Wednesday, 16 November 2011

(9.30 am)

(Proceedings delayed)

DR BRIAN McCLELLAND (continued)

Questions by MR MACKENZIE (continued)

(10.04 am)

THE CHAIRMAN: Good morning. Yes, Mr Mackenzie?

MR MACKENZIE: Thank you, sir. I apologise for the delay in starting. It has allowed me to discuss Dr McClelland's supplementary statement with him. I do apologise for keeping everyone waiting.

Dr McClelland, could we return, please, to your main statement, which is [PEN0170754], and we got to page 16, I think, 0769. We had reached question 8, where we had asked:

"The steps taken by the SNBTS, and when, to prepare for the introduction of surrogate testing, including the evaluation of any surrogate tests and the preparation of guidance on testing and counselling donors."

You explain in 8.1 the different studies undertaken and the matters considered, and you then set them out individually in 8.1:

"The clinical features associated with elevated ALT levels and positive Hepatitis B core anti-body in Scottish blood donors."

You refer to the study by Dr Gillon and colleagues published in 1988 in Vox Sanguinis, [LIT0011857]. We don't have to go to that, the study in donors testing for ALT and anti-HBc.

Over the page, a few lines down:

"It was predicted that using both the screening tests would exclude 4.4 per cent of donations and the authors concluded that the findings did not justify initiating surrogate testing until a prospective controlled trial had been done."

8.2:

"The extent to which ALT levels fluctuate when donors are tested during the course of several donor attendances."

You refer to Dr Susan Lumley studying a group of donors:

"... who were donating plasma regularly by plasmapheresis."

Just to pause and ask, doctor, why was there a group of donors donating plasma regularly by plasmapheresis?

A. This was to increase the supply of plasma for the production primarily of Factor VIII. This is a method, which I'm sure the Inquiry has already heard about, which allows an individual donor to donate substantially greater quantities of plasma than can be obtained by the

use of conventional whole blood donation. The relevance here was that these are people who attend quite frequently and, therefore, there was an opportunity to test routinely obtained blood samples at relatively frequent intervals and thus to see the temporal fluctuation of the levels of any parameter in the blood.

Q. Yes. How frequently did the donors attend approximately?

A. I can't say in relation to this particular group of donors, but possibly monthly. The limit for donation by plasma in the UK is roughly equivalent to a monthly donation.

Q. I think the study was undertaken in 1987. Where was this practice taking place of plasmapheresis? Was it just Edinburgh? Was it elsewhere in Scotland or what?

A. It was a fairly standard practice in many countries. I'm sure that -- it had been done for quite a long period primarily to collect plasma from particular donors who had high levels of antibody to, for example, tetanus or the rhesus antigen. It was used for production of immunoglobulins that had high levels of that particular antibody, and the reason for using plasma collection, plasmapheresis, in these donors was that they are small in number and their own plasma was of particular special utility.

Q. We should perhaps just go to the paper. I think the reference is [PEN0170776]. We will see this is a letter dated 30 April 1991 from Dr Gillon to Professor Cash.

Dr Gillon is digging out the results of the study Susan Lumley undertook in 1987. Could we go to the next page, please?

We can see a retrospective survey was carried out of all Edinburgh plasmapheresis donors past and present. I'm just wondering, doctor, plasmapheresis was being carried out at Edinburgh obviously in 1987. I'm just wondering what was the purpose of that? Was it for the production of immunoglobulin for particular patients or was the purpose of plasmapheresis at Edinburgh in 1987 to generally produce more plasma to send for fractionation generally?

A. Oh, I think in 1987 it probably was primarily for hyperimmunoglobulin.

Q. If we just scroll through the paper it might tell us. Perhaps just carry on going through. The next page, please. Just carry on going until we see the word "immunoglobulin". Hopefully it might appear but equally it may not.

Perhaps if we go to the end of the paper to see the conclusion.

THE CHAIRMAN: Can we stop there? On "possible aetiological

features".

MR MACKENZIE: Yes.

THE CHAIRMAN: We seem to have quite a variety of factors. I would like to go back to paragraph 8.1, when this paragraph is finished, but it's fairly clear that quite a lot of people had aetiological features for ALT variations that wouldn't have been terribly helpful in forming a conclusion about NANBH.

A. I think we may need to actually take a look at -- I have a feeling that I may have forgotten some relevant information preparing this statement. I think that Dr Gillon in the paper that you have recently referred to, the paper in Vox Sanguinis, may also have included data on platelet donors.

But just sticking with this, can you just take me back to the first page for a moment? I don't wish to mislead the Inquiry and I'm just beginning to wonder if these are patients who underwent therapeutic plasmapheresis or were they -- no, they were donors.

Sorry, plasmapheresis is also used as a treatment procedure, and just looking at that table made me think, have I misread this and confused donors with patients? But, no, these are donors.

Q. Yes. The point simply interested me, doctor, because my understanding, rightly or wrongly, was that

plasmapheresis was not generally carried out in Scotland, at least in the 1980s, with a view to collecting --

A. That's correct.

Q. I think we can leave that paper. But, sir, I think there was something --

THE CHAIRMAN: Yes, I'm slightly concerned about the general validity of findings that depend on a very small population, obviously not selected on a particular basis, almost casual, on this presentation of it, and how one can extrapolate from that to any general proposition, Dr McClelland?

A. This was a group of individuals -- let's just call them that -- who were selected on the basis that they were there donating -- undergoing this procedure and blood samples were available, and they were patients whom we had very close -- donors with whom we had very close contact. So consent and so on was not an issue. They could be informed of what was being done. It's not representative of the general population.

THE CHAIRMAN: Indeed, my immediate concern --

A. It's not claiming to be representative of the general population.

THE CHAIRMAN: That's right.

A. It's simply stating in this group of people the ALT

levels fluctuated. It's not saying anymore than that.

THE CHAIRMAN: It's almost like asking whether one could determine the driving characteristics of the general population by focusing on those who use Rolls Royces. The method of transport in that case and the method of extraction in this case are the things that distinguish people, but one would be very slow generalise on the results.

A. Well, absolutely, and this is precisely an example of the sort of confounding factor that I was referring to yesterday, when we were talking about prospective randomised controlled trials.

THE CHAIRMAN: I understand you are going back to Dr Gillon's paper.

MR MACKENZIE: Yes, sir.

THE CHAIRMAN: Because I think on paragraph 8.1 there are some very interesting question that arise. There we have got 82 per cent of donors with alternative explanations for raised ALT, which suggests that only 18 per cent of the small number of 2.4 per cent are going to give rise to data that bear upon the prevalence of NANBH. Is that right?

A. I'm not a statistician, as I will have occasion to say later on this morning again. I think in very simple response to that, I would say that there are, of course,

many factors that will produce a transient or prolonged elevation of a particular liver derived enzyme in the blood. Some of them are very common factors. So the fact that an individual has an ALT and has an explanation for it that can be divined by a clinical assessment tells you absolutely nothing about whether they may or may not have Hepatitis C in their blood.

Perhaps I should qualify that. It's probably not true to say it tells you absolutely nothing, but the correlation between having Hepatitis C and having an elevated ALT is totally not a simple one.

THE CHAIRMAN: I think that's the point. There is another putative explanation, which makes it difficult to generalise. Thank you, Mr Mackenzie.

MR MACKENZIE: Thank you, sir. Sir, I don't propose going back to Dr Gillon's paper further. We have got him coming tomorrow so we could perhaps put it to him again tomorrow.

Doctor, paragraph 8.2, sticking with the statement that:

"Dr Lumley found that donors' ALT levels fluctuated from one attendance to the next," is that true as a general proposition in relation to healthy donors who are not infected with Hepatitis C?

A. I think this is perhaps the point that Lord Penrose was

alluding to.

Q. But from other studies. This can't be the only study carried out.

A. From other studies -- and I'm not, I have to say, familiar, I have not researched all the studies that may have looked at serial measurements of ALT in healthy individuals representative of the public or representative of blood donors, there probably are very few because these are awkward things to do. But my understanding is that if one were to do such a study, one would find fluctuating -- a proportion of people who had ALT levels which would flicker in and out of the range that would be deemed to be a positive test in terms of a surrogate test --

Q. If one drinks alcohol, if I have a drink the night before or today --

A. If you were to measure the ALT levels of the Scottish male population on 2 January, you would find a very high proportion with elevated ALTs.

THE CHAIRMAN: Do they have to be able to stand up to be measured?

A. No, you can take blood samples in the horizontal position.

MR MACKENZIE: Exercise and other perhaps transitory reasons for elevated ALT --

A. Absolutely.
Q. -- so it does seem consistent with what else we know that healthy donors' levels of ALT may fluctuate and that is perhaps a factor which may create a problem in using ALT as a surrogate test for Hepatitis C?
A. It's one of many reasons why the use of this generic type of test can at best be a partial solution to the problem, because the fact that you sample an individual at 6 o'clock on a Thursday evening and their ALT level is comfortably within the normal range does not tell you that if you sampled them a week later, at 2 o'clock in the morning, their ALT level may not be elevated. In contrast to testing them for the presence of, let's say, Hepatitis B surface antigen, which if they have it on Monday, they will have it on Friday and they will have it three months down the line.
Q. Another difficulty, perhaps, just developing that a little is that for a donor who in the late 1980s did have Hepatitis C that infected donor's ALT levels may fluctuate as well?
A. Unquestionably they did.
Q. Even for infected donors, their ALT level would not always be elevated?
A. That's very well established.
Q. Yes, thank you.

THE CHAIRMAN: Dr McClelland, we have seen graphs, of course, that trace the peaks in ALT and the trough in ALT. If one looks at that and simply measures ALT at a given point, the relationship between peak, trough and the date of measurement must be purely casual unless some factor has been introduced into the definition of your group at the outset, such as after a heavy weekend or whatever.

If it is purely casual like that, aren't the ranges of variation such that generally inferences are very difficult to draw?

A. I'm not entirely sure that I understand what you mean by "the casual relationship" --

THE CHAIRMAN: Not planned. The relationship between the date on which the particular individual peaks and the date on which his ALT is measured --

A. Right.

THE CHAIRMAN: -- is not something that is predictable from generalities. It must be built in to the selection of the class.

A. I think that's probably a very complex question, but I think there are two factors that come to my mind. One is that -- assuming one had a continuous readout of the ALT level, it would be seen to fluctuate with a periodicity, which could be related to -- for example,

could show diurnal variation, as many biological variables do. It could show seasonal variation. It should show variation of the menstrual cycle or whatever. Or it could be completely random, which means we don't know what are the factors that are triggering it. Or it could be episodic relating to identifiable causes or factors like having a good drink.

The second factor is the periodicity of the sample because we almost never have, only in very specific samples would you have a continuous readout or even hourly or daily samples to work on.

So actually the practicalities of obtaining the data make it exceptionally difficult to develop a good understanding of, first of all, the pattern of variation, regular or irregular and, secondly, the association of those different levels with any other identifiable factor. And this is not just true of ALT levels, this is true of almost every biological variable that you choose, that our understanding what is the normal value is almost always, in my experience, when I have tried to scrutinise carefully and understand what a so-called normal range meant, I have become less and less confident that it was really very soundly based or could be seen to be truly representative of the population.

PROFESSOR JAMES: Could I just add for most of those biological variables, for example, ALT, this is a statistical concept, so a normal range for ALT is plus or minus two standard deviations from the level which is found in a population, which is thought by the lab or by the inventors of the test to be the most reliable "normal" where they have said nobody has got a cold on that day, nobody has had a heavy drink on that day et cetera, et cetera, but "normal", nonetheless, is a statistical concept, plus or minus two standard deviations. So people do accept that "normal people", a few normal people, might "normally" have an ALT, for the sake of this argument, that is marginally above the "upper limit of normal".

A. Absolutely. This is a big topic.

PROFESSOR JAMES: It's very, very boring but many, many people have spent many happy hours trying to define these things.

THE CHAIRMAN: Yes, I quite enjoy boring things, Dr McClelland. But I don't think I want to pursue this too far. It is quite clear that there is a generally accepted standard based on quite a changing but ever-increasing population, whose data feeds into it. That's all right.

My concern in asking the question was not about the

base data from which one would measure variation, but the chances of finding on the day, as it were, one carries out a more limited test consistency of data that could reliably be used as a measure perhaps. And "casual" simply means there is not a finite defined relationship or set of circumstances; it depends on all sorts of factors, many of which you have listed, that would undermine the exercise. I think that's enough boring material from me.

MR MACKENZIE: Thank you, sir. Could I move on, please, doctor, to answer 8.3, the question of:

"Evaluations of a system designed for testing large numbers of samples. Laboratory testing of ALT levels and the establishment of reference ranges for the Scottish blood donor population. Age and sex distribution of ALT levels in the donor population."

You refer, doctor, to:

"An evaluation of a commercial analyser (an Eppendorf EPOS) was conducted by the SNBTS West of Scotland and reported in 1987."

The reference, without going to it, is [PEN0170841].

Is that essentially, doctor, the evaluation of test equipment with which to undertake surrogate testing?

A. It was. That was the primary purpose, but it also, because there was quite a substantial sample of donors

covering a span -- both sexes and a span of age, it also was quite a substantial source of, as it were, baseline data, addressing the questions we have just been discussing, on the actual observed ALT levels in the population of donors. So it had a second utility, which was to allow a more confident prediction, I think, than we could have made before as to the probable loss of donations from testing, and also the point that Professor James raised with me yesterday, the number of donors who actually would be identified and would require counselling and care related to the observation of the positive result.

Q. Yes. We can see that samples from 5,000 donors were taken, and you say:

"Because ALT level is a continuous variable, the definition of a positive result must be based on a judgment essentially arbitrary as to how an individual's test result relates to the results from the representative population and for any practical large-scale application such as blood donor screening, a threshold value must be set, above which a sample is considered to be positive and the West of Scotland study showed that if the threshold level was, for example, set as the population mean plus 2.25 standard deviations, giving an ALT value of 55, then about 2.3 per cent of

donations would be considered positive and would require to be discarded."

Over the page, please. You tell us:

"The West of Scotland study mentioned above also analysed the effect of age and gender on ALT levels, providing data that indicated that the threshold ALT levels may well require to be adjusted to be age group specific for males and for females."

I think that's because, in short, men were found to have higher ALT levels than women. Is that correct?

A. That's a consistent observation and also there is a trend -- you know, an age-related difference, which actually from the point of view of a donor screening test was making this whole thing begin to look really quite complicated. It's not just a yes or a no, you need a sort of statistical algorithm to decide what is -- if you are going to relate this to the biology of the population, you actually need to select a threshold level of ALT which is appropriate for that person's age and gender, touching again on the points that Lord Penrose was referring to.

Q. What is the age relation to ALT --

A. I can't remember whether it goes up or down with older age. It probably goes up.

Q. With older age?

A. I can't remember.
Q. I think we do know that ALT levels are higher in males than females. Why is that?
A. Many things are different between males and females. Whether, if you did this study now with the changing pattern of drinking in females, you might well find a rather different result.
THE CHAIRMAN: Drink is a factor, isn't it?
A. Yes, of course.
THE CHAIRMAN: It did occur to me that there ought to be a higher level tolerated in Scotland than in some other parts of the world simply because of the reputation Scots have, as males get older for drinking excessively. So, you know, you can almost imagine the patients saying to the doctor, "No, no, doctor, it's not disease, it's the drink that has contributed to my condition".
A. Absolutely.
MR MACKENZIE: Thank you. Then again:
"In 1988 the SNBTS undertook a multi-centre evaluation of the same equipment for ALT determination and concluded that results were consistent between the centres, taking a threshold value of the population mean plus 2SD would lead to about 5 per cent of donors being excluded, whereas a slightly higher threshold of mean plus 2.5SD would exclude about 1.5 per cent of donors."

So to pause, there, if surrogate testing had been given the green light, then SNBTS would have been in a position, at least in terms of identifying suitable equipment, to introduce such testing?

A. Yes. I mean, this was an important assessment because something we haven't really touched on is the technology, the methodology, for measuring these enzymes is actually quite tricky. I think we saw a document yesterday from an evaluation carried out in three centres in England, which expressed concerns that in one of the three centres the ALT values were systematically different from in the other two, and that was why the approach that we would have had to have planned to adopt, if we were going to start testing, was to use the same equipment throughout, control it carefully and be confident that a positive result in Inverness, the same sample would also be a positive result in Glasgow or Edinburgh.

Q. Yes, thank you. Then in your statement you say:

"I have no recollection of being involved in or being aware of work on the preparation of guidance on testing and counselling donors. However, I'm sure that there was concern about how we would manage donors rejected on the basis of a surrogate test, since we suspected that in most cases the test would not indicate

the presence of infective non-A non-B Hepatitis."

I think you indicated yesterday really that little or no thought had been given to that stage of counselling donors and what to tell them and what to do with them in terms of recommended treatment. Is that essentially correct?

A. It's probably not quite fair to say little or no thought had been given, but what Professor James asked me was when we really sort of bottomed this out, and we certainly hadn't. We hadn't got to the stage whether we should have been doing this at this stage or not is an arguable point. We knew that we would have something of the order of 4,000, probably about 4,000 individuals who would fall into the category of having had a donation deferred for an elevated ALT level, and we were aware that that was going to be a very significant burden of work. But we certainly had not sort of prepared a systematic sort of management plan and costed out the stuff involved, looked at the implications for the other hospital departments and GPs and all that stuff we had not done.

Q. Hypothetically speaking, if you had been given the green light to introduce surrogate testing, so the service did then have to bottom that out and start drafting guidelines and protocols for counselling and recommended

treatment for donors, do you think that is a matter which could have been properly addressed or would that have been an insurmountable obstacle?

A. I'm absolutely confident it could and would have been addressed and that, you know, there were a lot of strategies that could have been adopted to allow testing to begin while some of that work was being done. It wouldn't have all had to be fully in place before one started testing.

At the simplest level you could commence testing setting the cut-off level somewhat higher, which actually probably in retrospect, with what we know now, would have been exactly the right thing to do, because the higher ALT level was probably more strongly -- well, we now know it was almost certainly more strongly predictive of Hepatitis C being present and that would, as you can see from these figures, have dramatically reduced the number of donors. So there are many ways this could have been handled.

So I think my answer to your question is, if there would have been a decision that testing would start, we would have clearly needed a few months, probably, to get all the equipment and everything in place and operating and staff trained. We would certainly have had to take some measures in terms of training donor staff,

preparing information for them and so on, but we had quite bit of experience of sort of working through that sort of thing some years previously with the introduction of HIV testing. I think we would have found our way through that fairly effectively.

Q. You mentioned the setting of ALT levels. Am I right in thinking that the higher the ALT level one chooses, the specificity of the test is increased? One is more likely to find a true positive, whereas the sensitivity is decreased?

A. The sensitivity is certainly decreased. I'm on fragile ground here because I haven't reviewed this, but I think that there are later data, which does indicate -- I think it's probably evident in the Canadian study by Blajchman and colleagues, which I mentioned later on, that the higher ALT levels were more strongly predictive of Hepatitis C being present. I would have to go back and look at the paper.

Q. So we would have to be cautious with that. So why did you say that you would perhaps have started with a higher ALT level?

A. I simply threw that out as one of the strategies that we could have adopted, because we would have reduced the number of donors and donations that had to be managed in the first six months while we were getting our feet

under the table with this new technique. I'm not saying that we would have done that but there was that and other things that we could have done.

Q. So it could have been set at a higher level initially for practical reasons?

A. Yes.

Q. I see. Thank you.

Then question 9, please, moving on. We then ask:

"Estimates made at the time of the likely cost of introducing surrogate testing in Scotland."

I'll come later with Professor Cash to look at the bids for funding, but you do say in your written response that:

" ... providing a reliable cost estimate of a surrogate testing programme would have been a difficult exercise. While the cost of equipment, reagents and personnel would have been relatively straightforward to determine, the costs that could be created in a blood donor programme would have been more difficult to predict. In addition to the costs associated with obtaining perhaps 5 per cent more donations to replace those discarded because of surrogate test results, there would have been the costs of care and management for a large number of donors who would find themselves deemed unacceptable to donate."

We are back, then, to the 4,000 approximately rejected donors.

A. Yes.

Q. Then over the page, please, page 19, question 10, we asked:

"Why surrogate testing of blood donors for NANBH was not introduced in Scotland."

You explain:

"I think there are many connected reasons."

You attempt to summarise them. Firstly:

"There was a persisting belief among most SNBTS (and NBTS) transfusion professionals that NANB hepatitis was a much less common consequence of transfusion than it appeared to be in the USA, and that it was generally not a particularly serious condition. I have dealt with this more fully above."

In terms of when these beliefs were held, doctor, what sort of time period do you have in mind?

A. Well, I was really relating this statement, which is an expression of opinion, I have to say, to the period, let us say, between 1980 and 1988, when Hepatitis C testing began to emerge. I do think that that coloured quite a number of the decisions or perhaps non-decisions.

Q. Thank you. Paragraph 10.2 you say:

"Medical advisers in the SHHD appeared to have

shared this view."

Do you say that just from reading the documents produced as part of the Inquiry or is that a perception you held at the time?

A. That statement is based essentially on reading the documents because I honestly can't remember to what extent I had any understanding of the views held in the department at that time.

Q. Thank you. 10.3, you say:

"This belief undoubtedly prevented serious consideration being given to undertaking a robust prospective clinical assessment of the effects of surrogate testing at a time when it should in my opinion have been undertaken."

When you say "at a time when it should have been undertaken," what time period do you refer to there?

A. I think we have covered this pretty fully yesterday but I think that was the very early 1980s because, as we said, it would have taken probably three years with a fair wind to get complete -- or have preliminary data from a study of adequate size and power. If it had been started much later than 1984 or 1985, its results would have converged with the emergence of Hepatitis C testing which, as the Blajchman paper shows very clearly, makes surrogate testing irrelevant.

Q. Even as at July 1987, the time of the letter to The Lancet "Surrogate testing irrational perhaps but inescapable", even at that time, so before Chiron had announced discovery of the Hepatitis C genome in March 1988-ish -- even in July 1987 at that time I think you thought it was too late to start a prospective study.

A. Yes, as I've said, I think any time probably after 1985, it would not have impact -- it would not have actually provided any gain in patient safety, unless there was some fairly spectacular preliminary results earlier than one would have planned or expected that would have motivated a decision to introduce surrogate testing. The point is, would any patients have been spared getting hepatitis? That's my judgment.

Q. I suppose that's looking back at things now with the benefit of hindsight, given we know the Hepatitis C test became available roughly in 1989, looking back one can say, well, it would have been pointless to start a prospective study after a certain date. But I'm just really trying to clarify your thinking at the time.

In the 1980s it appears there came a time where you thought, well, we should simply introduce testing rather than start a prospective study, and certainly by the time of the letter in July 1987 that appears to have

been your view.

A. Yes, I mean, my thinking about this fell into two epochs with, as we discussed yesterday, a gap in the middle when we were all fully exercised with AIDS and non-A non-B Hepatitis. From my perspective, it rather fell off the agenda.

I think by the time we came back to it, which was towards the end of 1986, by that time I think my feeling was that we actually just needed to get on with it. Obviously I had no knowledge at all at that time of whether or when some more definitive test procedure would be available. I had no inside track about what was going on in Chiron.

Q. Just to pick up on that, was the AIDS experience a factor in coming to the view that by late 1986/1987 surrogate testing for NANBH should be introduced, or was the AIDS experience and surrogate testing for NANBH two completely separate matters?

A. I don't know to what extent I consciously would have related the two at that time. I can't remember. But there is absolutely no doubt that the sort of learning through the AIDS experience and the realisation that something could be there in our donor population for years before we even realised that there was a problem, you know, the whole of that, I think, was a very

powerful factor, in my own thinking, that we would have to be more proactive in being able to do things. And in the case of non-A non-B Hepatitis, it was arguably more pressing because we knew there was something there, we had known for quite a long time that something bad was happening.

Q. Then in paragraph 10.4 you say:

"SNBTS and NBTS medical professionals were unconvinced that surrogate testing would offer material safety gains and were concerned that it would lead to the loss of donors and donations and difficult problems in the subsequent care and management of donors rejected on the basis of a surrogate test result.

10.5:

"Requests to the SHHD for funding to undertake surrogate testing were repeatedly turned down by the SHHD."

I'll go over that with other witnesses:

10.6:

"The 1988 multi-centre study of surrogate markers in blood donors was in my opinion essentially an irrelevance, yet it appears to have distracted a great deal of effort that could have been better directed to a dispassionate re-evaluation of information that was already available and that strongly challenged the

belief that non-A non-B Hepatitis was a non-serious condition that was rarely transmitted by transfusion.

"10.7. Perhaps most importantly SNBTS was not supported by SHHD in its expressed desire to adopt what Justice Krever would go on to describe as the 'precautionary principle' by introducing surrogate testing for non-A non-B Hepatitis."

Over the page, please, doctor, we asked:

"If surrogate testing for NANBH had been introduced in Scotland, the extent to which the incidence of post-transfusion NANBH/hepatitis C is likely to have been reduced."

You do go on to develop your answer, doctor, in a supplementary statement, which we will come to shortly, but just, firstly, if I may finish your first and main statement, you explain in paragraph 11.1:

"A number of studies provide some suggestions as to the possible impact that surrogate testing might have made to the risk of transmission of hepatitis by transfusion."

You deal first with the risk for recipients of blood components, and then for recipients of coagulation factors:

"For patients transfused with blood components."

You refer to a Canadian paper, which we should

perhaps go to, it's [LIT0013223].

We can see this is a paper published in 1995 by part of the Canadian post-transfusion hepatitis prevention study group. I think if one takes things chronologically, if we start in the right-hand column under "Introduction" we see:

"A prospective study of post-transfusion hepatitis in Canada in 1984-85 showed an overall post-transfusion hepatitis frequency of 92 per 1,000 allogeneic blood recipients, with a post-transfusion frequency of Hepatitis C of 31 per 1,000 recipients. Since 1985 many measures were introduced by blood collection agencies worldwide to try to improve the safety of the blood supply. These included the introduction of screening for HIV ... and direct questioning of blood donors about relevant medical information and lifestyle."

Reference in 1986 to the USA agencies introducing surrogate screening.

And then at the start of the next paragraph -- or rather the end of the last paragraph the decision in America was made:

"... without the benefit of data from prospective intervention studies showing efficacy ..."

Of surrogate testing.

Then:

"Because of the lack of such evidence, the Canadian Red Cross Society and some blood transfusion services in western Europe did not screen blood donors for NANB surrogate markers. We thought a randomised double-blind trial was needed in Canada to assess the frequency of post-transfusion hepatitis and to see whether the withholding of donor blood positive for the NANB surrogate markers would reduce the frequency of post-transfusion hepatitis.

"While our study was in progress, the genome of HCV was elucidated. Testing blood donors for antibodies to HCV was introduced in Canada in May 1990. Subjects were involved in our study before and after the introduction of HCV testing."

That's by way of introduction.

If we go to the left-hand column, please, we will see a summary of the results of this study which is being reported.

In the second paragraph down, in the left-hand column, we see:

"From 1988 to 1992 4,588 subjects were enrolled into two study groups that received allogeneic blood from which units positive for NANB surrogate markers were either withheld or not withheld. We also assessed a simultaneous non-randomised cohort (650) of subjects

who received only syngeneic."

What's the pronunciation?

A. Syngeneic, it's their own blood. "Autologous" is another word.

Q. I see:

"All subjects were followed up for six months and assessed for the presence of post-transfusion Hepatitis due to Hepatitis A, B, C, non-A/B/C, Epstein-Barr virus and cytomegalovirus. Withholding of blood containing NANB surrogate positive units reduced the overall post-transfusion hepatitis rate by 40 per cent and the Hepatitis C rate by 70 per cent. Most of the benefit of NANB surrogate testing was due to reduced frequency of Hepatitis C virus after transfusion before all donor blood was screened for anti-HCV."

The bottom left-hand column:

"Our study indicates that screening of blood donors with the NANB surrogate markers was of value in reducing HCV infection before HCV screening began but subsequently the value of screening cannot be clearly established."

It's not an entirely easy paper, I don't think, doctor.

If we can then, please, go to the discussion at page 24, which is 3226, the second last page. We see

the bottom of the right-hand column "Discussion". In the second paragraph:

"During our study ..."

There is some repetition here:

"... withholding of NANB surrogate marker positive units reduced the overall post-transfusion hepatitis rate by 40 per cent ... the introduction of HCV screening ..."

The second line from the bottom:

"Nonetheless our data suggest that NANB surrogate testing in Canada before May 1990 would have reduced the frequency of NANB hepatitis, especially that caused by HCV."

The next paragraph:

"The drop in the HCV hepatitis rate from 31.3 per 1,000 to 12.6 per 1,000 between 1984-85 and 1988-90 appears to have been associated with improved methods for the screening of blood donors, since the drop occurred without NANB surrogate markers. In the USA a similar reduction in HCV hepatitis was reported over the same period in association with the introduction of NANB surrogate marker testing."

That's the paper, doctor.

What points do you take from it, and feel free to do that with reference to your written answer or simply to

do it orally?

A. I'm not quite sure how you want to do this because this will come up when you move on to my second statement. It might be more economical of the time if we did it in a oner. This is a complicated paper and the more I look at it, the more I have realised there are some issues in interpreting the data.

Q. I think we will perhaps leave it. That's our first taster of it. We will leave it and put it to one side and come back to it when we look at your supplementary statement. I'm grateful. That may be the better way to do it. Thank you.

Just reverting to your main statement at page 20, I think we have covered most of what you say in page 20.

Over the page, please, paragraph 11.5. We come back to Scotland and the Crawford and others paper published in 1994.

You say, paragraph 11.5:

"During the first six months of donor screening for Hepatitis C antibody in Scotland, 181,000 donors were tested and 0.088 per cent were confirmed to have Hepatitis C antibody. Among the Hepatitis C-positive donors, 59 per cent had ALT levels above the upper limit of normal. Although this study did not determine ALT levels in donors who were Hepatitis C negative, the

findings suggest that the use of ALT screening would have allowed the detection of a substantial proportion of HCV-positive units."

I don't think we have to go to the paper. We have looked at it before but I'll give the number. It's [PEN0020582].

We are going to come on shortly, doctor, to your supplementary statement, but are you still of the view that the findings of the Crawford paper suggest that the use of ALT screening would have allowed the detection of a substantial proportion of HCV-positive units?

A. I think one has to take that in conjunction with the Canadian paper really, and I was aware obviously of the Canadian paper when I wrote this. In the absence of that one sort of genuinely prospective study, accepting its limitations, I think one would be somewhat less confident in making that prediction. However, that is precisely the type of data on which the American authorities made the decision to start surrogate testing, if you like.

Q. Okay, I think we will come on to develop your view on this a little when we come to your supplementary statement. Maybe I could just finish your main statement in the time we have left before 11 o'clock.

In paragraph 11.6 you look at patients treated with

plasma-derived coagulation factor products, and you say:

"It is generally accepted that surrogate testing would have offered little or more likely no safety benefit to patients treated with these products. This is a consequence of the large number of donations included in each manufacturing batch of product and the introduction of heat treatment."

You refer to a SNBTS document, the number is [PEN0130220]. We don't have to go to it.

Doctor, we have heard discussion of the question of viral load. Would surrogate testing have offered any benefit to pooled plasma-derived products by resulting in a reduced viral load?

A. I'm really not competent to answer that question. I don't know.

Q. Okay. Then in question 12, finally in this session, we asked:

"If surrogate testing for NANBH had been introduced in Scotland, the percentage of donations that are likely to have been rejected and the extent to which, if at all, that is likely to have caused difficulties in maintaining a sufficient blood supply..."

In respect of ALT testing you say:

"If the level of ALT that had been set as the threshold for a 'positive' result was the population

mean plus 2.5 SD (about 45 IU), the loss of donors would have been of the order of 2.5 per cent. If anti-HBc had been used in addition, losses would, according to Dr Gillon's study, have been about 4.5 per cent."

Finally you say:

"It is worth noting that a German report ..."

I will give the reference but not go to it, it's [PEN0170869]:

"... describes much higher ALT threshold levels of 134 IU for males and 89 IU for females. Using these higher threshold levels, only 0.25 per cent of the donors exceeded the threshold. Information is being sought about the ALT thresholds in use for donor screening elsewhere in Germany."

Have you had any success, doctor, in obtaining any further useful information from Germany?

A. Yes, the short exchange of emails I gave you yesterday was as far as I got with this. What I did ascertain from the medical doctor who is in charge of donor testing in the Frankfurt Red Cross centre, in North Rhine-Wesphalia, which is the biggest German centre, was that these ALT levels were indeed applied across the German blood services. From the start of ALT testing, which I believe to have been round about 1990s, although he did not give me that information, I think it will

appear somewhere in evidence available to the Inquiry, the ALT testing was terminated across Germany in 2006.

I did seek further information, first of all, about the levels of deferral, because this is only a brief reference to 0.25 of donors being deferred, and also about any evidence that they might have comparable to the Canadian study to look at the association of ALT levels with the presence or absence of Hepatitis C, because they continued ALT testing for quite a number of years after Hepatitis C testing was introduced.

So the data exists in Germany, but after a very encouraging initial response to my first questions, subsequent attempts to get the supplementary information met with a resounding silence. But what is interesting, and it relates to our earlier brief conversation, is that the higher ALT levels you can see clearly here a much, much smaller proportion of donors that were excluded.

So one could say that the German in a sense voted with their feet, or on the basis of the evidence which I don't know, to choose these high levels, either because they believed those would be more predictive, they would be more, as it were, specific for infectious units, or because they were being pragmatic and not wanting to get too big a problem with donor deferral as

a result of ALT testing.

Q. Yes. I think you mentioned that had ALT testing had been introduced in Germany some time in the 1990s. Is that correct?

A. Yes, I have on a feeling -- I glanced through the other statements around this block and I think in Professor Leikola's statement, he actually gives information about the time -- the starting of testing.

Q. I had understood, and I may be wrong, that at least some parts of Germany were ALT testing since the 1960s.

A. That's entirely possible because the system is actually quite heterogeneous in Germany, particularly in earlier years there were university-based -- university hospital based blood collection centres, Red Cross centres. So I think it's only relatively recently that there has been a much more sort of standardised regulatory regime for the transfusion services.

Q. I see. So when you talk of ALT testing in Germany in the 1990s, is that a reference to across all of Germany?

A. That may just be wrong actually. I don't know. I vaguely recall seeing a document in the last few days which gave a date, and I think it might have been one of Professor Leikola's papers.

Q. So in short we should perhaps look to other sources?

A. Please. I can't answer that at the moment.

Q. Thank you, sir. That may be an appropriate point to break.

THE CHAIRMAN: Yes, thank you.

(11.02 am)

(Short break)

(11.26 am)

MR MACKENZIE: Thank you, sir. Dr McClelland, I would like to turn now, please, to a supplementary statement you produced for us. It's [PEN0172651]. I should perhaps explain that we initially sent out a set of routine questions for our various witnesses and then, having considered the responses we sent out a set of supplementary questions to try and focus on particular points.

Question 1 we asked:

"Should a large-scale prospective study, as originally proposed by Dr McClelland in 1981 (ie along the lines of the US ... studies ... including the follow-up of recipients), have been carried out in the UK in the early 1980s (or at some point thereafter) with the following aims:

"(a) to assess the prevalence of post-transfusion NANBH in the UK.

"(b) to evaluate surrogate markers for the disease.

"(c) to investigate the natural progression and

seriousness of the disease.

"(d) to produce a library of 'known' infected sera with which to evaluate any future assays which became available?"

Your reply at 1 you say you have not changed your view in the years since this was originally proposed such a study.

You still believe that:

"... such a study should have been carried out in the UK. A true prospective study was carried out in Canada recruiting patients between 1988 and January 1992."

You explain:

"This study probably provides the best available evidence on which a judgment of the value of surrogate testing might be (or have been) made."

We will come back to that paper.

Question 2 we asked:

"If such a study had been carried out to what extent is it likely to have met the objectives set out in 1 above? To what extent would such a study have provided more information on which to base a decision on whether surrogate testing should be introduced?"

I think really the second part of that question, what we were seeking to ask, was whether such a study

could have led to more informed decision-making.

In your reply 2 you say:

"The outcomes of a study of this nature would have depended entirely on the quality of the design and research protocol ..."

Et cetera:

"These in turn would have been in large part a function of the resources both intellectual and financial -- that were devoted to the study and of the extent to which government and influential figures in the health service communicated the importance of the study to participants ..."

You go on to say:

"I think it is clear from the documents held by the Inquiry that the proposals that I submitted in the early '80s were at most outlines -- intended to illustrate the kind of study that was required. A successful study would have required the engagement of people with the knowledge and skills to design an effective study with adequate statistical power, cost it, obtain funding and carry it to completion."

Just to pause there, doctor, would a study of that type have been required to have been carried out at a UK level rather than a purely Scottish level?

A. It certainly would have needed to be a multi-centre

study, just because of the size of enrollment that would be required, I think, to achieve a study of adequate power and statistical power. As I think we already said yesterday, it would have been an expensive, difficult and long study to do. It could not have been accomplished by one or two individuals based in one regional transfusion centre with small financial inputs.

Q. I think you explained yesterday that such a study would really have required support at the highest level, at government level.

A. Yes, both to fund it and as I tried to imply in this statement -- I mean, this is wisdom, this is knowledge that I have now that I did not have in 1981 but, you know, I have in the latter part of my career in various capacities been involved in a number of large clinical studies and learned to understand just how much resource is needed. I did not have that understanding at the time that I put these proposals forward.

Q. I see. With the understanding you have now about the complexities of designing and effectively implementing such studies, with the benefit of hindsight, do you think it would have been practical to carry out such a study in the early 80s?

A. Well, I think, there is kind of two answers to that. I think if -- and that's why I included quite a long

paragraph about this here. I think if the study had been done to a high standard, it could have, as I have said to you, produced useful answers in terms of three of the four objectives but probably would not have been informative about the long-term health effects of Hepatitis C infection, simply because that requires a very long follow-up of a large population and would have been very difficult to do. However, I think it's quite possible that if there had been a moderate degree of interest in the study, a study would have been done that was too small and underpowered and might not have yielded conclusive results.

Q. I understand.

And question 3, doctor, we refer to the conclusions of the work of Drs Dow and Follett, and we refer to certain documents in footnote 1 on page 2 of your statement. I'll go on to look at some of these documents with Dr Dow next week.

But we asked:

"Did the conclusions of Drs Dow and Follett place sufficient emphasis on the likely prevalence and seriousness of post-transfusion NANBH? In particular, as well as having regard to reported cases of the disease, did the work of Drs Dow and Follett have sufficient regard to the fact that most cases of NANBH

were subclinical and were unlikely to be detected without prospective follow-up (by biochemical testing) of recipients?"

You say in your reply:

"I cannot recall the extent to which I was aware of these findings before Dr Dow's May 1986 report to the SNBTS directors. However, I am confident that I would have realised then that the studies were not designed in a way that could determine the prevalence of clinically silent post-transfusion hepatitis or obtain a reliable epidemiological picture of the severity of the condition."

Obviously, doctor, you would have seen Dr Dow's May 1986 report to the directors. Is that correct?

A. Yes.

Q. Do you remember seeing that report at the time?

A. I don't have any recollection now, but I think I was present at a meeting at which it was presented.

Q. We refer to three documents in the footnote, a final report of 1984, a thesis of 1985 and the special report of May 1986. Have you looked at these reports recently?

A. I don't claim to have read them all in great detail but I'm fairly familiar with the principal findings.

Q. Thank you. Over the page, please on to question 4 we asked:

"In the second half of the 1980s, did SHHD medical officers place sufficient weight on the likely prevalence and seriousness of post-transfusion NANBH?"

In footnote 2 on this page we refer to particular documents:

"To what extent did their views in that regard influence their opinion on whether surrogate testing of blood donors should be introduced?"

You reply that:

"In responding to this question I would like to refer to my previous witness statement."

In that you stated your:

"... personal opinion that professional staff in the transfusion services did not fully appreciate the scale and importance of NANBH before the advent of the HCV test."

When you refer to 'professional staff in the transfusion services", is that in both England and Scotland?

A. I think that applies to both, yes.

Q. A general comment.

You have also described your:

"... views as to why the problem may have been under-recognised. Medical officers in the SHHD would have had no reason to be expert in hepatitis and

I imagine that they would have depended on information from those considered to be experts. It seems clear from a number of documents included in the detailed chronology ..."

That's the chronology compiled and sent by the Inquiry to yourself and other witnesses:

"... that officials in SHHD and some of the professional advisers felt that the Dow and Follett work provided evidence that NANBH following transfusion was not a serious issue in Scotland at the time. Advice from other sources in the UK may also have tended to underestimate the prevalence and seriousness of NANBH."

What did you mean by "other sources"? Anything in particular?

A. Well, there was a fairly small group of experts, virologists mainly, who were members of all the relevant committees and some of whom were quite frequently party to decisions or non-decisions around the introduction of testing. So I think advice and opinion was coming from, if you like, a professional community defined by having an interest in this particular topic.

Q. You then say:

"I have not seen documents that suggest that importance was attached to obtaining information from the USA, Canada or elsewhere that may have challenged

the reassuring received view from the UK."

When you say "information from the USA, Canada or elsewhere", can you give an indication of the sort of information that you mean?

A. Well, I think the Inquiry has already seen a huge amount of information that had been built up, for example, from the TTV study and similar activities, pointing to the importance of non-A non-B Hepatitis in terms of both how common and how serious. I'm merely trying to respond to question 4, perhaps slightly overpolitely, and I say I think it's entirely reasonable that the rather small cadre of medical staff in the Scottish Home and Health Department at that time couldn't be expected to be experts in hepatitis.

It does seem, you know, looking with the wisdom of the retrospectoscope that they were guided very much by one single piece of work, which was the Dow and Follett research, and didn't show -- there wasn't much to see in the documentation that they had actually seriously tried to take a more independent look at the literature and the information that was available. That's all I was trying to say.

Q. I see.

THE CHAIRMAN: It's a difficult area, this, because I suppose it's not just the availability of information

but one's approach to it and the understanding of it that would instruct a view on how serious NANBH was at any one time. How do you resolve this? It's not an easy equation to define.

A. It's not, and that's why I'm not intending to be overly-critical here. I think you just have to be prepared (a) to -- as with anything like this, to take a look at what has been written and to look at the literature. It wasn't a difficult thing to do, even in 1986, shall we say, before the internet was available and so on. It was quite easy to go to the library and look at a few current journals, and at that time there was masses of stuff being written and published about this, and then pick up the telephone and ask a few other people what they thought about it. It's not rocket science really.

I think this is the way one tends to form a judgment about a complicated technical issue that is not bang centre in one's own field of expertise. I'm not sure whether that's answering your point or not, sir.

THE CHAIRMAN: I think at some stage I'm going to have to take a view about what was realistic and what might realistically have been expected of those who had administrative and advisory roles round about this period, and I suspect that there will be many factors

that enter into that. I'm not sure actually that it's all that easy to say you can just wander along to the library and pick up the relevant material. I'm not sure that the library would necessarily have been arranged in such a way at this time to enable one to pick up the material. Nowadays I wouldn't expect to see many of the publications on the shelves, it would all be computer terminals.

A. I would say so. In some ways it was possibly easier in the early to mid-80s because you could go to a library and it had journals on racks and you could go and pick up one called "T for transfusion", or "N for New England Journal of Medicine". Now you have to grapple with the knowledge network or Ovid or something.

THE CHAIRMAN: I don't think I can take it very far at the moment. I can't get you to take the decisions for me.

MR MACKENZIE: Thank you.

Dr McClelland, in question 5a we asked:

"If surrogate testing of blood donors, (ie testing for elevated ALT and/or anti-HBc) had been introduced in Scotland what percentage of donors are likely to have been deferred."

You reply:

"This would have depended entirely on the rules adopted for the performance and interpretation of both

ALT and Hepatitis B core antibody ... tests. Perhaps the best data on ALT for Scotland is the report on the evaluation of ALT testing ..."

You give a reference. We don't have to go to it. It is [SNB00024423]. This was a report by Drs Robertson and Cuthbertson, evaluating the Eppendorf EPOS system we referred to earlier:

"This reported a threshold ALT level of 2.5SD above the mean value would lead to a loss of 1.5 per cent of donations and at a lower cut of 2SD above the mean the loss to be about 5 per cent. Gillon et al in their 1988 Vox Sanguinis article [SNB00083536] reported that 2.4 per cent of 1,742 donors had ALT levels above 45 units and anti-HBc was detected in 2 per cent. There was no overlap between donors with raised ALT and those with anti-HBc.

"A reasonable estimate would be that the combined application of ALT testing at the 2.5SD level and anti-HBc testing would have led to the loss of 3-4 per cent of donations in the mid-1980s. These numbers may have underestimated the longer-term effect on donor attendances because later research has shown -- perhaps not surprisingly -- that donors who are rejected on one occasion are unlikely to return to volunteer again and this tends to have a cumulative effect that is

not measured by the initial rate of deferral."

We then asked:

"Could a sufficient blood supply have been maintained?"

Your view was that for the Southeast Scotland region at least a sufficient blood supply could have been maintained to meet clinical requirements.

I think I'm right, doctor, that at least at some points in the 1980s, your region were transferring red cells to London to help them?

A. Yes.

Q. It's against the background perhaps, am I right in thinking, that a lot of plasma was required to produce blood products but perhaps less components were required for routine transfusion purposes? It's a very inelegant question but ...?

A. So long as one is depending or was depending primarily on the collection of whole blood and not depending on the plasmapheresis procedure that we were discussing this morning, then with the rising requirements for Factor VIII, if you collected enough bags of whole blood to meet the targets that we had been set for plasma, then we had too much red cells.

Q. Yes.

A. Well, we had more red cells that were needed for

sensible transfusion of the patients in the population that our region served.

Q. Yes. The next question is more difficult and it's a longer answer. We then asked:

"To what extent are cases of post-transfusion Hepatitis C likely to have been prevented (having regard, for example, to the finding that in the first six months of HCV screening the prevalence of HCV and Scottish blood donors was 0.088 per cent and that elevated ALT levels were found in 59 per cent of HCV-positive donors)?"

That's, of course, a reference to the Crawford paper of 1994, [PEN0020582].

Page 5 you begin your answer. You say:

"My response to this relates to patients who were transfused with blood components."

You then in the next paragraph say:

"The question breaks into two main parts: (a) how many individuals were infected with Hepatitis C as a result of transfusion of a blood component and (b) what proportion of Hepatitis C transmissions could be avoided by the use of surrogate testing with ALT and anti-HBc."

So you then look at the first part of that question:

"What was potentially preventible -- ie how many

patients were being infected with Hepatitis C by transfusion each year before HCV testing began? In the UK we have no direct knowledge of the number of transfusion recipients who became infected with HCV in any year before the start of HCV testing.

"We do know that when routine HCV testing began in September 1991 a positive HCV test was found in about one in 1,000 (0.09 per cent) of attending blood donors. This figure reflects the true prevalence of HCV in SNBTS donors in 1991-2 and is, to my knowledge, the only reliable prevalence data that we have. For any earlier years, an estimate of the number of HCV-positive donors would have to be made, in turn necessitating estimates of the factors that are believed to influence prevalence."

One factor is information from Health Protection Scotland:

"Because of the increasing incidence of injecting drug misuse, the prevalence of HCV in the Scottish population is believed to have risen substantially over the period 1970-1991 and that this is believed to have accounted for an increasing prevalence of Hepatitis C infection in the Scottish population."

Just pausing to look at other factors which may play a part in trying to estimate the likely HCV prevalence

in blood donors prior to September 1991, we know that in roughly 1983 there were the beginning of steps to try and exclude donors at a higher risk of transmitting HIV and presumably those steps became increasingly effective or stronger as time went on. Does that seem reasonable?

A. They may well have had a cumulative effect, as it were, within the community. I mean, certainly, as the Inquiry has already seen, there were progressive modifications and refinements and some extensions of the donor exclusion criteria in relation to HIV. Unfortunately, of course, we don't have any direct evidence of the effect that that had on either the prevalence of Hepatitis C in the donations that were collected or on the rate of non-A non-B Hepatitis in recipients. But I have later on referred to a letter written to the New England Journal by Professor Blajchman and his colleagues comparing information from the United States and Canada over a similar period, and his interpretation of the data are that in the United States there was a substantial fall in the rate of non-A non-B Hepatitis in recipients, which was attributed to the introduction of surrogate testing. But over a comparable period in Canada there was a comparable reduction in the rate of non-A non-B Hepatitis in the recipients, in the absence of surrogate testing. Those were -- is attributed to

the effect of the AIDS-related donor selection measures.

That's about the best data I could find to address the question, but I can't map that directly on to what happened in Scotland or the rest of the UK.

Q. No.

THE CHAIRMAN: Can I just ask for some clarification about this paragraph that we have just ended on? You say that the increased incidence of injecting drug misuse is related to increasing prevalence of HCV. I think I can understand that. The drug users are part of the population and so you increase one element, you increase the overall position. But does this read through to the donor population?

A. I have to be very clear that this statement, starting from "My understanding" to the end of that paragraph, is really based on discussions that Dr Gillon and I had with Professor David Goldberg and his colleagues in the course of preparing a document, which has been separately submitted to the Inquiry at your request, sir. And two points: first of all, they have a publication which I haven't cited because I felt it was more appropriate to Professor Goldberg's evidence, based on statistical modelling and it is on the basis of that they have made the statement that the number of injecting drug users has increased sharply from the

early 1980s.

Secondly, if I understand correctly Professor Goldberg's thesis, the main driver of the prevalence of Hepatitis C infection in the community in Scotland is injecting drug misuse.

THE CHAIRMAN: I think I understand that, but I think you will be aware of Professor Simmonds' analysis of the phylogenetic trees related to the transmission of HIV in Scotland.

A. Yes.

THE CHAIRMAN: And as I recollect it, the drug abusing population did not contribute to the infection of haemophilia patients and indeed only had one single original source. I'm speaking from memory and not from having the article in front of me. But if that were so and they were not contributing to the transmission of HIV, that would only be because they were not part of the blood donor population contributing to the sources of blood products, would it not?

A. Well, I would obviously very much like to think that the drug injecting community were not part of the donor population. And in earlier evidence to the Inquiry I did make the point that although it was only in 1983 or 1984 that we formally introduced an exclusion for drug users, in fact the practice in the Southeast of

Scotland centre -- and I am sure it was the case in other transfusion centres -- had been based on a recognition of evidence of drug injection was a disqualification, it was just less formal.

THE CHAIRMAN: Really it's only this last sentence or two that worries me, because as presented it might give rise to the inference that one could read through to a relationship between drug abuse and the spread of Hepatitis C among blood donors -- sorry, in blood donations and that worries me just a little on the whole information I have, including your earlier evidence about the extent to which these people had been excluded.

A. I entirely accept that, sir, and, yes, I don't wish to add anything to that.

PROFESSOR JAMES: Can I just pursue this a fraction? The evidence from the States and Canada that you have alluded to suggests that improved screening of donors did actually very significantly reduce the incidence of post-transfusion non-A non-B Hep C in roughly the decade between 1981-1982 and 1991, when HCV screening came in. While I accept what the chairman says, as a matter of fact from my understanding, your screening of donors and indeed in the UK as a whole but particularly here in Edinburgh and in Scotland really did get tighter in

a progressive fashion over that period of time. It wasn't just one step, and it was perfect sort of thing.

So would you like to hazard an estimate of whether a similar sort of -- I mean, for the figures for 1991 that, you know, we have got the famous 0.0088 per cent (sic) from Dr Gillon's original survey, the implication might be that actually the prevalence among donors in the early 1980s might have been two or three times as great as that, or do you think that's just too speculative or a reasonable inference to draw?

A. I'm not sure I have understood you. Are you asking whether I think it's possible that the prevalence ten years -- say 1980 would have been substantially higher than it was in 1991.

PROFESSOR JAMES: Yes, in the general population and in particular in the donor population.

A. I think it would be pure speculation. I made the statement here that I think the only modestly reliable prevalence data we have is the 1991 figure. I did write, you know, before the Inquiry hearings started, when we were just beginning preparation -- I did spend some time with Peter Simmonds to try and get his take on what one could or could not say about essentially two questions. One was when Hepatitis C might have appeared in the community in Scotland. And, secondly, what one

might deduce from the sort of phylogenetic evidence about how the population of Hepatitis C carriers might have increased and as a result of what factors. And I have to say that the only conclusion that I was able to draw from that discussion and reading what are actually for me quite difficult scientific papers was that one could be reasonably confident that Hepatitis C has been present in the community for a long time.

As to quantitating it or producing any confident assertion as to what may have influenced its prevalence, I wasn't very much the wiser having had that discussion.

PROFESSOR JAMES: Quite a lot of people are not much the wiser after technical discussions with Professor Simmonds.

THE CHAIRMAN: That's not a reflection of Professor Simmonds.

PROFESSOR JAMES: No, no. Speaking for myself, it's my own inadequacies over a number of years.

THE CHAIRMAN: Having looked at his phylogenetic trees as best I could over quite a considerable period of time, I'm not sure that I understood more than the few sentences in which he actually indicated his conclusions. So it's not a criticism but it is difficult.

PROFESSOR JAMES: In summary, you can't make any estimation

as an analogy with the sort of estimations that they were making in Canada and the United States?

A. Unfortunately we can't because we don't have any of the data because we didn't do the studies.

PROFESSOR JAMES: Yes. I agree with you that that's a matter of regret.

THE CHAIRMAN: Let's wait and see. Yes, Mr Mackenzie.

MR MACKENZIE: Thank you, sir.

I think one point of clarification for the record, I think Professor James referred to the incidence of HCV in donors as being 0.0088 per cent and, of course, it's 0.088 per cent. I should clarify that.

Just the point in short perhaps, doctor, that looking at that paragraph on page 5 we have just discussed that one certainly can't exclude increasing injecting drug misuse as a possible factor in an increased prevalence of Hep C in the Scottish donor population?

A. I think that's undoubtedly true.

Q. So it may be a factor. What weight we place on it is perhaps very difficult to say?

A. Yes.

THE CHAIRMAN: I suppose we do have to bear in mind that it's not just the drug abuser who may be the source of a donation that transmits, it can further down the line.

A. Absolutely, and this is part of Professor Goldberg's hypothesis, I think.

MR MACKENZIE: Thank you.

Then at the bottom of the page, doctor, you say:

"However, Ebeling and Leikola (1991) cite a number of studies that show that the overall incidence of post-transfusion hepatitis has declined in the 1980s ... this is partly due to changes in transfusion practice towards fewer units per patient and also to a reduced infection risk per unit.'."

I should perhaps pause and explain, sir, that Dr McClelland has a number of additional references at page 10 of his supplementary statement, which we will find reference numbers and put into courtbook in due course. We hope to do that on Friday to the extent that we can find them.

To return to the top of page 6:

"This trend is demonstrated by the rates of PTHC in Canada that were observed in two studies in 1984-5 and in 1988-92 (HCV antibody was measured using archived samples for years before HCV testing began in May 1990). Feinman et al (1988) reported that the rate of PTHC in Toronto was 31.3/1,000 blood recipients in patients recruited to the study in the period 1984-5. Blajchman et al (1995) in a multi-centre study in Canada found

a PTHC rate of 12.6/1,000 in the recipients of blood in the absence of surrogate testing."

Is table 1 a reference to Blajchman's table 1 or your table 1?

A. That's a reference to the table 1 in the Blajchman publication. I apologise for the confusion.

Q. Not at all.

Then:

"Donohue et al (1992) reported a falling rate of PTHC in the USA (from 38/1,000 to 4.5/per 1,000) and attributed this to the effect of surrogate testing. However, this conclusion was challenged by Blajchman et al (1993) who suggested that the observed fall was due to changes in donor selection related to AIDS, since Canada had seen a similar decline but had not introduced surrogate testing."

I think really, doctor, all of that discussion perhaps indicates the difficulties in trying to come to any firm views about this period and whether if surrogate testing had been introduced the extent to which things may have been affected?

A. I think absolutely, and I think it's extremely important to be aware that there is this evidence that actually prevalence -- sorry, the effect on patients might have been very considerable without the introduction of

surrogate testing. I have gone into some detail on that because of the question that was asked. I think it has to be answered with that background.

Q. Thank you.

Then you say:

"How many donations with HCV could have been detected by the use of ALT testing and HBcAb testing?"

"The study that is probably most informative is that of Blajchman in Canada."

I think we can then take the rest of that as read because I think we have looked at this study now.

I think over the page you reproduce a table from Blajchman --

A. May I just clarify, this is not a reproduction of the table, this is my table 1, and I have extracted what I thought was relevant data from a much more complicated table in the Blajchman paper. And I have to say also I think I may have made a typo because I can't quite square the arithmetic in the 0.0 Hepatitis C rate. There may be a typo there, for which I apologise. I need to cross-check this for the record with the table in the full paper to --

Q. I'm not sure you have made an error, doctor. If we go to the paper, please, it's [LIT0013223].

A. Thank you.

Q. It's at page 3225 we see the table at the top of the page.
A. That's the one.
Q. If we go to the second column from the right -- second entry, we do see 0.0. Read across to the left, we will see other figures.
A. Yes, I can explain this. It's the distinction between the overall post-transfusion hepatitis events and those which were specifically Hepatitis C related.
Q. Right. So --
A. So what this table is saying is in the withhold group, which means the group of patients who received blood that had had an ALT and core test done and all units which were positive for ALT or core had been removed, ie patients who received, let's say, ALT and core negative blood, the rate of Hepatitis C transmission was zero with confidence in intervals of 0.7 [sic] -- 0 to 7.4 per 1,000.
Q. I'm not sure, doctor, I understand everything in the table, but I think it would take quite a lot of time to go through it in detail, but what in short do you take from the table, doctor? What's the point you seek to tell us from the table?
A. I think the important -- there are a couple of points, and this is why I tried to condense this into the

smaller table in my answer. There are two epochs in this study. There is the period before Hepatitis C screening was introduced, and then there is a period after hepatitis screening was introduced, during which the researchers continued to apply the protocol for their trial, ie to randomise patients to receive blood that had been ALT tested and positive units withheld and blood that had not been influenced by the effect of ALT or core testing.

So they started a randomised study to compare tested and untested blood, and then about a sixth of the way through the recruitment to that study Hepatitis C testing came in, but they continued with the protocol and, as it were, superimposed the Hepatitis C testing on that.

This is where my statistical skills become woefully inadequate, but I felt it was only safe to look at the data for the period before Hepatitis C testing had been started, and the number of patients there is relatively small. However, there were nicely matched numbers and, as far as I can tell, quite well-matched groups of recipients in this period. So 397 patients received blood that was not subject to the effect of surrogate testing. 402 received blood that was subject to the effect of surrogate testing. There were eight events as

defined by elevations of liver enzymes in the recipients in the no test group, and only two events in the test group.

The important figures, though, in relation to the question, which is specifically about Hepatitis C, is in the penultimate column on the right, which is that the rate of Hepatitis C in the recipients of the untested blood was 12.6 per 1,000 with a wide range of 4 to 29. Whereas in the 400 recipients of untested blood there were no transmissions of Hepatitis C.

I'm not at all confident to comment on the statistical power of that observation because the number of patients in that group are quite small, and I'm not really, certainly at the moment, prepared to comment on the significance of any results that were found in the period after Hepatitis C screening had started because I haven't got my head around that.

But what you can say is that the -- what this data appears to show is that once you have started Hepatitis C screening, then the surrogate testing had no statistically detectable effect on the rate of post-transfusion Hepatitis C. Whereas, before you had Hepatitis C testing, surrogate testing has an effect, an apparent effect, on the rate of Hepatitis C in the recipients. However, I always thought that if you see

the 95 per cent confidence intervals overlapping, as they do here, the statistical confidence in the finding was not that high, and I feel that is reflected in the discussion or the final conclusions of the paper, which says:

"Our results suggest that ... surrogate testing would have reduced the rate of Hepatitis C in the patients."

I would stress that this is not a simple paper and the more I looked at it, the more I felt less confident in the conclusions I can draw from it. And I would hope that if the Inquiry feels it is important, they would seek the input of someone with greater skills in this -- more competent than me to evaluate it.

Q. In particular, in the question of statistics, it is a statistician, I think, is the area that you are talking about?

A. Yes.

THE CHAIRMAN: Can I just say that I think there was an error in your answer and that the 400 you refer to are the 400 where there was testing. You use the two groups as untested -- in your answer here, you won't see it on the screen.

A. Okay.

THE CHAIRMAN: But the distinction in the first two lines is

between those where no testing was applied and those where testing was applied. Is that right?

A. It's entirely possible that I have --

PROFESSOR JAMES: It's the ambiguity there in the word "testing". What you meant by "testing" was that they were screened for ALT and for antibody.

A. Screened and withheld.

PROFESSOR JAMES: Correct, yes.

THE CHAIRMAN: It's just in the answers recorded. I simply want to make sure that you are not recorded as saying something that didn't work.

A. Thank you.

MR MACKENZIE: Thank you, sir, I think that's correct.

Then returning to your statement, doctor, you then look to apply that to Scotland, if we believe the conclusions of the Canadian authors are correct.

Sir, there are a few pages still to go and it gets quite complicated again. I wonder if I may seek a very short break of five minutes, if that wouldn't be too inconvenient.

THE CHAIRMAN: No, a recovery period is quite in order.

(12.14 pm)

(Short break)

(12.22 pm)

MR MACKENZIE: Thank you, sir.

We reached page 7, Dr McClelland, and you set out there that in the months of September 1991 to February 1992, following the commencement of HCV screening in Scotland, 0.088 per cent of 159 donations were designated positive and 95 per cent of the donors of these units returned for further information and tests:

"More than half (59 per cent) of the donors in whom HCV antibodies were detected had elevated ALT levels, suggesting indirectly that as many as half of HCV-positive donors might be directed and excluded by detection of a specified elevated level of ALT. If the findings of the Canadian study were simply applied to the Scottish donor HCV prevalence of 0.088 per cent, then up to 70 per cent of the HCV-positive units would have been removed. For the estimate below I have used the assumption that surrogate testing would have allowed 50 per cent of HCV-positive units to be withdrawn."

Do you think that's a reasonable assumption, or is it one one should be extremely cautious about, or what?

A. I think one should be very cautious about all of these numbers. Primarily because, as I said just before the break, there are wide confidence intervals around these numbers. So I mean, the figures from the Canadian study, if you applied the confidence intervals, it could

be 0 to 100 per cent, rather than 70 per cent. That's why you need a statistician.

THE CHAIRMAN: The 50 per cent is just a working hypothesis?

A. It's to allow me to do what I think is an illustrative calculation. It's nothing more than that.

MR MACKENZIE: Thank you.

You say:

"To gain an idea of the impact of this partial removal of infective units in terms of the numbers of infections in transfusion recipients, I have used data from a SNBTS account for blood database. Since AFB is a recent development ..."

Approximately, doctor, when was that brought in?

A. Well, this has been in evolution for about ten years, but it's only actually for the years 2010/11 that the thing has matured to the point where we can be confident that we actually know the number of patients. We know accurately the number of patients who actually received a transfusion of one or more blood components.

As with the 1 in 1,000 figure for prevalence, which I feel is solid, this is the only number that I feel is, in terms of the number of recipients, solid and we clearly have to consider then, if one is trying to look at other years, when the true figure for other years might be.

Q. Okay. You then say:

"Table 2 lists number of assumptions that have been made to provide an illustrative example. Errors in these assumptions may lead to over or underestimates of the number of infections."

So table 2 "Blood components transfused to patients in Scotland":

"Data from account for blood 2010-11.

"Number of blood component units."

Does that include or exclude plasma products such as albumin?

A. That's blood components as we define them, ie excluding any fractionated plasma product. It's red cells, platelets, plasma and cryoprecipitate. And any one of those products counts as one in these data.

Q. Yes.

A. And the basis of that -- the logic behind that is that we assume that the even if -- you know, the probability of any component of the blood containing Hepatitis C is the same as the probability of the parent donation containing it.

Q. Each of these type of components you have mentioned would be capable of transmitting Hepatitis C?

A. Yes. Equally -- I think we would say equally capable of transmitting.

Q. You then look at -- so number of blood components, all types transfused. So we are not looking at number of donations or units collected, we are looking at the number of blood component units actually transfused?

A. Yes. If I could just explain, the Blood Transfusion Service obviously has data about the number of units that are placed into stock, that are shipped to hospitals, but it is dependent on the hospital blood banks for information about what is transfused to patients and what isn't. So this part of the -- the reason it has taken so long to build this database is it involves setting up systems which each of the hospital blood banks in Scotland, with one small exception, which is not material, daily or twice daily send an automated report to the central data warehouse, which is based on units of blood -- of each component that are confirmed to have been transfused.

Q. Thank you.

A. The data that we did not have accurately or reliably before.

Q. We see the number of blood component units transfused as 207,439.

We then see the number of patients who received one or more blood component units as 36,875.

If one were to have asked that question as in the

late 1980s, how many patients received one or more blood component units? I appreciate there isn't data available, but do you have a feel for an approximate number?

A. This is a number that has been obviously very important for a long time, and the estimate that I have tended to use over that period, up to about the early 2000s, I tend to work with an estimate of about 50,000 recipients, based on piecing together various types of information that we had.

In 2005 -- and I think this is in a document which is probably in the Inquiry's papers -- I produced an estimate for the Crown Office, Procurator Fiscal Services, and at that time I used a figure of 40,000 recipients, which was based on slightly more information, because by that time we had done the first two pilot iterative projects that led to the account of blood database. So we were a bit more confident of the figure then and it actually came down.

THE CHAIRMAN: Dr McClelland, it has got off screen. Could you just remind me what component units comprised, red cells, platelets?

A. Yes, the terminology basically from one whole blood unit one can produce red cells, platelets, plasma, or cryoprecipitate, and the convention we have used here is

that any one of those would be a component unit.

THE CHAIRMAN: I understand that for your first line. You then have the number of patients who received one or more blood component units.

A. Yes.

THE CHAIRMAN: Is that the same definition?

A. Yes.

THE CHAIRMAN: You have got a problem for me.

A. Just to be clear, if I was the patient and I received one bag of plasma, the plasma obtained from one blood donation, I would count that as one unit in this table.

THE CHAIRMAN: Yes, but in reality, is that the way life operates or are the components, as you have defined them, not processed in many cases before they get back to the patient?

A. They are always processed. The unit of -- the bag --

THE CHAIRMAN: But think of the cryoprecipitate.

A. Right.

THE CHAIRMAN: What happens to the cryoprecipitate in number terms to get the number of patients who receive one or more components of cryoprecipitate?

A. Well --

THE CHAIRMAN: I find that difficult to imagine.

A. Cryoprecipitate is usually supplied for the patient in that sort of standard dose of six donation units. The

cryoprecipitate of six donations will either be -- I think the current practice now is actually that that is mixed into one bag before it's supplied to the patient. In earlier years they were supplied as separate bags. It's immaterial for the purpose of this table, this would be counted as six because it contains some of the blood from six separate blood donations.

THE CHAIRMAN: Here we are dealing only with components that actually get into patients as such?

A. Yes, we are excluding from these numbers components that might have -- outdated in the hospital blood bank or been damaged or discarded or something. We are not counting those at all.

THE CHAIRMAN: Right. I think I understand that so far. I think we will look over the page in due course, no doubt, to your table, to see how the spread comes.

A. Yes.

MR MACKENZIE: Thank you, sir.

Doctor, 207,439 blood component units are transfused. Number of patients who received one or more blood component units, 36,875. In terms of looking at the average number of blood component units received by each patient, do we simply divide the 207,439 by 36,875?

A. That's correct.

Q. We can see, I think, your handwritten calculations

suggesting a figure of about five?

A. It's about five.

Q. Then in the next line down:

"Possible outcome of surrogate testing for NANBH, assuming 50 per cent reduction of transmission of HCV."

Looking at the number exposed with no surrogate testing, 36,875 -- we can see where that comes from -- times 0.00088, which is the prevalence of HCV upon the start of donor screening in Scotland in September 1991, results in a figure of 32?

A. May I just interject for clarity?

Q. Yes.

A. I think I should have -- that heading should have been assuming 50 per cent reduction of transmission, but also assuming a recipient of one blood component, I think for clarity, if you think of this as being the risk calculation for a patient who received a single blood component, be it a red cell or platelet, or a (inaudible) or plasma, because we can then dissect out the effect of multiple components.

Q. Yes.

THE CHAIRMAN: So this is the risk per unit, if one can use that rather crude way of looking at it?

A. Yes, what we are doing is taking the risk per unit and applying it to the risk per patient, and to make it

simple, assuming the patient only gets one unit.
THE CHAIRMAN: But if one looks at the reality, we'll come to the effect of your table --
A. We will come to the effect of multiple units.
MR MACKENZIE: So the calculation, 36,875 times 0.00088 is the risk per unit. Presumably the more units one receives, the higher the risk of a particular patient receiving HCV?
A. I think we should be very careful about terminology. The risk per unit is 0.088. It's one in 1,000 essentially. This calculation here tells you something different; it's the risk -- it's the product of that risk per unit and the number of patients who actually get transfused and, therefore, it gives you an estimate of the number of patients who actually got infected, who actually received a Hepatitis C-positive unit.
Q. Yes. And in simple terms, the more units one receives, the more likely one will receive an infected unit?
A. Yes. My understanding -- and I did consult about this but not with the most authoritative people because I couldn't find them in the time available -- my understanding of this is that the risk of a patient receiving a positive unit is essentially additive; it is the sum of the -- it is -- the risk is additive. So if you get one unit, the risk of getting a positive unit is

one in 1,000. If you get two units, the risk of getting a positive unit is two in 1,000. If you get ten units, the risk of a getting a positive unit is 10 in 1,000. That's what intuition would tell you. But intuition and statistics don't always go too well together. But I did check that out and I believe that to be correct.

THE CHAIRMAN: I think it's consistent with evidence we've had before --

A. I'm relieved to hear it, sir.

THE CHAIRMAN: -- about the progressive risk being additive.

A. I would stress, because we will come back to this in a minute, that is the risk of a patient receiving a Hepatitis C-positive unit.

PROFESSOR JAMES: I think these terms here in this table 2 are -- you have explained them but they are actually as they stand quite misleading. I have just done some quick sums, and as a matter of fact the total number exposed is -- you multiply 32.4 by 5.6 approximately and that comes out to around about 340 individuals exposed no surrogate testing.

A. That, sir, is why I suggested that I think we needed to amend the heading here, to -- I was trying to separate out the risk of exposure due to a single unit. And I think you are right, I think I have missed a step in the logic here. I have jumped a step in the logic here.

PROFESSOR JAMES: For the records here, it is rather important that we don't go away with the impression that, for example, the number exposed with surrogate testing using the assumptions you have made is 16, as a matter of fact the number exposed with surrogate testing, making the assumptions that you have, is around about 170 actually.

A. Yes. It may be a little bit more complicated than that but that's probably closer to the mark. I'm not sure that the average is the right multiplier to use here, as you will see when you look at the table.

PROFESSOR JAMES: You have just said that this is additive. So if you get five units for the sake of this argument, you are five times more likely.

A. Correct.

PROFESSOR JAMES: So the calculations that I have just made, which I'm not suggesting are more than plus or minus 2 or 3 -- the calculations I have made are on that assumption. There is nothing more complicated in those calculations?

A. Absolutely. What you have done is take the average and assuming that the average is the correct -- five units per patient approximately is the right number to take, it may not be the right number to take.

PROFESSOR JAMES: Okay.

A. But, yes, in principle I agree.
PROFESSOR JAMES: That's the best we can do, though, isn't it?
A. Yes. Yes. Well, no, it's not the best we can do --
PROFESSOR JAMES: Oh good.
A. -- if we carry on, we can do better.
PROFESSOR JAMES: Thank you.
MR MACKENZIE: Thank you.
So, doctor, I think Professor James was putting to you that in terms of the number of patients exposed without surrogate testing, the total number exposed, one would make a calculation something along the lines of 32.45 times 56, I think it was.
A. I think --
Q. Times five --
A. -- for clarity in --
Q. 5.6 I think it was, yes.
A. For clarity in the evidence, I think it might be safer to actually split this up, as I suggested, and to say this -- which will require this table to be modified, but to be quite clear that this calculation is based on the assumption that each patient only gets a single unit, which would allow us then to go, as it were, over the page and say: but what about the real numbers of units that patients get? It's simply that if we

conflate two parts of the calculation, it might actually be very difficult to interpret later on. That's my suggestion.

Q. We will come over the page shortly, doctor. What I'm particularly interested in is the figure, even if it's simply an approximate figure, for the total number of patients exposed, and I think you did agree that Professor James' approach of 32.45 multiplied by approximately --

A. The average number of units. It's a perfectly reasonable starting point.

Q. Yes. Reverting to your table, we can then, I think, understand the number exposed with surrogate testing on the 50 per cent hypothesis. We can see how you reach a figure of 16 on the assumption --

A. It's simply halving it.

Q. On the assumption a patient received one unit. And equally we can understand Professor James' calculation, looking at the total numbers of patients, it would be 50 per cent of about 340.

A. I'm less confident in the second one because to calculate the effect of the surrogate testing in the recipient of multiple units, bearing in mind the partial effect, I'm not certain whether that calculation is straightforward or not. I'm sorry, I'm out of my depth

for this.

Q. Me too.

PROFESSOR JAMES: Me too.

MR MACKENZIE: I'm sure this won't be the final word on the question of statistics.

We then see the assumptions you have made in carrying out that working example. We can simply read them all for ourselves, I think.

Over the page we come to an interesting table at page 9, the effect of the amounts of blood received by an individual patient.

You say:

"A proportion of patients receive very large numbers of blood component units. For these individuals, the risk may be materially increased, and the impact of testing may have been greater."

We can see the table you have produced is again from the account for blood in 2010 to 2011.

A. Correct.

Q. And --

A. That's a direct printout from the database.

Q. Looking at each column, we can see the left-hand column "Units per patient per year". So, again, we are looking units of blood actually received by patients. And then we can see the number of patients transfused. And,

finally, the total number of units transfused, which is essentially, I think, a multiplication of the figures in columns 1 and 2.

A. It is the multiplication.

Q. One can see, for example, 12,603 patients received two units, and one can see the spread. One can see between 2,199 patients received 11 to 20 units. I think we can just let the figures perhaps speak for themselves.

Is there anything else you wanted to draw attention to from the table, doctor?

A. Not at the moment.

THE CHAIRMAN: I find it quite difficult that there are no figures at the bottom end of the table for number of units transfused. The number down to ten comes somewhere under 100,000, which would mean 107,000 or thereby for the remaining sections, and averaging it out down to 50 you get another 65/66,000 or thereby. It rather suggests that an awful lot of units were transfused into the 293 who got over 50.

A. That is correct. I mean, there is -- it's one of these sort of --

THE CHAIRMAN: The sort of exponential --

A. It's one of these 20/80 situations where a small proportion of patients get a very large proportion of the blood components and that actually, when you think

about it clinically, is kind of what you would expect but it is -- this is why I was guarded about the use of the average.

THE CHAIRMAN: That's what I was --

A. It's highly skewed population -- distribution, I should say.

THE CHAIRMAN: A purely arithmetical average is not terribly reliable here.

A. We can produce these data for the other columns. It was just going to make the table very long and unwieldy. The purpose of this was just to sort of offer the Inquiry an approach to the question, which is a difficult question.

MR MACKENZIE: And by the 20/80 rule, you mean that just very simply and unscientifically about 20 per cent of patients receive about 80 per cent of the blood.

A. Yes, it's a fairly well recognised distribution.

Q. Yes. Under "Conclusion" you say:

"I am very much aware of the risks of making a simplistic attempt in the absence of sufficient data, to estimate the possible effect of something that was not done 10, 20 or 30 years ago. I suppose the essence of question 5c is how one would interpret the evidence if I or one of my family was the patient likely to require a transfusion. This is a test that I have often

resorted to over the years in trying to make a judgment on difficult questions like this one. Using that test, I have little doubt that if I needed a transfusion today in a situation where there was no Hepatitis C tested blood available, then I would, on the basis of the evidence that we have, prefer to receive blood that was negative in one or both of the surrogate tests for NANBH than to receive blood that was positive in one or both of the tests."

You asked that question, doctor, if you needed a transfusion today in a situation where no Hepatitis C tested blood was available, but how about if you needed a transfusion in 1987 and with the knowledge of non-A non-B Hepatitis at that time, would you still have preferred to have received blood that had been screened negative for surrogate markers?

A. I think unquestionably, yes. I didn't ask myself the question at that time, I don't think, but I think the answer would have been the same.

Q. Yes. I suppose the additional question, doctor, you say you would have preferred to have received blood that was negative than to receive blood that was positive. How about a choice between blood that had been screened and was negative for the surrogate tests and blood which was unscreened?

A. Yes.
Q. The same answer to the question?
A. It's a variant of the same question.
Q. Yes, thank you. We are not done with the statistics yet, doctor. Over the page, please. We can see this is an extract from a paper prepared by the SNBTS in response to questions from the Crown Office and Procurator Fiscal Service in 2005. Who was the author of this response?
A. I wrote this.
Q. Thank you. We can see that the Deputy Crown Agent in a letter of 21 June 2005 raised a number of questions.

One question, 3, was this:

"An estimate of the prevalence of the virus in donated blood in the UK until such times as a screening test was successfully introduced in 1991 ... information regarding the process of selection of donors to minimise any such risk."

You then in your response said:

"Prevalence of HCV in donated blood before the start of HCV testing."

There is a reference to the first four months of Hep C testing. We have seen that before. The prevalence of 0.09 per cent.

Then you give three estimates. Estimate 1 is this:

"Patients exposed to HCV by transfusion."

You say:

"From work currently being carried out and still subject to verification, we have estimates based on data for 2002 that currently covers 77 per cent of the blood supplied to Scottish hospitals."

Is this essentially the start of the account for blood exercise?

A. Yes, this was the early, first or second, step in that, yes.

Q. "... and this shows that blood components were transfused to about 31,000 patients. On this basis, for the whole of Scotland in that year about 4,000 patients would have received a blood component transfusion. If we assume first that the figures were similar in 1990 and second that very few patients would receive more than a single unit of HCV-positive blood component, then the number of patients exposed by blood component transfusion in one year can be estimated as 0.09 per cent of 40,000, ie about 40 individuals."

That, again, was dependent upon that important assumption that very few patients would receive more than a single unit --

A. I think in retrospect -- I may just say by way of explanation, this was -- I hadn't actually intended to

include this with the statement. It was because of the glitch that I handed you this yesterday, and it was my own copy that I had stuck this on the back.

I included this merely because of the question that was asked earlier on about how would we estimate the transfused population, the number of recipients, to just -- because this was the only other documented thing I could find of an earlier estimate of the transfusion population, which at that time was 40,000. So it has found its way into the evidence, but it was actually unintentional.

I had intended to make reference to that, as I have done in my oral statement. I'm not sure that the rest of the other estimates are particularly relevant to the -- that's entirely up to you, sir.

Q. Estimate 1, is that essentially similar to the working example --

A. It is.

Q. -- we have just looked at?

A. And it's subject to Professor James' comment.

Q. Just one final point as regards estimate 1, the final sentence:

"The study mentioned below suggests that around 50 to 60 per cent of these would have become infected with Hepatitis C."

Why isn't that about 100 per cent? Why is it 50 to 60 per cent?

A. That was the -- gosh, I think that is based on the very extensive work done by Dr Kate Soldan, which has been published a number of years, which -- and I honestly can't remember what -- where that figure -- how that figure emerges from her work.

Q. Is your position -- I have to say that I hadn't realised until now that these two pages were included in error essentially. Is your position we should be a little cautious in relying upon these estimates?

A. Yes. I'm not sure that they're terrifically helpful to the Inquiry, they are rather old, and the only reason I had initially thought of including it was because of -- anticipating the question of how would we have estimated the population of transfused patients in earlier years, and this was the only previous -- the only earlier estimate that I was able to lay hands on. I just would be concerned that the other paragraphs may actually be non-contributory and waste rather a lot of time.

I'm sure the Inquiry has already heard extensively about the Soldan work.

THE CHAIRMAN: We have heard a lot about Soldan in the past. I think perhaps the best way to approach it is that you

are not relying on the information in these two sheets --

A. Absolutely not.

THE CHAIRMAN: -- to support any proposition at this stage.

A. No, absolutely not.

THE CHAIRMAN: If you are not supporting it, I don't think we need to be overconcerned about it.

A. Thank you, sir.

MR MACKENZIE: I am grateful, sir.

THE CHAIRMAN: There is no need to analyse the degree to which they might be reliable.

A. Thank you. I did just want to -- reverting to the previous page, if we could for one moment, to the last page -- yes, this one.

I had been deliberately vague in the statement at the top of the page, and I referred to the impact of testing because what we are actually concerned about is the number of patients who get Hepatitis C and the severity of their disease, and while we have clarified with Professor James' help the -- a view of the risk of a patient receiving a Hepatitis C-positive unit, the relationship between receiving more than one Hepatitis C-positive unit and the risk of contracting Hepatitis C and the severity of the subsequent disease is not simple, and it would be for the Inquiry to

decide, you know, to what extent it wishes to explore that.

MR MACKENZIE: I have no further questions, thank you.

THE CHAIRMAN: I think we will leave it just now, Dr McClelland. I think that already we have heard quite a lot about factors that could complicate the situation.

Mr Di Rollo, what is your intention?

MR DI ROLLO: We do have some questions actually. Mr Dawson is going to deal with them. I don't know whether you would wish to start now or after lunch.

THE CHAIRMAN: I don't imagine Mr Dawson is going to finish in five minutes.

MR DAWSON: I don't think so, sir.

THE CHAIRMAN: Perhaps we should just start anyway and see how you get on.

Questions by MR DAWSON

MR DAWSON: Hello, Dr McClelland. If I could just ask you some questions, first of all, about a passage in your first statement that you were asked some questions about this morning but in a bit more detail. This is the passage at paragraph 11.6, which can be found on page 21 of [PEN0170754].

This is the section in which you were asked to give your views as to the likely impact upon different kinds of patients of surrogate testing, in particular in this

paragraph you are talking about:

"Patients treated with plasma derived coagulation factor products."

You say:

"It is generally accepted that surrogate testing would offer little or more likely no safety benefit to patients treated with these products. This was a consequence of the large number of donations included in each manufacturing batch of product and the introduction of heat treatment."

And you make a reference to a further SNBTS document.

Just to tease that out a little bit more. I assume you are talking about concentrate treatment there. Is that right?

A. Plasma derived coagulation factor products.

Q. Concentrates.

A. Concentrates. Yes.

Q. Am I right in thinking that your reference, first of all, to heat treatment would mean after heat treatment came in in 1987 it afforded such a protection to haemophiliacs that surrogate testing would have been of no additional benefit?

A. I think once it became clear that non-A non-B Hepatitis was effectively not occurring, though it was a product

that did not transmit, then surrogate testing would probably have been of little relevance.

Q. If one were to look at the period before heat treatment against non-A non-B were effective, because we have looked at that period during this evidence as well, would I be correct in understanding your position that due to the large number of donations that would go into each pool, your position would broadly be the same, ie that surrogate testing with its obvious limitations would not have offered any benefit in reality to the those receiving concentrate?

A. Broadly, yes. I think -- again, there may be some statistics involved in this because it might depend a lot on pool size, and I suppose it is conceivable -- it's conceivable that with -- let us assume the prevalence of Hepatitis C was one in 1,000 and you found half of them, and the pool size was 1,000 -- it's probably bigger actually, I can't remember the pool sizes at the moment. So it's -- on a probabilistic basis there might have been an occasional batch that would have been protected from containing Hepatitis C. I'm really not sure that I'm competent to answer that. Again probably not a simple calculation. But I don't think -- what I think one can say with confidence is that it would not have afforded any reliable degree of

protection to recipients of, say, Factor VIII concentrate.

THE CHAIRMAN: We will stop at that, Mr Dawson. I think it is necessary.

(1.01 pm)

(The short adjournment)

(1.45 pm)

MR DAWSON: Thank you, sir.

Dr McClelland, I was just asking you before lunch a couple of questions about the possibility that surrogate testing would have had an impact upon safety for those being treated with concentrates.

I would like to ask you whether you think that surrogate tasting would have had any such advantages for haemophiliacs being treated with cryoprecipitates over the relevant period, on the basis that those patients, of course, wouldn't have had the advantages of heat treatment that we have discussed?

A. I think that the same -- broadly the same arithmetical arguments apply. Severe haemophilia patients receive -- requiring a lot of treatment would receive an awful lot of donations worth of cryo. I think -- you know, it all depends on one's estimate of the effectiveness of surrogate testing on, as it were, interdicting Hepatitis C-positive units. And as I think we discussed

this morning, the evidence for that is not particularly solid.

So I would have thought that very much, as I said before lunch about non-heat-treated concentrates, it might have had a marginal effect but, as it were, over a long-ish period in which a patient -- certainly a patient with severe haemophilia would get either repeated doses of concentrate or repeated doses of -- you know, 20, 30, 50, donations in cryo. I suspect that it wouldn't really have made very much difference.

Q. In one of the earlier sections, the C3A section, we heard evidence about a particular kind of patient. This patient was someone who, say, in the mid-1980s, had not received treatment with concentrates before, and we had contemplated with Professor Ludlam the possibility that such a patient might be treated with cryoprecipitate.

A. Yes.

Q. He told us that there would be a point at which such a patient, if receiving a lot of cryoprecipitate would lose the benefit of the small pool on the basis that they would be exposed to an accumulating number of donors.

Do you think it would be logical to say that such a patient -- I should say, first of all, do you agree with that proposition that there would be a point at

which such a patient being treated with cryoprecipitate would lose the benefit of the small pool in the way that I have described?

A. I think it's arithmetic. It's not necessarily totally simple arithmetic, but if you accept the sort of numbers we were discussing this morning, say, about 1 in 1,000 donations would have the capability of transmitting Hepatitis C, then your probability of getting a positive donation will be a product of the number of donations you get.

Q. Does it follow from that, that that patient, whom I have described, if one had surrogate testing, which would have, I think you said, had some impact on the number of positive donations getting through, if you like, that patient would be able to receive more treatment before they reached the point where statistically they would be likely to be infected?

A. I would have put it slightly differently, but it probably amounts to about the same thing, that if we accept with all the reservations that surrogate testing had some effectiveness in reducing the risk of receiving positive donations, then -- let's say it was, you know, as I said this morning, 50 per cent effective, then you would reduce the probability of getting a positive donation at any given dose level by a factor of about

50 per cent.
Q. Okay.
A. That's a very simplistic view but that's what I could come up with.
THE CHAIRMAN: Mr Dawson, you should remember that there is a problem about statistics that while they may be valid for a general picture, you cannot extrapolate back to the individual example.
MR DAWSON: Indeed, sir.
THE CHAIRMAN: I wouldn't like to see a lot of time taken up --
MR DAWSON: I'm moving on from that. I think that's something that we may wish to explore elsewhere, sir. I'll move on from this series of questions.

I think it would be fair to say, from your evidence so far Dr McClelland, that the patient that you really had in mind when thinking about surrogate testing was predominantly the blood transfusion patient. Is that right?
A. Yes.
Q. Could you tell me -- you may have given evidence to this effect before, but say around about 1986 to 1988, how much of the blood that was being collected ended up being used at the PFC and being made into concentrates?
A. Sorry, what year?

Q. Say around about 1986 to 1988. I don't want anything specific --

A. A very large majority of the plasma -- a large majority of the plasma derived from the whole blood collections would have ended up at PFC, because back as far as 1975 Professor Cash had introduced a policy initially in Edinburgh of essentially converting from whole blood being transfused to red cell concentrates, and that spread over the whole service. I can't tell you now from cold precisely what proportion of whole blood and red cell concentrates were used in other parts of Scotland but I would say a majority -- probably quite a large majority of the blood that was collected had its plasma separated and that was sent for fractionation. As we have heard many times before, that was a driver for the whole of the Scottish Blood Transfusion Service.

Q. Would it be correct to say that a proportion of the total blood that was collected would effectively go down the PFC route and a proportion of the total blood that was collected eventually ended up being transfused into blood transfusion patients?

A. That might -- that could be seen as quite an ambiguous statement. The blood that was -- the majority of the blood was separated into red cells and plasma. A large proportion of that plasma went to the PFC. Some of it

was used as direct, clinically transfused plasma, some of it was made into cryoprecipitate. But the red cells and the platelets from those donations would be transfused.

Q. There is a degree of overlap, if you like, then, between the two categories. Is that right?

A. I'm not sure that's a very helpful description.

Q. What --

A. I don't know quite where you are driving with it.

Q. What I'm trying to explore is there appears to be an argument that one of the reasons that surrogate testing was not introduced would be that it would be a very large-scale operation and it would cost a lot of money. What I'm trying to investigate is whether it might have been possible to introduce surrogate testing on a smaller scale --

A. I see.

Q. -- for the blood that had been collected for those for whom there would be the greatest benefit, ie the blood transfusion patients?

A. No, that wouldn't have been a runner. It would have been actually operationally much easier to introduce it for every donation than to introduce it for a subset. No question about that.

Q. Thank you. Can I just ask you some questions about

material that was covered yesterday, in particular the thought process behind the recommendation in the 3 March 1987 SNBTS directors' meeting that surrogate testing be introduced. You remember, we looked at that minute in particular, and then we looked at The Lancet article of 4 July 1987 in some detail.

In your evidence yesterday, you suggested that ultimately there required to be some persuasion of those within the government that this was a good idea and in particular you suggested that there required to be well-argued and rational advice for the government to take that course. Is that accurate?

A. I can't remember precisely what I said yesterday but that to me is common sense. I would not expect the government -- either the minister or his or her advisers to take a decision other than on the basis of well-argued and rational advice.

Q. Would it be correct to say that between the beginning of the 1980s, where we have looked at the efforts that you were making to try and institute a large-scale prospective study and the meeting that I referred to in 1986, that you had changed your view about whether surrogate testing should be introduced without that type of study?

A. I think if you put it slightly differently, as I said

yesterday, I had been very keen that we should do a proper evaluation of the effectiveness of this in the early 1980s, and although in, say, 1986 or 1987 I had no prescient foreknowledge of the emergence of the Chiron discovery, I felt that what had happened to change my view since perhaps the 1980/81 sort of era was that, first of all, more evidence had accumulated from more studies, most of them broadly analogous to the TTV studies. And, secondly, that the Americans had, having argued the toss about the pros and cons of doing this testing for a long time both very publicly in the literature and obviously behind closed doors and on the Blood Products Advisory Committee, had decided that they had no option but to go ahead and do it without the benefit of a proper trial.

I think those were the two factors that, you know, made me feel there was little point in pursuing the -- what appeared to be a fairly thoroughly lost cause of the clinical trial and that the evidence had built to a point where we really had a duty to start.

Q. I actually wanted to ask you a little bit about the factors that were influencing your choice at that stage. Can we look at that in a bit more detail. Just to be clear, can we perhaps look at the transcript for yesterday, in particular page 143?

I just wanted to refer, first of all, to the question and answer at the bottom of that page.

Mr Mackenzie asked you:

"At this time in July 1987 ..."

This is the passage when you are talking about The Lancet article which came out on 4 July:

"At this time in July 1987 to what extent was patient safety a factor in your consideration?"

And I think your answer was:

"Answer: It was the factor in my consideration."

Was that a correct recollection of the emphasis that you put on -- which one doesn't quite get from the page?

A. Yes, I think so.

Q. Okay. I also just want to refer you --

A. If I said it was the factor -- that's not strictly true because obviously there were other factors which were specifically mentioned in that letter.

Q. Indeed. I just wanted to refer you -- indeed. I just want to refer to you another couple of things you have mentioned already.

The first is one can see there on page 141, going over to 142 you gave some evidence to the effect that:

"... even in the absence of a proper -- you know a definitive prospect of randomised controlled study to provide a real answer, that there was sufficient

evidence -- the evidence which had convinced the Blood Products Advisory Committee of the FDA that surrogate testing needed to be introduced and led to the decision in the United States was, while not complete and not definitive, very, very difficult to ignore and I had no conviction that the epidemiological situation, the sort of prevalence, the amount of Hepatitis C -- or non-A non-B Hepatitis infection in the UK was really that much less than it was in America, in 1986, because, you know, commercial paid donors had stopped. They had introduced similar changes in donor selection in relation to AIDS that we had, and I felt if, in the light of, you know, those two major changes, the United States felt it had to introduce this testing, we were in a very, very poor position to not follow suit in the UK, unless we had convincing evidence that it really genuinely wasn't a problem."

I referred, first of all, to the emphasis upon patient safety. This might be deemed as a second prong to your rationale that by this stage you had become convinced that the American position and the American conclusions were in fact relevant to the position in Scotland. Is that correct?

A. Well, yes, I mean, I think it would have been -- I couldn't see then and I can't see now any reason for

not taking very serious note of what was emerging in another part of the world with, okay, probably a rather different epidemiology, a history of paid blood donation and so on. But, yes, I thought it was relevant information.

Q. If I could turn to what seemed to me to be perhaps the third prong to your rationale. This is on page 147. At the top of the page, going from line 2. You say there:

"As I recall, the only studies that looked at surrogate testing and concluded that it didn't have any effect, if you look carefully at them actually, the number of patients enrolled was very small and probably not sufficient to draw any conclusions from at all as a statistical basis."

I think the third -- as I have described it as the third prong, what you are saying there -- and please correct me if I am wrong -- is that any of the small studies which tended towards the conclusion that surrogate testing wouldn't be a good idea, you didn't think were as important as the US studies that we have just referred to. Is that right?

A. I think that's broadly true. I mean, there is a huge literature -- and I may have missed some studies out but I would probably stand by what I said yesterday, yes.

Q. I have described it as a three-pronged rationale:

safety, the increased importance of the US studies and the limited significance of the smaller studies.

What I would like to ask you about those three prongs is to what extent did you communicate to the Scottish Government through the SHHD the fact that your view in 1986 was based on these three prongs, if you like?

A. I honestly can't recall whether I personally was communicating as an individual to the Scottish Government about the -- to the SHHD about this. I think 1986 was a period when we still had fairly regular representation, participation, from the department in the directors' meetings of the SNBTS, and I think I would have assumed really that my channels of communication with the department, as one of five regional directors, was either through the national director, Dr Cash, and/or through the discussions that took place in the documents around the directors' meeting and the coordinating group, to which the department was party. I don't think I would have seen it as my role, unless it was an issue to which -- for which I had been sort of specifically delegated a job on behalf of the SNBTS, to go and make direct representations to the department.

Q. What about generally speaking? Obviously that's your

individual position. What about as a part of the SNBTS directors group? Do you feel that this rationale and this three-pronged logic was something that was communicated to the SHHD at that time?

A. Well, as I say, I can only answer that in terms of, you know, what is documented, you know, what was minuted as discussions that took place at the directors' meetings in the presence of department officials, which obviously they would have been aware of, and what was documented -- what is documented in terms of communications on behalf of the directors to the department through our national director.

Q. Could I just take you to a document that we didn't look at yesterday, as far as I recall. This is [SGH0028127].

If we could flip over to the second page, we can see that this is a memo by Dr McIntyre, dated 6 April 1987, and one can see from the first full paragraph there, just to orientate this in time, where it says:

"The directors of SNBTS are unanimous, and are now pressing fairly strongly, that this screening should be instituted; though perfectly aware that it would be costly and could not abolish transmission completely, they could then claim to have taken all steps open to them to reduce transmission. Before embarking on such an expensive programme it would seem logical to

participate in the proposed research and to delay any further action until the results of this were known."

So just to orientate that in time, this is the month after the meeting during which you had made the recommendation or you had decided upon making the recommendation.

A. Yes, I'm familiar with this.

Q. Could I just go back to the first page, please? One can see that this is a document which was circulated by Dr McIntyre to a number of people with whom you will be familiar within SHHD at that time, Dr Scott et cetera, et cetera.

One can see from the third paragraph that there is an attempt to summarise really what the background to this argument is. In particular it says:

"In USA, largely one suspects because of the fear of litigation, there has been a great deal of pressure to introduce this indirect screening for 'non-A non-B Hepatitis' and we understand this is likely to happen soon. A similar situation is said to exist in Germany.

"SHHD was asked last year to meet the expenditure of £810,000 annually to establish screening of all blood donations with the intention of reducing transmission of non-A non-B Hepatitis by blood and blood products. Approval was not given as the research already conducted

in the West of Scotland with CSO funding indicated the impact there of transfusion association 'non-A non-B Hepatitis' was not great; also that the indirect screening proposed would be expensive, could not in any event abolish the transmission of this 'Hepatitis' by blood and blood products and would lead to a loss of a perceptible amount of 'innocent' blood which nevertheless failed to pass the screen. We also wished to await DHSS thinking on this subject.

"DHSS have now invited their Transfusion-Associated Hepatitis Working Party, which includes two Scottish members and an SHHD observer, to consider this issue. The Working Party noted the research already conducted in the West of Scotland and advised that instead of embarking at once on expenditure amounting the UK to perhaps £6-8m, research should be commissioned to expand the previous Scottish research; it is agreed that the impact of this 'Hepatitis' differs considerably in different countries. The research is planned to take place in three English centres and one Scottish centre (Edinburgh)."

Then over the page:

"The English component has been presented to the research management division of DHSS, a formal application has been encouraged and is now being

prepared with a view to a start in September 1987."

Then we have the paragraph I have read already.

And then in the final paragraph:

"If recipients of this minute are agreeable that this is the correct line to adopt, then the Edinburgh SNBTS will be asked to prepare a detailed proposal along similar lines to that of their English counterparts. Consideration will require to be given as to how the cost of the research estimated to be in the order of £25,000 can be met. If this line of approach is considered to be inappropriate, the Transfusion Associated Working Party at DHSS would require to be advised as soon as possible since the working party would presumably then recommend expanding the English component; this would leave Scotland without locally derived fresh information to illuminate decision on the proposed screening."

So does it appear from this, which is the SHHD position, if you like, or an expression of that in the aftermath of your recommendation in March 1987, that those at SHHD had understood the three prongs to your argument, as I have described it?

A. I don't think this really enlightens -- or enlightened me very much on that question.

Q. There doesn't seem to be any suggestion here, certainly

as I read it, that the US research, which was influencing you at this time, was something which the SHHD thought should be influential to the extent that surrogate testing should be introduced?

A. I think the paragraph on the previous page is rather dismissive to be honest. One suspects largely for medical/legal reasons and so on.

Q. Indeed. What I'm trying to investigate here, Dr McClelland, is the extent to which the thinking which had driven you to come to the recommendation you did in March had been communicated to SHHD to the extent that they understood why it was you had made that recommendation?

A. I think the -- not entirely but I think the second complete paragraph -- first complete paragraph on this page we have on the screen at the moment is reasonably clear. I mean, what Dr McIntyre is saying is that the directors were saying, as they were, "We wanted to feel confident that we had taken" -- I don't think it's so much claim, "I think we wanted to feel that we had taken all possible steps to reduce transmission". And that's basically what I said yesterday.

Q. Given that you said yesterday that there were a number of conflicting view points at this point and there was a requirement that a full and rational view be put

forward to the SHHD, does it look from this document as if the full reasoning that you had based your recommendation on had got through to them?

A. I really can't answer that.

Q. Could I just ask you -- I don't think we need to go to the document but obviously when I refer to The Lancet letter of 4 July you know which document I'm talking about?

A. Yes.

Q. Why did you feel it necessary to write a letter to The Lancet at that time in those terms?

A. I think, as I said -- I think I said yesterday it was the sort of -- I think I was getting quite frustrated actually, to be honest, and I felt that it was not something that I had done a lot in my career but that it was appropriate for something that I felt quite strongly about to try and stir the pot a bit, and in the hope that perhaps by putting the particular set of arguments that were contained in it -- because there were several quite materially different points in that letter -- putting that into the public domain might in some way stimulate a reconsideration of the importance of getting on with this. I think that was probably extremely naive in retrospect but I think that was probably what was in my mind in drafting that letter.

Q. In whom were you trying to stimulate a reconsideration?
A. I'm not sure how, as it were, targeted I would have been in my thinking about that because, you know, if I tried to answer that question from now, I would say, well, the potential targets would have been, you know, opinion formers and the people to whom those opinions have to be relayed to get a decision to make a change in health policy.
Q. So presumably that would include the SHHD people and also -- I think at that time there were perhaps some people, although perhaps not directors, people within SNBTS that didn't hold the same view as you?
A. Well, yes, and there were a number of people who were advising, as you are very familiar with, people who were advising the department in London and there would be some advice coming to the department in Scotland, and clearly there was liaison between the departments. So I thought, putting these arguments as clearly and strongly as we could into a national, large circulation general medical journal might provoke some thought which might in turn provoke some action. It was probably rather naive but that, I think, was probably what was in my mind.
Q. Thank you. Could I just move on to a related but slightly different topic, something that we covered to

a certain extent this morning, and that's to do with the practical arrangements for surrogate testing and the extent to which those had been thought through at the time of your recommendation. I think we covered this to some extent this morning, and you have mentioned in your evidence a number of things which decisions would have had to have been taken about and practical measures would have had to have been put in place for surrogate testing to get up and running, for example a decision about the cut-off for the ALT, there would have to have been provision for counselling. You have mentioned training of staff and equipment. Are these the kinds of things --

A. Yes, absolutely.

Q. -- which practically one would have had to be thinking about?

A. Absolutely.

Q. In one of the previous sections relating to the introduction of HTLV-III antibody testing, you gave some evidence about an algorithm that had been created in order to lay down a template for how the testing would work.

A. Yes.

Q. Was any similar plan of action, if you like, thought through for surrogate testing?

A. It was certainly thought about. It didn't reach the stage of formal debate at the level of the directors meeting and so on, but certainly those of us who were interested in this had -- we were aware of the -- I mean, having been through the process of developing a sort of decision chart for HIV -- which, of course, we subsequently had to do for Hepatitis C -- we were fairly aware of all the directions, all the questions that started to arise and became -- compelled one to address them when you started to explore this decision process. So you, know, we were in a position to hit the ground running with developing that. That's not to say it would have been easy, and it would undoubtedly, had we had to do it, would have gone through many iterations. But, as I said this morning, I think we were actually quite well equipped in terms of past experience and so on, to get on and deliver this relatively quickly.

Q. So that would tend to suggest that you were aware of what questions needed to be asked and what issues would need to be dealt with because of your previous experience of doing this kind of thing. What I'm more interested in is the extent to which you had formulated what the answers would be to those questions, by the time that you made the recommendation in March 1987.

A. I don't think I can add very much to what I said this

morning. We had not done a great deal of formal work on it.

Q. Okay. Can I take it from the fact that you hadn't done a great deal of formal work that not a great deal about this type of thing, the practical arrangement, had been communicated to SHHD?

A. I think that the department were clearly very well aware of the main elements of the -- the issues that would be problematical about the loss of donors and the creation of a population of individuals who would have suddenly got an abnormal screen test.

This is not revolutionary stuff. It's exactly what happens when you initiate a new screening programme. And medical folks in the department would have been perfectly familiar with those issues.

You start -- you have many identical issues arise. So I think they were probably perfectly well informed -- informed with a level of sort of detail that was appropriate at the time to make a judgment on those issues.

Q. Okay. Thank you very much.

There is just a couple of other areas I would like to cover with you quickly. The first is in relation to a passage which you produced in your original statement, which one can see at page 11 of [PEN0170754]. This is

something that we looked at yesterday.

Am I correct in my understanding that this is you reproducing a note that was drafted by Harold Gunson and which had been made available to people who had attended the Working Party on Transmission Associated Hepatitis in November 1986?

A. Yes, this is -- it's a scan or a -- an image, facsimile of the one -- probably, the second, page of a four or five-page document that was produced by Dr Gunson as the working paper for that document, which I think is -- I'm sure it's in the court book.

Q. I'm right about the authorship?

A. Yes, I just was too lazy to type it all out again.

Q. No, it's helpful to have it there.

I think in your evidence yesterday you said that this document in particular was one which had been persuasive as regards the development of your thinking towards recommending introducing surrogate testing. In particular, I think you said that the numbers that were being used in this document had been influential. Is that correct?

A. That is correct, yes.

Q. And do I take it that the similarity in the numbers here and in the numbers which one might find in The Lancet article is not coincidental and that this is the source

of the information in The Lancet article?

A. It may well be. I honestly can't remember. But if they are the same numbers, then I'm sure that's where I got it. But I can't remember the answer to that.

Q. I just wanted to ask you if you could be a bit more specific about precisely what it was within this that had been so influential on your thinking, other than the author of it, of course, but the actual detail?

A. Well, I think it may well be the first time that I had seen a calculation of the number of infections that could be occurring based on what we then knew. I'm not sure -- I certainly should have done that calculation myself before but I'm not sure that I did, and I think the scale -- I think I probably would have seen this in the context of what we'd seen with HIV, where actually, although it was a terrible problem, the numbers of infections were very, very small. Compared to this they were tiny. But I cannot honestly say I remember the eureka moment when I read this and thought, "My God, these are big numbers". I think seeing those numbers, as it were, in cold blood probably was a factor in my trying to push on a bit more.

Q. Can you help me with what Dr Gunson's position was when this paper was presented about the attractiveness or otherwise of introducing surrogate testing?

A. Well, only insofar as there is an illegible scribbled note of mine from the meeting, which I haven't yet tried to decipher fully, and there is that interesting note that we were reminded of yesterday in which I think it was Dr Forrester asked the chairman, Dr Gunson, if he would introduce testing if it was free of charge and he said, "No, I wouldn't".

Q. That's really what I'm asking about, Dr McClelland. I'm trying to reconcile how it is that you could be presented with this material and that have apparently a significant influence on your thinking towards favouring surrogate testing and the reference that you have made to the note made by Dr Forrester, which would tend to suggest that Dr Gunson was not in favour of surrogate testing to the point where he said he wouldn't introduce it, even if it were at no coast?

A. Looking back at this while I was preparing these reports, I found this very hard to square. I would not wish to conceal that at all. I think I have said it in my statement. I find it very difficult looking back with the wisdom of hindsight to understand how a group, of which I was a member, could have this very well-prepared, well-argued, well-sourced, well-informed paper presented to us with these quite disturbing numbers and then proceed to agree to do yet another

study of prevalence in donors.

I think I probably -- I cannot now say why I didn't make more of a fuss. I know I arrived very late for the meeting and possibly felt it was inappropriate for me to make a fuss at the meeting, but it was following -- you know, it was after this, I think, that we began to push again for some action.

But if you are asking me to tell you precisely what was the relationship between seeing this document and the action that I -- that was -- the decision taken by the BTS directors, it would be pure speculation because I don't have a clear -- I don't have any memory of my thought processes over that period.

Q. Thank you. Could I just clarify one final matter with you, Dr McClelland? Am I right in saying that throughout the period when one was considering surrogate testing with all its inevitable disadvantages on the basis that it wasn't a true test but was a surrogate test, that it was only ever being considered as an interim measure until something better might come along?

A. No, I don't think one can really say that. I think that might have been a hope. I don't think I ever considered it as an interim measure because I didn't know it was -- you know, I had no -- I mean, what I knew about the development of specific tests at that time was gleaned

from the work that one of my own staff had been doing, which was proving that it was incredibly difficult, and through him the knowledge of a lot of other groups around the country, some of whose work -- around the world, some of whose work I had read, some of whom I had met, who were all finding it incredibly difficult. So my own knowledge at the time was not such to make me expect an early resolution to this problem.

And I think that would have been the position of most people because actually the breakthrough, if I can use that term, that led to Houghton and his group discovering the Hepatitis C test was dependent entirely on what was very novel technology, which I and most of my colleagues didn't know anything about at the time. You know, the sort of reverse engineering of a virus from -- starting off with an antibody was science fiction, as far as I was concerned.

Q. Okay. And would it be --

A. So I don't think I had an expectation that there was going to be an early emergence of a super-duper specific test.

Q. I think it has occurred to me that I may have made an assumption, that I should probably ask you about, in my earlier question, which is, it is, is it not, inherent in the nature of surrogate testing that there is going

to be a degree of unreliability about it?

A. Well, the use of something like the ALT test, it's absolutely inherent.

Q. But the fact it's a surrogate test means it is never going to be the test you would really want in an ideal world?

A. I think the sense in which the term is used means you are measuring something which you hope is associated with the presence or absence of something else.

Q. Which adds in an extra layer of complication, if you like, in terms of its accuracy?

A. Yes.

Q. So if someone were to say that, "I don't like surrogate testing because of the fact it's not going to eradicate all of the non-A non-B Hepatitis in the donor population", that would probably misunderstand the parameters within which one should be discussing a test of that nature?

A. Sorry, could you repeat the statement again? I missed a bit. The hypothetical statement that you made.

Q. If someone were to say, "I don't like surrogate testing because of the fact it's not going to eradicate all of the non-A non-B Hepatitis in the donor population," that would probably misunderstand the parameters within which one should be discussing a test of that nature. What I

mean by that is a surrogate test is always going to have a degree of inaccuracy?

A. Yes, I think the question is a much more general one: would you use a treatment that does not guarantee to cure 100 per cent of the disease? Would you reject it because it only cures 50 per cent?

Q. Thank you. There is one final area I would like to explore with you, and to do that I would like to return again to the statement -- sorry, the transcript from yesterday.

I would just like to refer you to two particular answers that you gave. The first is on page 144.

This is the question which comes immediately after the one which I referred you to earlier and it's in the context of the way in which you had presented the argument along with the other centre directors in the July 1987 Lancet article.

You said there, in your answer in line 2:

"The objective was to try and get testing started."

If I could just refer you to another passage from page 106 towards the bottom of that page, in line 18. This was in the context of you answering some questions about the multi-centre trial, you will recall that no doubt, and you refer there to the fact that:

"It did seem rather like a way of buying time

actually."

What I wanted to ask you was you seem there to be presenting two schools of thought, if you like. One is the objective that we have to get on with things and the other is the school of buying time.

What I wanted to ask you was, would it be fair to say that around this issue there were really two camps; one was the we have to get on with it camp and the other was the buying time camp?

A. I don't know that that really -- that makes it sound very polarised. I don't recall it being like that. There was a certain amount of perhaps inertia.

I think if I was to criticise the -- you know, the sort of -- with the wisdom of hindsight, there was perhaps a very large preoccupation on all the problems, which maybe was more influential in people's thinking than perhaps thinking about the potential safety gains that could be gained.

I don't think it was polarised. I don't think anybody was -- perhaps I shouldn't have said that. I certainly didn't feel anybody was explicitly trying to buy time, trying to prevaricate. But I did feel, as I was trying to say here, that this was a study that was actually quite -- you know, relatively easy for the transfusion services to do because in a sense the

clientele was entirely under their control. But I didn't see that it was actually going to help us with making a decision about what to do. It might at most have told us a bit more about the magnitude of some of the problems but it wasn't going to tell us anything about the magnitude of the some of the benefits.

MR DAWSON: Okay, thank you very much, Dr McClelland. Thank you, sir.

THE CHAIRMAN: Mr Anderson?

Questions by MR ANDERSON

MR ANDERSON: Thank you, sir. Dr McClelland, could we look together, please, at a document that you haven't been shown thus far? It's [SNB0059240].

MR JOHNSTON: Sir, I wonder if I could interrupt for a moment. As I think it may have been made clear by the Inquiry team, I do have an objection to the line that I think Mr Anderson is going to pursue resting on this document and the reply to it.

THE CHAIRMAN: It has not been made clear, since I would have resisted any attempt to make anything clear before hearing you. Mr Johnston, it has been made clear that you have an objection but I have not read the letter and I don't know what it is yet. So help me, please, to understand what it is.

MR JOHNSTON: Yes, I'm grateful. We can see, looking at the

letter, and I should say, as I understand Dr McClelland has seen the letter and I don't think there is any need for him to disappear in the course of what I hope will just be a brief discussion, but we can see from looking at the letter that there is a suggestion that the individual officer should be removed from duties which include interface with the Scottish Transfusion Service. That seems to be supported by reference to a position he took at the last BTS subcommittee meeting in relation to a particular project, namely a collaborative research agreement, and exception is taken to the way in which he approached that.

That's a very short summary of the large second paragraph on that page.

THE CHAIRMAN: Yes.

MR JOHNSTON: Perhaps it's enough simply to note that the entire issue, so far as one can see, has no bearing whatsoever on topic C2 or indeed, so far as I can see, anything else with which this Inquiry is concerned.

The remaining short paragraph on the page mentions another event that happened earlier, which relates to a delay in the AIDS validation studies of plasma-derived blood products and, again, it's suggested that the approach taken by the particular officer led the directors to have little or no confidence in him.

That clearly, one could say, is something that falls within the scope of the Inquiry in general, albeit not topic C2.

Then moving over the page to page 9241, we can see that Dr Cash points out that not all the fault lies on one side, and he accepts that others may perhaps have to share in this. And he points out there has never been such a difficulty with predecessors, which I think must be a reference to Dr Bell.

The reason I object to this -- I should say there is a reply to it, sir, and I'm not sure, in order to address the issue you would prefer to see that also.

THE CHAIRMAN: I think you are probably giving me the flavour of the correspondence without going into the detail.

MR JOHNSTON: In that case, the reason I object to it, sir, is quite straightforward, it is simply that without any prior warning, as I understand the line that is sought to be pursued, we are going to end up in a position where an individual is subject to criticism. No prior warning of any such issue was given to me before about 11 o'clock this morning.

Equally, it is not clear how it has any bearing on the C2 topic, as I have already said. There are, of course, as we know, various memos in relation to C2, of

which this particular person was the author and, of course, if there are specific complaints about advice he gave in relation to C2, then those memos, of course, can be discussed with him, and indeed with others who have a view on them. But in my submission it's simply inappropriate to single him out for criticism in a rather abstract way and not in a way that has any connection with the topic that's actually before the Inquiry in this phase of the hearing.

THE CHAIRMAN: Yes.

MR JOHNSTON: Those are the reasons for which I object, sir.

THE CHAIRMAN: Mr Anderson, what do you say in response to that.

MR ANDERSON: Yes, I'm obliged, sir. Perhaps I should explain that this letter from Dr Cash to Mr Morison and its reply were, I think, stumbled upon by the Inquiry team some time last week. They were intimated some time after 5 o'clock on Friday and discovered by those instructing me on Monday. I saw them and I suspect my learned friend Mr Johnston only saw them for the first time yesterday.

I think it's important to make clear that it's not my desire that this matter be ventilated, nor is it the desire of Professor Cash or indeed Dr McClelland. It's a decision taken by the Inquiry team, and I make no

comment upon that, but I think it's important to understand that it's the Inquiry team that sees these letters as relevant and wishes to explore the contents of these letters with Professor Cash and, I'm sure, with Dr Forrester, who is due to give evidence, I think, on Monday of next week.

My purpose in seeking to put this to Dr McClelland is that, of course, he is mentioned in this letter, both implicitly as being one of the SNBTS directors, and also expressly in about the fourth line of the second paragraph.

THE CHAIRMAN: Can we go back to that, please, so that I can see what the reference is?

MR ANDERSON: If one returns to page 1 -- that's 9240 -- what is said in the second paragraph is:

"I cannot begin to understand the problem but the quality of Dr Forrester's remarks at the last BTS subcommittee meeting in the context of the Sandoz Collaborative Research Agreement were regarded by my colleagues, particularly Dr McClelland and myself as bordering on insulting."

Then, sir, you will see just about three lines from the foot of the first page, it says:

"Taken together along with other episodes of only minor importance, I must, with regret, conclude that the

SNBTS directors have little or no confidence in the person who currently provides the vital medical link between the operational part of the blood transfusion service and SHHD."

Now, as I say, I'm not responsible for these letters coming before the Inquiry but since they are before the Inquiry, it does seem to me with respect, to be helpful that the matter is investigated to some degree, and rather than have perhaps the unedifying prospect of Professor Cash saying one thing and Dr Forrester saying another -- and I have no idea what he will say, of course -- it seems to me that it would be helpful to have Dr McClelland's comments on this.

THE CHAIRMAN: So that on one view we might have two saying one thing and one saying the other? It's very strange, Mr Anderson, that you should have introduced a letter with a view to attracting an objection which you are then in effect asking me to sustain. I'm not quite sure where I am. Perhaps I should ask the Inquiry team for their observations on this, unless you have got much else to say.

MR ANDERSON: The only thing I would say, sir, is I'm seeking to emphasise that I did not introduce the letter.

THE CHAIRMAN: This is the first time I have seen it and it

came to me as a result of you asking a question. So, so far as I'm concerned, you have introduced it.

There is a mass of material at this Inquiry that I have been spared due to the diligent work of the Inquiry team in making sure that I only get to see what they think is important, and this is the first time I have seen this one, Mr Anderson. But I get lots of surprises. So I'm not too worried about that.

MR ANDERSON: We have all been spared, I have no doubt, sir. But, as I say, I'm not producing this. It's the Inquiry team that is producing it and I'm quite happy if the matter is not investigated, but if it is to be investigated --

THE CHAIRMAN: I think now that I have seen it, it's so intriguing that I can't see it being left out of account altogether. This sounds very much like something up the hill I would be saying I repel the objections, subject to relevancy and competency, Mr Anderson.

MR ANDERSON: Ultimately, that would have been my position, sir, but if you remain undecided, that would be an option, that you simply allow it under reservation.

THE CHAIRMAN: I'm not sure that the reservation is strictly accurate in these circumstances but, of course, I'm open to submissions at the end of the day that some particular topics should not be referred to in a final

report for particular reasons. If I may, I think I would rather we get on with it but preserve every person's interest in arguing that it's not material at the end of the day.

Internecine battles, I would have expected to hear about. I can't imagine any major public department operating for a long time without generating them, and if this turns out just to be something like that, perhaps it will disappear off the dyke with a lot of other snow. But let's wait and see. I think you should ask your questions now and, Mr Johnston, you will not be prejudiced in the ultimate analysis if this proves to be totally irrelevant.

MR JOHNSTON: Thank you, sir.

MR ANDERSON: I'm much obliged, sir. I don't intend to take up much time on this.

Dr McClelland, we see this is a letter of 21 August 1986. It's addressed to Mr Morison of the Scottish Home and Health Department and it's addressed "Dear Hugh ..."

Can you help us first of all with this. Do you know who Hugh Morison was?

A. Not perhaps with a degree of precision which you would wish, but he was a very senior civil servant in the Scottish Home and Health Department whose

responsibilities, portfolio included the SNBTS.

Q. All right. Can you tell us who Dr Forrester was and what his duties were?

A. Dr Forrester is a medical doctor who was -- again, I'm not sure what his precise -- he was a medical officer. I don't mean any disrespect if he was of higher status than that but he was one of the medical professional team in the Scottish Home and Health Department. I honestly don't remember what his grading, what his precise position was, but he did have -- my recollection is that Dr Forrester [sic -- Dr Bell] had the retirement or illness -- I can't remember whether Dr Bell left his post because of illness or retired but following Dr Bell, Dr Forrester became the medical sort of liaison person between the department and the Scottish Home and Health Department.

Q. I'm obliged.

A. That's my recollection of his relationship to BTS.

Q. You will see that the letter starts off:

"I must once again request that consideration be given by appropriate colleagues in SHHD to give Dr Forrester duties which do not include an interface with the Scottish Transfusion Service."

In the second paragraph it goes on:

"I cannot begin to understand the problems but the

quality of Dr Forrester's remarks at the last BTS subcommittee meeting in the context of the Sandoz collaborative research agreement were regarded by my colleagues, particularly Dr McClelland and myself, as bordering on the insulting. They also revealed a depth of scientific/medical understanding that was remarkably and disturbingly shallow."

Before getting on to what may or may not have been said at the meeting, could you help us with this, please, doctor: what was the Sandoz Collaborative Research Agreement and who were Sandoz?

A. Sandoz was a large Swiss-based multinational pharmaceutical company, who had a longstanding interest and commercial and research and development activity in the field of immunoglobulin; that is antibody therapy for a variety of disorders. So they were a very large pharmaceutical company but had a major division which specialised in an area which was very -- of very great interest to the SNBTS because we also had a manufacturing activity in that field.

Q. What was the Sandoz Collaborative Research Agreement?

A. This was an agreement which was established and endured for four or five years, quite substantial sums of money were involved, and the purpose was to develop monoclonal antibodies; that is antibodies made by manipulation of

cells outside the body, directed at bacterial -- parts of the chemistry of bacteria which cause a condition called endotoxemia. This is part of the bacterial wall of a particular class of bacteria, which was known -- is known to be an extremely -- cause profound disruption to the physiology of the body, which, in its most severe form, produces, you know, rapidly lethal shock, and in a less severe form is an ongoing problem in critically ill patients such as occupy many intensive care unit beds.

It's a manifestation of bacterial infection, which is not amenable to antibiotic therapy, not simply maintainable to antibiotic therapy. Therefore some form of biological-based therapy designed to interrupt the effect of this fragment of the bacteria was a very important therapeutic target.

Q. All right. I'm not sure we need to know much about the science, in fact, Dr McClelland, but we see in capital letters, the Collaborative Research Agreement. Who was the agreement between?

A. It was between the -- the players were the Sandoz company and a research team in the Scottish National Blood Transfusion Service. The actual signatories to the agreement was probably the Common Services Agency, but I can't honestly remember

the contractual details.

Q. I think that's all we need to know for present purposes. What is referred to here are comments by Dr Forrester at a meeting. Do you remember this meeting?

A. I do actually.

Q. Can you help us with what the comments were and why they may have provoked this response from Dr Cash?

A. Yes, I do remember quite clearly because it was -- I was actually quite upset at the time. We were endeavouring to explain to the committee, which had to approve this agreement, the nature of the science, probably along the lines I have just summarised for you, and I certainly can't remember the exact words but the recollection that I have is that Dr Forrester was actually very, very dismissive of this and said it was completely irrelevant and I think, you know, the implication was that we were completely out of date in terms of even thinking that this was a problem worth addressing.

The committee of obviously non-scientific people was clearly a bit nonplussed by this and, yes, I was very disturbed because he was wrong. And he was not only wrong but was -- I felt, as John Cash said in his letter, what he said was actually very insulting to both our ability and our integrity that we should be putting

forward a serious agreement on something that apparently was valueless.

Q. The next sentence is that:

"They [the remarks] also revealed a depth of scientific/medical understanding that was remarkably and disturbingly shallow."

Would you associate yourself with that comment?

A. Well, I think -- my recollection is that what actually emerged when there was some discussion of this after the meeting, was that Dr Forrester was actually talking about a different condition.

THE CHAIRMAN: I'm sorry, I didn't hear that.

A. I'm sorry, I think my recollection is that what emerged when there was some discussion after the formal part of the meeting was that Dr Forrester had actually misinterpreted what we were proposing and was referring to a completely different condition, to which his remarks probably were apposite.

THE CHAIRMAN: A different medical condition?

A. Yes.

THE CHAIRMAN: Yes.

MR ANDERSON: It goes on to say:

"Dr Forrester made identical comments at the commercial interface steering group on 6 August and when challenged made it quite plain that his view that the

clinical importance of endotoxic shock/overwhelming coliform septicemia was of historical interest only and was nowadays quantitatively a trivial matter, had been formed after appropriate consultation and was 'the official SHHD view' on the matter."

Do you remember that incident?

A. I honestly don't remember those precise words being said.

Q. If we go to the final paragraph on that page, please, doctor, it says:

"Taken together along with other episodes of only minor importance I must with regret conclude that the SNBTS directors have little or no confidence in the person who currently provides the vital medical link between the operational part of the Blood Transfusion Service and the SHHD."

You were one of the SNBTS directors at the time. Was it right to say that you had little or no confidence in Dr Forrester?

A. I'm not sure that I would express it in those words. I think, looking back, what I was probably aware of -- and this is -- I say this because it is verifiable, my recollection was that in the -- if I can say the era of Dr Bell, he was a regular -- and I think I said this morning -- a contributing participant to the SNBTS

directors meetings and there was regular and easy contact. I do not recall that being the case during the period that Dr Forrester occupied the same role.

Q. Why was it different?

A. I don't think Dr Forrester attended -- this is why I feel it's perhaps -- it may be worth looking at some minutes to check, but my recollection was that he was less regularly present at our meetings and, as it were, there was this less sense of easy communication with the department during his period in that office.

THE CHAIRMAN: Mr Anderson, we are going to have to have a break for the benefit of the stenographer unless perhaps another second or two would do you.

MR ANDERSON: A minute or two.

I take it you would not have seen a draft of this letter or the letter itself before it went out?

A. I'm sure I didn't, sir. I think I first saw it in the course of looking at papers for the purpose of this Inquiry.

Q. What Dr Cash is effectively asking is that Dr Forrester be moved sideways, as it were, and be removed from duties in relation to the Scottish Transfusion Service. If you had seen this letter before it had gone out, would you have supported it?

A. I might well have done. Probably -- whether my reasons

would have been entirely dispassionate or not -- but I had been quite disturbed by this incident that's referred to in the first paragraph of the letter or two incidents actually.

Q. No doubt there is one specific reason for this incident which gave birth to this letter, but in it Dr Cash says that the comments of Dr Forrester "reveal a depth of the scientific/medical understanding that was remarkably and disturbingly shallow". Had Dr Forrester ever manifested that problem previously? Was that a concern in other words from the point of view of the SNBTS?

A. I cannot say, sir, that I have any recollection of that.

THE CHAIRMAN: I don't want to stop you inappropriately.

MR ANDERSON: We will leave it there.

THE CHAIRMAN: Do you have further questions on this to ask, Mr Johnston.

MR ANDERSON: No, I'm content to leave it at that.

THE CHAIRMAN: No, no, Mr Johnston, do you have questions to ask about it?

MR JOHNSTON: I think just a couple of very short ones.

THE CHAIRMAN: I think we will break because I would like just a little bit of information whether the research into monoclonal antibodies that you have described had any direct or indirect connection with the raising of antibodies to any of the conditions that I'm concerned

with and the same would apply to any other aspects of this work.

Incidentally, did you have contacts with Professor Charlie Brown at Heriot-Watt at this time?

A. Yes.

THE CHAIRMAN: Perhaps you might like to tell me whether there was a relationship there too.

(3.03 pm)

(Short break)

(3.19 pm)

THE CHAIRMAN: Dr McClelland, I have had the benefit of a little tutorial on just how serious a condition endotoxic shock was. So, Professor James will come back to that later and we needn't take time on it at the moment.

Sandoz would be interested, given the nature of that condition and the problem that it caused, in finding a monoclonal antibody that could be exploited commercially if they could get it.

A. Yes, absolutely.

THE CHAIRMAN: Did they approach you or did you approach them?

A. I think they originally approached us.

THE CHAIRMAN: Would that be to try to get access to some material that perhaps was derived from a patient or

patients who had had endotoxic shock and recovered from it?

A. No, I don't think it was. I think it was because one of my colleagues, Dr Robin Barclay, had developed an interest in -- purely for the reasons of SNBTS work -- I may be factually incorrect. My recollection is that Robin had developed some quite nice techniques for studying human blood donor plasma to detect plasmas with high levels of antibody to various endotoxin components, because our original thought, which had emerged from a previous idea that Professor Cash and I had worked on before, was that we might find donors with naturally high levels of endotoxin -- anti-endotoxins IGG specifically, which might provide -- we might be able to make sufficient quantities of intravenous immunoglobulin to allow a pilot clinical trial with a product which was in essence already licensed and that that would be a bridging step towards -- sort of proof a principle test that didn't involve all the huge regulatory problems of using an artificial antibody.

THE CHAIRMAN: So yours didn't involve the artificial antibody?

A. It did, because we went on -- we had in parallel been pursuing them on -- because we had already done a lot of work on monoclonal antibodies for a whole variety of

other applications. So we had --

THE CHAIRMAN: And some of that, I think, I have read about in PhD theses and so on --

A. Quite possibly.

THE CHAIRMAN: -- because Charlie Brown was interested on this.

A. I think he may have actually supervised one of -- we certainly did have --

THE CHAIRMAN: Dr Horsley(?).

A. Yes, that's right. We did have discussions I'm sure with Professor Brown about manufacturing aspects of this as well.

THE CHAIRMAN: But having gone through that, I think it's fairly clear that it has got nothing to do with monoclonal antibodies to any of the infections and other things that I'm concerned with in this Inquiry.

A. No, we did give some thought, as did others, to the possibility that monoclonal antibodies against HIV might have some relevance, but rapidly concluded that it was probably a non-starter. I think that's probably correct.

THE CHAIRMAN: And that was in common with other people?

A. In common with other people.

THE CHAIRMAN: Now, Mr Johnston, I don't know if that helps you at all.

MR JOHNSTON: Thank you, sir, that removes the main question I wanted to ask. And the only one other in fact, if this is the right time to ask it, whether Dr McClelland recalls whether the Collaborative Research Agreement actually went ahead?

A. Oh yes, it did, it operated for quite a number of years and in fact produced some very promising products but eventually was terminated very amicably, as Sandoz made a commercial decision not to pursue the line of investigation. There had been a huge fanfare about another monoclonal antibody produced in the United States with the same objective, which we actually were confident wouldn't work because it was directed against the wrong thing, and it failed very spectacularly and blew the market away for quite a number of years. So Sandoz -- it was a very civilised divorce actually.

THE CHAIRMAN: Scots and Swiss it had to be reasonably civilised, I suppose. You don't want to ask.

MR JOHNSTON: I have no further questions, sir.

THE CHAIRMAN: Thank you very much.

MR MACKENZIE: Thank you, sir, the next witness is Professor Cash. We won't finish him today but I think we can make a useful start.

THE CHAIRMAN: Yes.

PROFESSOR JOHN CASH (continued)

Questions by MR MACKENZIE

THE CHAIRMAN: Good afternoon, Dr Cash?

A. Good afternoon.

MR MACKENZIE: Good afternoon, professor, I apologise you have been kept waiting today.

Now, professor, we have asked you to attend to speak to topic C2, the question of surrogate testing for non-A non-B Hepatitis in the 1980s. You have provided us, professor, with some statements but before we go to them, what I would like to do, please, is to take you in chronological order to one or two documents where I think you can assist us.

I would like to start, please, by taking you back to 1981, to the Advisory Group on Testing for Hepatitis B, of which I think you were a member. Could we, please, look at [DHF0030037]?

I think we can faintly see this, professor, is a third report of this group. It's dated 1981. If we can then go to page 0041, please, we can see the members of this group and we can see, professor, that you were, of course, a member of this group and no doubt you will recall that?

A. Yes, I do, yes, thank you.

Q. I think this report is relevant to us for one reason. Can we then, please, go to page 5, which is -- rather page 4 to start with, 0045, and we can see in paragraph 22 under the subheading "Tests for non-A non-B Hepatitis viruses", it states:

"Non-A non-B Hepatitis viruses are a common cause of PTH in the United States and are thought to have been responsible for cases of PTH in the UK. Hepatitis due to these viruses is common among haemophiliacs and follows the administration of imported and occasionally of British Factor VIII and Factor IX. There is evidence for the occurrence of sporadic cases of non-A non-B Hepatitis in the general adult population and in association with cryoprecipitate therapy in the UK."

Over the page, please, paragraph 23 states:

"There are at the present time no screening tests for detecting non-A non-B Hepatitis viruses in blood donations."

Then paragraph 24:

"We recommend that research is undertaken in the UK to determine the extent and severity of PTH due to non-A non-B Hepatitis viruses. Unless this is done, we will not have the knowledge on which to base any possible future recommendations about screening blood donations for these viruses."

Do you recall that sort of discussion, professor, as part of the workings of this group?

A. I don't honestly but I would take the view that that's a pretty accurate minute. I don't recall.

Q. Indeed, it's not a minute, it's an official report of the group.

A. I beg your pardon, yes.

Q. So presumably the members --

A. Some pretty distinguished people there, Sheila Sherlock.

Q. Yes. And then finally and for completeness, can we go, please, to page 8 of the report, which is 0049, the summary of principal recommendations are set out.

Number 9, towards the bottom of the page, we can see one principal recommendation of the group was that:

"Research should be undertaken in the UK to determine the extent and severity of post-transfusion hepatitis due to non-A non-B Hepatitis viruses."

Professor, we heard from Dr McClelland yesterday about his membership firstly of a Medical Research Council group, a Working Group on Post-Transfusion Hepatitis, which met in 1980 and 1981, and Dr McClelland had submitted a study proposal to this working group to undertake essentially a study in the UK of the sort that was undertaken in America.

Do you remember, professor, whether Dr McClelland

discussed these proposals with you at the time?
A. Oh, yes, indeed. I was on the main committee, and Harold Gunson set up a subcommittee, post-transfusion hepatitis, and Brian was asked to serve on that. We also had another committee, a subcommittee, that I was chairman of, so, yes --
Q. And when you say you were a member of the main committee --
A. Yes.
Q. -- that would have been the MRC Blood Transfusion Research Committee?
A. That was the resurrected committee. It had been disbanded several years before and then had been resurrected under the chairmanship of Harold Gunson. Previously, Pat Mollison, Professor Pat Mollison, was the chairman.
Q. So you were certainly well aware of Dr McClelland's study proposal to the MRC working group in 1980/1981?
A. He discussed it with me and then I saw it when it came up to the parent committee.
Q. What was your view on the proposal?
A. I was strongly -- I was leaving it to them to get on with it but at the main committee I was strongly in support. I mean, I believe we couldn't even think seriously about surrogate testing until we had done some

important research, and much of that needed to be a replication in the UK context of the TTV study in the States. So I was very supportive.

Q. Do you remember the views of the other members of the Blood Transfusion Research Committee, the main committee --

A. I can't remember. To be absolutely honest with you, no, I can't. I think they would have taken the view -- but this is again conjecture -- that as the subcommittee was packed full of people who were pretty well expert in this area, they would, I'm sure -- certainly I know, Harold Gunson, who was chairing the parent committee and the subcommittee could have expected them to say, "Yeah, it seems a great idea".

Q. We know that the MRC Blood Transfusion Research Committee was disbanded. And if we could, please, go to a letter in that regard, [SNB0025864], we can see, professor, this is a letter from Helen Duke of MRC to yourself, dated 19 July 1982, stating that:

"The committee had fulfilled its remit and should be disbanded."

What was your reaction to receipt of that letter?

A. I was exceedingly angry, for very good reasons, which if you are interested, I will come to, and I hotfoot down to Manchester to speak to Harold Gunson, who was the

chairman, to find out -- because I couldn't believe what I was hearing. My first intimation that it had been disbanded was not from Helen Duke, it was a call from Harold Gunson and I was extremely angry.

Q. Do you know why the committee was disbanded?

A. Well, there is Helen Duke's reason, and we can easily discuss that. But I went down to Manchester and spoke to Harold and he insisted that I didn't speak to my colleagues about it, but he made it absolutely clear to me there were two reasons. First, my own personal interest. I was heading a research group in that MRC unit that was looking at the use of albumin in the acute intensive care area and the acute bleeding area which we use in vascular surgery. We had set up a major, multi-randomised double-blind trial for the use of albumin versus -- which cost millions of pounds worldwide -- versus salt solutions which cost 4p a patient. And the answer we wanted to know, was it more effective the albumin or was it less and was it dangerous? Because there was enough information to know that that was a real issue.

That was a study which was eventually done in Australia and New Zealand 20-odd years later and it demonstrated that albumin, as Professor James is nodding, is a complete waste of money and it initially

suggested -- or it came back on that that it actually was dangerous in some clinical situations.

As a consequence of that, 25 years later, the albumin market and the fractionators collapsed, and Harold Gunson told me I have no -- just listening to him, the DHSS was heavily lobbied by the pharmaceutical industry that are interested in making -- the albumin market and were -- forcibly made the point that they did not wish to see this research take place. That's what Harold told me.

The other thing he told me that DHSS was strongly opposed, for whatever reason -- he didn't explain -- to his hepatitis working group and so the notion that Helen Duke is saying this is being reproduced elsewhere, the albumin was certainly not. And if you ask: what about the hepatitis? All I can say is that, as in the papers here, before the MRC research committee got into the hepatitis group, with Harold Gunson in the chair, there was an ad hoc meeting that took place in 1979 and out of that ad hoc meeting emerged four major project grants. It's in the papers. You need to ask who got them, which -- you know, did Harry Zuckerman get them. Did Sheila Sherlock? So in other words the MRC as a result of this ad hoc thing, before Harold Gunson was allowed to take over, had already committed resources

for other people to be doing research.

So that may have been the reasons for the hepatitis going down. But Harold was sure DHSS did not want to get into surrogate testing.

Q. Okay. Looking on then to what your reaction to the disbanding of the committee was, if we can go to another letter, please, [SGH0010087] we can see this is a letter dated 23 July 1982 from yourself, professor, to the other SNBTS directors and Mr Watt advising that:

" ... the MRC has disbanded their Blood Transfusion Research Committee."

You deeply regretted this development but stated that the time was now:

" ... opportune to consider the establishment of a UK Transfusion Services' Research Committee."

Am I right in thinking professor that, in short, that didn't happen?

A. No.

Q. And I think we have heard evidence -- you shook your head at that question.

A. Absolutely.

Q. I think we do know that the CBLA had a research committee in blood transfusion but that wasn't a true UK-wide research committee?

A. No. If you had asked me would Dr Lane, who was an old

friend of mine, support the notion of an albumin multi-centre randomised trial, the answer was certainly not.

Q. Okay. So there are these matters in the background perhaps. What we do know is that in 1982 the UK Blood Transfusion Services, I think again partly or largely through your prompting, set up a Working Party on Transfusion-Associated Hepatitis and you recall that?

A. I do. I'm sure that Harold Gunson was just as positive as I was.

Q. Yes.

A. I can't take all the credit.

Q. And certainly one feature of the documentation does seem to be -- and correct me if I am wrong -- that you seem to have had a good working relationship with Dr Gunson?

A. Yes, on the whole I did. We had some fundamental differences, which may, for instance, come out when we talk about Hepatitis C donation testing but, yes, we wined and dined together, he slept over at our house and so on, and I did at his house. So, yes, I would say we were good friends.

Q. Now, Dr McClelland was a member of the UK Blood Transfusion Services Working Party on Transfusion-Associated Hepatitis and we have heard from Dr McClelland how again he put forward a study proposal

suggesting a prospective study in the UK, looking at donors and recipients with a view to looking at the prevalence of post-transfusion hepatitis and the question of surrogate testing. And I think that proposal was drafted by Dr McClelland in 1983. Do you remember that, professor?

A. I don't know him drafting it. I do recall vividly -- we were in regular contact, Brian and I -- that he was going to have another crack because this, he thought, might be a different environment outside the MRC. Little did he know, however ...

Q. Is that something you would have supported at the time?

A. Oh, absolutely. I supported this notion right from the 1979/1980.

Q. Now, I would like, then, please, to go forward to 1985, if I may, and refer you to a document [SGH0018259]. These are minutes of an Advisory Committee on the National Blood Transfusion Service, so I think it's the NBTS in England and Wales, not Scotland.

A. Yes, indeed.

Q. We can see that you were a member of this advisory committee --

A. Yes.

Q. -- professor. Can you help us just a little, what was this advisory committee? What did it do? What was its

purpose, just very briefly?

A. You may remember that there was this immense shemozzle. You've had the DVD that we have looked at, self-sufficiency and so on in the 75s -- in 1975. And then in 1980, the minister, who is now part of the current government, stood up in Parliament in December to talk about self-sufficiency and so on and so forth.

It was very clear, because I was really quite close to Ed Harris, he came up here on several occasions on the invitation of Graham Scott, I think, and we had a number of discussions, and there is quite a lot of correspondence between Ed and myself. He's the deputy chief medical officer.

And it became very clear, and Ed was very clear, that the problem -- there were some very severe problems in England and Wales. There was the BPL rebuild, and they just didn't have the plasma that we all felt was needed. So they set up an Advisory Committee of the National Blood Transfusion in England and Wales to look at these issues. And what emerged over the months and months and months was that this committee was going nowhere.

There was really -- the fundamental problem, I believe, there was no political will to actually resolve the issues that they had down there. And if you

chase the database, you will discover eventually it was just disbanded, it disappeared. And just before it disbanded, I resigned. It was a complete waste of a day going down there. And I wrote to Ed and apologised.

Q. Thank you, professor. A particular item I would like to look at is on page 3. It's 8261. Item 14, the bottom of the page we will see is headed "European Community Directive on product liability". I think this is a reference to a Council Directive dated 25 July 1985, which was going to bring in strict liability in the UK, and I think the UK have three years from July 1985 within which to implement the Directive.

We can see the entry in the minute states that:

"It was reported that this Directive would be binding upon the United Kingdom, imposing a legal liability upon the 'producer' of defective products; this liability was not believed to extend to the donor but advice on this point was being sought."

We know, professor, that this Directive was implemented in the UK by the Consumer Protection Act 1987, which came into force, I think, in March 1988, at least in respect of the strict liability provisions. But my question, professor: was this the first occasion on which this European Directive on product liability came to your attention? Was that something you had been

aware of before?

A. I really would be speculating a little. One of the problems I have constantly is I was buzzing all the time with European colleagues, Jussi Leikola, who I know is -- Pim van Aken and so on and others, Alfred Hassig in Switzerland. And these guys, unlike myself or indeed Harold Gunson, were heavily involved in Council of Europe business and deliberations. And very often they would tell me, "Oh, by the way, John, this is coming along, this is coming along". So the question, is this the first time? I honestly, genuinely don't know. I doubt it. But clearly here it's recognised, it's in a minute of a DHSS meeting.

Q. So certainly by this date, November 1985, obviously you were aware of this Directive, which was on its way?

A. Yes, indeed, and you will recall, sir, previous discussions about the whole question of Crown immunity, the whole question of John Watt getting very worried and the directors getting worried as to who's going to be legally liable in this context. We saw ultimately it was going to be taken out, we assumed, of our government's hands and would become part of a European initiative.

THE CHAIRMAN: Could we just pause on the terms of this item because I find them rather strange. It says that:

"Liability was not believed to extend to the donor but advice on this point was being sought."

What was the focus of discussion here?

A. I don't remember, sir, but there is the very famous Scottish -- I was about to say "trial", but it was a Scottish case in which I was heavily involved, High Court, in which relatives sought to get the names and address of a donor that they believed had lied or whatever in giving information to us that was HIV positive. It was very famous, and I had an amazing day in the High Court up there, and the judge eventually ruled that they would not give the name and address of the donor.

So the notion that somehow the donor would be protected in case of liability, as I have always understood, sir, had in the event, as lawyers are always telling me, to go -- there needed to be a case and a judgment made, and certainly I was heavily involved in that. I got a lot of ribbing from his Lordship.

MR MACKENZIE: I think --

A. He is quite famous actually. Sorry.

THE CHAIRMAN: I don't think I want to pursue someone who has given you a ribbing. But at this stage was it just accepted that people like yourselves, the SNBTS, would be liable --

A. Yes.
THE CHAIRMAN: -- and this is considering the extension of liability to the donor?
A. And the donor was protected. We were to be -- discovered that the donor might not be protected but the judge eventually said they are.
MR MACKENZIE: And the point perhaps is, who is the producer of a unit of blood? Is it the donor who donates it and/or the SNBTS? It may have been the short point, yes.

Moving on, please, professor, to [SNF0010135], this is a minute of the meeting of the SNBTS directors on 25 March 1986, if we can please go to the last page, it's 0142. We can see under item 5 "Surrogate testing for non-A non-B". We can see reference to the FDA advisory panel's recommendation in the US in February, recommending surrogate testing in the United States.

It appears to be, professor, that it's that which brought the question of surrogate testing towards the front of the agenda for the SNBTS. Does that seem fair? Is that how you remember it?
A. Yes, I would only go -- you have heard the word "Lieutenant Colonel Tom Zuck" in this Inquiry. Bill Bayer, Kansas City, a remarkable man in San Francisco. Also we were buzzing very closely together.

And, again, I must have been aware that the FDA were moving in this direction. So we didn't sit there waiting for the FDA. We were beginning to think we were going to have to think about this.

Q. We can see after a full discussion, which I think somebody may have mentioned earlier, that the secretary of --

A. Miss Corrie, yes.

Q. Shorthand perhaps for strong opposing views held.

A. Yes.

Q. I'm not sure if that would necessarily apply here?

A. I can't remember. It wouldn't surprise me, sir. It wouldn't surprise me. That's a Morag Corrie code, for lively discussion.

Q. "So after a full discussion the directors agreed to give consideration to funding someone to undertake research. Dr Cash would think about the possibilities in association with Dr Fraser and make some proposals to the directors."

I think the next document of interest, professor, is to go down to England and look at the set of minutes of the English directors in April 1986. This is [DHF0021290].

We can see the names of those present have been blanked out, but there was representation from the

SNBTS. I take it that would have been you, professor?

A. Almost certainly. I can't be sure. What we know is the chairman was certainly Ian Fraser.

Q. If we go to page 7, which is 1296.

A. I should say, I notice that somebody was welcomed as the first RTD to represent Scotland. I just saw that. So the odds are it wasn't me. Just one of my colleagues. But it doesn't matter, we would have been fully briefed.

Q. Thank you, professor, for pointing that out, of course.

Under item 16:

"Should the NBTS carry out a study on NANB hepatitis.

"The chairman reported that this had been discussed by the Scottish directors and that he had agreed to raise it with RTDs [blank] reminded directors of two previous attempts, one by the MRC and one by the Transfusion-Associated Hepatitis Working Party, to study this problem. After discussion it was agreed that this should not be pursued because of lack of time and resources."

Is that consistent with your understanding of the feeling, the opinions of the English directors towards the question of carrying out a study into non-A non-B Hepatitis?

A. Yes, in fact Ian Fraser wrote to me and virtually the

same wording applies.

Q. And if we could then, please, come back to Scotland and look at the Scottish directors meeting of 25 June 1986, which is [SGH0016286]. If we may go to page 5, please, 6290, under topic (i) "Surrogate testing" at the bottom of that, underlined:

"It was agreed to await the outcome of Dr Fraser/Dr Contreras' joint deliberations and to discuss the matter again at that time."

Under (k) "Product liability" we see:

"Following recent discussions and the attendance of a legal office representative at the coordinating group to advise directors on the implications of this legislation, Dr Cash advised colleagues he had taken up the matter with the general manager."

Can I pause there, please, professor, and ask: what advice did you seek or receive in relation to how the Consumer Protection Act may impact upon the SNBTS?

A. What I was keen to know is that if we hadn't, for instance -- there were other things as well -- incorporated surrogate testing into a programme, was this going to be a matter that would be a cause of concern in the event of the patients and relatives taking the service to court? That was a fundamental -- and in that context, if they did, who would be

responsible, held responsible, for this, in the event that we, as operational managers, had said we need to be doing X and we were not allowed to do that? It was to try and begin to get some clarification. And the general manager of the CSA at that time was Jim Donald and he was very supportive to getting that sort of ventilated and discussed.

Q. Do you remember ever receiving any legal advice on the implications of the Act?

A. No. Well, I need to be -- we got two opinions -- I need to be very careful -- from the CLO. One related to -- I think they both actually related to (a) the directors in general, but then John Watt saying, "Are we legally liable in terms of product liability?" We did and I know the Inquiry archives have got both these opinions. If you haven't, I can certainly make them available to you.

Q. When you say the question was "Are we liable?", does that mean the opinion was on personal liability?

A. Yes, it was, I think.

Q. Rather than --

A. I think you may be -- yes.

Q. There is perhaps also a question: would the SNBTS as an organisation be liable as the producer of a donation which caused infection? Is that an issue on which legal

advice was ever sought or obtained, can you remember?
A. I'm not sure. It may have been about -- I can't recall, I'm sorry, sir. Certainly in 1988 there was this extraordinary meeting in the Scottish Office in September of that year, in which the Scottish Office convened a meeting of interested parties to discuss the potential of litigation in relation to HIV.

Chris Ludlam was there, as I recall, the general manager of the CSA was there, I was there, in which people were giving their opinions as to whether if that arose there would be weaknesses in what we had done and not done and so on and so forth. And the whole question of -- and I think I have raised it on a number of occasions -- who was actually responsible for the safety of blood was not discussed. It was raised by me but we didn't get a clear answer, even to the extent of all of us are responsible and we need then to work closely together. It was a difficult meeting, I recall.
Q. In the second half of the 80s, what was the procedure or mechanism for the SNBTS obtaining legal advice? We see reference here, the general manager of the CSA. Would you contact, firstly, the CSA, who would then pass the request on?
A. Yes, we would do that, sir, and I can't remember for sure but I could well understand -- and I'm fairly sure

this was -- it was made pretty clear to me, the notion of John Cash directly writing to the CLO was not appropriate; it was the CSA that should be the route, and I accepted that.

Q. Okay. Then over the page, please, of the minute, at page 6, we can see at the top of the page:

"The question of whether or not BTS would be liable in terms of paragraph 56C of the Directive had been raised wherein it is stated that the producer has a defence if he can show that he 'did not manufacture the product for an economic purpose, nor distribute it in the course of his business' and Mr Murray of the SHHD believed that this statement would not exclude BTS liability in the event of litigation. This and other questions would hopefully be answered when the draft statutory instrument became available for comment. It was noted that much depended also on the result of early court cases."

Et cetera.

The underlined part:

"As had been previously agreed at a coordinating group meeting, Dr Cash would take this matter up at the NBTS Advisory Committee, which included DHSS representation."

Two questions, professor, one small, one larger.

The small question is, what does the underlining in the minutes represent? Simply that that was a matter which somebody was to take forward or to action? Or did the underlining represent a matter of significance or importance?

A. No, I suspect it's my secretary underlining, "do something".

Q. Yes. The slightly larger question: you were to take the matter of product liability up at the NBTS Advisory Committee. Can you remember doing that?

A. I can't honestly remember, sir.

Q. We may come to some minutes which may assist in that regard.

A. Yes.

Q. Thank you. We know that in America surrogate testing was commenced by the various blood bank organisations in 1986.

A. Yes.

Q. Could we then, please, look at a letter from yourself to Dr Fraser of 28 August 1986. It's [SGH0016269]. We will see it's a letter from yourself, professor, on the question of surrogate testing for non-A non-B and you say:

"I have a feeling that as the drums are beating louder and louder in other parts of the world on this

topic the Brits remain fast asleep. I may be wrong but I would like to be better briefed on the matter."

Presumably the reference to the beating drums elsewhere is a reference to America having introduced surrogate testing?

A. Yes. We knew the French -- you know, it was all happening and people are bubbling around and thinking about it. Yes. Yes. I think the Australians got off and they had a bad sticky start but, yes.

Q. And you go on to say that you raised the issue:

"... at a SNBTS directors' meeting some months ago and it was agreed that Dr Fraser would explore the idea of setting up a UK prospective trial. I recall you saying to me that you pursued this at the NBTS directors' meeting (I am afraid I wasn't there) and it went down like the proverbial led balloon."

A. Sorry about the language.

Q. Then:

"I'm bound to conclude that I feel we cannot leave the matter as it is and would value your comments on the suggestion that we (you and I) get down in the near future to plan a 'consensus meetings' designed to look at the issues associated with NANB donation testing."

Et cetera:

"The purpose of the meeting to which all UK BTS

directors would be invited, would be to see whether we can reach conclusions which would enable us to make some clear operational decisions and that these would be transmitted to the various Departments of Health."

Can you remember, professor, was your position at this stage that you supported the introduction of surrogate testing or that you wanted more information upon which to make a decision?

A. (Inaudible). Can I just enlarge a little on that, sir.

Q. Please.

A. The surrogate testing issue, as I'm sure Brian McClelland has told you, it was a hugely important and very difficult position -- situation. First of all, we had no benefit -- no notion, of the real benefit it would bring to the patients in the United Kingdom.

We knew at that time that in the United States' big study there were very substantial variations, geographical variations in the nature of the beast. And the question was: where did the UK sit in this big variation? And when I tell you that that was a key element of data that we were short of, there was the other side, there was the cost, there was the sheer money, and I and a lot of my colleagues were very concerned if we spent £800,000, that wouldn't be extra from the Treasury, that would be taken from somebody

else's pocket in the NHS and somebody would have to pay for that.

So we needed to be able to -- there was a cost related to the whole exercise. There was also a cost, as I'm sure Brian has said, in terms of donors. You know, a vast number of donor a knock on -- Jack Gillon knocks on my door and says, "I am afraid I have some bad news", you know, and the fact of the matter was we knew then that a large number of these people -- had either been out having a good bevy in the pub the night before or overweight or been on treadmills and goodness knows what, but the message from Jack would have been "It's bad news". And exposing vast numbers of our donors and relatives and families to this misinformation when we didn't even know if there was serious benefit to what we were going to do was a great cause.

The second think that was worrying me, in 1987 we discovered for real when the Scottish Office announced it was going on open a private hospital, in Clydebank of all places, to treat wealthy folks from the Middle East. When that happened there was an absolute explosion and for a moment we got into very serious trouble with our donor people in the West of Scotland, such that our blood collection went down. And I recognised that our donor panel, although in Scotland was very strong

numerically, I sensed that if it was messed about with by aspiring politicians and civil servants, we could get into quite serious trouble.

So the other cost was our donor panels. If the word got out, "If you become a donor, you may be labelled", and the guys saying, "We are not sure -- you may be labelled," that will have impact on your dental care, your GP and everything else, and that was absolutely nonsensical, we would be in serious difficulty.

So this was a big decision. It wasn't like HIV donation tests in a sense. This was making a major tactical moral position and we needed the data. So I supported that getting the data very strongly.

Q. Yes. I think the transcript has missed -- you said "Can I just enlarge on that very much the latter". So I think when I had originally asked you -- I think your answer, after I asked the question -- your answer was:

"Very much the latter. Can I just enlarge upon that?"

A. Absolutely, we needed the data desperately. But as we all discovered, it became eventually evident, largely due to the leadership of Brian McClelland, that the tide was going out, that we were going to lose, if we couldn't get engaged, generating the data, it was going to be too late.

THE CHAIRMAN: Mr Mackenzie, the stenographer really needs to stop now.

MR MACKENZIE: We can stop there, sir.

A. Sorry, I do apologise.

THE CHAIRMAN: The stenographer is not terribly well.

MR MACKENZIE: Sir, Professor Cash unfortunately isn't available tomorrow but I think we will be able to accommodate him another day within our forthcoming timetable.

THE CHAIRMAN: We are getting slightly out of time and order.

MR MACKENZIE: We are.

THE CHAIRMAN: I think we will just simply have to make the best of it.

MR MACKENZIE: Yes.

THE CHAIRMAN: Professor Cash has got a little tutorial on toxic shock -- sorry, Professor James has and I don't think this needs to be taken down. So I wouldn't worry about it. It's just for people's information.

(Off the record discussion)

THE CHAIRMAN: I hope that provides some context for the issues which might arise.

(4.12 pm)

(The Inquiry adjourned until 9.30 am the following day)

I N D E X

DR BRIAN McCLELLAND (continued) 1
Questions by MR MACKENZIE (continued) 1
Questions by MR DAWSON 91
Questions by MR ANDERSON 124
Questions by MR JOHNSTON 143
PROFESSOR JOHN CASH (continued) 144
Questions by MR MACKENZIE 144