 2009.

Oddly enough, I was the first person to write in them because when I retired I saw him on one occasion in my *ad hoc* presence in the clinic but the notes for the period from 2009, I think it was, backwards were not provided for me. So the only handwritten notes I saw were those from 2009 onwards, in which I happened to have written the first entry. But I had to rely on the brown envelope and the contemporary notes.

Otherwise, I have tried to get hold of notes and haven't had a response other than the fact that the notes were being sought, other than the brown envelope systems for two of those patients. So for one patient I have had no information of any kind.

So I can't say more than that. I've tried repeatedly to get hold of the notes of rule 9 requests

and so far I have had that level of difficulty.

Q. Then moving on to the relative risks as between cryoprecipitate and commercial concentrate, or indeed NHS concentrate, you refer to the possibility that if someone has lots and lots and lots and lots of cryoprecipitate, there may be risks created. Would you accept, however, that the risk from each bag of cryoprecipitate was very, very small indeed?

A. Yes, of course. The point of Peter Kernoff's paper in Gut or Christine Lee and Peter Kernoff I think, and I think I mentioned this earlier, if you did get a bag which was infected then you'd get a full dose. So if you think that the level of exposure to the virus makes a difference, then that might be important.

I mean, for instance, in Covid-19 there are people who think that the load of virus you get at the point that you're infected might have some relevance to your short or long-term prognosis. Now, whether that was the case, of course, I don't know.

But we know, I think, and you have the records, that I think there may have been one or more seroconversion to HIV from cryoprecipitate. I'm not sure we have that evidence but --

Q. Not in your experience at The London?

A. Not in my experience at The London but, I mean, I'm

talking about the national experience.

Q. But would you accept, as a matter of generality, the magnitude of the risk from the use of cryoprecipitate is completely different from the risk of concentrates, and indeed, that's the very reason why cryoprecipitate was being recommended by UKHCDO for certain cohorts, and indeed why you were using it for children and others?

A. And why I wrote that paper for six patients being treated under my care from '82 to '84 which we've looked at. So yes, I agree.

Q. Then could we have up on screen, please, Henry, JEBA0000008, please.

This is a document you saw this morning, Dr Colvin. It's quite faint but if we could zoom in, please, 4 July 1979. The name is redacted but it seems likely it's from you as it's given the title of senior lecturer of haematology in July 1979?

A. Yes.

Q. It's a description of an incident occurring when a doctor, whose identity is not relevant for present purposes, was on call in the department and the child with mild haemophilia was given a bottle of commercial concentrate, in that case Hemofil. If we can just look at the second paragraph -- and you've said for

reasons that are unclear that that concentrate was given, and you have said this:

"I am rather unhappy about this incident for a number of reasons. Firstly I understand that through no fault of [their] own [the doctor] was unaware of the arrangements for treating haemophilia at The London ... did not even know where the concentrate was kept. As you know there are instructions for staff treating the disorders of haemostasis of children and general instructions for treating haemophilia available. Secondly I am anxious that children with mild haemophilia should not receive the commercial concentrate because of the substantial hepatitis risk."

Then there is a reference to cost.

So it would seem clear from this that, as at 1979, one of the factors influencing your decision-making in relation to the treatment of children with mild haemophilia was the risk of hepatitis?

A. Yes. Of course I was wrong about the commercial concentrate because the NHS would have been just as good at transmitting the hepatitis.

Q. Concentrate *per se*, I think we can safely say, substantial hepatitis risk?

A. Yes.
Q. In relation to what went wrong on this occasion, was this, as far as you can recall, a one-off or was there a broader systemic problem that you became aware of?
A. I don't think there was a broader systemic problem in '79 that I was aware of. I do think that I could and should comment on the great difficulties that haemophilia centres have in making sure that the right people look after their patients.

So we had a regulation at The London that if anybody came to the Emergency Department with a bleeding disorder, they should immediately call the registrar on call for haematology, and the reason for this was that triage wouldn't help, because you had to wait for however long, maybe even for triage, in the Emergency Department, and when you had had your triage, the triage person wouldn't necessarily fully understand the importance of haemophilia or might not even have heard of haemophilia really.

So the rule was that if somebody came to the Emergency Department of The London Hospital, then whatever the complaint, if they had a haemophilia card with them or if they said, "I've got haemophilia", the registrar would immediately be called, day or night.

Now did this always work? I can't necessarily

convince you that it did always work because people sometimes fail. But we had made every effort to make sure that that happened and if you don't do that, then you will get serious problems.

And equally, you can't have somebody on parade 24 hours a day, 7 days a week, with haemophilia experience. It's a rare disease and we have to accept that we do our best to make sure that people are properly looked after with haemophilia. It's the responsibility of the director to try to make sure that this is never a systemic problem.
Q. I asked you a moment ago about the relative risks of cryoprecipitate versus concentrate. Can I just ask you to think further about the relative risks of NHS concentrate and commercial concentrate.

Would you accept that, on a like-for-like basis if only one treatment were required, for example, that the risk of NHS factor was, in fact, lower than the risk of commercial factor?
A. For hepatitis C?
Q. Yes.
A. No, I wouldn't.
Q. Why is that?
A. Well, if there are thousands of donations in a batch of Factor VIII concentrate and the prevalence of

hepatitis C in the community is 0.3 per cent, then there's 100 per cent chance of being infected. There is another argument which I think I address in my --
Q. Forgive me. On one treatment only?
A. Yes. There's another argument which I give in my submission which is about the subtype of virus with which a person is infected. And that's an issue which I think you would have to ask Professor Hay or Professor Preston because I know that Professor Preston, in particular, was very interested in any difference between the subtype of hepatitis C that might be imported from other countries and the type of hepatitis C that might be endemic in the United Kingdom, or whether that made a difference.
Q. You will recall, Dr Colvin, I asked you earlier today about your evidence in your statement that it wasn't realistic to revert to cryoprecipitate for all patients in all circumstances. I don't need to go back over that.

One of your answers was about how realistic it was to go down that route. If one thinks about a patient who only rarely requires treatment -- so not someone on home therapy, not a regularly treated patient -- the question is: would it not be realistic to at least discuss the reversion to cryoprecipitate

with that category of patient?
A. Well, of course, if one is very rarely treated, one perhaps very rarely sees the patient, and then you'd have to address the issue when the patient came to see you. If you had a very rarely treated patient who required a small amount of treatment to treat a particular bleed, then cryoprecipitate would be an appropriate therapy.
Q. In terms of balancing risks, the decision to run risks, you've set out your view in a number of parts of the material about the benefits of treatment outweighing the disadvantages or risks of treatment.
A. Yes.
Q. Would you accept that, as a matter of general principle, a patient may view those risks very differently from how a clinician may view them?
A. Oh, yes. Of course, I understand that entirely. And when we were talking about consent earlier, we talked about whether this consent was a one-way or a two-way process.
Q. Because you might think, as, for example, you said in your paper with Dr Kernoff in August 1983, that the benefits of treatment outweighed the disadvantages. But a patient faced with a possibility, for example, of a premature death as against pain, discomfort,

injury from a bleed may form a very different view, and that's why it's so important for them to be at the centre of the decision-making process.

A. Well, I mean, particularly in today's world, quite properly the patient is at the centre of care. Now, whether in the -- 30, 40, 50 years ago we fully understood that the patient was at the centre of care in the way we should have done I think is less clear.

I still interview patients for admission to medical school, and one of the questions often asked by my colleagues, and I may even ask it myself from time to time, is: in a particular scenario, who is the most important person in this scenario? And the correct answer for the student who wants to be a doctor is: it's the patient.

Q. Was that not the thinking in, say, the first half of the 1980s, that the patient was the most important person in the scenario?

A. I'm not saying it wasn't. I'm saying that attitudes, I think, have developed over the last 50 years in a way that patients are now more clearly at the centre than perhaps they were in the earlier part of the middle of the last century. I think this was a process that continued over that period. The fact that we correctly believe that the patient is the

centre of the consultation may not have been as appreciated as it should have been in the past. I think it's hard for me to say anything other than that.

Q. Others have used the term "paternalistic" to describe, in some respects at least, the approach to clinical decision-making in earlier decades.

Do you think that is a fair description of clinical decision-making in this context in the '70s and '80s for haemophilia clinicians?

A. I think it is, and I think it's also very interesting to reflect on what it means because I think that the correct putting of the patient first does create a burden on the patient which can be difficult for the patient to bear.

So if you take paternalism at one end and total patient choice at the other end -- and of course I completely agree with the idea of patient choice and autonomy. When you're ill yourself, you may take a different view in the sense that -- I was referring to my arthroscopy a while back, and I wasn't, I think, given a really clear choice between, shall we say, physiotherapy, arthroscopy, and knee replacement. My surgeon said, "You need an arthroscopy". And I said, "Okay. You're the doctor. Let's get on with it".

Now, obviously, he tried to give me a choice between the things that he thought would be sensible, but maybe orthopaedic surgeons are particularly paternalistic. I hope I don't abuse orthopaedic surgeons by making such a remark. But it is the case that many of us, including myself, quite appreciate that the doctor is the expert, and we want to be told what the doctor thinks the best treatment is.

But -- and I think this is terribly important -- we need to give patients the best information and the choice of what we do next, and we don't always get that right. Sometimes we don't. That, of course, is sadly a fact of life. But my answer is that I do believe in patient choice. I do believe that in the past it was more paternalistic, but the process of paternalism wasn't always completely disadvantageous because it took the responsibility of what happened next away from the unfortunate patient.

Q. Dr Colvin, I have asked you previously about UKHCDO guidance or the absence of it. I'm not asking you to go back to that.

Would it have assisted clinicians to make better decisions in the latter part of the '70s and the early part of the '80s in particular if there had been clear

guidance from other sources, such as the Chief Medical Officer, or the Department of Health and Social Security?

A. Yes, I believe it would. I have written and helped to write very many guidelines/documents in the field of haemostasis and thrombosis in the period probably dating from the early '90s when I became more nationally active, if you like.

I thought that these guidelines/documents were extremely valuable because they were based on expert opinion. But many of these guidelines/documents were challenged by people who quite rightly said, "These are not statistically valid pieces of guidance. They are just a group of self-styled experts who tell you what to do because they think they know what they are talking about."

I think I also mentioned in medico-legal work that these documents tend to get relied on because they are often the best bet about what is the correct approach at the time. Now, of course, there has been a tendency in medico-legal work, because of the burden principle, for doctors to get away with, if you like, anything that is regarded as being something that a reasonable group of doctors would do. And I think Montgomery has changed that view, in terms of consent,

to the importance of warning and discussing material disadvantages as well as material benefits.

So I think that very clear guidelines/documents are actually a big responsibility for those expert groups because when you get down to looking at the medico-legal cases, the guidelines/documents give you a framework on which to provide your expert witness opinion. But, having said that, if the doctor whose practice is being criticised has written very clear reasons for what he or she has done and has not abided by the guidelines, I believe that is a defence in the sense that the guidelines are guidelines. They are not the law.

Does that answer your question? I am sorry. It's list --

Q. Yes, yes.

The last matter I wanted to ask you about, Dr Colvin, requires us to go back to a couple of documents. RFLT0000050, please, Henry. I want to go back to the January 1983 meeting that you attended at The London Airport hotel. We looked at one record of it. This is a second record.

So this is a note that was put together by the managing director of Immuno, Mr Norman Berry, and we can see from it that the location of the meeting in

January 1983 was the Excelsior Hotel, Heathrow Airport. We can see this is being sent to Dr Kernoff who you worked closely with.

If we go over the page, we can see Mr Berry's summary of discussions of the meeting. Professor Bloom is described as being in the chair. Then there is effectively a presentation by doctor -- I don't how you pronounce that -- Eibl of Immuno. The particular issue here was about the so-called hepatitis reduced concentrates, and we can see there's a detailed discussion. If we go to the second page of this, Henry, or the next page, we can see there's discussion then of -- in the numbered paragraphs of a range in particular of ethical issues being put forward, the use of children for the trials, who would benefit, difficulties in carrying out the trials, the practicability or otherwise of testing in chimpanzees.

Then if we go ever the page, we can see in the second half of the page there's a summary from Professor Bloom at the end. Now, that's Mr Berry's note. I want to go back to the other note and then ask you a question. So if we go back to the other note, please, Henry, which is PRSE0002647. This is Dr Frank Boulton's note. We looked previously in some detail at the section of it on AIDS, and I'm not going

to go back over that because I read the key parts of it when I was asking you a question. But we can see, again, the first two and a half pages of this note deal with Dr Eibl talking about methods of chemical inactivation of viruses. Then if we go to the next page, we see, for example, considerable discussion about chimpanzee testing, observations and contributions from Professor Zuckerman. Then we see under the heading "Clinical trials design", considerable discussion over the nature of the design. Then we know from the rest of the note there's lunch, and then an after-lunch discussion about AIDS.

Then if we can go to the last page of this, please, Henry. Again, we see the list of attendees. It's predominantly but not exclusively Reference Centre Directors, but there are others present too, obviously, including yourself.

I just wanted to show you those again and press you as to whether you have any recollection of this meeting now because it does seem to have been a somewhat unusual meeting. Not common to meet at Heathrow Airport hotels, I would have thought?

A. No.

Q. Not common to have the great and the good of haemophilia and hepatitis care meeting with one

particular pharmaceutical company?

A. Sure.

Q. And it's an important meeting, and there aren't many people I can ask about it.

A. I have no recollection of the meeting, I'm afraid.

Q. None at all?

A. I have no recollection of the meeting.

MS RICHARDS: Sir, those are my questions, subject to anything further from behind. Whilst I check that, I understand that you, sir, may have a handful of questions for Dr Colvin.

SIR BRIAN LANGSTAFF: Just one or two. I think you may be being asked one question here, Ms Richards.

(Pause)

MS RICHARDS: Before you ask your questions, I am asked just to go back to one particular issue, and I'll do so. We talked about -- I asked you about diagnosing haemophilia and von Willebrand's and you talked about some of the difficulties.

A. Yes.

Q. The particular question I am asked to put to you is: in the 1970s, so not earlier than that, was there a standard model of diagnosis for VWs and haemophilia, was there a standard set of tests or diagnostic approaches that would be undertaken by you?

A. There was always difficulty in knowing who did or didn't have von Willebrand's disease, and one of the anxieties at UKHCDO was that many of those who had been diagnosed as having von Willebrand's disease might not actually have it, might even be normal persons.

The diagnosis of haemophilia itself can be very difficult if the level of haemophilia is very mild and the scientific advances that led to the effective diagnosis of von Willebrand's disease and its distinction from haemophilia continued throughout the time I was a haemophilia doctor.

I do not recall the exact date of the work that resulted in the discovery of the so-called von Willebrand factor and von Willebrand factor antigen. I think they were in the early 70s because I think they were beginning to establish this when I became *au fait* with haemophilia care.

But the results of haemophilia, normally low Factor VIII level, normal von Willebrand factor, and the results of von Willebrand's disease, low Factor VIII level, low von Willebrand factor, were relative rather than absolute. These were not helped by the fact that things like blood group could affect the level of Factor VIII and von Willebrand factor in

the blood. They weren't helped by the fact that if you had an thyrotoxicosis, an overactive thyroid, your levels of factor and von Willebrand factor went much higher and might look normal. They weren't helped by the fact that if you were trying to look at a screaming child who you were trying to get a blood sample from, that in itself would lift the level of Factor VIII clotting activity in von Willebrand factor and therefore would tend to negate the results of your test.

Now, one of the criteria for diagnosing von Willebrand disease was that you had three levels, taken at separate times, that made you think that this was von Willebrand's disease rather than haemophilia and there is a form of von Willebrand's disease that looks exactly like haemophilia, unless you do some very sophisticated tests.

So, yes, there was a system for distinguishing between mild haemophilia and von Willebrand's disease in the 70s, but it became more and more sophisticated as the years went by and different sub-types of von Willebrand's disease became diagnosed.

Also it was the case that people who had very mild haemophilia were being diagnosed who would never have been diagnosed in the past.

So I fear I've repeated my answer.

Q. It may be it's a question I can take up with others, thank you, Dr Colvin.

Sir.

SIR BRIAN LANGSTAFF: I just want to go back, if I may, to your description of why it is that you don't entirely agree with or didn't agree at the time with Dr Winter's approach to heat-treated product.

A. Yes.

SIR BRIAN LANGSTAFF: What you said, I think, was you thought that you would not use commercial heat-treated product because of three things. The three things you identified was, first of all, the donor pool, the way that the donor pool was selected. The second was the size of the pool. The third, I think, was the possible effects or side effects of viral inactivation.

Considering the donor pool, the way selected, what was your impression of the donor pool from which the United States, certainly, commercial concentrates were drawn?

A. So the first thing I would say is that my view wasn't expressed in quite that way. What I said was that the safety of a product was dependent on the donor pool and the way it was selected and the size of the pool.

But it wasn't -- I didn't say "I wouldn't do this because that", but I think that's perhaps a niggle.

But the point that I was making was that if the donor pool, as it was, was very highly infected with HIV infection, then it might not be possible to inactivate the HIV by the heat treatment method that had been chosen. Because the viral inactivation acts on a principle of log kill. That is that you have logarithms of reduction. So if you have 10 to the 6th particles and your system of viral activation will deal with 10 to the third particles, you are still left with 10 to the third particles. So it means that if you have a very high level of contamination then a particular process of viral activation may not be effective because of the failure to address the number of logarithms of infection that are in that donor pool.

SIR BRIAN LANGSTAFF: So the first question mark over commercial concentrate was the nature of the donation input, if I can call it that way, the donor selection.

Now the Inquiry will hear, or will know, does know, that on 24 March 1983 the FDA in the United States made a recommendation to the pharmaceutical companies. But recommendation is something of a euphemism, perhaps, because it's a bit

like meeting a policeman who says, "Well, I recommend, sir, you had better move on"; you don't normally disagree if you are prudent. So it was something -- a recommendation with potential teeth.

What it recommended was that there no longer should factor concentrate be manufactured from donors in the three particular groups: those who were men who had sex with men, homosexuals; those who came from Haiti; and those who used intravenous drugs.

Was that widely known in circles in which you moved shortly afterwards?

A. I think I was aware, and certainly had been made aware in this Inquiry, that those who were selecting donors were encouraged to change their donor policies. Whether they actually did so, I don't know, but my impression was that they had promised to do so.

Q. So there would be a difference, it would follow, between the post 24 March -- post March, if I can call it that, plasma used for making product and the pre-March?

A. That's possible.

SIR BRIAN LANGSTAFF: It's the Inquiry's understanding that there was a degree of concern expressed, at least in public circles, picked up I think by the press and a statement made in Parliament, about the possibility

that because commercial companies used plasma products which had been manufactured and stored for a while, a couple of years perhaps, back stock, that that might no longer be able to be sold in the United States.

It was thought -- an actual implication of the recommendation was that if you had old -- product made from old plasma, you would simply not supply it or withdraw the stuff that was already out there. That would be, one would think, a logical interpretation?

A. Yes.

SIR BRIAN LANGSTAFF: Basically, the UK thought or some people in the UK thought that the plasma companies might try to dump --

A. I understand that --

SIR BRIAN LANGSTAFF: -- this product in the UK, and it became, as I say, from the information the Inquiry has, a matter of some concern. At least reflected in a Parliamentary statement by Baroness Masham at one stage, saying it wouldn't happen.

Shortly after that -- if we can just have, Henry, if we may, the DHSC0002231_036.

I appreciate you haven't seen this. This is a letter from, as you can see, the Joint Parliamentary Under-Secretary of State at the DHSS has written to the General Secretary of ASTMS, and if we go down to

the last paragraph:

"Regarding blood products from the USA ..."

Can we just highlight that, Henry.

"... in March this year the US Food and Drugs Administration ... initiated new regulations for the collection of plasma designed to exclude donors from high-risk groups. Although future supplies of [Factor VIII] both for export and for use in America will of course be manufactured from plasma collected in accordance with these regulations, there is still a quantity of stock, some already in this country and more in America awaiting shipment here, which has been made from 'pre-March' plasma. The FDA has recently ..."

This letter is, as I say, in August, 26 August 1983:

"The FDA has recently decided not to ban the use of similar stocks intended for the USA market because to do so would cause a crisis of supply. Obviously, the same considerations apply here."

Now, just pausing there, this is talking about supplying the UK with plasma which was thought to be an unsafe basis for American concentrate of whatever form. Was this ever discussed, to your recollection, by the UKHCDO?

A. Not at all. But, of course, I wasn't in the -- I wasn't in the inner sanctum of the UKHCDO at the time. I was a foot soldier.

SIR BRIAN LANGSTAFF: Did you ever hear any discussion of this?

A. Not at all. No, no. Absolutely not. I mean, this is the first I've seen such a letter. I was never aware of this idea of dumping Factor VIII in the UK and I wouldn't have been. I'm not making the excuse that I was a foot soldier but I was just one of the UKHCDO people. I wouldn't have had access to any kind of reference centre data. Peter Kernoff would have done but I wouldn't have done.

SIR BRIAN LANGSTAFF: I just imagined that you might have listened to the press or the TV and picked up something of this going on because of your particular interest in.

A. Not to my knowledge, sir.

SIR BRIAN LANGSTAFF: But you didn't?

A. No.

SIR BRIAN LANGSTAFF: Well, if you had been, I imagine you would have taken steps to ensure that none of that stuff was used in your practice?

A. Indeed. And I mean, when we get back to 1986, when I'm sitting in my room in Milan, I had this awful news

that a heat-treated concentrate has transmitted HIV and, in that case, I did take immediate telephonic action.

SIR BRIAN LANGSTAFF: Now, there appears to have been general consensus by the end of 1984 that heat-treated product was probably safer. You had un-heat-treated product, and we saw the letter that counsel showed you earlier, where you said in effect that you were going to go on using up the existing stock. That's BART0000676. I think that's the right reference.

No, it's not the one I --

A. I'm familiar with the other. I don't need to see the --

SIR BRIAN LANGSTAFF: So why exactly was it that you didn't do what you've accepted was perhaps the logical thing to do once it's been decided that a product is less safe than it might be, why did you go on using it up, rather than withdrawing it straight away?

A. So I think the argument is based on my discussions with Peter Kernoff at the time (and I think I have already answered the question, I am happy to answer it again) and that is that if it were to be the case that these commercial products which had been heat treated were not safe, from the HIV point of view, then all my HIV negative patients would become HIV positive, and

I'd have an even bigger crisis on my hands.

I've mentioned this in terms of the decision that Mark made (which I don't criticise because he said himself it was the most difficult decision he'd probably ever made) and that was that he said that he was personally convinced that this was the right thing to do but that conviction was based on zero evidence.

SIR BRIAN LANGSTAFF: Can I then go back to the period between May 1984 when Mark Winter and Jeff Savidge were going to use heat-treated commercial product and that period between then and December, you still had a certain amount of Factor VIII concentrate which came from America --

A. Yes.

SIR BRIAN LANGSTAFF: -- in use because you had to have. You didn't want it, but you had to have it to make sure there were supplies.

Did you think that perhaps changing that to use heat-treated commercial concentrate would involve no greater a risk to the patients and might involve less?

A. I didn't know it existed.

SIR BRIAN LANGSTAFF: But you didn't know about Mark Winter's decision at the time?

A. No, not at all. I don't think anybody else did either.

SIR BRIAN LANGSTAFF: Well, it's quite interesting that you say that. Presumably, there wasn't much discussion then around this time, that you can recall, about the possibility of products becoming heat treated.

The reason I ask that is that it was in March '83 that Hyland were licensed by the FDA in the States to distribute heat-treated Factor VIII.

A. But they were --

SIR BRIAN LANGSTAFF: It had been around on that basis --

A. They were trying to produce a lower hepatitis risk product, and I wasn't available -- I'm sorry, I wasn't aware that such a treatment was available in the United Kingdom and, as I understand it, Mark's use of this product was based on his being provided with a supply which was enough for him and for Professor Savidge's -- Dr Savidge's use.

But it wasn't available to anybody, I think, as -- I mean, I wasn't aware it was available at all and I don't believe -- of course, sadly, Peter died some years ago. But, I mean, you could certainly ask Christine Lee, perhaps, if she was aware of the availability of this product. I don't believe that it was available to the nation as a whole in any way at all.

SIR BRIAN LANGSTAFF: If anyone had wanted it, could they have obtained it on a named patient basis?

A. I've no idea what the level of supply was or what availability it was thought could be made.

SIR BRIAN LANGSTAFF: Subject to availability.

A. Sorry?

SIR BRIAN LANGSTAFF: Subject to availability.

A. Yes, but I've no idea about that, and of course, it is also the case -- and I made this very clear -- that my own particular interest was, where I could, to use the NHS.

SIR BRIAN LANGSTAFF: Yes.

A. I think that very much guided my thoughts because I wasn't keen to use commercial products. Of course, I used commercial products, and I used them in the period that you're talking about (and I recognise that) and, if that had serious consequences, which I think it did, then I'm very sad that that happened. But I wasn't aware of the availability of the concentrate that Dr Winter was describing, and I don't think there was any actual evidence that it was going to be effective.

SIR BRIAN LANGSTAFF: Yes, I see. Thank you very much.

The next two or three questions are really -- may seem to be quite random, but the first is this:

you knew of the risk that factor concentrates might give rise to HIV -- sorry, to causing AIDS before the discovery of HIV --

A. Yes.

SIR BRIAN LANGSTAFF: -- or the identification of HIV. Did you monitor at all any of your regular patients for signs of the opportunistic infections which were characteristic of that condition?

A. I didn't specifically monitor any patients, and I don't think any of my patients showed any clinical signs of the condition.

I mean, I think the characteristics of infection, or the progression of infection such pneumocystis pneumonia or throat infection with Candida were the two big signs, I think, of AIDS in that time. Because nobody, I think it's fair to say, with haemophilia ever developed Kaposi's sarcoma. It was actually a transmissible agent that was transmitted by another route.

So I think that the characteristics of AIDS which might have shown up were pneumocystis pneumonia, that you couldn't possibly miss, and infection with candidiasis in the throat, which I think you also wouldn't miss.

However, it was the case -- and I know this --

that there were people who got pneumocystis pneumonia not with haemophilia in my community because I'm sure they didn't. But at that time before AIDS was really well recognised, people were going down with pneumonia, and it took a long while for it to be appreciated that they had pneumocystis pneumonia because they weren't known to be homosexual or drug addicts or drug users.

So I am confident that nobody under my care developed the characteristics of AIDS in that period.

SIR BRIAN LANGSTAFF: Thank you. The next thing -- you said that 8Y, when it came into regular use, was a really good product. Can you just help. What's so good about it?

A. Well, it was effective. It did what it said on the tin. When you gave it, you got a Factor VIII level. Of course, you'd expect that. It was reasonably easy to make up because some products were less soluble than others, so it was reasonably easy to make up. And it -- although intermediate purity, there were people who thought, and this included the people at the Royal Free and I think in Birmingham -- there were people who thought that it had a very low incidence of causing inhibitor development. Now, if this was the case -- and it may have been but it's hard to prove --

it would be extremely valuable because when you treat somebody for the first time, about 20 per cent of people will develop inhibitor to Factor VIII, and then you are in serious trouble. Two things may happen: either the inhibitor may go away, or it may get worse. If it gets worse, then until the recent advances in science, haemophilia became almost untreatable.

So to have a product which had a lower incidence of inhibitor development in newly treated patients would have been a big advance. The difficulty is that it's quite difficult to prove that when you've got quite limited numbers. But the people at the Royal Free and I think in Manchester -- or, sorry, Birmingham, and even our own sort of private experience that wasn't published, was that people treated with 8Y seemed to have an incidence of new inhibitors when treated for the first time which was lower than that which was associated with the commercial products.

However, later, when very high purity products became available, there were those who thought that they had other advantages. For instance, there were people who thought that the very pure products were less likely to cause immunosuppression, in terms of not immunosuppression caused by AIDS but

immunosuppression caused by concentrate exposure.

Then when recombinant factors became available, there were different ways of making recombinant factors, and one of the recombinant factors was slightly altered in its structure of Factor VIII in a way which some people thought increased the likelihood of an inhibitor developing.

Now, we now go back to the concept of added value in the commercial world, where all commercial companies feel that it's their job to provide evidence that their product is better than the opposition's product, and everybody's, quite rightly, suspicious of such claims unless they can be absolutely proven.

Generally speaking, the idea that one product was more or less likely to produce inhibitors than another was hard to confirm at a scientific level. But those who used 8Y, particularly, I think, Birmingham and the Royal Free, and maybe us to a certain extent, got the impression that it was very fit for purpose and that it was derived from UK sources, which we generally thought was a good idea, and that it might be less likely to cause inhibitor development, despite the fact and even, possibly, because of the fact that it wasn't totally purified. Because it was possible that the presence

of the proteins that you described, fibrinogen and Factor XIII, a bit of immunoglobulin maybe, conceivably could have protected the active agent from the immune system.

Now, this is all speculation and I don't think there's any hard scientific evidence, but I think I could see how that might be true.

SIR BRIAN LANGSTAFF: Thank you very much.

That's all I think that I have to ask you.

Ms Richards?

MS RICHARDS: Sir, I have no further questions but is there anything that Dr Colvin would wish to add?

A. Well, I would like to make a very brief final statement, and my brief final statement is that I deeply regret that patients under my care were infected with any viral agent and I also deeply regret that there were patients who I looked after who were infected by viral agents from other centres.

One of the things I tried to do was to provide comprehensive care for my patients and their families and of course I looked after many families across a number of generations and that was a real challenge, which I tried to meet.

I would like to thank the nursing staff at the hospital for their dedication and the contribution to

care they made in very difficult times. I have been asked for the names of those nursing colleagues and I'm happy to pay tribute to Sheila Hayden, Debra Jones, Wanda Little and Cathy Woosey, who were the main nurses who helped me to look after my patients.

The last thing I would like to say is that I had an enormous admiration for my patients and their families, who showed dignity, endurance and enormous courage in the face of dreadful illness and personal tragedy.

SIR BRIAN LANGSTAFF: Thank you very much.

Can I finish by saying this, that you've been particularly useful. Your evidence has been particularly useful to the Inquiry because your experience and recollection covers the mid-'70s to the early 2000s, coming into it, dare I say it, as perhaps one of the newer and younger doctors to practise in that field at that time as a consultant. And can I thank you for the lively and informative way you've given us access to that perspective that you have and to your thoughts.

I'm very grateful to you for having done so and for having had the -- being prepared to come here to give evidence, despite the challenges of Covid. So thank you very much.

A. Thank you, sir.

MS RICHARDS: Then, sir, tomorrow the plan is that I will conclude the presentation on Cardiff, and then we'll move on to, and I expect to complete in the course of tomorrow, the presentation in relation to St Thomas' Hospital.

SIR BRIAN LANGSTAFF: So Cardiff we finish, St Thomas' we complete.

MS RICHARDS: St Thomas' we start and I am anticipating we will complete.

SIR BRIAN LANGSTAFF: You hope to complete.

MS RICHARDS: Yes.

SIR BRIAN LANGSTAFF: Very well. 10.00.

(5.17 pm)

(Adjourned until 10.00 am the following day)

INDEX

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

F:

(80) request... - see

F:

F:

F:

F:

F:

F: