Tuesday, 20th October 2020

(10.00 am)

SIR BRIAN LANGSTAFF: This morning and tomorrow we have Professor Lee.

MS RICHARDS: That's right, sir.

PROFESSOR CHRISTINE ANNE LEE, affirmed

Questioned by MS RICHARDS

MS RICHARDS: Professor Lee, I'm going to start by asking a few questions about your career. You studied medicine at Oxford; is that right?

A. Yes.

Q. Then you held various house officer posts in the early 1970s?

A. Yes.

Q. You began your haematology training in September 1974; was that at St Mary's, London?

A. Yes, St Mary's, Harrow Road, London, yes.

Q. And you were a registrar there and the consultant was Dr Fielding; is that right?

A. Yes.

Q. And that was something that you did until 1976. You then moved to St George's as a senior registrar in November 1976?

A. Yes.

Q. You remained there until December 1982?

A. Yes.

Q. You told the Lindsay Inquiry that it was at St George's you became particularly interested in haemophilia; is that right?

A. Yes.

Q. Now, you spent, during that time, I think, some ten months or so at the South London Transfusion Centre?

A. Yes, I was working part-time. So I was working two and a half days a week there for my training, for MRC Path.

Q. And that was the Tooting centre?

A. Sorry?

Q. Was that the centre in Tooting, the transfusion centre?

A. Sorry, yes.

Q. Can you recall who the director was of the transfusion centre?

A. No.

Q. You've said in your statement that you would have understood, at that time, the risks of transfusion acquired infection. I'll come on to knowledge of risks of infection in more detail later but can you recall what, if anything, was discussed or talked about that issue at the transfusion centre?

A. It would have been the generality of the infections that were known at that time.

Q. Do you recall any particular impression of the centre at Tooting and how it was run?

A. No.

Q. Do you recall whether there were any shortages or supply issues? We've heard from Dr Winter that by the time he was dealing with the Tooting centre, there were difficulties because they were covering both the south east and south west regions; were you aware of that?

A. No.

Q. You then moved to the Royal Free Hospital at the end of 1982 and beginning of 1983; is that right?

A. Perhaps I should explain. I was given the appointment at the end of 1982. I actually took up the appointment at the end of January 1983.

Q. The post was entitled "Research Senior Registrar"; is that correct?

A. That's right.

Q. How did you come to apply for that job?

A. The job was advertised by being sent to all the haematologists in the London region. And Peter Flute, Professor Peter Flute, gave me the description of it, and that has been submitted in the papers.

Q. Yes, we'll look at that in a moment. Had you known or worked with Dr Kernoff or Dr Howard Thomas before?

A. Sorry, had I?

Q. Had you known or worked with either Dr Peter Kernoff or Dr Howard Thomas before?

A. When I did a neurology SHO post, in 1971, for six months, at the Churchill Hospital in Oxford. I was doing neurology and Dr Peter Kernoff at that time I think was doing his MD on inhibitors with Dr Rizza, and the Haemophilia Centre at the Churchill Hospital -- which incidentally is where I trained, in Oxford Medical School -- and the Haemophilia Centre was on the same site. And occasionally, I would see him because we had ward rounds to train us to take membership of the Royal College of Physicians. So that was how I knew him at that time.

Q. And had you known Dr Howard Thomas before you applied?

A. Dr Howard Thomas was a colleague of my husband in the Department of Medicine at the Royal Free, when Dame Sheila Sherlock headed up that department, and he was a professional colleague of my husband, and I did -- we did know him socially.

Q. We're just going to look at a couple of documents, the application Dr Kernoff made for a grant for the post that you took up, and the job description.

Henry, could we have, please, WITN0644062, please.
This is one of the exhibits to your witness statement, Professor Lee.
If we see the whole page, please, Henry.
This is:
"A Grant Application to Action Research for the Crippled Child."
Could you just explain what Action Research was?

A. It was a charitable trust that awarded monies for research, and as you can see from the title there, at the time it was called Action Research for the Crippled Child, which is rather insensitive and that has now been dropped but, as I understand it, I think it still exists. And I imagine it was applied for because haemophilia before treating caused great disablement.

Q. We can see that the application was -- this is November 1981, in fact. So obviously before you saw the post advertised and applied. It's called Aspects of the Natural History of Liver Disease and Haemophilia. I'm going to ask you to turn to a few pages. I want to get a sense of the work you were doing at the Royal Free under this post.
If you go to page 4, please, Henry.
We can see under the heading "Objectives" -- if

you want to zoom in on that paragraph, please, Henry.
You can see the whole paragraph under the heading "Objectives".
Sorry, can we see the whole of that section. Thank you.
So there are three objectives there set out as the objectives of the post:
"To use a newly-developed radioimmunoassay for immunological markers of non-A, non-B hepatitis to study (a) the natural history of acute and chronic [non-A, non-B] hepatitis in haemophiliacs; (b) the antigen/antibody content of different preparations of clotting factor concentrates; (c) the transmissibility of [non-A, non-B] infection to household contacts of haemophiliacs; (d) the antigen/antibody content of different preparations of pooled immunoglobulin."
Just pausing there, is that an accurate description of part of what you were doing when you took up the post?

A. This work I was not doing. This work was conducted in the Department of Medicine on samples, and it -- I wasn't involved in that.

Q. At 2.2, were you involved in what's set out there, which involves an ultrasound scanning, or was that, again, in the Department of Medicine?

A. No, I'm sure we're going to move on to this, but these were two ways in which progression of liver disease could be looked at by non-invasive methods, so avoiding liver biopsy, essentially. And there is a paper in my submission that records the results of this, but it was published much later, and it was effective to some extent.

Q. Yes, we'll come on to look at that paper at a later stage of your evidence, Professor Lee. Just trying to get a sense of what the aim of the work was.
2.3 was:
"To make a detailed evaluation of patients treated with pooled immunoglobulin ..."
And then:
"To make a detailed evaluation of the response to steroid therapy and patients with severe chronic liver disease. If appropriate, to establish a multi-centre controlled trial to assess the possible benefits of steroid therapy."
Was that part of the work which you undertook?

A. No, I need to just take a little bit of time here.
Most of the patients had been seen before I -- my post, essentially, was looking retrospectively at the data. The study that I analysed, obviously, was Dr Kernoff's supervision. The patients had been

treated, and most of them had been treated actually before I even set foot in the centre, and I think this document you've got is the application.

Q. Yes.

A. And actually what was done was not necessarily in the application. I'm sure we're going to move on to discuss it, but the information in this significant paper that came out of the evaluation had largely been in the notes before I even started.

Q. Yes. We will come on to the paper, but at the moment I'm just trying to get a sense of what the work was that you were asked to undertake?

A. Yes, but I just want to emphasise again that this is the application for the grant, and actually what I ended up doing was not necessarily what's in this application.

Q. Well, if we just look at a couple more passages in this and then we'll look at the job description, which was the job description that you answered.

A. Yes, I think that's probably a better description, really.

Q. If we look at the next page, please, Henry, before we leave this document, we can see -- if we have the first half of the page, please.
We can see there there's a description of, at (c):

"Patients to be studied are registered at the Royal Free Hospital Haemophilia Centre."

You did some work at a later stage with Dr Rizza. I'll come back to that. But for the purposes of this study, and the work you were initially engaged to carry out, it was all relating to patients at the Royal Free, was it?

A. Yes.

Q. And we can see there it said:

"All age groups are represented. About 200 patients receive infusions of blood products in any one year. All patients receiving infusion therapy are reviewed at frequent intervals and detailed information about types and amounts of blood products given, and sequential changes in biochemical and immunological tests, is available for a period of several years in most patients, and a lifetime in many."

If we skip down a few lines, it then says:

"A large bank of stored sera is maintained."

Is it correct that the stored sera had been maintained since 1978?

A. Yes, the stored sera came into practice in 1978, when Dr Kernoff took up his tenure as director of the centre. And I think it's important to just expand on

this a little. It had become reported, and people were aware, that there was a transaminitis, abnormal transaminases, in people who were having concentrates. And Dr Kernoff had come from Oxford. He had done research through Dr Rizza. And the idea of storing sera was in order to, in the future, if a test became available, be able to see what was happening.

I think at this stage, it might also be important to make a point about the first paper which was published, in the year of my birth, in 1943, by Paul Beeson, who was the Professor of Medicine when I was a student there. He, at the end of that paper, which reported seven cases of people who had got jaundice and the common factor was a blood transfusion -- this is 1943, no tests -- and he said it's important that doctors begin to understand this, and they should note down the details of the transfusion, and they should store a specimen of it for future use.

So the idea was really to understand what was happening, and it became a regular part of the whole practice of treating and caring for people with haemophilia at that time. And this went on, and it proved extremely helpful in understanding these diseases.

Q. Again, we'll come back to that in more detail at a later stage.

If we just go on to the next page, please, Henry. Go to the bottom of the page under the heading "Reasons for support requested".

We can see, again this is the grant application, but what is said there is that:

"The medical registrar will be involved in the day-to-day supervision of patients included in the clinical studies, and in the collection and analysis of clinical and laboratory data."

So that's a description of what was intended for the post that you then took up. So you were involved not simply in the collection -- or the intention was that you would be involved not simply in the collection and analysis of data but also day-to-day supervision of patients. Is that right?

A. No, that's not true. What I was involved in, the patients had already been treated in the context of a bleeding episode. And I was involved in analysing the liver function tests, the treatment they'd had, and their clinical symptoms from the notes and from the results in the notes. I can't remember the detail of it, but I think there was only about one or two patients within that short frame. The study, if you

look, finished -- it was '78 to '83.

And the actual patients who'd had treatment that I followed, there are about two, out of the -- over 58. These patients had all been treated for something that was bleeding associated. They weren't chosen. And the significant thing about this study is that these people had not been treated a lot before, so they were first treatment patients. So it was possible, at that time, the only way you could diagnose non-A non-B hepatitis was you had to have an increase in the liver function tests within one month of twice the normal level, plus exclusion of other causes of hepatitis. And the medical feature of non-A non-B hepatitis is that it could be asymptomatic, and transaminases were essentially what's called a yo-yo response; they went up and down. So unless these tests were taken about every two weeks for three months, you would miss it.

This is what had already been done in these patients under Peter Kernoff's direction, and I took up the remaining ones that were put into this paper, and I analysed what had gone before. The reason for stopping in '83 is because there was a lot of information that needed to be got out there for the benefit of the patients and people treating them.

Q. Again, we'll come back to the details of the study. I'm just trying to get a sense of the work that -- the day-to-day responsibilities you had, Professor Lee.

So if we look at the job description. WITN0644061. So if we go to the heading "Duties of the post", bottom half of the page, please. It says there:

"The main duty of the post-holder will be to take a primary responsibility, under the direction of Dr Kernoff and Dr Thomas, for the implementation, coordination, follow-through and completion of the research projects which are described in the attached protocols. He/she will be located in the Haemophilia Centre and will need to acquire a thorough knowledge of the principles of diagnosis and management of patients with congenital coagulation disorders by involvement in the service and academic work of the centre. There will be a strong clinical component to the job which will include frequent contact with patients with liver disease and their relatives, making appropriate arrangements for their investigation follow-up and collection of necessary samples and possibly establishing combined 'haemophilia/hepatitis' clinics. He/she will be responsible for the collection, recording and analysis of data which will accrue as the studies

progress and the final assembly of results for publication".

If we just go to the next paragraph, please, Henry, it then continues:

"An important component of the post will be to liaise between the Haemophilia Centre and the Liver Unit."

Then various matters set out there, including:

"It will be necessary for the post-holder to familiarise him/herself with aspects of liver disease and its management by participation in Department of Medicine meetings, seminars and ward rounds."

Then if we just go over the page before I ask you a question. Sorry, if we can go to the next part of the document. Under the heading "Study and training":

"The post-holder is expected to gain wide experience of clinical and laboratory aspects of bleeding disorders during the tenure of the post. And there will be many opportunities to participate in departmental activities which are not directly associated with the research projects."

So is that an accurate description of the job that you took up, Professor Lee?

A. Not entirely. I think the first thing that hits me that I would like just to make a comment about is the

bit about liver biopsy.

And before I took up this post, there had been a death from a liver biopsy. And that certainly was not part of what I took on. The only patient who, at that time, had a liver biopsy was the lady that is reported -- or who had fulminant hepatitis, and it was covered with cryoprecipitate. But I certainly was not trained to do liver biopsies. I never did liver biopsies.

The detail of the extent to which I, in practice, got involved in all these things was quite -- was not quite as extensive as it would appear there, because I took over in the end of January in '83, and almost immediately we were consumed by the issue of HIV.

Q. This document suggests that whilst the completion and analysis of the research project was an important part of -- a key part of the work that the person was being employed to undertake, it was also expected that there would be a clinical component, regular dealings with patients, and learning thoroughly the principles of the diagnosis and management of bleeding disorders. Was that part and parcel of what you did?

A. I think -- I'm sure all the descriptions of the centre are there in my submissions, and almost by osmosis you had to get involved in this. It was a very big

centre. So there were teaching sessions and meetings, planning, about what was happening and who was coming and things like that. But I would just emphasise that the main thing I was doing was analysing data, and I just got -- I know we're coming on to it, but it's relevant.

The study was over a five-year period from April 1978 to March 1983. And this information was already in the patient notes, and the study was retrospective in the sense these people were not recruited to go into a study. They were people who came with a bleeding problem, and then they were retrospectively identified. And because there had been the collection of the samples and the results of the liver function tests, it was possible to retrospectively analyse that information.

The five-year period went from April 1978, when Peter Kernoff was appointed, and it ended in March 1983 for the purposes of this study. I don't recall exactly the number of patients that I was taking two weekly specimens from, but clearly it wasn't very many because there's only a month that they had their treatments.

Q. I understand your evidence, but for the purposes of that study, the time period of the patients being treated was coming to an end at the point in time at

which you took up your post in January 1983.

But following that, what clinical role did you have at the Royal Free, 1983, 1984? Did you, for example, sit in with Dr Kernoff when he saw patients with bleeding disorders?

A. No. No. I think it also has to be understood that the care at the centre was a multi-disciplinary team. So there were the consultants, the clinical assistant, Dr Eleanor Goldman, who I know is making a submission; there were the family therapists; there was a physiotherapist, there were the laboratory staff. And the main day-to-day clinical work was conducted by the senior registrar or registrar who was rotating through the department from the department of haematology who was learning about haemophilia. New patients who came were seen by Dr Kernoff. I didn't see new patients.

Q. Could we have on screen, please -- this is your evidence to the Lindsay Tribunal, Professor Lee -- LIND0000326. Could we please go to page 3. You'll see you were being asked, as I am, about the various posts you'd held, and then you give an answer a few lines down:

"Essentially, I cared clinically for this group of patients really since the beginning of 1983."

The evidence you gave to the Lindsay Inquiry

suggests you had had a clinical role for patients with bleeding disorders from 1983 onwards.

A. I think a clinical role of taking blood tests or discussing patients within a team is what I was talking about. But I was not the person who saw new patients. I was not the person who made the decision about what treatment should be given to a particular patient. Dr Kernoff was the director at that time, and I was doing research under him, under his direction. And clearly, I would help out if there was some -- somebody who needed to have blood taken or to be clerked, but I just emphasise that it was a team approach.

I would also like to just put on record, when I gave information to the Lindsay Inquiry, it was 20 years ago. I was working as the Director and Professor Of Haemophilia at that time. So I had access to documents. I was -- I knew my information. You are asking me about a period in 1983 which, on my account, is 37 years ago?

Q. It is.

A. And it's difficult to remember these things.

Q. Do you have a sense or recollection of how often you had direct patient interaction, other than for the purpose of taking bloods, in the period 1983/1984?

A. No, I don't have a recollection of the complete number of cases, but I can tell you that once HIV infection began to impact -- for example, I can remember a patient. He was the oldest patient who died of HIV infection, and I did look after him when he came up to the centre because we were totally confused about what he had. So we had to think of all the possible things to test. In retrospect, what he had was Pneumocystis carinii, but we didn't know that.

So that was the kind of situation that I might be called upon to do clinical work. But the practicalities and organisation at that time was: Peter Kernoff was the director. He made the decisions. And there was a junior -- well, not so much junior, really; it would be a senior registrar/registrar -- in the department so that when the patients came up, as they could do at any time, that would be the first person who saw them and reviewed the problems, and they would probably discuss with Dr Kernoff what should be done.

Q. So a new patient typically would be seen by Dr Kernoff. Existing patients who were returning for review appointments would be seen ...?

A. It would depend on what the clinical problem was.

You also probably need to know that there was

a very well-trained senior nurse. And at that time, I can't remember how many, exactly, nurses there were, but certainly one who very sadly died two years ago. But the nurses also would be very involved in the first -- seeing the patient as they came in.

For serious situations which the patients, many of the patients -- all the patients who were treated in this paper, I'm quite certain Dr Kernoff will have seen them.

I think it's also important that you know -- you know, I was working part-time. I wasn't on call. I didn't -- I was working four days a week then, I think.

Q. And you --

A. So I think it's very difficult for me to know what Dr Kernoff was doing 24 hours of the day.

Q. Yes. I'll take you through some documents in due course and ask you about them which show some of Dr Kernoff's thinking at the time. But just going back to the question of how the care was organised and structured at the Royal Free in this 1983/1984 period.

You've mentioned team discussions. Was that a regular occurrence, that there would be what would now be known probably as multi-disciplinary team meetings to discuss patient care?

A. Can I just refer to a document here --
Q. Yes, of course.
A. -- which you have. This is on record.
In order to just give you an idea of the sort of team working we had --
Q. Sorry. Can I just stop you, Professor Lee, so others can follow? Is that the document: "Haemophilia centre haemostasis unit"?
A. That's right.
Q. Henry it's WITN0644069.
A. This was actually written in 1987 when I returned as a consultant, but it gives a description of the centre. Like, for example, by 1970, the centre had 180 patients, and this was housed in a prefabricated extension and the end of a ward at the old Lawn Road Hospital.
In 19 -- in 1972, they've got 220 patients. The present director, Dr Kernoff, took up post in 1978. And I think to give you an idea of the growth is important, because that was also reflected in the team that came together. So this is -- by 1987, you've got 981 patients in the period 1987-88. And this required more staff, so you've got Dr Kernoff and now Professor Tuddenham, who mainly was in the laboratory doing research, trying to purify Factor VIII. You had

an associate specialist, Dr Eleanor Goldman who tended to see mostly the children, and she was a trained family therapist. You have Riva -- she was 90 two weeks ago, and I think is giving evidence to you. And Riva Miller, who was called a social worker, but she was a trained family therapist and went on to develop the same-day testing clinic for HIV in the hospital, and also to advise the Blood Transfusion Centre in North London about HIV.
There was also a full nursing team. There was the nurse manager, she was called -- there was Patricia Lilley. There was a data processing officer, because Peter had a computer put in in '79/'80. There was an office manager. There were secretaries. There was a reception coordinator, and there was a full laboratory that did all the specialised tests for our patients and also had to do the specialised haemostasis tests for the whole hospital. We had a renal unit. We had a big transplant unit, and we had a big emergency department, so that was a big part of it.
In terms of the team -- would you like me to tell you about the teamworking?
Q. I'm interested in particular in the years 1983 to 1984 for the purposes of my current questions. I understand that most of the people that you've

mentioned would have been in post then -- so Riva Miller, Dr Goldman, Dr Kernoff, Professor Tuddenham.
In terms of the clinical decisions that were being taken about, for example, what treatment a particular patient should receive, was that something that was discussed in team meetings, in some sense?
A. I don't recall that, no. I think those decisions were made by Dr Kernoff.
Q. We'll come back to a number of these matters, Professor Lee, but just completing the basic chronology of your career, in November of 1984, you worked -- went to work at the Charing Cross and Westminster Medical School and Queen Mary University Hospital; is that right?
A. I hope it's possible to put this into context. I think it's important because you might wonder why I left something that was very important to me, and I really cared for those patients. I had two young children. I lived in Richmond which, for those of you who live in London may know, is very close to Queen Mary's Hospital, Roehampton, which was then a district general hospital that was a university hospital. And there was a post of a senior lecturer in haematology at Charing Cross medical school but essentially taking

up the single-handed haematology post at Queen Mary's Roehampton. And the previous consultant in haematology had been a laboratory-based haematologist, and I was to change the service because this was happening in haematology for it to be a clinical and a laboratory service. It was a very attractive post to me because there was no definite post at the Royal Free. I was doing research. And it was actually quite difficult for me to make this decision, but I did it for family reasons and proximity reasons, really.
So I applied for the post, and I became the consultant. I was there for three years, and during that time, because I was a senior lecturer, the head of the department very kindly -- this is at Charing Cross -- the professor at Charing Cross very kindly let me go on secondment for one day a week back to the Royal Free, essentially to try and write up information that we had because it was very important to get this into the public domain for the patients and the people caring for these people.
So I think that was 1986, as I remember it, that I started going a day off a day a week. And then, during that time, the workload of HIV and non-A non-B hepatitis was becoming enormous, and Peter Kernoff made the case

within the Royal Free for a consultant where the -- most responsibilities would be to manage this group of patients. And he eventually succeeded in getting the funding for that, and I applied for it and took it up in November '87.

I think -- will you enable me now to just sidetrack a little bit to what I was doing in this post about HIV? Because it is relevant.

Q. Yes.

A. Or would you prefer me to keep that --

Q. No, I'll ask you about that in a moment. Can I just ask two questions before you do that?

A. Yes, sure. Sure.

Q. The first is: in relation to the period 1984 to 1987 when you were at Queen Mary, my understanding from the evidence you've given elsewhere is that you were working initially one day a week at the Royal Free?

A. Yes.

Q. Then from 1986, you did two sessions per week at the Royal Free; does that sound right?

A. One day I did. I used to drive over from Richmond to Hampstead. And maybe that was called two sessions, I don't know.

Q. It's simply the terminology --

A. I don't think I was paid. I wasn't paid for that. It

may be that -- because of the regulatory thing within the hospital, Peter had to put in -- because I was working there, he might have had to put some kind of documentation into the hospital, because, you know, hospitals don't let people just go and work in a department without the authority. So I imagine that's where the two sessions came. But I was not paid. My salary was as a senior lecturer at Charing Cross Hospital.

Q. And in terms of the work you were undertaking at Charing Cross or Queen Mary University Hospital, did you have any responsibility during that period for the care of patients with bleeding disorders?

A. I think -- you know that -- well, I'm sure you'd know -- haemophilia occurs in 1 in 10,000 people. During the three years I was there, there was one little boy who presented, and I actually referred him to Peter. That was the only person. But, of course, you know, haematologists, bleeding disorders aren't just haemophilia. And, of course, also, that time -- we're talking about '84 -- our knowledge was perhaps rather rudimentary about other inherited clotting factor deficiencies.

But we -- at that time, I didn't have anybody specifically with haemophilia, but because I was the

consultant haematologist, if anybody bled in the hospital, I was responsible for trying to diagnose it and organise -- tell the laboratory what they should test for. But again, the laboratory at Queen Mary's, Roehampton, was nothing like the laboratory at the Royal Free. So we were not doing -- we did very simple screening tests for bleeding disorders.

Q. So you were a general haematologist --

A. Yes.

Q. -- in a district general hospital --

A. -- I looked after the whole of the haematology. And as I say, it was quite a challenging post because haematology was changing. You know, we're doubly trained in haematology. We have what's called MRCP, Membership of the Royal College of Physicians, so we have to have done medicine, and we have to have done a very full laboratory training which is medical -- MRC Path.

And at about that time -- previously, haematology had been within pathology, had been a pathological discipline, but now we were moving to a situation, actually like Scotland had always had, where the haematologist was a clinician and a laboratory person. And I had to change that hospital laboratory to that.

Q. You had wanted to make an observation about the work

you did in relation to HIV and AIDS?

A. Yes. We'll be talking about this, I'm sure, but the -- as I remember it, the National Blood Transfusion Service wanted to -- clearly wanted to test blood for HIV, and this became possible in the beginning of 1985. They were very concerned that people would therefore go and give blood in order to get a test. And in order for that not to happen, in every -- I think every region or district general hospital, I'm not quite sure of the size of the areas, they appointed two AIDS counsellors, and they were to provide a testing service for HIV, as it were, in the community. And because, when I went to Queen Mary's, they knew that I'd had some experience, I was appointed one, and I had an HIV testing clinic that I had to see people with.

The other quite extraordinary thing, and I think this is important because it does set the scene of how AIDS and HIV really overwhelmed not just haemophilia, where it was quite tragic, but it overwhelmed the whole country. And I was told to go and address every secondary school in the borough of Richmond upon Thames to educate them about AIDS.

Q. You returned to the Royal Free full-time at the end of 1987, November 1987 --

A. Yes.
Q. -- as a consultant?
A. Yes.
Q. And then you took over as acting director in around April of 1991 after Dr Kernoff was taken suddenly ill?
A. That's right.
Q. And you then became the director of the centre in April 1992 until your retirement at the end of 2005.
A. Yes.
Q. We'll come on to these in more detail, to some extent at, a later stage, but you were a member of the UKHCDO from 1991 to 2005?
A. Yes.
Q. Or before 1991?
A. 1991 to 2005.
Q. And you sat on various working parties and groups? We'll look at couple of those.
A. Yes.
Q. And you were on the medical advisory panel of the Haemophilia Society between 1993 and 2005.
A. Yes. I think I was asked in my statement about this. I think what I remember is that David Watters used to phone me periodically, but I can't remember regularly going to meetings of that.
Q. I want to ask you a few questions about St George's,

first of all, before we turn to look in more detail again at the Royal Free.

So you were at St George's between 1976 and 1982, with the exception of the time you were on secondment for training purposes to the Regional Transfusion Centre. That's --

A. Can I interject a little bit? And I'm sorry to get personal about this, but I was actually working part-time, so 6/10ths I was working, and it was under a scheme called the Married Women's Scheme. And I had two pregnancies during that time, so I was on maternity leave at two stages during that time.
Q. St George's was a designated Haemophilia Centre, not a reference centre, but a small Haemophilia Centre at the time?
A. I don't know about that because Peter Flute, Professor Peter Flute was the Professor then. I don't have knowledge of that.
Q. I just want to get a sense of the number of patients and the kind treatments used there. And we can do that, I can prompt your memory, Professor Lee, by looking at the annual returns. It's HCDO0000143_003, please, Henry. If we go to the next page, please.

This is just to get an idea, because we may not have anybody else that I can ask anything about

St George's of. This is the 1983 return, so you'd still have been there in 1983 -- no, I'm sorry. You'd moved on at the beginning of 1983, but this may give us a flavour. You'd been there in 1982.

We can see there 31 haemophilia A patients and 4 von Willebrand patients, so a much smaller centre than the Royal Free.

A. Mm.
Q. If we look at the products that were used, we can see a small amount of cryoprecipitate, some NHS human Factor VIII concentrate used both in hospital and for home treatment, and then the predominant product used there was Armour Factor VIII. And we can see the quantities there: 270,755 per hospital treatment; 309,680 per home treatment.

Decisions as to what concentrates to use at St George's, were those taken by Professor Flute?
A. I do not know.
Q. Do you have any recollection of what the treatment policies were at St George's at that time?
A. No. I could just comment on these figures.
Q. Yes.
A. I saw them for the first time, I think, last evening. You see that there's NHS Factor VIII and commercial here. It's roughly a third was NHS, and two-thirds

was commercial, and the national figures were similar to that. But I have never seen that document before. I would have no part in any decisions that Professor Flute made, and of course --

SIR BRIAN LANGSTAFF: If you look at the figures, it looks more like a fifth.
A. Sorry?
SIR BRIAN LANGSTAFF: If you look at the figures, the total there for NHS, as opposed Armour, it looks to me to be something more in the region of a quarter or a fifth.
A. Yes, maybe. I mean, I think it's not really appropriate for me to comment on Peter -- Professor Flute's figures, you know. I wouldn't have had anything to do with this.

I mean, I suppose, to follow up on that comment, I mean, it just emphasises that much more commercial Factor VIII was being used than NHS Factor VIII.

MS RICHARDS: As senior registrar and completing your training, effectively, under Professor Flute, you would presumably -- I appreciate what you said about periods of maternity leave and being part-time, but when you were there, you would presumably have been involved in the clinical care of patients?
A. Yes, but not an awful lot, actually. A lot of -- to

do MRC Path training, you had to rotate through various specialities. So some of the time would be spent -- as we've seen from the transfusion centre, there was a whole big several months spent at the Royal Marsden to learn about leukaemia, and within St George's, I would occasionally have seen a patient. But my main clinical contact with patients was actually at a district general hospital that was part of the St George's network, St James', Balham, which was general haematology, and there I would see patients on the wards, and I ran an outpatient clinic, but I didn't do those things at St George's. And, of course, there was a large amount of microscope work to screen films.

I also, at St George's, was very involved in being trained in the laboratory to do Factor VIII assays and things that are relevant to haemophilia. But that wasn't in the context of doing work for patients; it was to actually learn how to do it, because when we did our MRC Path exam, we had a three-hour practical session doing such tests.

Q. And which year did you undertake the MRC Path exam?

A. I finally achieved the MRC Path exam in 1982, so then I was fully accredited as a haematologist, but before that time I wasn't accredited.

Q. If we then move on to the Royal Free.

If we could have up on screen, please, Henry RLIT0000022.

Put this up as much as anything as an aid to memory, Professor Lee. This is a seminar you participated in, or helped organise, I think, in 1998. So that's when events would have been fresher in your mind.

A. I didn't just participate in it. I actually helped organise it.

Q. If we go to page 34, Henry -- oh, I'm sorry, I'm using the internal pagination, so can you go on 10 pages. Thank you.

I just want to get a sense of practices at the Royal Free in the 1970s. Obviously aware that you weren't there, Professor Lee, but you've talked about it in this seminar.

So we can see you ask a question:

"Was it right to think that cryoprecipitate was a thing that really pushed home treatment?"

And then you go on to say, after Dr Matthews' response:

"In our own Centre [Royal Free], Katharine Dormandy really made a major contribution. I think I am right in saying that she made cryoprecipitate in the old hospital at Lawn Road in the labs there, and that the patients

were actually started on home treatment with cryoprecipitate. In fact, if you go back and look through the notes of some of our older patients, the social work contribution was to raise the money to buy the deep freezers enabling them to have it at home."

Then if we just go to the top of the next page before I ask the question, you then --

SIR BRIAN LANGSTAFF: Just give Professor Lee a moment --

MS RICHARDS: I'm so sorry, Professor Lee.

SIR BRIAN LANGSTAFF: She's trying to follow it both in the document and on the screen.

A. Page 34 I've got ...

MS RICHARDS: I'm so sorry.

So you asked a question about cryoprecipitate being a thing that really --

PROFESSOR LEE: Which page is that, please?

MS RICHARDS: It's page 34, using the pagination at the bottom left-hand of the page. It is on the screen.

PROFESSOR LEE: Yeah, okay, sorry.

MS RICHARDS: You asked a question about cryoprecipitate --

SIR BRIAN LANGSTAFF: Just a moment.

You may find it easier to look at the screen. I don't know if it's easier -- you're working with.

PROFESSOR LEE: I think I will use this.

SIR BRIAN LANGSTAFF: Okay.

PROFESSOR LEE: As long as I have the page numbers, I'm fine.

MS RICHARDS: So you asked that question, Dr Matthews, who was at Oxford, responds about cryoprecipitate making a big difference because it was easily made, although he observes it wasn't the ideal material but many centres found it a very useful material for home treatment.

Then you made an observation about Katharine Dormandy, who had been the director at the Royal Free in the 1970s. And you observed that cryoprecipitate was made in the labs at the Royal Free, patients were started on home treatment with cryoprecipitate. And you refer to the notes of some of your older patients showing social work efforts to raise money to buy deep freezers.

Then if we go to the top of the next page, so it's the top of page 35, you then say that:

"In our Centre we were a bit slow to use large full clotting factor concentrate because it wasn't really until you [Professor Tuddenham] and Peter Kernoff came that people were started on this treatment, because Katherine had been so taken up with the cryoprecipitate."

Now, I can obviously ask Professor Tuddenham about this as well but your understanding was that cryoprecipitate had been a significant part of the treatment policies of the centre in the 1970s under Dr Dormandy?

A. Yes, I think I would like you to refer to this paper. I can give you the number if you like. Do you want the number now?

Q. Yes, please.

A. It's the history paper that was written for the 50th anniversary of the KD Trust, and it's on the -- it's 644061.

Q. I'm not sure it is, I'm afraid.

A. All right, well, on my witness statement --

Q. Not your fault at all, Professor --

A. On my witness statement it's 0655067.

Q. Yes.

A. Okay.

Q. "Blood borne infections and haemophilia: the worst of times."

A. Yes, and this --

Q. Professor Less, can I ask you just to pause for a moment so we can get it up on the screen so others can follow?

A. Sure.

Q. Henry, it's WITN0644067.

Which page would you like to refer to?

A. I think the first page is talking about Katharine Dormandy, and the references are very relevant and you have these in my deposition.

Katharine Dormandy pioneered making cryoprecipitate at the old Lawn Road Hospital. And the paper that describes that is Bennett et al, in 1967, referenced in this paper. I was actually working at the renal unit at the Royal Free when the old Lawn Road Hospital was there, so -- and it was a two-storey old Victorian hospital, and she commandeered the whole of a haematology ward, the old haematology ward, to make the cryoprecipitate on site. And then eventually the North London Blood Transfusion Service took it over. And she pioneered home treatment with cryoprecipitate. And at that stage the Haemophilia Centre was a caravan that had been paid for by the Haemophilia Society in 1965, and she had about six beds at the end of a ward bay on the ground floor, and people -- the patients with haemophilia would come up to that place if they needed treatment, or bleeds or whatever, but they also came up, eventually, when it went on to home treatment, to collect cryoprecipitate.

If you read in some of these -- this paper of hers, she describes how, in order to take the cryoprecipitate home, it needed to be kept cool, and she had card ice, you know? And that was given to the patients and they'd have cardboard boxes to kind of insulate this cryoprecipitate, because the problem with cryoprecipitate is that, you know, Factor VIII decays over 12 hours. So if you have cryoprecipitate, the actual amount of Factor VIII in it depends on, one, the speed with which you get the blood that's taken out of the patient, to centrifuge it, to get off the plasma, to freeze that plasma down to a low temperature, and then warm it up to a slightly -- sort of 4 degrees, and then decant that over to another bag. This is a process. And she had this going in this haematology ward at Lawn Road. And then those bags have to be put in a deep freeze.

And all this time, every time that material is out of freezing, the Factor VIII is decaying. And then the patients who have driven -- often those patients came from quite a distance to the Royal Free -- they have to take this home to put in their own deep freezers, if they've got a deep freeze.

And they had these boxes, the home-made boxes that would be -- card ice put in and insulated to take them

home. And all the time the Factor VIII is decaying. And I think that is very well described in the paper, Bennett.

Q. Thank you.

So throughout the 1970s, Dr Dormandy essentially establishing and operating a home treatment programme using cryoprecipitate?

A. Actually, I'm sorry to -- you asked me another question, didn't you, before that? About that she was using cryoprecipitate, and Dr Kernoff and Dr Tuddenham --

Q. I'm just going to come to that.

A. Oh, sorry. I'm sorry.

Q. Don't worry.

So Dr Dormandy's post was taken over by Dr Kernoff and Dr Tuddenham in 1978. They were appointed co-directors of the Royal Free at that point?

A. Well, that's not entirely true. Peter Kernoff was initially appointed director, and I think he was interviewed by Katharine Dormandy, and then he took up a research -- he was doing research, I think, in North America. And in the interim, Katharine Dormandy was diagnosed [redacted] and she had a terminal illness, and they then managed to get a senior lecturer post to be her replacement, although she

hadn't actually died yet.

And Dr Tuddenham, you can ask him about this tomorrow, but it's my understanding, and this understanding comes from Peter Kernoff, Dr Tuddenham was appointed by the new Professor of Medicine, Professor Hoffbrand, and he was made co-director in Peter Kernoff's absence. He was actually appointed as a senior lecturer.

So I think, at that stage, Peter Kernoff's understanding was he was director, and when he came back from America he found out he was co-director.

Q. If we look at your evidence to the Lindsay Tribunal again, LIND0000362, please.

If we go to page 5 please, Henry.

Actually, we'll just pick it up -- to put it in context -- the bottom of the previous page, the second page.

So if we pick it up down there, Professor Lee, we can see, just about halfway down the page, you were talking about Dr Dormandy and the use of cryoprecipitate, and you referred to the notes and there being records of letters from social workers trying to get deep freezers and patients talking about having that treatment at home.

And then you were asked the question at the bottom

of the page:

"For how long would the patients have continued to use cryoprecipitate for home treatment at the Royal Free?"

If we go to the next page, you say:

"... at the Royal Free, they went on using it probably for longer than other places."

And you've explained why: Dr Dormandy was very enthusiastic about cryoprecipitate. And then you say:

"[The] two co-directors were put in ... they were young doctors, and they came in in 1978 and very rapidly changed everybody to concentrate. There had been some people who had had concentrate before then, but I would think that up until 1978, the majority were still probably still on cryoprecipitate."

Then you're asked about this being a change of policy, and you answer yes.

What understanding did you have, from subsequent conversations or discussions or however you gained it, about the reasons for this change of policy? Why did they switch very rapidly to concentrates?

A. I think you should ask Professor Tuddenham that question tomorrow. I just need to just go back a bit about Katharine Dormandy. And why she was so enthusiastic about continuing cryoprecipitate was

because she had made it, and she had pioneered home treatment. And also, she -- and this is listed in the blood-borne infections paper, she'd done a study with some American workers in North America, in Worcester in the United States, and they were treated exclusively with the new concentrates, whereas Katharine Dormandy was treating people with cryoprecipitate.

And what she found was that while both groups of patients, so that's the people on the concentrates in North America and the people just receiving cryoprecipitate at the Royal Free, while both groups of patients had raised transaminases, that's a liver function test, this was more common in those treated with concentrate. This was consequently shown to be due to be due to hepatitis C.

And in the comment at the end of this paper, published in 1977, Dormandy noted:

"The long-term significance of the various abnormalities recorded here is unknown. Aggressive therapy should continue."

And I don't know, I think you'd have to ask Professor Tuddenham, but I think it was probably the convenience and the efficiency. And so when you treat people with the concentrate -- I described this fall of

the Factor VIII, making cryoprecipitate, let alone everything you needed to use this at home and also to draw it up in hospital. I'm sure that it was the much more effective treatment for bleeding. And that's probably what drove them to changing people. And it wasn't just them, it was the patients asking for it and the whole of the treatment community.

I am going to -- I think it's important to go through this again, if you will allow me. I just want to expand a bit about cryoprecipitate.

It comes in a plastic bag, and it's a very sticky solution. And when people treated themselves at home, if they, say, had a bleed, they would need probably ten bags of this. So the process of this was using two big syringes and having to draw this stuff into a syringe. And the volume that the patient had to then -- they were taught how to self-in fuse, and you imagine trying to -- it's not easy. The volume is enormous. Not only was the volume enormous but the efficiency, the amount of Factor VIII in it, you didn't know exactly how much Factor VIII was in it. It depended on how long that whole process I've described to you took.

You know it also depended on the Factor VIII, the original donor, and some transfusion centres even got people running up stairs to raise the Factor VIII in the

plasma.

So when the concentrates came, you had two little bottles, and it was a very small -- much smaller volume, and you actually knew how much was being given. So you knew how much the patient would have in his bloodstream. It also meant that people could take the bottles to work, or they could go on holiday. You know, this had been virtually unheard of because you had to have a deep freeze full of cryoprecipitate.

So I think that that was the reason that there was the complete change, and I don't think they could be criticised for that at that time.

MS RICHARDS: Okay.

I note the time, sir, is this a convenient point to take a break?

SIR BRIAN LANGSTAFF: It is, yes.

We take a break in the mornings. It will be 45 minutes. That allows everyone to have a proper socially distanced refreshment. So we start again, shall we, at 12 o'clock.

MS RICHARDS: Yes, and if you could give --

SIR BRIAN LANGSTAFF: Professor Lee, you're giving evidence. The rule is that in a break, you must not discuss your evidence, either the evidence you have given, or you suspect you may be asked to give, with

anyone. That includes those near to you and it includes counsel, it includes family, but you can talk about anything else you like. I'll see you at 12.

(11.15 am)

(A short break)

(11.59 am)

MS RICHARDS: Professor Lee, I was asking you about arrangements at the Royal Free Centre. The Royal Free was part of the North East Thames region but also, I think, to some extent, part also of the North West Thames region; is that right?

A. Yes. This was, of course, prior to us becoming a trust in 1991, but there were two regions and patients came from both areas.

Q. We know that there were regular meetings of a haemophilia working party of the North East Thames region which were attended by Dr Colvin and Dr Kernoff. What regional transfusion centre or centres did the Royal Free predominantly deal with in the eighties?

A. The North London.

Q. Was that the one in Edgware?

A. Yes.

Q. Okay.

In your evidence to the Lindsay Tribunal, you

described Dr Kernoff as a real stickler for detail. Could you just expand upon that?

A. He -- well, I suppose he -- everything he did needed to be recorded. And I suppose it's really underscored with the fact that one of the very early things he did at the Royal Free in 1979/80 was that he recruited a computer person to get a computer that could record all the treatments that patients had, and the numbers of the batches of concentrate.

And, you know, to set that in context, nowadays all of us computers in our hands, mobile phones. I didn't have a computer on my desk at the Royal Free until 1994. The idea that there was a computer in 1980 is quite extraordinary.

So that was one area, that he was a stickler for detail. The other thing which I was on the receiving end, and we're going to go on to talk about the way we disseminated the information that we were finding, I wrote a lot of that, and he would go through it with a fine-tooth comb, and make sure that the facts were right, the dates were right, and I suppose that's stickler for detail, really.

He also -- it's quite extraordinary. I mean, these days we have clinical governance, everybody talks about governance. Dr Kernoff, as early,

I think, as 1980, actually set up a management structure in the Haemophilia Centre and had regular management meetings of the key individuals who were leading each section, so the laboratories the nursing, the medical, the counselling. And I think that was also kind of reflection of his stickler for detail.

The other thing that I think is also reflects it is that, you know, these days we can go online and get any kind of information, through PubMed or anything like that. In those days it was all paper bound. And in his office, he had a filing cabinet and he had file cards, and any publication that he was aware of, or that was important, it was put on this file card, and -- so that he had a kind of almost -- a very primitive kind of PubMed, if you like.

So he was a stickler for detail.

Q. Do you happen to know what became of that card file?

A. No.

Q. If we have up on screen, please, Henry, again the transcript of Professor Lee's evidence to the Lindsay Tribunal, LIND0000326.

And if we could go to page 15, please, Henry. Just picking it up halfway down the page you, were asked questions -- and I'm going to ask you similar questions in a few moments, Professor Lee -- about the

treating practice at the Royal Free, starting with 1983. And you explained that you were working very closely with Peter Kernoff and you say this:

"... I was party to his thoughts. And we used to discuss these things in that period.

"But it was his clinical decision ..."

And that's correct?

A. Yes.

Q. Before we look, then, in a little more detail at the treating practices, just one further question about Dr Kernoff.

You've mentioned that he worked in Oxford under Dr Rizza or worked with Dr Rizza. What, if anything, can you tell us about the relationships he had with other reference centre directors, in particular Dr Rizza and Professor Bloom?

A. Professor who?

Q. Bloom.

A. I know particularly about Dr Rizza, I don't know that I know about his relationship with Professor Bloom.

And the relationship with Dr Rizza was that -- you know, Oxford was the pioneering centre in the UK for haemophilia, with Rosemary Biggs and MacFarlane, and then the UKHCDO was set up, initially as group of

doctors in the country who knew about haemophilia. Because it's so rare, she had recognised your average GP or even hospital haematologist was unlikely to see a person with haemophilia, and therefore it was important to bring together people who had knowledge in order to be able to know what was the best treatment for people.

She was at Oxford with MacFarlane, and then MacFarlane retired, and then she retired, and Dr Rizza came down from Scotland to be the director. And Dr Kernoff, as I understand it, was doing work towards an MD at London -- for London University -- which is where he trained, at the London. And I think initially his topic was to look at why people with haemophilia had haematuria, blood in their urine, but actually he changed, because it became quite a problem, an emerging problem, I suppose, in haemophilia, and that was inhibitors. He did a thesis on inhibitors.

And Dr Rizza was his supervisor, and I'm sure that Peter, Dr Kernoff, learnt about haemophilia through Dr Rizza.

Oxford at that time was beginning to make concentrates. MacFarlane had realised that if you were going to provide enough blood to make concentrates for the haemophilia population of the UK, which then,

I think, had been -- approximately 500 had been identified, patients, you would need an enormous amount of blood, and therefore they started making concentrates from bovine concentrates, animal concentrates.

And in fact, the very first tooth extractions for haemophilia were actually done under bovine concentrate. And, you know, for a patient with haemophilia, your teeth -- if you had a tooth out, it was terrible. And people could bleed to death with that, so this was quite remarkable.

One of the problems about that early concentrate, bovine concentrate, is that it developed inhibitors, antibodies. So Peter, Dr Kernoff, had moved in towards trying to look at what are these antibodies about, because of that experience. And Dr Rizza had clearly been the clinical person looking after such patients and involved in the development of these concentrates following on from MacFarlane and Biggs.

So I think the relationship was really like an apprentice, really. And, you know, I think in that history document, Dr Rizza describes about sitting by patients' bedsides transfusing cryoprecipitate. So Peter, Dr Kernoff, would have learnt about haemophilia from Rizza.

And, you know, clearly, certainly in my own career,

those of my teachers who are still alive, you go on having a respect and a collaboration with them in the sense of maybe asking them for advice or whatever. So I think that explains their relationship.

But I do not know the relationship between Professor Bloom and Dr Kernoff.

Of course I knew Professor Bloom. Extraordinary physician. And he -- I mean, his contribution to people with bleeding -- inherited bleeding disorders, was that he began to understand about von Willebrand disease. I can remember in my training, and revision for MRC Path, there was still an idea that von Willebrand factor, they didn't -- the idea that it was separate and was significant was ill understood, and it was largely Arthur Bloom who understood that. And he actually came and taught us on that training course in London at that time.

But to go back to your first question, I don't know about Dr Kernoff and Professor Bloom.

Q. I'm going to ask you about the treatment policies at the Royal Free, largely from around 1983 onwards. And similar question to those you were asked at the Lindsay Tribunal, but before we do that, I just wanted to look at one earlier document with you. It pre-dates your time at the Royal Free, Professor Lee.

I just want to go through it with you.

It's BART0000913, please.

You'll see, Professor Lee, this is a letter from Dr Kernoff to the local Health Authority, Camden and Islington Health Authority, in June of 1980, and it gives -- it's a discussion about what are going to be the purchasing relations in terms of the commercial concentrate.

So we see the first paragraph refers to:

"... the views of the medical members of the adjudication panel -- [himself] and Dr Colvin -- on the tenders submitted for this contract."

Now you've described how, when you became director, you didn't have a role in contractual relationships or choice between tenders, and we'll look at that later.

Were you aware of this adjudication panel system? Was it a system still in operation when you took over as director?

A. I don't know. This was 1980. I wasn't there. I don't know.

Q. By the time you took over as director, was it a panel that you had to dismantle, or was it something that you knew nothing about?

A. When I became -- I had nothing to do with the purchasing or the decision about which concentrate to

make -- to have -- until 1991 when I became director. And when I became director, I -- it was the same time as the Royal Free became a trust, so there's no more North East Thames and North West Thames. There was a completely different -- the Royal Free had to get its own money in. And from that point onwards, the purchasing was actually done by the supplies department. And then it was changed to the chief pharmacist, but largely, actually very good advice I had from my husband, that I should never get involved in negotiations about which pharmaceutical company, or those kind of negotiations.

There was a manager -- he was the chief scientist -- called Angus McGraw, and he -- eventually, they changed from supplies to the pharmacist of the hospital, and together with Angus McGraw, they did all the negotiating. And the only decision-making, from the point that I was making the decisions, was that I would make a decision about which was the right type of concentrate. So the types of concentrate were: is it going to be plasma derived? Is it going to be high purity? Is it -- later is it going to be recombinant? And that, as far as I was able, was done through what was the guideline from the UKHCDO at that time. So it was a type. I didn't have anything to do with the

decision as to which company to go with --

Q. If we go back to --

A. -- or the prices, which was -- I think I should just -- I think it's important, if you'll allow me, to just talk a little bit about what I know from 1991 about buying.

The Royal Free, when it became a trust, was very clear that managers looked after money, doctors look after patients, which is probably how it should be. So the contracts were not in the medical hands. And other institutions, other hospitals, who weren't actually trusts yet, but then they became trusts, some of the directors actually had the ability to choose which concentrate to have, which company to go with.

And there was a deal of profit that could be made by the hospital, because the -- there's one particular trust that I know of that the purchasers would pay a price for the concentrate, and the director of that trust -- and I think it would be inappropriate of me to name names. I'm just explaining the system that could have gone on. The director of that trust had negotiated a price A. The purchaser was asked to pay a price A plus something or other, so that trust or that hospital made enormous profits out of haemophilia.

Q. There maybe some --

A. I have to say that, and that was not the case at the Royal Free, but the negotiation of contracts was very difficult because they had to negotiate with GPs sometimes, or small health authorities.

And there was -- I remember we had a -- this is moving on a bit, but I think it's relevant. We had a little boy who sadly had an inhibitor, and the cost of his recombinant -- they were treated with a thing called recombinant 7A for inhibitors. I'm moving on a bit. But the cost of that child's care, which was totally appropriate and enabled that child to go to school and to do things which, you know, you hoped all children with haemophilia would be able to do, completely wiped out the whole budget of the authority that he lived in. And in my -- which maybe we're moving on to, but that was the reason I wrote probably to the BMJ leader about the economics of haemophilia care, that a single institution like the Royal Free, that had a tenth of all the people with inherited bleeding disorders in the country -- the Royal Free was having to negotiate these concentrates in order to get the money in to pay for the concentrate. It was very, very difficult. And it was a poor management system that had been brought in by the

purchaser-provider divide that happened in the '90s.
Q. I'm going to come on to that, but I'm going to take you back to the first half of the 1980s, Professor Lee.
We can see from this letter, and there are plenty of other examples of it, that Dr Kernoff, in contrast to what you've described of your own practice, and Dr Colvin, were actively involved in decisions about which concentrates to use. But I just want to explore with you some of the points he makes about the treatment policies at the trust in this letter.
So we can see in the second paragraph:
"The present buying policy of the Royal Free and London hospitals" --
A. Can you --
Q. Yes, it's the second paragraph. It begins "In essence". We can see that that --
A. No, it's not -- is this a letter to Mr Jones?
Q. Yes.
A. Well, I've only got the first line, is it?
Q. Ah. I'm sorry.
A. And the number 2:
"We feel rather strongly that the contract should be shared."
Q. Is that the only thing you can see on your screen?

A. Yes. I've only got down to that.
Q. Ah, I think the other screens have the --
A. Okay, now I've got the full --
SIR BRIAN LANGSTAFF: Mine doesn't. Mine has exactly what Professor Lee has.
MS RICHARDS: Mine has the whole second half of the document. Are you able to read it in that form?
A. No. It's just -- the letter only has page 1.
Q. Yes, we'll start with page 1 and move on.
SIR BRIAN LANGSTAFF: I now have the whole.
MS RICHARDS: If we pick it up in the second paragraph, we can see there that the present buy-in policy --
A. No, I still haven't got it. It just says:
"We feel rather strongly that the contract should be shared between two companies" --
SIR BRIAN LANGSTAFF: No, I think the second paragraph, Professor Lee, on the page. It begins "In essence".
A. I hadn't got that. It's now come up.
SIR BRIAN LANGSTAFF: You may have if you go back -- have you got the start of the --
DR LEE: Oh, I see. Sorry. I was looking at number -- it's got numbers 1 and 2. I'm sorry. "In essence." Okay, sorry.
MS RICHARDS: So we can see there:
"The present buy-in policy at the Royal Free was

described as sharing purchased between Immuno and Armour."
That's as at 1991. And then if we go back to the full letter, please, Henry, the next paragraph says:
"We had taken account of several factors other than the tender prices alone."
So we can see the price was a factor for Dr Kernoff. But then he says this, and I wanted to ask you about this:
"There is generally considered to be a medical hazard in exposing patients to a brand of concentrate which they have not previously received in that the likelihood of contracting hepatitis seems to be increased. Our policy has therefore been to attempt to maintain individual patients on particular brands."
Now, were you aware, when you joined the Royal Free, that that was the policy at the Royal Free?
A. Yes, that certainly was the policy, and the idea behind that policy was that maybe you could limit exposure. And throughout, we always had a policy that patients were kept on the same batch, the same number of concentrate, until that batch ran out.
Q. And then if we could go on to the third page of the letter, please, Henry. We can see in the paragraph that's numbered 6, so towards the top of the page,

Dr Kernoff says this:
"In common with the directors of most other large Haemophilia Centres in the UK, I have in the past both sought and accepted financial support for research and educational purposes from all the companies now making tenders. We're currently receiving support from Immuno, who paid the publication costs of the Royal Free 'Haemophilia Centre Handbook' which we're now selling to augment our research funds. The maintenance of our academic programme has always depended to some extent on assistance from commercial companies, and it is my intention to continue to seek such support and, if funds are offered, to accept them."
Again, dealing with the period in the first half of the 1980s, so in terms of your work there -- it would have been 1983 and 1984 predominantly -- are you aware what funding was received from pharmaceutical companies by the centre or by Dr Kernoff in that period?
A. No. I think the description in this letter I am sure is true, but I didn't know the detail of it. This is 1980, isn't it?
Q. It is. I'm just seeking to --
A. I wasn't there.
Q. I understand that, Professor Lee. I'm just seeking to establish from this what the position was in 1980 --

A. I think it's highly likely, and again, I said that Dr Kernoff was pretty much, as we said, a stickler for detail, and I'm sure he provided the information which this document is based.

Q. Then if we go to the next paragraph, please, Henry. Just have the whole of the next paragraph. If we pick it up about halfway down, he says this:

"I do not view Factor VIII simply as a commodity which forms part of a general hospital supplies list. It is an essential human blood component whose use is accompanied by serious risks to patients and whose methods of production by commercial companies are the subject of intense medical, ethical, and political controversy."

Are those issues about medical, ethical, and political controversy, are those matters which you and Dr Kernoff ever discussed?

A. I don't know that I specifically discussed with him, but I can understand from this letter, and I think what's also happening here, is that we're in a transition period. The reason that initially the contracting was done through suppliers, you know, who did everything from toilet papers to, I don't know, soap or whatever, was because it was blood, cryo. And all the blood went -- it was suppliers that did it.

I mean, actually, I don't know when you had to start paying for blood. I'm sure you had to start paying for it by the time you became a trust.

But what's happening is that you're transitioning into a drug, a therapeutic product, from blood. And I think the managerial and financial implications of that changed. That's why gradually, and by the time I got there, it changed from supplies to a pharmacy because you've got a drug.

So I think what is written in this letter is true, but what you're asking me is: was I aware of that in 1980? No.

Q. Really more, were you aware of it from your interactions with Dr Kernoff from 1983 onwards when you were working with him?

A. I don't know that we would have had discussions about purchasing products. It was, as you like, below my pay grade. I wasn't a consultant; I was a senior registrar, so I don't think I would have had any discussions with him.

SIR BRIAN LANGSTAFF: I don't think the question is really about purchasing; it's about the nature of the commercial product. So the issue, I think, is whether you may or may not have discussed at any stage with --

A. You mean whether it's Immuno or Armour or what --

SIR BRIAN LANGSTAFF: Whatever. Just call it "pharma", if you like. Pharmaceutical. Commercial companies.

A. No, I wouldn't have had those discussions in 1983.

MS RICHARDS: Did you ever have discussions with Dr Kernoff, whether in 1983 or later on during the 1980s, about what he talks there of the intense medical, ethical and political controversy? Methods of commercial production of concentrates?

A. I don't think so. I don't recall anything.

Q. When you arrived in 1983 -- I'm just going to ask you for your assistance on, in broad terms, the treatment policies --

A. Sorry. In broad terms?

SIR BRIAN LANGSTAFF: You're moving away from the microphone.

MS RICHARDS: I'm so sorry. My apologies, Professor Lee. I'm going to ask you in broad terms about the treatment policies of the Royal Free in 1983/1984. So the matters that you were able to assist the Lindsay Tribunal on I'm going to ask you about.

A. Could we have that -- the Lindsay Tribunal statement that I made --

Q. Absolutely.

A. -- because that was made 20 years ago, and at the time that I made that, I also had access to information

within the centre, and I -- you know, it's very difficult for me to recall that now, so I'd be grateful if you would put up what I said to the Lindsay Tribunal.

Q. Yes. I should say, we don't have the written statement you provided to the Lindsay Tribunal, but we do have the transcript of your oral evidence, and we can have that on screen. It's LIND0000326. If we go to page 15. I'm not going to ask you at the moment about specific parts of the transcript, but if we need to look at it, or you need to look at it to refresh your memory, we can find any useful sections.

Do you have any recollection of, broadly speaking, the proportion of patients in 1983 who would have been on home treatment at the Royal Free Centre?

A. No.

Q. Do you know whether home treatment at the Royal Free Centre by '83/'84, was that for adults and children or just adults?

A. Both, I think.

Q. Do you know whether it was -- a home treatment was only used for patients with severe haemophilia A?

A. Yes. I know that because it's mostly the practice still today. You have to train somebody to treat themselves, and as many people in this room will

attest, it's not easy, and if you're not doing it regularly -- so somebody with mild haemophilia would, you know, maybe not have a bleed or a problem; they'd have it very infrequently. So they would lose their practice. So you trained up the patients, and of course the parents, which was not so easy.

So the question you're asking me, whether it was the same for all patients, I think it was the severe patients who were on home treatment.

Q. And then dealing first with adults with severe haemophilia as a cohort, what products -- I don't mean the specific pharmaceutical companies, unless you can remember those. What products were used for the treatment predominantly of adults with severe haemophilia A? Was it commercial concentrate, or NHS concentrate, or something else?

A. It's very difficult for me to be precise about this, but the generality of my memory, and I think what's recorded here, we were very fortunate to be able to have all our children on NHS concentrate, and that was really, I suppose, for two reasons. One is they're small, so there was not a supply problem. And the other is that, although at that time there wasn't any evidence that NHS product was to be preferred, we didn't have any evidence that commercial concentrate

transmitted more than NHS concentrate, but many parents had an instinct that they would prefer NHS concentrate for their children. And that was done, and we were able to maintain that.

And, you know, in retrospect, it was fortunate, because I think the only child we had to care for with HIV actually acquired his infection, I think, abroad somewhere when he was given commercial concentrate.

As far as the adults were concerned, I believe it was a mixture. But again, it's very difficult for me because I -- you know, I didn't make the decisions, and that wasn't my day-to-day work.

Q. Again, this is territory you largely covered in your Lindsay Tribunal evidence --

A. Yes.

Q. -- so that's one of the reasons why I'm asking you.

A. I would just remind you that at the time I gave the Lindsay Tribunal evidence, I actually was able to have access by telephone to the people within the centre to review the treatment records. And that information was not information that I remembered or information that -- in that time period, or information that had been because of my decision, but I was able to get the evidence from records. And so that's -- I hope I'm not appearing to be difficult, but I would really

prefer for the -- what I said then to be taken as what was happening because, you know, I can't remember after 20 years.

Q. Yes. You'll appreciate not everyone will have had the opportunity of looking at the Lindsay transcript --

A. Yeah, sorry.

Q. -- which is one of the reasons for asking you in this forum.

So children, NHS Factor VIII; adults with severe haemophilia, commercial concentrates, or NHS concentrates or both.

A. Yes.

Q. In terms of adults with moderate or mild haemophilia -- you said in your Lindsay evidence it was difficult to distinguish between the two, not diagnostically but in terms of your recollection of treatments --

A. Yes. I think just to clarify that, I mean, severe haemophilia is less -- it's defined as less than 2%, but basically it's nothing, and non-severe haemophilia is above 2%.

Q. So for adults with non-severe or mild or moderate haemophilia, what was the general approach to treatment for those adults?

A. I think we're talking about 83, so by then, we did have access to DDAVP, I think.

Q. Yes.

A. Professor Mannucci's paper was published in '77, and I was actually quizzed on it during my MRC Path exams, so -- in the early 80s, so I think we had DDAVP by then.

And DDAVP you can give to people who have non-severe Factor VIII deficiency. You can't, of course, use it for anybody with Factor IX deficiency. It acts by raising the Factor VIII. You can also use it for people with von Willebrand's disease, but at that time we hadn't worked out what it was all about, von Willebrand's disease. And some people didn't respond and some people did respond, and it was only later that the different types emerged because we had molecular diagnosis.

So DDAVP was beginning to be used where it was possible, but if -- particularly in the context of von Willebrand's disease, I would say, if a patient had an enormous bleed, like, you know, some of the nosebleeds that people had, which are called epistaxis in the papers, it could be torrential. So to use DDAVP was just totally inappropriate. And there were also people who just didn't respond to it.

Q. So for the patient who is a non-severe haemophiliac, and for whom you've judged that DDAVP would not be

appropriate in those particular circumstances, what would be the next line treatment in 1983/84?
A. Factor VIII.
Q. Concentrates?
A. Yes.
Q. Not cryoprecipitate?
A. No.
Q. You told the Lindsay Tribunal, and we can look at this if we go to the next page, we pick it up at out of the third of the way down, you were asked the question -- this here was about children, but you were asked the question:
"... were any of your children receiving cryoprecipitate as distinct from concentrate?"
You said:
"No ... I have carefully checked on that issue, and the only person with severe Haemophilia A who received cryoprecipitate during that period was, if you like, a kind of conscientious objector who didn't want to have concentrate."
Pausing there, what did you mean by "conscientious objector"?
A. It was somebody who didn't want to have blood products. I don't think -- is "conscientious objector" the right word, actually? The parents

didn't want any blood product.
Q. You describe it there as a patient who received cryoprecipitate. So someone who didn't want concentrates.
A. Yeah, I -- quite honestly, I think it's very difficult for me to remember precisely what that means.
SIR BRIAN LANGSTAFF: Let me tell you how I read it.
Somebody who on principle -- I think is the words I would replace "conscientious objector" by, but on principle didn't want to have concentrate. It doesn't say what the principle is but that's the sense of it that it conveys to me at the moment.
Is that about right, do you think?
A. Yes, I really -- I do remember when I was giving the Lindsay Tribunal evidence actually phoning back to the Royal Free for them to check, I think probably either the treatment records, the Oxford returns, you know, or to find out that fact, because I was being asked that fact, and that was the information I got at the time, I think.
MS RICHARDS: So that was the use of cryoprecipitate for a single severe haemophiliac. You were then asked:
"Whether for children or for adults, the choice was if somebody was a person with mild haemophilia and who could be treated with DDAVP, that was done [which

is the answer you gave me a few moments ago] if not, they were treated with concentrate, either NHS or commercial --"
And you answered "Yes".
Do you know why cryoprecipitate was not used as the second line treatment for a mild haemophiliacs if DDAVP was not an option?
A. Well, I think I partly have rehearsed the answer to this question in that the practicalities were very difficult. But more than that is the issue of exposure, that to treat a bleed you need ten -- what are we talking about? We're talking about --
Q. Mild haemophilia.
A. Mild haemophilia ... no, I think it's more about the risk of not getting the right level and not having effective treatment, I think, is probably the decision-making process here.
And, you know, it relates to the fact that the Factor VIII in the cryoprecipitate was not known, in that you couldn't measure it, and it would probably have had a fall-off by the time you got it into the person. And also, it may also have been the issue of the bleed they got, you know. The treatment just wouldn't be so effective.
But, you know, again, the decision-making process is

Dr Kernoff's. It's very difficult for me to know exactly how he came to that decision. And I -- I think it would have been very difficult, in some situations -- in many situations -- to use cryoprecipitate effectively and to stop the bleeding, and to prevent the problem.
Q. Was prophylaxis a feature of treatment at all at the Royal Free in the first half of the 1980s or was that something that only came later?
A. No, prophylaxis, we started prophylaxis at the Royal Free in 1994. It depends, in a way, what you call prophylaxis. You know, regular prophylaxis for children was begun in 1994, where you gave three treatments a week to keep the level up to a certain level, to stop bleeds, but sometimes there was a certain kind of prophylaxis used if people had a particularly important day, you know, that was -- in their life, that they needed cover. I'm trying to think of an example.
I mean, for example, I suppose if you had a job interview or something like that. But it wasn't used widespread in the sense that we now know about prophylaxis.
Q. Haemophilia B was the predominant treatment NHS Factor IX concentrate?
A. Yes. And the reason for that is that it's sixth less

common than haemophilia A, and there was not a supply problem.

Q. And then you've touched on this already, but von Willebrand's disease, what was the predominant mode of treatment in 1983/84?

A. Well, it depended what the bleed was, but if it was a severe bleed it would be concentrate. And you'd try and use DDAVP if you felt that that was going to be effective.

Q. Just look at the 1983 returns for the Royal Free which would have been completed by Dr Kernoff and/or Dr Tuddenham.

Henry, it's HCDO0000184_006, first of all, please.

We can see here it's the annual return for 1983, the directors are identified as Drs Kernoff and Tuddenham. Total number of haemophilia A patients treated during the year, 128; carriers of haemophilia A treated during the year, 4; total number of von Willebrand's disease patients treated during the year, 24.

Then if we look for the haemophilia A patients, we can see for hospital treatment, there's an amount 1,443 bags of cryoprecipitate which someone has translated into units.

Then we can see NHS Factor VIII concentrate at

193,965 units for the hospital treatment.

And then the total use for home treatment that's given there, would that have been predominantly children, in all likelihood?

A. No, I don't think it would necessarily be all used for children. That seems quite a lot. I think there would have been adults who were on home treatment with NHS, not necessarily all of them. I don't know.

Q. Then we can see the figures for use of commercial products, so in that year, Factorate, Koate, Hemofil and Kryobulin were all used, and the concentrate used most was Factorate, just over a million units of hospital treatment and 1.2 million for home treatment. And then Koate, 1.2 million for home treatment.

If we go further down that, we can see "Porcine Factor VIII". I'll ask you a little more about porcine later, but what, at that time, was porcine Factor VIII predominantly used for?

A. Inhibitor treatment. And it's important, I talked a bit about -- or do you want to leave this for later, about porcine?

Q. We'll come back later to porcine and one of the papers you wrote about it later, if we may, Professor Lee.

Then we can see reference to some other products, including DDAVP.

And then von Willebrand's disease we can see cryoprecipitate was still being used for von Willebrand's disease, it would appear, in hospital, and then smaller amounts of NHS treatment used, hospital and home.

If we can then go to HCDO0000184_051, please.

I won't go through the details of the inhibitor treatments, Professor Lee, but we'll just look at haemophilia B.

So this is the annual return for 1983 in relation to haemophilia B treatment. Thirty-one patients treated throughout the year, one carrier treated, and we can see there that the bulk of the material used there is NHS Factor IX concentrate.

Are you aware of 1983 representing any particular change in approach to treatment or, as far as you know, was this broadly consistent with what had been the treating practices at the Royal Free?

A. No, I -- you know, I just say again that I was a senior registrar in the department. These decisions and what was going on with the treatment was managed by the directors. So I think what is here is here, and I'm sure it's true.

Q. If we just look at WITN0644070, please, Henry.

This is a document from much later, it's

a 1995 document that you prepared.

If you could just go to page 20, which is the last page of this document, please.

You provide there a table showing "Clotting factor concentrate obtained 'free' in the context of therapeutic trials", and that gives figures from 1988 onwards to 1995, which was the date of the document, with a total value attributed to it of £1,572,000.

Do you know what the position was at all pre-1988? Were clotting factor concentrates provided free to the Royal Free for therapeutic trials?

A. No, this is -- covers the time period that I was in charge. I was a consultant. I became a consultant in 1987. So prior to that, I didn't -- I don't know what was obtained.

Q. Do you know at all -- and again, I'm asking you now, again, about 1983/'84, what the arrangements were at all for obtaining NHS concentrates?

A. No.

Q. You don't? You don't know whether that was from the Regional Transfusion Centre or directly from BPL?

A. No, I don't know.

Q. You've alluded to the fact that there were possibly supply issues in relation to NHS concentrate because you focused the supplies you had on children and then

some for adults. Do you recall any more detail about what those supply issues were? Was it something you discussed with Dr Kernoff or he discussed with you?

A. No. I do have memory that he tried to save up concentrate if he knew somebody had got to have an operation. That's the vague memory I have.

Q. NHS concentrate?

A. I don't know which particular one it was. I just know that he would do that, because I think there was a supply problem in general sometimes. But I think it's probably not appropriate that I make statements about this period because I would say again that I wasn't responsible for making any of these decisions, I wasn't looking after the patients that needed to have an operation. It wasn't my job, you know. So I think what we have to do is rely upon what's in the returns and things --

Q. And your evidence to the Lindsay Tribunal, which you've said --

A. -- yes, yes -- (overspeaking) -- what is said in the Lindsay Tribunal, which, as I say, I said it 20 years ago, at the time that I said that, I cross checked with people who were then alive, like the chief nurse in the unit, who -- and she -- I phoned up and cross-referenced, and she checked out things. But

I think it's very difficult for me now to give reliable information about that period. I'm quite happy to give you the detail for when I was in charge, in 1991, and how it was done. But I think it's unfair, it's probably unfair for Dr Peter Kernoff, to say why the decisions were making what he was doing, you know, because I don't know.

Q. Professor Lee, to make it clear, I'm not asking you to speculate.

A. Yes, well, that's why -- I don't want to speculate.

Q. The reason I'm asking these questions is because you told the Lindsay Tribunal that these were matters that you discussed, that you were to some extent privy to Dr Kernoff's thinking, and you did give evidence to the Lindsay Tribunal about what the treatment policies were, in broad terms, in 1983 and 1984.

A. Yes, but what I would also say is that we are now in 2020, so this was 20 years ago. And I think the information that I provided for the Lindsay Tribunal at that time was what I remembered 20 years ago from 17 years before that. We're now 37 years, and I'm sorry, I think that what I said in the Lindsay Tribunal was as true as I knew at that time, and from my standpoint now, I stand by what I said. But I don't think it's fair for me to speculate, in

a way, because trying to remember back 37 years, when I've been retired 15, is quite difficult for me. But what I said to the Lindsay Tribunal was true at that time.

SIR BRIAN LANGSTAFF: Not only at that time, so far as you know, you've just said that you think it's -- that you have no reason to think that anything said there was inaccurate?

A. No.

SIR BRIAN LANGSTAFF: Thank you.

MS RICHARDS: You've said Dr Kernoff was a stickler for detail and for records. What, if any, policy or practice was in operation at the Royal Free during the 1980s for recording what particular products patients received and what particular batches patients received?

A. This is the 1990s?

Q. 1980s. You returned as a --

A. 1980s, okay. Well, I have explained to you that there were the beginnings of a computer in '79/'80, but every patient had a treatment record which was in addition to the medical record, and all the notes and all the treatment records were all kept in the Haemophilia Centre. And in those treatment records, which the patients had at home, and we had access to

them in the centre, they had to record the date, the type of bleed, the number on the concentrate bottle, and how much used.

They had those forms at home. Initially, they would post them back, and within the centre, they were filed in this treatment file. And of course, if we were administering treatment in the centre, because sometimes a patient might come up with a problem and they needed treatment in the centre, we would record in those treatment files. And gradually, from 1980 onwards, that treatment was put on a -- on the computer, which was a pretty primitive computer to start off with.

And you showed us the records that were sent to Oxford. We called them the Oxford returns. And there are a large number of patients we looked after and a large amount of treatments. So that detail eventually was -- we were able to put the numbers down because we had a computer. But it was still there in the records, and the data manager within the centre, Francoise Kendall, one of her big jobs was to go through these records and, you know, add it all up.

Gradually, we were able to appoint additional people in the centre. I think we had two people eventually. We had a -- we were so lucky because we -- the patients were so lucky. We had a big cold room where all the

concentrate was kept, and eventually we had a good stock control, so there was a record of what's here, what numbers are here, what's gone. So I think that answers your question, does it?

Q. Yes. The treatment records from the 1980s that you've described --

A. Yes.

Q. -- so the physical treatment records, and leaving aside whatever there might have been transposed to the computer --

A. Yes, yes.

Q. -- were they part of the patient's medical records, or were they kept separate?

A. No. They were -- the medical record was, you know, like we all remember medical records, was a big file. And then there was a separate folder which had these sheets of paper in. And within the centre, initially when I went there in '83, when there were less patients, we actually had a whole bank of filing cabinets, alphabetical filing cabinets, where these records were kept together.

When we were able to have the new extension opened in 1994 by the Duchess of Kent -- and, in fact, just to add in, that was the last decision Peter was able to get out of the Trust before he was ill. It wasn't a Trust

then, but the hospital was to go forward with this extension. We were able to put in that extension, a huge area -- it was about the size of this platform here -- where the notes were put in filing cabinets, hanging in filing cabinets, but the notes were with the treatment folder. So if you saw a patient, anybody saw a patient within the centre, they had those notes.

Q. And when you retired in 2005, did those hard copy treatment recordings still exist?

A. Yes, I think -- it's a tragedy, actually. Those treatment records enabled us to have an enormous information about haemophilia, which wasn't just so that you would get publications; it was so that you could inform your care and understand what was happening. It was there on paper. Sadly, I think the -- certainly, the hospital record has now been computerised because I know my rule nine request, we've had to, you know, get them. I don't know what's happened to the treatment records. And I tell you, it is a real tragedy. I mean, maybe they've been digitised. I don't know.

Q. Well, we can no doubt ask that question of the Royal Free.

Sir, I'm about to move on to a topic that won't be capable of being resolved within the next two to three

minutes, so shall we break for lunch now?

SIR BRIAN LANGSTAFF: Yes, let's break for lunch until two o'clock. So an hour for lunch. Back at two o'clock. The same rules apply as they do at any break.

(12.59 pm)

(Luncheon Adjournment)

(1.58 pm)

Q. Professor Lee, I'm going to ask you about the developing knowledge of risk of non-A non-B hepatitis as the next topic.

First of all, as part of your general medical training and then your haematology training, what do you recall you learnt about the risks of viral transmission from blood and blood products?

A. Sorry, the risk of?

Q. The risks of transmission of virus through blood and blood products, to what extent did that feature in your medical training?

A. Well, going back to when I was a house officer -- sorry, a medical student at the Radcliffe, at Oxford, Professor Paul Beeson was the Professor of Medicine. And I remember very distinctly going on ward rounds, almost the first ward round we went with him as a student, he would -- he asked us what the risks of

blood transfusion are. And we very quickly learnt to say hepatitis.

And of course the reason he focused on that was because he was -- I said earlier, he was the man who in 1943, as probably a very junior doctor, had noticed that seven patients he wrote up who had become jaundiced, there seemed to be a common feature that it was due to transfusion.

And he was the person at the end of that report that said that it was very important that doctors recognised this problem, and recorded details of what they'd had transfused. I think scientifically speaking, looking at it, you could argue, you know, maybe it was hep B. But it was probably more likely that it was non-A non-B hepatitis. And of course, you know, there were no tests, at that time. I suppose that was my first knowledge.

Then, of course, the Australia antigen, as it was called, was identified in 1965, and I remember this was the measure of hepatitis B, it was called Australia antigen because it was first identified in Aboriginal people. I can remember as a medical student, we had -- he's died now -- we had a professor -- no, he wasn't a professor, he was a gastroenterologist/hepatologist, Ralph Wright, and

I can remember going with a bottle to get a patient's blood who had come in, when I was a house physician, for access to this Australian antigen which he had in his laboratory.

I think my knowledge -- I think, if I remember rightly, and maybe I don't need to remember the details of this, and we can refer to it, but the test for hepatitis B came in 1973, so by the time I did my blood transfusion training, which is 1979, I think I would have certainly been aware of hepatitis B. But of course by then, we had a blood test for it, so ...

In terms of -- of course I knew about hepatitis A, because it's orally -- you know, it's a problem not necessarily from blood transfusion, although later it transpired that one of the concentrates transmitted it. I think non-A non-B hepatitis, to be perfectly honest, I don't think I was particularly aware of it until I was aware of this research post. I think that's probably true.

Q. I'm going to ask you in a moment to look at a small number of papers from the 1970s.

A. Yes.

Q. You may not have seen them at the time but they're a set of articles that we've asked other clinical witnesses to look at so far.

A. Yes.

Q. Before I do that, generally speaking, in the 1970s and 1980s, what journals or periodicals, magazines, would you read in order to keep yourself up to date with medical developments?

A. I think for the most part, in that time, I would more read textbooks. You know, there was a particular volume for haematology called Postgraduate Haematology that was the sort of bible, if you like, for MRC Path.

In terms of journals, I mean, we -- my husband and I -- had a life subscription to New England Journal, so that would have been in the house. British Medical Journal was also in the house, I would that have read. Lancet also. We took Lancet at home, I think. And in terms of other relevant journals for haematology, I would have read the British Journal of Haematology. That would have been in the library at St George's Hospital.

Again, I think what's really changed so much over this time is the -- you had to go to paper journals. I mean, now you go straight into the Internet and can find anything. And the other thing, I suppose, is really the delay in publication. So sometimes you would rely on hearing things at meetings, and sometimes there would be departmental meetings, but it wasn't really

very focused, really. When I did blood transfusion I think hepatitis B I would have really known about, and hepatitis A to a certain extent, but that was it.

I think the other thing to say just about blood transfusion, when we did our training in blood transfusion a lot of it was on the basis of looking -- doing the laboratory things of looking at antibodies and antigens. You know, you had to identify whether somebody had an antibody to a certain blood antigen, so it was quite complicated and we spent a lot of time doing that. So, I think --

Q. Okay.

You've already indicated, I think in the responses you gave this morning and just now, that it had generally been known, certainly from the 1940s onwards, that hepatitis was a major risk of blood transfusion. I'm going to ask you to look at -- it's four or five short articles.

If we could first of all, please, have, Henry, PRSE0001431.

I'm going to show these to you or extracts from them, they're all fairly short, Professor Lee, then ask you some questions about them.

The first is a Lancet article from August of 1974, it's:

"Long-incubation post-transfusion hepatitis without serological evidence of exposure to hepatitis B virus."

There are just two passages, I think that will suffice for current purposes.

The summary explains that:

"An agent other than hepatitis B virus seems to be the cause of 36 (71%) of 51 cases of post-transfusion hepatitis identified during prospective biweekly serological follow-up of 204 cardiovascular-surgery patients. The sera of the 36 cases showed no evidence of the antigen or antibody response expected to accompany response by HB virus and to be detectable by the assays used. Incubation periods and clinical and epidemiological features were inconsistent with hepatitis A. Cytomegalovirus-associated seroconversion was no more common among the HB-negative cases than amongst HB-positive cases or amongst patients who did not develop hepatitis. The data suggest that a large proportion of long-incubation post-transfusion hepatitis is unrelated to hepatitis B and that control of post-transfusion hepatitis will require identification of a hepatitis virus(es) type C."

Then if we go to the last page, please, Henry, left-hand column, towards the top of the page, the first

main paragraph beginning, "The fact that":

"The fact that non-B hepatitis cases are less frequently associated with serious acute illness does not imply that such cases are of lesser importance. Long-term complications of acute hepatitis B infection, such as chronic hepatitis, cirrhosis, and hepatoma, have been reported to follow mild anicteric infections more frequently than severe icteric cases; consideration must thus also be given to the possibility that non-B hepatitis may play a role in the aetiology of some forms of chronic liver disease."

Pausing there, Professor Lee, first of all, I'm not expecting you necessarily to remember if you read this at the time, but would you have been reading the Lancet at the time? Is this the kind of material you're likely to have come across in 1974?

A. I think no is the answer, because I was just reviewing my CV and trying to -- in 1972 to '73, I was a research registrar in renal medicine to now Sir Keith Peters, and then, for domestic reasons, really, I left that post, I had an MRC training fellowship but I left that post-to work as a general practitioner, because I thought that would fit in with my domestic responsibilities.

I think it's very important -- and this is in the

College of Physicians recordings that I made, which were essentially to look at how women went into medicine in that period, early sixties, early seventies. There was no opportunity, really, to have children in creches or nurseries and things, and so my husband and I felt that it might be a better career path for me. And I actually trained in general practice for a year, until September 1974. And then I realised that general practice wasn't for me. I'm very privileged to have had that experience because I think it helped me understand people and patients and other concerns. I went back into my first love, really, which was haematology, because before I went to university, I'd worked for eight months in the post-graduate medical school in the haematology laboratory.

So I went into haematology, and that was when I started at Harrow Road, so St Mary's Hospital, Harrow Road, with Dr Jack Fielding. And that was a district general hospital, September 1974. I don't think we had any patients with haemophilia. And I certainly would not have been aware of that article then. But I know the article and I probably, I think, became aware of it when I took up the research post in 1983.

I think it's also important to -- for me to remember and to let people know what the knowledge was and -- around about that time.

I was a Senior Registrar, part-time Senior Registrar at St George's, and then I said I'm going to take this post to look at non-A non-B hepatitis. And amongst my colleagues, there was almost a universal thing: what is that? Why are you doing that?

So, to answer your question, I probably became first aware of this paper when I took up the research, because it's a very significant paper, but up until then, I would not have seen it, and probably very few of my colleagues would have been aware of it.

Q. Well, I understand the basis for the first part of your answer, Professor Lee, that you wouldn't have seen it at the time. What's the basis for your suggestion that your colleagues, in haematology, would not have seen it at the time?

A. The basis is my personal memory of when I said I was leaving to go and do this research. So I'm talking about the group of doctors that I worked with --

Q. At St George's?

A. -- at a very significant department of haematology, headed by somebody who worked -- who specialised in haemostasis.

Q. You rightly said that this was a significant piece of work, something which you realised later. It's significant because it's one of the first articles which identifies the existence of what then, over the next few years, became referred to as non-A non-B hepatitis, isn't it? So that's it's key significance: that this is the emergence of non-A non-B hepatitis, and an understanding that there was some other form of post-transfusion hepatitis. Would you agree?

A. Yes.

Q. And the second part of its significance is that observational warning on the last page, that -- I'm going to paraphrase and put it in perhaps more colloquial terms: that clinicians shouldn't assume that non-A non-B hepatitis is going to be something mild or insignificant. Would you agree?

A. Well, I agree what's written there, yes.

Q. If we could then have up on screen, please, Henry, PRSE0001794.

This is an article by Craske, Dilling and Stern in August of 1975 in The Lancet:

"An outbreak of hepatitis associated with intravenous injection of Factor VIII concentrate."

We can just, again, look at two passages. I think this is one, again, that you're familiar with,

Professor Lee.

So the summary tells us there was:

"An outbreak of jaundice associated with three out of four batches of a commercial brand of freeze-dried Factor VIII concentrate ... at the Bournemouth Haemophilia Centre between April and June, 1974. Seven cases of non-B hepatitis and four of hepatitis B occurred within 6 months of the first use of this product. Two patients contracted both types of hepatitis ... nine ... became ill out of a total of twenty regularly seen at the centre, eighteen of whom received commercial Factor VIII concentrate."

So here we have a report of non-B hepatitis in haemophiliacs published in The Lancet.

And if we go on, please, Henry to page 3, and look towards -- in the left-hand column, second half of the page, towards the bottom of the page, please, Henry, the paragraphs numbered (1), (2) and (3), we can see that Dr Craske and his colleagues say this:

"In the meantime, some or all of the following measures might help to lessen the frequency of jaundice.

"(1) Commercial Factor VIII concentrates should be reserved for the treatment of life-threatening bleeds in all haemophiliacs and for covering major operations."

And then it goes on to make two other observations.

Is this is an article that you think came to your attention at the time?

A. In 1975, I think, did you -- wasn't it?

Q. Yes, it's August of 1975.

A. In 1975 I was working for Jack Fielding, I think, and I was also pregnant with our first son, and I don't think I would have been aware of this article then. I certainly, of course, became aware of it as soon as I started looking at this problem, and again, I would remind you, in 1975, I think that was when Katharine Dormandy was in charge of haemophilia.

Q. Yes.

A. And of course, if you read -- subsequently I read her articles, and it was quite clear that she must have been aware of that and she must also -- because she wrote that paper where she compared the rate of transaminitis in her patients treated with cryoprecipitate, and the American, but I don't -- I certainly wasn't aware of this paper in 1975.

Q. I absolutely understand that, Professor Lee. I'm trying to explore these papers to some extent generally with you but drawing on the fact that you consequently specialised in haemophilia. Would you agree that this is a report that ought to have been given serious consideration by those caring for

patients with bleeding disorders in 1975?

A. I don't know, really, I don't think I can comment -- I think what it says I agree with.

The question you're asking me is if everybody caring for people with bleeding disorders would have known this. I don't know. You know, I wasn't involved in that at that time.

Q. Yes. My question is not whether they would have known it as a matter of fact, because obviously you can't speak to that, but whether you would accept that they should have known it. This was a significant publication in terms of haemophilia and non-A non-B hepatitis.

A. I think that's probably right. But, you know, I think it's very difficult for me to comment on those people at that time because, you know, knowledge moves, and I don't know how long -- I mean, this kind of information might have been presented at meetings. I don't know. I wasn't -- you know, I wasn't at those meetings.

Q. No, and we have access to meetings such as those of UKHCDO.

Do you recall whether -- later, once this became an issue that you were going to be researching with Dr Kernoff, do you recall ever discussing with him the

recommendation that we see here from Dr Craske and others that commercial Factor VIII concentrate should be reserved for the treatment of life-threatening bleeds and for covering major operations?

A. I don't remember specifically discussing it with him because, as I said, you know, my responsibilities were not in deciding what should happen; it was trying to find out what was happening at that time.

Q. If we could then have, please, PRSE0000381. This is a publication in the Yale Journal of Biology and Medicine in 1976 by Purcell, Alter and Dienstag. If we just have, please, Henry -- to page 246. So it's the fourth page.

There's just one passage I wanted to invite your attention to, Professor Lee. And we see, by this time, the terminology of non-A non-B hepatitis is being used. Third paragraph down:

"Although type non-A non-B hepatitis is associated with less severe acute illness than type B disease, the long-term prognosis for the two diseases may be similar."

And then the passage goes on to discuss transaminase elevations. I just invite you perhaps to read that to yourself, Professor Lee, and then I'm going to pick it up in the last sentence of that paragraph:

"Chronic non-A non-B hepatitis is not necessarily a benign infection and may be the cause of a significant proportion of chronic hepatitis" --

A. I haven't got there.

Q. I'm so sorry, I'm going too fast.

So that others can follow, I'll read the whole passage:

"Thus elevation of transaminase values persisting for six or more months has been observed more frequently following non-A, non-B hepatitis than following type B hepatitis. Others have reported similar results. Transaminase elevations have been documented for several years in some patients. Three such patients at the NIH underwent liver biopsy. Two had histopathologic changes in the liver compatible with chronic active hepatitis, and the other was diagnosed as having chronic persistent hepatitis. Thus, chronic non-A non-B hepatitis is not necessarily a benign infection and may be the cause of a significant proportion of chronic hepatitis not identifiable as type B disease."

This a publication 1976. I'm afraid I don't know the month without --

SIR BRIAN LANGSTAFF: It's a bit of a publication in February.

MS RICHARDS: Yes.

SIR BRIAN LANGSTAFF: And that's the best we can do.

MS RICHARDS: I'm sure we can find the precise date, but in any event, it's published in the Yale Journal of Biology and Medicine. 1976, I think, was the year you moved to St George's. You've described the broad nature of the work you undertook there.

Do you think you would have come across this article at the time?

A. 1976 is a very significant time in my life because our elder son was born on [redacted] 1976, and I was on maternity leave, and I spent the time trying to negotiate a part-time post. I had no job, and in those days it was quite difficult if you wanted to have a part-time post. And I think I eventually started at St George's Hospital on October 14, 1976. So I certainly was not aware of this paper then, but I did become aware of it, of course, because it's a very significant paper and, you know, I knew Harvey Alter, eventually, and I think this last sentence is very important. It says, you know:

"Thus, chronic non-A non-B hepatitis is not necessarily a benign infection and may be the cause of a significant proportion of chronic hepatitis not identifiable as type B disease."

The significant word is "may". May. And the whole

point of studying this -- there was no test -- was to find out more about it. I mean, the whole -- all people at that time, and this was knowledge that I would have, you know, become aware of as I started doing the -- putting the data together. People were aware of these transaminitises. There was an idea that it might be some kind of metallic thing in the concentrates. But they were aware of the transaminitis, but the knowledge of what was going to happen was just not there. Indeed, even the acute infection, until we published our very detailed follow-up, the -- what happened when you have an infection was not so clearly defined.

You know, a lot of people who have their first attack, it was asymptomatic, and as I think I said earlier, the only way, at the time that we were doing this, to prove as best you could that somebody had had it was to do sequential transaminases. And to have a baseline, you needed to know what the level was before somebody had the treatment. And, of course, we didn't know the long-term consequences really until -- I would suggest the knowledge didn't really become defined as to long-term consequences until the 1990s. And perhaps our long-term study in 2000 actually was very significant because what it showed was that for people who didn't have HIV co-infection, the progression rate was very

slow.

So I hope that answers your question.

Q. Yes. Would you agree that, whilst you're right to point out the use of "may" and to observe that there was still much that was unknown --

A. Yes.

Q. Would you agree that, again, for clinicians practising in the field by 1976, this, from a very authoritative source, should have been, at the very least, a warning against any assumption that non-A non-B hepatitis was somehow mild or insignificant?

A. No.

Q. Why?

A. Because I don't think they would have read this journal. Sorry, this is the Lancet one?

Q. No. This is the Yale Journal of Biology and Medicine.

A. Yes. I mean, I doubt whether anybody in this country reads the Yale journal. I don't think they would have been aware of this.

Q. How would important research such as this from the States have been disseminated in the United Kingdom, then, in the late 1970s?

A. I would think that somebody like Dame Sheila Sherlock would have probably attended the American association meetings of hepatology. That's probably how it would

have got disseminated. But I don't think this would necessarily have been disseminated amongst haemophilia physicians, this particular paper.

Q. Let's look at one more from Harvey Alter which is now the following year, 1977. Henry, this is NHBT0000092_002, please. This is headed "International forum". It's published in Vox Sang 32 in 1977, and it appears to be an address by Harvey Alter. I'm just going to ask you to go to the end of it, to the second page, please, Henry. Last paragraph of the paper, so the bottom right-hand column:

"Although non-A non-B hepatitis is, on the average, less acutely severe than type B hepatitis, it can cause severe acute disease, and, more disturbing, it appears to have considerable propensity to progress to chronic hepatitis."

Then he says:

"The main thrust of post-transfusion hepatitis research must now be directed at developing detection methods for the non-A, non-B agent or developing some reliable method of viral inactivation or removal which would be independent of testing."

Were you familiar, first of all, with this journal?

A. Sorry, what was the journal?

Q. If you go to the first page, it's Vox Sang.

A. No, I didn't read Vox Sang.

Q. Would you expect haematologists to be broadly familiar with research or --

A. In 1977?

Q. -- advice of this kind?

A. I doubt whether -- not every haematologist would have read Vox Sang. I think it might be quite helpful, if I am allowed, for us to -- am I allowed to do this -- to help answer these questions about what people were thinking and what they knew around this time because, you know, it was 1977. I wasn't moving in these circles.

Q. No, but I'm very keen to keep this chronological, Professor Lee --

A. Yes, but can I --

Q. Materials from the 1970s, absolutely.

A. I just -- you've had this article, and I just feel it's important for people to get a feel for what people knew and didn't know. Haematologists, I'm talking about, and haemophilia physicians. In fact, the most -- the person who has contributed the most to haemophilia care in the whole world, I would say, and that's Professor Pierre Mannucci, who is now 80. And you have this.

Q. Yes. What's the reference, please?

A. The reference is witness 755071, and --

Q. So that reference -- could you give me the title of it?

A. It's "AIDS, hepatitis and haemophilia in the 1980s: memoirs from an insider".

Q. If you give me a moment, Professor, I. Know we have it, but it's not necessarily --

A. I may be able to give you a different reference that is -- your references are all different.

Q. It's WITN0644071, please, Henry.

A. This is wonderful. Thank you very much.

To answer your question, really, you were asking about what would you expect people to know? And I think what Mannucci's writing here really encompasses it, really, that through the 1970s it was recognised that the use of concentrates -- coagulation factors made from plasma pools -- were associated with hepatitis. And then it goes on to talk about the first tests of liver function tests, and I think it quotes the Alter paper. And then I would just point you to the second page, the next page, and I think it's important that -- that sentence that he writes:

"It was only in the mid-1970s it became clear that hepatitis was frequent in haemophiliacs and it was only in mid-'80s that it was shown to be progressive and

severe in one-sixth of patients."

Q. If we look at the -- it's right to note this was an article written by Professor Mannucci, I think, in 2003, or published in 2003. I'm going to ask you some more about this later.

A. You've got the wrong one. That's why, is it?

Q. The top of the first page, to cite this article, and then we see the date --

A. Okay.

Q. -- 2003. But if we go to the second page which you were referring to, Professor Lee, you'll see in the paragraph above there, he talks about:

"A relatively benign picture of non-A non-B hepatitis initially emerging from studies being questioned by three subsequent studies published in 1985 and 1986."

A. Yes.

Q. The issue I'm exploring with you, or seeking to explore with you, Professor Lee, is whether it's right to characterise the understanding of non-A non-B hepatitis in the second half of the 1970s as being a relatively benign condition.

A. I think, for some people, it was a benign condition. It probably was, and it was probably right to characterise it. I think the whole problem was that

it was very difficult to sort out who was going to have a benign prognosis and who was going to progress quickly to liver disease. And that only really evolved later.

Q. We'll look at some of your later studies in due course, Professor Lee, but if I can return you to the second half of the 1970s, there are just two further documents I'm going to ask you to look at.

RLIT0000228, please, Henry. This a publication by Hoofnagle and others called "Transmission of non-A non-B hepatitis", and the date is 1977. This from the Annals of Internal Medicine. If we could go, please, Henry, to page 6. It's the long paragraph in the right-hand column, beginning "Several clinical and epidemiologic features of non-A non-B hepatitis". Thank you. I'm just going to read certain parts of this, Professor Lee, and then ask you about it.

"Several clinical and epidemiologic features of non-A non-B hepatitis have become clear from studies such as the present one. First, non-A non-B hepatitis closely resembles type B hepatitis. The incubation period, the clinical symptoms and signs, and the potential for chronicity appear to be similar to type B hepatitis. Undoubtedly, what was once referred to as serum hepatitis included both type B and non-A non-B

hepatitis."

That's the first point. Second:

"Non-A non-B hepatitis appears to be spread predominantly by the parenteral route. Most cases have been described in association with transfusion, intravenous drug use or serum inoculation."

Then we can skip to the third point:

"Third, non-A non-B hepatitis appears to be associated with a chronic carrier state and chronic liver disease."

Then it goes on to talk about the particular study.

"These 'implicated' blood donors were, for the most part, asymptomatic, although liver function tests and liver biopsy examinations frequently showed evidence of underlying chronic hepatitis."

Then:

"Finally, non-A, non-B hepatitis appears to be common. Three of the five infectious donors studied here transmitted this non-A, non-B hepatitis."

Then it goes on to talk about the screening of blood donations.

If we can just go a little further down, Henry. Then goes on to say:

"... at the present time, more than 90% of post-transfusion hepatitis is due to non-A non-B

hepatitis."

Professor Lee, this doesn't appear to be consistent with Professor Mannucci's description of a benign picture in the 1970s of non-A non-B hepatitis, does it?

A. Well, I would just comment, and I think we need to perhaps talk about the work that I did for -- with Dr Kernoff and Dr Thomas, and that is that certainly the acute attack in some of those patients was totally asymptomatic.

And I would also like to fast forward to tell you that come the year 2000, so we're talking 30 years after, there was a very low number of people who had progression of their hepatitis in those who were not infected with HIV. HIV was a terrible co-infection that speeded up the progression of this condition.

I think it's also quite difficult, in 1977, to talk about progression when there is not a test, and in certain -- I think it's very difficult. And, you know, I think it's very important to understand that part of the rationale, if you like, of trying to understand what happened following concentrate treatment -- which had been given because it was necessary because the patient had some kind of life-threatening bleed or needed an operation or whatever, acute operation -- it's important to understand that we needed to know what was happening,

you know.

And I don't -- I've got a bit lost with your question, actually. I'm getting tired.

SIR BRIAN LANGSTAFF: Would you like a break?

PROFESSOR LEE: I think I probably would like a bit of a break, if that's possible. Just five minutes or something.

MS RICHARDS: Or we could take the half hour break now, sir.

SIR BRIAN LANGSTAFF: That would be a good idea, I think. It's not an endurance course; it's evidence. So by all means, if you feel tired, you just let us know, and you've done that.

So take a break now for half an hour. Allow everyone to get a cup of tea. A bit earlier than usual, but that's what we'll do. So be back, please, at 20 past 3.

(2.42 pm)

(A short break)

(3.16 pm)

MS RICHARDS: Professor Lee, there's one further article from the 1970s I want to ask you to look at.

It's PRSC00003622. And this is The Lancet, September 1978. It's a report by a Professor Preston and others, "Percutaneous liver biopsy and chronic

liver disease in haemophiliacs".

If we look at the summary, and this should come up on the screen in front of you, Professor Lee, the summary says:

"Systematic screening of forty-seven haemophiliacs in Sheffield revealed abnormal liver function tests in thirty-six (77%), with a tendency for these abnormalities to persist. To assess the importance of these abnormalities, percutaneous liver biopsy was carried out on eight symptom-free patients under Factor VIII cover. A wide spectrum of chronic liver disease was demonstrated, including chronic aggressive hepatitis and cirrhosis. The liver pathology bore no relation to clinical history or to biochemical findings. Hepatitis B virus markers were common, but evidence suggests that this is not the only factor contributing to the development of liver disease. The high incidence of chronic liver disease seems to be a recent development and is probably related to factor-concentrate replacement therapy."

Then if we go to the third page, please, Henry, under the heading "Discussion", we see -- a little closer.

We can see it starts by saying that:

"77% of our treated haemophiliacs had abnormal liver

function tests and a history of a hepatitis-like illness was listed in 50%."

And then this contrasted with earlier reports.

Then if we go to the next paragraph, please, Henry, we can see it talks about biopsies, and then halfway down the paragraph it says:

"We also found a wide spectrum of chronic liver disease including benign self-limiting chronic hepatitis, potentially treatable aggressive hepatitis, and established cirrhosis. All our patients were symptom-free at biopsy and it was impossible to differentiate between the different forms of liver disease on the grounds of biochemical abnormalities. Since the patients undergoing biopsy had been arbitrarily selected it is reasonable to conclude that in a large proportion of haemophiliacs receiving treatment with Factor VIII have important chronic liver disease."

Then one final paragraph.

Next column, please, Henry. Go up a bit.

Picking it up six lines down into the first main paragraph, Professor Lee:

"In addition, non-A non-B hepatitis may well be an important factor and observations in four of our eight patients support this possibility."

And then that is developed in the rest of that paragraph. And then we have the conclusion that histological liver disease is common in haemophiliac patients, and then again it goes on to talk about assessment by use of biopsy.

Now I'm not at the moment asking you about the practicalities or, indeed, the risks of liver biopsies, which you've already alluded to and we may come back to, but just in terms of the importance here of the findings of evidence of chronic liver disease, would you agree that this was a significant paper?

A. This was a significant paper, but I think it's important that it's realised that there were other studies.

I think the other thing I've had time to reflect on when I was -- had the break, is I don't think this is a memory test, and I am a great believer in looking at documentation. And if I'm allowed, could we look at what Mannucci writes on this matter.

Q. Is that the same document we were looking at before?

A. Yes, it's --

Q. Yes. So Henry, it is WITN0644071.

A. I would -- the next page, please. I'd point you to the second paragraph. As we've discussed, it talks about, you know, unequivocal evidence of the existence

of structural liver disease in people with haemophilia and elevated transaminases, we've talked about that.

But I think what's important is the bottom of that paragraph where he talks about:

"A prospective biopsy study was undertaken by me with hepatologists Colombo and Rizzetto in 10 haemophiliacs with non-A, non-B chronic hepatitis followed up for more than 6 years. The study, published in 1982, demonstrated no case ..."

No case.

"... of progression towards cirrhosis or hepatocellular carcinoma."

So that was that his experience. I'm not saying this is universal; what I'm trying to show is that different patients were showing different progression rates and not everybody was showing every kind of abnormality.

And then I think the other significant thing that he states here is:

"The relatively benign picture of non-A non-B hepatitis initially emerging from these studies was questioned by three subsequent studies published in 1985 and 1986."

So.

"A large retrospective study of liver biopsies

collected by Aledort ... provided histologic [change] ... in 15% of cases."

So 15% of the total. And it's worth noting that within that worldwide collection of liver biopsies, one of his patients had died of the liver biopsy.

Then I think this is the study you're talking about, isn't it? I think?

Q. No --

A. Well, this is from the same group:

"In an 8-year prospective study conducted in Sheffield, histological signs of cirrhosis were found in nine of 79 haemophiliacs (12%) ..."

I mean, what I'm trying to show you is that there was a realisation this was a problem, but there was debate at that time as to how severe this problem was, and what was happening. And some of the papers published, who had done biopsies, showed abnormalities. Some of the studies did not show abnormalities.

And of course the other issue, which I think I took up with Professor Preston in the debate over doing liver biopsies, because it is relevant, is that liver biopsies were very often done on more severe patients. So it wasn't at all clear as to how many of the patients were progressing. There was no doubt there was some kind of problem. And some of the experience at this time --

and, you know, Mannucci's study was showing that.

What these people were looking at is, you know, when you treat a patient with clotting factor concentrate, you're doing it for a reason. You're doing it to stop severe bleeding. And what they were weighing up is how severe is this transaminitis, or non-A non-B hepatitis? We don't know. And is the risk of not treating them, of them not receiving concentrate, is that risk worth taking? That was the debate.

And I think the other important thing in this paper that he writes is on two pages, 2068, it's on the paper.

Q. It's two pages further on, Henry.

A. And this called "The fallacy: a retrospective knowledge". And he sets out in the table --

SIR BRIAN LANGSTAFF: Just a moment. I think it's at the bottom of the --

DR LEE: Yes, but I think we need --

SIR BRIAN LANGSTAFF: You're looking at your paper -- (overspeaking) --

DR LEE: No, no, no. There's a table at the top.

SIR BRIAN LANGSTAFF: I see.

DR LEE: Can we just see the table at the top first? He sets out in this table a very good chronological order of how knowledge developed, both for hepatitis and AIDS. Now can we go back to the bottom which you --

and it's talking about:

"The fallacy of retrospective knowledge. Table 1 summarises the chronology of the development of knowledge about hepatitis and AIDS in haemophiliacs. To sum up, even though the problem of hepatitis was known since the 1970s, there was no reason to believe that this adverse effect of haemophilia care was heralding the much more ominous AIDS. On the whole, everybody was muddled in the period between the early 1980s and the first cases of people with haemophilia and AIDS were reported."

Now, I know you're talking here -- you've separated off hepatitis, but there is absolutely no doubt at that time that there was a debate going on about how severe these changes were. Was everybody going to get this? Was it vastly progressive? And that was being weighed about -- it's all a balance of risk. It was being weighed up about whether treating the patient -- it was important to treat the patient, to give effective treatment.

I mean, some of the reasons -- people would have died of bleeding if they hadn't had these concentrates. So it's a balance of risks.

MS RICHARDS: I'll come back to balance of risks, Professor Lee, but first of all, Professor Mannucci

entitles this "The fallacy of retrospective knowledge". He is, of course, writing retrospectively at this point.

A. Sorry. He's writing what?

Q. He's writing retrospectively in this article. If we look at his table at the top of this page and we just look at hepatitis, because we're going to come on to AIDS separately, we can see, for example, none of the materials which I've just referred you to are there set out in his chronology of the main events in hepatitis. He refers to some early reports, '70 to '72. 1975, he cites his own piece of work. 1977, he cites liver biopsy. And the reasons you've explained, liver biopsies were not something that could be frequently undertaken. And then he goes on to desmopressin, DDAVP, and then to the 1980s. So it's not, by any stretch of the imagination, a comprehensive chronology of the development of knowledge of non-A non-B hepatitis, is it?

A. I really find that very difficult, what you've just said. This man lived through treating his patients through all of that period. And there is absolutely no doubt around this time that there was a debate as to what transaminitis meant. There is no doubt that it was clearly stated in some of these papers that

this hepatitis occurred, but the debate was also about how serious the transaminitis was. Was it worth stopping the treatment because it was terrible, or was it more important to go on using the treatment?

And, you know, as things transpired, the later information we had showed that, actually, for many patients, it was a very, very slow progression. And, indeed, for some patients -- quite a few, actually -- they lived through an era where we had treatment for hepatitis, so it could be cleared. So are you saying that in this period, that treatment should have been stopped because they get hepatitis and maybe they have the problem of bleeding?

Q. Professor --

A. And these people -- can I just finish? I'm sorry.

Q. Of course.

A. But these people benefited from the clearing of their hepatitis in the end. And I just will emphasise again that in this time there was debate about the seriousness of the transaminitis. I don't think anybody doubted it was there, but there was debate about the progression and the seriousness. And I would say again, Preston's paper where he biopsies -- he worked alongside a very able hepatologist called Dr David Trigger -- it's more

likely they were biopsying the patients who looked as if they were more badly affected.

Q. Yes.

A. So you have a skewed vision.

Q. You and Professor Preston I think have crossed swords, if I may put it that way, in relation to that issue in the past --

A. I don't think it's a question of crossing swords.

Q. I disagree --

A. I'm sorry, I don't like that. I am a physician. I cared desperately about my patients. They're not my patients; it's the patients. And throughout my career, I have wanted to do the best for those patients. And for me, the best I can do for patients is based on evidence and also safety and efficacy. And I do not -- and the reason you're using this term "cross swords", which I don't really like because it was just showing our experience and questioning whether it was right to risk death from a liver biopsy and provide cover to cover the biopsy. I don't think that's crossing swords. It's trying to fight for care and investigation of patients to be relevant.

Q. Would you accept that you and Professor Preston disagreed about whether his was effectively a selection of the most serious cases?

A. I think what I would say is that I put into the public domain the experience we had had. And as a physician, the way I have approached looking after and caring for patients has been that I need to know what is going on, but I need to know in a way that is not risky for them. And what I have -- we're going to perhaps talk about it, I don't know, but what I wrote up in that letter was an experience -- it wasn't when I was there that the person died of the liver biopsy. It was during the time that Dame Sheila Sherlock was investigating these patients with liver biopsy.

But having had that experience, there was no way we were going to expose patients to the risk of death from bleeding. And I do not like the idea that you are suggesting that I crossed swords with somebody. You know, I'm a doctor, and we're all in the thing together. We're all trying to find out knowledge, to find out what is best for our patients. We're not fighting.

Q. Professor Lee, let's leave aside the term I used.

A. Sorry?

Q. Leave aside the term I used.

A. Thank you.

Q. The question I was simply putting to you is: are you aware and do you accept that Professor Preston

disagreed with your characterisation of his research? That's all.

A. I think -- I didn't disagree with the results he got. Not at all. Not at all. In fact, they were very helpful. What I did disagree with was the approach of doing unnecessary liver biopsies. And for the reasons I've said. You know, there was a patient that died.

But what I would also say is that, as we went through the '90s, and then we had CT scans and imaging, the consultant hepatologists that I worked alongside were able to do what's called transjugular liver biopsies where the needle went down through the vessel into the liver, and they could do that under an image. They can see where the needle goes. When you do a liver -- I've never done a liver biopsy. When you do a liver biopsy, in the days that these were being done, the needle goes into the liver. The liver is an incredibly vascular organ, so you could hit a vessel. It's quite dangerous. And also, each one of those liver biopsies had to be covered with a large amount of concentrate, so that's another issue. That's probably a minor issue because, I think, you know, that shouldn't be an issue, but it's the safety issue.

And I think it's terribly important that we're

pursuing science here, as applied to haemophilia care, which then makes the care of those patients good and safe, but you can't do anything unless you know what's going on.

Q. Professor Lee, we may be slightly at cross-purposes. I'm not seeking to challenge your view that liver biopsies could be dangerous, or your view that they are not something that should generally be undertaken. That's not the purpose of my questions. I'm looking simply at the question of what was known in the second half of the 1970s and what was not known; the emerging picture in relation to non-A non-B hepatitis.

Professor Preston's work was a part of that picture. An important part of the picture. I'm not suggesting it was a complete part of the picture, and you pointed to some of the other studies here.

Can I ask you to look at two descriptions of non-A non-B hepatitis --

SIR BRIAN LANGSTAFF: Before you do that, can I just ask a couple of questions about this paper by Mannucci in the 1990s? Can we go down to the bottom of the page, please, Henry? Under "The fallacy of retrospective knowledge", the second sentence:

"To sum up ..."

So what he appears to be saying, this sums up the

fallacy of retrospective knowledge:

"Even though the problem of hepatitis was known since the 1970s, there was no reason to believe that this adverse effect of haemophilia care was heralding the much more ominous AIDS. On the whole, everybody was muddled in the period between the early 1980s ..."

That muddling, as I read this -- and please tell me if I'm reading it wrong. I read the muddling to be about AIDS and the link between hepatitis and AIDS. Because I think he's saying that hepatitis is one thing. Put that on one side. AIDS is another. That's the way it seemed. Have I got it wrong?

A. Well, not really wrong, but what he's trying to say is that ... one of the big problems was that mostly -- and this was knowledge that was gained later. The people who, mostly, who had big problems with liver disease actually had co-infection with HIV. And we now know that HIV was coming in, in '79. So I'm not saying all these studies, but some of these studies overlapped that period. So what he's saying is that we had no reason to know that then. I don't know if that explains it or not.

SIR BRIAN LANGSTAFF: I'm not sure it does because it's really a question of how one reads the words.

But the second question I want to ask you -- can

we go back to the very first page of this article, please, Henry. Just something that caught my eye at the bottom of the page. It's the very bottom right-hand side:

"Hepatitis. Emergence of the hepatitis problem."

This is what Mannucci says throughout the 19 -- through the 1970s:

"Recognised the use of concentrates of coagulation factors made from plasma pooled from several thousands of blood donations was often associated with hepatitis. However, the first large study of liver function tests was published in 1975."

Now, that's Mannucci's own study, I think, because that's the reference. And:

"Our survey of 91 multi-transfused Italian haemophiliacs found that 45% of them had elevated serum transaminases. Although non-A non-B hepatitis ..."

Can we turn over the page?

"... was expected to be responsible for transaminase abnormalities, a definite distinction between transfusion associated hepatitis and transaminitis could not be made at that time."

The last sentence:

"Our findings were subsequently confirmed and extended by a joint American/English study that

demonstrated that transaminase abnormalities persisted, supporting the views that they were a hallmark of chronic viral hepatitis."

Now, what he appears to be saying is that his own work in 1975 demonstrated that there was a condition which was associated with factor concentrates made from large pools and the development of chronic viral hepatitis. It doesn't say how serious the chronic viral hepatitis was, but that's what he appears to be saying. And then what does it go on to say after that, Henry?

A. I think --

SIR BRIAN LANGSTAFF: "Unequivocal evidence."

So he's saying that what he had suspected in '75 was unequivocally confirmed in 1977, and on it goes.

Have I got that right? Is that the way it would read?

A. Yes, but I think the other important thing in that paragraph is what I said earlier: the thing about the joint American/English study -- this was Katharine Dormandy's study with Levine in Boston. And I said earlier about this, that she had shown -- she had been worried about transaminitis, and she compared her patients who were treated with cryoprecipitate with the American patients who had been treated with concentrate. And transaminitis was in both, but there

was more transaminitis in the American patients. So this was a piece of information that was in circulation.

The other thing that's not actually written here, but I can tell you that there was a view held by some people that transaminitis may be accentuated by metallic things in the fractionation process. So what I'm trying to do -- you know, it's quite clear that there was -- they're descriptions of non-A non-B hepatitis. They're not really defined as non-A non-B, but going on -- but the extent of this problem and the rate of progression and the seriousness of it was not clear. There was evidences in both directions, and it was that kind of information that was informing the way people or haemophilia treaters were treating.

MS RICHARDS: Dr Kernoff, in April 1979 -- perhaps we'll look at this, Henry. BART0002487. Go to the second page, please, Henry. You'll see there it's a letter to Dr Colvin, 27 April 1979. We go to the paragraph numbered 2 under the heading "Types of therapeutic material available", and we'll go halfway down the paragraph. He's talking about commercial versus NHS concentrates. I don't need to ask you about that, at least not at this stage. Then he says:

"The clinical reason [referring to the NHS] is the

growing awareness that the probability that commercial concentrates are the higher risk of transmitting non-A non-B hepatitis than NHS material."

Then this the sentence, Professor Lee:

"This is a serious disease with long-term consequences."

I can take you to a reference the following month by Dr Kernoff and Dr Colvin which talked about it being serious with long-term sequelae and that the acute disease may sometimes be fatal.

Would you agree with that characterisation, as at 1979, recognising there's still much to learn, it was known that this was a serious disease with long-term consequences?

SIR BRIAN LANGSTAFF: Shall we use the word "believed"?

MS RICHARDS: It was believed.

A. Yeah. Can we begin at the beginning of that paragraph? The paragraph you just had, it's disappeared, has it?

Q. That's the paragraph:

"Types of therapeutic material available."

A. Saying that he wrote:

"The clinical reason is the growing awareness that the probability that commercial concentrates have a higher risk of transmitting non-A non-B hepatitis than

NHS material."

Dr Kernoff wrote that. It later transpired that is not true, and we're going to go and discuss that paper. That came after that. So what I'm saying here -- this is 1979 -- that even Dr Kernoff clearly moved his view.

Q. That wasn't, with respect, the question I was putting, Professor Lee, which is: do you agree with Dr Kernoff's characterisation, as at 1979, that non-A non-B hepatitis is a serious disease with long-term consequences? Is that a fair way of describing non-A, non-B in 1979?

A. I think all I would say about that sentence, and this is written in 1979 by Dr Kernoff, okay, is that he writes: "As far as is known." And I think that is the qualification: as far as is known. And, as we know, subsequent work shows --

SIR BRIAN LANGSTAFF: I think the "as far as it's known" is dealing with its prevalence, rather than the long-term consequences -- (overspeaking) -- if you just read it through:

"As far as is known is, at present, much less common in the UK than in the USA, et cetera."

A. Yes, okay. As far as is known: what I'd say to that is it is a qualification. This is written in 1979. And if Peter had been -- Dr Kernoff had been

alive in 2000 or 1994, he would have realised that, actually, there were some patients where the progression was very slow. Sadly, it was mostly the patients who had co-infection with HIV who progressed very rapidly. And, you know, we cared for those patients, and because all the clotting factors are made in the liver, it was very difficult.

So I think -- I suppose what I'm trying to say is this is written in 1979, and Dr Kernoff is aware of all these publications that you've shown. And he's balancing those publications up, and he's saying that it's a disease with long-term consequences. Of course, that's '79. You know, the long-term consequences for many patients didn't come until 2000. That's what I'm trying to explain. Is that helpful?

SIR BRIAN LANGSTAFF: Yes, thank you very much.

MS RICHARDS: Would you accept, in light of the material that we've looked at and any other material that you're aware of from the second half of the 1970s, that it should have been appreciated by haematologists, by the end of the 1970s at least, that non-A non-B hepatitis was a clinically significant condition which carried a significant risk of causing liver disease?

A. No.

Q. Why?
A. Because they weren't aware of that.
Q. Well, Dr Kernoff is saying it in terms. It is a serious disease with long-term consequences.
A. Yes, you asked me about all haematologists, didn't you? What was your question?
Q. I'm asking you whether you would accept that, in general, haematologists should have known by the end of the 1970s that non-A non-B hepatitis was a clinically significant condition which carried a significant risk of causing liver disease?
A. I think, I suppose what I would say in answer to that is, in an ideal world, they should have known, but what I would say is that they did not know. I don't think any of us really knew for certain what was going on. I mean that was why the study was being done: to understand it.

So I think the idea that everything was understood by the end of the 1970s is wrong. There were all sorts of emerging knowledge coming out. And in parallel with that, there was treatment, remarkable treatment. And that's what people were weighing up. And they were -- you know, one of the reasons for this study, this retrospective study that Dr Kernoff wanted to be looked into, for the information to be assembled

and published, was to find out more about what is happening, what is going on.

And of course, the -- we're going to talk about it, I know, but the most important thing that that study showed, and the knowledge up to that date had been completely wrong on it, was that whether the concentrate was made from blood donors in America or anywhere, or the UK, everybody got non-A non-B hepatitis on their first exposure.

And, you know, none of that knowledge was available in the late 1970s. That emerged and began to be presented at clinical meetings, I suppose, from 1983 onwards. But what was known -- you've shown it -- was the Craske paper, and, you know, the knowledge was building up.
Q. Professor Lee, you used the term "for certain", things weren't known for certain.
A. Yes.
Q. Professor Mannucci used the term "unequivocal" in the paper you've referred us to. Finding certainty, finding unequivocal evidence, may be something that might take many years of research and study. Clinicians will rarely have evidence of certain outcomes. They have to look at the risks. Would you agree?

A. Yeah, and I think -- yes. And I think what is important when you're considering risk, the only risk that you are considering or you're throwing at me, is the risk of this emerging picture of transaminitis, non-A non-B hepatitis.

Haemophilia treaters at that time, and indeed now, also have the risk to think about: what happens -- what happens -- if I don't give this patient treatment? What happens if this patient isn't having regular home treatment at home so that when he develops a knee bleed at home he can treat himself immediately so that that knee in future is not going to be a target joint and he becomes -- "crippled" is a word that I don't like using -- disabled.

And I think always in this risk -- and, you know, it goes for the whole of medicine. You're balancing risk. You know, what were haemophilia physicians trying to do? They were treating people who had an inherited bleeding disorder, and they had to balance up what is the greater risk: that I risk this person being at home and getting a cerebral haemorrhage? Or -- you know, I'm sorry to labour this point but it really is important. And I just -- I think it's important.

You've got this document somewhere, I've got a number. It's to try and convey what haemophilia is

like untreated. And it is relevant.

We're now in 2020, and what I want you to put up on the screen, if I'm allowed, is -- it's witness number 644002.
Q. Sorry, could you tell me what the --
A. The title of it is the -- it's from the textbook of haemophilia, it's a historical introduction that I wrote.

Can you find it?
Q. Unless it's exhibited to the long statement that you prepared for today, then it's not something that I can --
A. All right, well, am I allowed to read from it and provide this to you afterwards? I think it is probably in one of my Rule 9s.
Q. It's fine for you to read from it. We will have it but we don't have it electronically to display.
A. Will you forgive me if I -- I won't read too much. What I was going to say was, this is 2020, and this is a publication that came in 1937 from America. And it was --
SIR BRIAN LANGSTAFF: Is it the Birch paper?
A. Sorry?
SIR BRIAN LANGSTAFF: Is it the Birch paper?
A. Yes, yes.

So it was 113 people with haemophilia in 1937. And she -- actually, it's a he, with a strange spelling -- reviewed the causes of death from bleeding.

Just to give you an example, 15 out of these people died from circumcision, six died from epistaxis, seven from central nervous bleeding.

And what's even more awful was that most of these people died before the age of 15 years. I think only eight survived beyond 40. And what you're balancing is always risk of bleeding. I think we must not forget that people bleed. And it's often forgotten now because fortunately we now have recombinant products and our little boys can be treated with prophylaxis, and indeed now there's even gene therapy, so nobody ever sees a bleed. And of course cerebral bleeding was the most devastating thing of all. So that's what I'm trying to convey.

MS RICHARDS: 1937 was not 1979, and the choice was not necessarily one of, in every case, treatment with concentrates or no treatment. Amongst other things, cryoprecipitate was available, and for non-severe haemophiliacs, DDAVP.

A. Yes, let's go back to cryoprecipitate.

We know, then, that the incidence of hepatitis in

blood transfusions was one in 300. This was done from post-transfusion studies. If you treat somebody with cryoprecipitate, you very quickly accrue a large number of treatments. And indeed, the paper that you have here from the lady who was a carrier of haemophilia, you know, people -- women who are carriers of haemophilia, sometimes they have quite a low level of Factor VIII. And she had an operation on her knee, and was given cryoprecipitate. I'd need to get the paper, but I think in the end she had about 300 units of cryoprecipitate. She developed fulminant hepatitis C. She was in intensive care and nearly died. That was from cryoprecipitate.

And that lady went on with long-term hepatitis C, and indeed in the end she and her son both had type 1 hepatitis -- of course her son had severe haemophilia -- and they both cleared it with treatment, with interferon, later on.

So the idea that you could go and treat everybody with cryoprecipitate, and it was safe and effective, because people had a transaminitis or a non-A non-B hepatitis for which we did not know the progression of the seriousness, was really not a correct idea. Furthermore, I followed your questioning of Dr Brian Colvin, and following that question, I went

back to Katharine Dormandy's original papers, and I think it was the one in 1974 which shows in great detail what was needed to train people and to make cryoprecipitate.

You're suggesting, I think, that in the late 1970s, that this concentrate is stopped being used, and we start going back to cryoprecipitate.

Well, for a start off, if people are treating at home, they haven't got deep freezers anymore. In the hospital, the Blood Transfusion Service is not geared up to making this anymore. That's just a provision thing. But the most important thing, probably -- and, you know, there are many people in this room who were in receipt of concentrates, the convenience for the patients of these concentrates was extraordinary, because it was, you know, injecting -- I don't know, 20mls into your arm, having a little bottle that you shake up, that you can take when you go in your car somewhere, or you can go abroad or you go to work, the patients wanted it. You know, it ...

And the other thing that we haven't talked about at all -- I mean, we have kind of talked about it, I discussed with you how bovine Factor VIII was developed and how in Oxford they were therefore able to take people's teeth out.

As things moved on, operations could be done for these patients. You know, people with haemophilia are no different to any of us. They can get appendicitis. They can get things that you need to do an acute operation on. And having these concentrates absolutely transformed things because you could do that now.

So I hope that explains things, and why it would not be sensible or even practical to go on back -- back -- to using cryoprecipitate. It was not a safe product.

Q. It was a safer product than factor concentrates, whether NHS or commercial, was it not?

A. Well, it depends how you define "safer".

Q. In terms of risks of transmitting hepatitis to start with.

A. In terms of transmitting hepatitis, if -- once you've got over 300 units, it's almost inevitable you're going to get it. But, you know, if you're treating a patient who has bleeding, what is the greater risk? Do you want somebody with, say, a cerebral haemorrhage to die because, you know, they've been treated with ineffective cryoprecipitate that hasn't brought the level up?

And, you know, if you're operating on somebody, it's very difficult to plan how many units of cryoprecipitate you want when you can't even know

what's in the bag. It's just -- it's an extraordinary idea. And I think you have to balance the risks. And all you -- and -- sorry, I don't want to be aggressive -- but all that is being seemingly considered at the moment is the risk of non-A non-B hepatitis, which at the time there was very limited information. We didn't have -- the virus wasn't identified until 1989 so we didn't have a test. You are suggesting that it was important to avoid that risk, and then you expose the patient with the haemophilia to a much bigger risk. Because if you use the concentrate, you know what's in the bottle, you can precisely dose it. It's just a whole safer procedure.

And I think from the quality of life perspective, it would also be an issue for the individuals with haemophilia.

Q. Do you agree or accept that patients with haemophilia or other bleeding disorders who are going to be treated potentially with concentrates should be told that non-A non-B hepatitis was a risk of their treatment and that it was a serious disease with long-term consequences? To quote Dr Kernoff.

A. I think at the time that you're talking about this, I -- fortunately, maybe, for me, I wasn't in the

position of talking people through their treatment. I've explained that what I was doing was following up information that had been acquired from that treatment.

What I do know is that patients -- when the knowledge evolved, was that patients would be told there was a possibility of this transaminitis or hepatitis, but the treater, or Peter I suppose, at that stage, would have said, "We don't know what the long-term consequence of this was."

And I think I would just go back also to the point that, for most people, it was an asymptomatic disorder. So it wasn't an issue, you know. Their priority was to be treated.

But I think it probably -- I don't know, because I wasn't Dr Peter Kernoff in that period. I was, at that time, or certainly in the 1980s when I was doing this research, I was looking at data. I wasn't the person who was administering the treatment.

Q. The question I asked, Professor Lee, and I'm not sure that you've quite answered it, is not what as a matter of fact patients were told at the Royal Free, do you accept -- you're free to disagree -- do you accept that by the late 1970s or early 1980s, patients had a right to know, should have been told, that the

treatment they were being offered carried a risk of non-A non-B hepatitis and that, whilst much was still unknown, non-A non-B hepatitis was a serious condition with long-term consequences?

A. I don't agree with that. And, you know, I'm trying to remember back 37 years, basically. You know, you've quoted the papers, and what you've quoted is certainly true. But there was also a great, great unknown. So I don't agree with the totality of what you said there.

Q. Do you disagree with my characterisation of the condition, or are you disagreeing with the suggestion that patients should have been given that information?

A. I don't disagree with patients being given information. I think all patients should be given information, and that's all -- the way certainly I've practiced. But it's very difficult giving information when the information itself is being debated, and it is not entirely clear what is happening.

Q. So this is a hypothetical question because I understand that you were not at the Royal Free at this time. But bearing in mind Dr Kernoff's own characterisation of the condition as a "serious disease with long-term consequences", is that a piece of information which, in your view, should have been

given to patients at that time?

A. Well, I go back to what I said earlier. It was the knowledge at the time. He qualified it. So I don't know. I wasn't in the position of giving the information at that time, to the patient. I don't know what he told the patient. But when he qualified that, as far as is known, I'm sure he probably would have told the patient, "This is a problem". That was his way. He told patients things. So I don't -- but I -- again, I think it is inappropriate to ask me what Dr Peter Kernoff would have told patients.

Q. That's not the question, Professor Lee.

A. What is the question?

Q. The question, I'll say it again. The question is: do you consider, as at 1979, or 1980, that patients should have been told that their treatment carried with it a risk of developing non-A non-B hepatitis which was believed to be a serious condition with long-term consequences?

A. Can I answer that question. It's very difficult for me to say what people -- what consultants delivering care at that time should have said. Maybe I can say -- I never treated one of those patients first time. Ever. Then.

Q. In 1979?

A. No. I wasn't there.
Q. Yes. I know. I'm trying to understand the point you're making, Professor Lee.
A. I didn't. I -- I didn't treat any patient there until 1987. Okay?
So I've forgotten what the question is. I'm terribly sorry.
SIR BRIAN LANGSTAFF: It's the hypothetical question.
A. Yes, well, what I think I would say is that I can't project what I would have said then, but what I can project is what I would have said when I was in the position that -- if I was in that position -- in fact, I wasn't in that position -- of giving people untreated concentrate before 1985, and I'm sure that what I would have said is that there may be a problem, but we don't know what it means.
MS RICHARDS: Is this your evidence in relation to what is a matter of fact the position was at the Royal Free, 1983, and --
A. No, it's not a matter of fact because I don't know what Dr Kernoff said to patients, and I wasn't in the position to actually give treatment then.
Q. Professor Lee, if you'll let me ask the question, you'll understand I wasn't suggesting what you think I may have been suggesting.

As I understand your evidence so far, you're saying: as a matter of fact, you don't know what was or wasn't said to patients about non-A non-B hepatitis.
A. Yes, that's right.
Q. Is that not something that you ever discussed with Dr Kernoff, given that you were researching the incidence of liver abnormalities in these patients?
A. Can I remind you again that the study I was looking at, the information, the retrospective information I was looking at, I started looking at the end of January 1983, by which time most -- all the patients had been treated. What -- there was still patients who were being followed up.
Q. So is the answer to my question that you didn't have any discussions with Dr Kernoff about the information that was provided to patients at that time?
A. No. My job was to try to find out more information.
Q. As far as you know from your time at the Royal Free in 1983 and 1984, or indeed anything that you may have learnt subsequently about that time when you took up your consultant post there, were any changes made to treatment policies or practices in response to the risk of non-A non-B hepatitis?
A. Which period are you talking about?
Q. The first half of the eighties, but in particular '83

and '84.
A. No, I wasn't -- I wasn't in the position to -- that wasn't my role and it wasn't in my knowledge. And by the time I was in the position that I was a consultant, in 1987, the concentrates were all heated. So the practices were never part of my job.
Q. If we could just look at your evidence to the Lindsay Inquiry, LIND0000326.
If we could turn, please, to page 15, so the very bottom of that page, you can see you were asked about what the practice was in 1983 and you talked there about a very strict policy about using DDAVP for those with mild disease. And then if we go over the page, you say at the top of the page:
"For those who had severe Haemophilia A, there was not any positive stopping of treatment and people went on having both National Health Service and commercial concentrate."
Now the context of that question may have been the response to the developing knowledge of AIDS, but is it right to read that as being practices in relation to the treatment of patients at the Royal Free didn't change in 1983 and '84? That's --
A. I don't know about change, what I -- this evidence that I gave to the Lindsay Inquiry 20 years ago was

cross-checked with the people who were still -- who had been working at the Royal Free at that time, and the records.
Q. So, as you previously indicated, this is the most reliable evidence you've given on those questions of fact?
A. This is very reliable evidence, what I've said here, and, you know, I would remind you again that you're asking me questions in my memory about things that happened in 1983, which is 37 years ago. And what I would say about this evidence is that it is much more reliable. I'm then the Director and Professor of Haemophilia at the Royal Free, and more importantly, I have the experience and I'm able to find or cross-check the record of what was happening. But at the time that I was doing the research, or looking at the information from the patient notes and trying to work out what had happened when these people had their first treatment with concentrate, these decisions and if there were changes, they weren't my remit. All I can do is provide the evidence that was available. And I think it's very difficult for me -- you know, Peter's dead and ceased working in 1991. It's quite difficult for me to know what rules and changes and things he made. I don't know.

MS RICHARDS: Sir, I note the time. I'm going to move on to the next topic of questioning to HIV and AIDS.

SIR BRIAN LANGSTAFF: Well, let's give the Professor the opportunity to break now until tomorrow, if you'd rather.

PROFESSOR LEE: I would prefer that. I'm exhausted.

SIR BRIAN LANGSTAFF: I thought you might say that.

PROFESSOR LEE: Thank you very much.

SIR BRIAN LANGSTAFF: It is one of those days, I think. Shall we meet together then, at ten o'clock tomorrow morning. The rules that I've mentioned to you apply overnight as they do at any break.

PROFESSOR LEE: So I can't talk to my husband tonight.

SIR BRIAN LANGSTAFF: Well, you can talk to him. I wouldn't ever want to stop that, but not about your evidence, either what you have said or what you may yet say.

(4.20 pm)

(The hearing adjourned until 10.00 am the following day)

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