VOLUME VI

IN THE COURT OF QUEEN'S BENCH OF NEW BRUNSWICK

TRIAL DIVISION

JUDICIAL DISTRICT OF FREDERICTON

B E T W E E N:

HER MAJESTY THE QUEEN

- and -

ALLAN JOSEPH LEGERE

Voir Dire Proceedings held before the Honourable Mr. Justice David M. Dickson at the Burton Courthouse, Burton, New Brunswick, on the 3rd day of May, A. D. 1991.

APPEARANCES:

Graham Sleeth, Esq.)
Anthony Allman, Esq.) Appearing for the Crown
John Walsh, Esq.)

Weldon J. Furlotte, Esq.)
Michael A. Ryan, Esq.) Appearing for the Defence

Bonita DesRoches
Court Stenographer

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Voir Dire Proceedings held on May 3, 1991.

Continuation of Cross-Examination of Dr. John Waye

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(Accused Present)

CONTINUATION OF CROSS EXAMINATION OF DR. JOHN WAYE BY MR. FURLOTTE:

COURT: This is the continuation of the Voir Dire in the same case with everyone present that should be present. Mr. Sleeth is still excused. Mr. Ryan, welcome back. Okay, Mr. Furlotte, you're continuing the cross examination.

Q. Dr. Waye, on a matter we touched on yesterday about the effects that people smoking might have on their DNA and the changes, or the different airs that they

breathe in polluted areas. Did you have time to read that article I presented to you before court?

A. I haven't studied it. I've scanned it.

Q. Scanned it?

A. I just received it.

Q. Yes, and are you familiar with any type of research in the past on this topic?

A. The ill effects of smoking?

Q. Well, not the ill effects, but the effects it would have on DNA structure?

A. Not in that specific sense. It's a carcinogen.

Q. Would you be able to tell me the significance that this research might have on, say, the contamination or band shifting on DNA and in autorads?

A. None

Q. None whatsoever?

A. Not in my opinion.

Q. So does that just change the structure of DNA or does it contaminate it?

A. This article is not about the structure of DNA molecule, it's about adducts.

Q. Adducts, and what are adducts?

A. Adducts is defined in here, creation of adducts. What they did is they broke it down into the nucleotides, made them radioactive, put them into patients, and I believe this has been done in animals, and then they analyze what these nucleotides look like when they come out, and the nucleotides are sometimes altered. That's called an adduct.

Q. They compare it in here with something like -- in the last of the paragraph it says, "To measure DNA adducts in a person the way we now measure cholesterol, it would give an indication of cancer susceptibility.

My understanding of cholesterol is that it's some kind of build up in the arteries of fat tissue. Is that correct?

A. My understanding of cholesterol is probably about the same as your level of cholesterol. That's not my field of expertise.

Q. Now, if these adducts were to bind to DNA as I -- at least my interpretation of this. Would that be a proper interpretation, that the adducts from -- caused from smoking would bind to the DNA?

A. What they're talking about an adduct is the individual building blocks that make this chain, and this is just a test to measure the effects of some of these chemicals in the environment on the building blocks. Sometimes you can alter these building blocks, make a little change to a C molecule, and that's called an adduct, is an altered C.

Q. An altered?

A. An altered DNA molecule or an altered person or an altered RFLP.

Q. So it's not a form of contamination of any kind, it is just --

A. Contamination in the sense that we're talking in the relevance to DNA typing?

Q. Yes?

A. This article comes out saying that smoking, like all carcinogens can have chemical effects on the building blocks of life and comes up with a miraculous conclusion that there's ill effects of smoking.

Q. Okay, and there's no way it could alter the migration of DNA fragments through an autorad, through the gel?

A. Yes, when you look at the bottom line of what we're looking at, as we said yesterday, the creation of an adduct in the incorporation of an adduct could change a base pair in a particular cell and alter that base pair in that cell and the cell that's derived after it doesn't alter the RFLP pattern across four probes throughout an organism, human.

Q. Okay, that's just what I wanted clarified.

COURT: How do you spell that word 'adducts'?

MR. FURLOTTE: A-d-d-u-c-t.

COURT: The article isn't being marked as an exhibit?

MR. FURLOTTE: No, it may go in later when I present my own evidence.

COURT: I've been making notes here. Now, the notes I've made so far this morning read like these: "The article has no effect whatever on DNA testing. Cholesterol is not within the witness's field of expertise, and the article suggests smoking has ill effects, and adducts is spelled a-d-d-u-c-t-s." Now, that is as much as I got out of ten minutes of procedure here this morning. What the article is we don't know. There's nothing -- you know, anyone reading the transcript of this ten minutes this morning would wonder, "What are those people wasting their time about, or spending their time on?" I don't mean just you, Mr. Furlotte, I mean me as the presiding judge and all the other people and the Crown. However, go ahead.

Q. Did any of the matter dealt with in this article deal with your own expertise, any fields of your expertise? You say cholesterol is not your field of expertise?

A. Cholesterol is not in that article either.

Q. No.

A. In that particular article when they talk about nucleotides, I understand that; when they talk about altering the structure of a nucleotide, creating an adduct.

Q. That's an area of your expertise, is it?

A. It's not an area of my expertise. I can certainly understand what they're saying.

Q. To get back to case law we were dealing with yesterday, Dr. Waye, and I want to see what the different misconceptions that come out of expert testimony in the past. In the Spencer case at page 782, the judge found that -- in a statement he says, "When the radioactive probe finds an exact complementary base sequence, the probe binds to that location causing radioactivity to accumulate at the bonding site." So I think we've determined that that is a misconception, probes don't necessarily bind to the exact complementary sequence?

A. They can.

Q. They can, but they don't always.

A. They'll bind to sequences that are of high homology. They're either identical or they're very close to identical.

Q. Yes, but when we're expanding the technique and the limitations of these probes, the probes can also find, as I think we established yesterday, they can also find fragments that -- and bind to fragments which are not their exact complementary base pairs?

A. Yes.

Q. And they can be out by maybe anywheres, 85, 75 per cent?

A. Out by that much?

Q. Not out, well, let's say they could be out by 15 to 25 percent?

A. Depending on the conditions that you used, but not these conditions.

Q. Depending on your conditions, how far out could they be?

A. You want an exact number and I have to, again, you'd have to give me the probe, give me the probe sequence, give me the target, give me the target size, give me a lot of specifics to answer that question. It wouldn't be out by very much.

Q. Out of any probes that you've experienced with, out of all the probes that you've experienced with, what was the greatest amount that they would have been out, any one probe would have been out?

A. I'd have to go back to the lab and start looking at the target sequences that have been sequenced, look at the probes that have been sequenced, scan my notes over the years and do some calculations. It's not a very interesting question. It's not a question that I've ever really pondered to a great extent.

Q. It might not be interesting to you but it might be interesting to somebody else.

A. I just told you what I'd have to do to come up with that interesting answer.

Q. The bottom line is, Dr. Waye, probes do not always find the target that they're set out to find, do they?

A. You're reading a summary there where people are taking a procedure and they're simplifying it and saying the probe recognizes its exact same thing. The restriction enzyme does this exact same thing. Everyting is mentioned in absolutes. I've already told you that the probe will recognize its exact match. It'll also recognize things that are near matches. There's a tendency when people summarize an area of science to express it -- oversimplify it and express it in absolute terms. It's not the way it is then.

Q. Would you admit that when the judge found, as a finding of fact, that radioactive probes finds an exact complementary base sequence, that either he made an wrong finding of fact or he was presented improper evidence in court?

A. He overstressed the truth. He expressed one truth, it will find its exact same thing. He also had it oversimplified that a probe is like a piece of velcro, it'll find the other piece and it'll stick.

Q. But since we don't have the benefit of a transcript of that trial, we don't know whether it's the judge that overstressed the truth or it was the expert witness who overstressed the truth?

A. It would be fair to say that I'd probably have to look at the transcripts and figure out just what he was reading to come to that statement.

Q. Now, the judge also found in Spencer at page 72, he said that, "If the semen sample and blood specimens are from the same person, the probes will bond with DNA segments of identical lengths in identical positions resulting in two identical patterns of bends."

That, again, is a misconception, is it not?

A. Again, he's overstressed the situation. If everything is perfect and the technique worked perfectly and there was none of the what-ifs and could-bes, the fragments -- and they are from the same individual -- would be the same length and they'll migrate to the same position.

Q. I believe you mentioned yesterday that the system simply wasn't built for the purpose of -- isn't meant to find these types of patterns, are they?

A. To find these -- the system is not built to find these types of patterns?

Q. It's not built, or the purpose is not meant to be so precise in having identical lengths and identical positions?

A. The system is not designed to define the -- when I analyze a sequence, to define its precise base per measurement. That's what the system is incapable of doing. The system is quite capable of detecting pattern.

Q. Originally, isn't that what the forensic labs hope that the system would do?

A. No, they wouldn't have used that system.

Q. Isn't that what the forensic labs came to court proclaiming what the system could do?

A. Not to my knowledge.

Q. Isn't that what it appears that happened in the Spencer case?

A. They came to court saying that their system was capable of base pair resolution and defining matches that way; I imagine Mr. Spencer is a free man.

Q. What benefit would proficiency testing have to a laboratory?

A. Proficiency testing at the end of a person's training period where they're learning a technique would be an opportunity to have them run through the technique and the test is set up so that you know what the answer should be and if they've learned the technique properly and applied the technique properly they'll get the expected answer.

Q. Are you aware that in the past, proficiency testing had been done on Cellmark's lab.

A. Yes.

Q. And are you also aware that out of 44 samples, there was one incorrect match?

A. I can't remember the exact number of samples. There were somewhere around 50, I believe, they were given.

Q. Somewhere under 50?

A. Somewhere around 50 they were given. I can't remember how many they reported on, but there was --

Q. Around 50, under 50, there was one incorrect match if --

A. There was one incorrect match called, yes.

Q. Which, if such an error made within a lab, could very well convict an innocent person?

A. They mixed samples up. As I said yesterday, if somebody mixes up samples, analyzes the blood sample twice, things of that nature, you fundamentally apply the test improperly and you could get the wrong answer.

Q. So that's at a rate of two percent?

A. For that particular --

Q. One in, say, one in fifty?

A. For that particular proficiency test, for that particular investigator, on that particular day. That would be that error rate.

Q. Which, depending on how many proficiency tests you would conduct in a lab, that could very well be an average or it could be higher?

A. I think you'd have to conduct more than one proficiency test.

Q. But at least it's quite possible that labs do make mistakes, as such?

A. You just showed an example, yes.

Q. And that applies to every lab, just not Cellmark's lab?

A. The possibility of somebody mixing up a tube.

Q. How is that deficiency built into your data base when you draw out your conclusions at the end, the statistics of probability? How do you allow for these errors of this kind? How can you be conservative? Or is this built into your data base to allow for this type of error?

A. You perform the test properly and you report the test as conducted properly.

Q. How do you give an accused person the benefit of this type of error?

A. I'm trying to understand what you're saying. You want me to somehow statistically correct for the possibility of somebody mixing up tubes?

Q. It would be nice.

A. Again, I'm not going to speak as I had set it up, but to follow your line of logic, you'd have to start off with the premise that it's a possibility that I mix up the tubes or I didn't mix up the tubes.

At the end of the experiment you come up with a conclusion that I identify a sample coming from this person, but I could have mixed up the tubes, or I couldn't have mixed up the tubes. Following that, again, logic, and I use that loosely, you'd have say -- I'll be conservative and I'll assume each and every test I performed, I performed incorrectly and I'll throw out all my results, which makes going to work fairly boring, or useless.

Q. But it's relatively impossible for an accused person to come to court and prove that tests were mixed up? Would you agree with that?

A. To prove which?

Q. That proved that there was a mixing of samples before the tests were conducted? It's virtually impossible for an accused person to come to court and prove that the lab technician somehow mixed the samples? Will you agree with that?

A. Yes, they generally don't come to the lab and watch what we're doing.

Q. Okay, so what I'm concerned about is if you come to court and you say, well, the statistically probabilities that there's only one chance in five million or twenty million or thirty million that -- or you can go 500 million -- that any individual -- that somebody else out there could have the same DNA as the accused person. What's the benefit of all that if there's a tube, or a five percent chance that you're wrong to begin with?

A. I think you have to look at what we're actually reporting. At least, in my experience what I've reported is I've taken -- I've used this technique,

I've found my one or two band patterns across five probes. I've done the statistical calculation, and I report the frequency of that profile in the population, period. Now, whether I analyzed the blood sample twice by accident, which I'm confident I didn't do; nevertheless, even if I did do that, I'm reporting on the frequency of a profile. The other issue is really a yes or a no issue. It has nothing to do with that frequency.

Q. Okay, let's get back as to what the value of that statistical calculation would be. When you say there's one chance in five million, one chance in 500 million, that somebody else out there would have the same profile as an accused person, what population are you comparing that with?

A. It'll be stated in your report.

Q. You're not even comparing that with the world population, are you?

A. Again, it'll be stated in the report. Read the sentence at the end of the report. It'll give you a reference population.

Q. No, that's reference population for your data base.

A. Correct.

Q. But when you're comparing -- if you're comparing one in a million, that there's only one in a million chances somebody else out there is going to have that or -- if you're comparing that with a population of five million people, it doesn't give much of a chance for somebody out there to have the same genetic structure, profile, only five chances, would that be right?

A. The point of the test is to show if things are rare or not rare. If the numbers at the end of the

report said one in five million, that doesn't mean that there are five people in Canada that have it, it means that the chances of finding somebody with it. You'd have to look at an enormous number of people.

Q. There's a chance of finding five people out there with it?

A. You have a very simple way of dealing with probabilities and most people fall into this thing. They'll say, "There will be five people out there that have this."

Q. No, no, there doesn't have to be five. I agree with you.

A. And conversely, people really enjoy when you exceed five billion because then you draw the eroneous conclusion that there isn't another person on earth with that pattern, and that's just oversimplifying probabilities. What we're trying to do is to find whether something is rare or common, period.

Q. Would it be safe to say that specific DNA test results are only as reliable and accurate as testing procedures used by the particular laboratory and technician?

A. What was the beginning part of that, testing procedures or the results?

Q. I'll re-read it. That specific DNA test results are only as reliable and accurate as testing procedures used by particular laboratory and technician?

A. That applies to everything. You can have a very reliable procedure if done correctly will give you a reliable answer each time. If you put it in the hands of an incompetent person, you've taken a

reliable procedure and you're going to, on occasion, get unreliable results because you've put it in the hands of an incompetent operator.

Q. So we should be looking at one, let's start with the technician who does the testing, the possibility of his making an error?

A. If the technician did the test. Generally the technicians don't do these types of tests.

Q. We should be taking that into consideration, though, shouldn't we, the possibilities?

A. Well, if the policy of the lab is that the technicians aren't doing the tests, it's not really a possibility.

Q. You're assuming an awful lot again, Dr. Waye.

A. I can only testify on my own experience and when I did case work at the R. C. M. P., I was the case worker, not the technician, and the technician wasn't involved in the test procedure. So the possibility --

Q. I know you have a lot of confidence in your own work, but that doesn't mean everybody else is as competent as you are.

A. The point is whether my technician is competent or not, they didn't do the test, so their competency doesn't enter into my test results.

Q. Is it true that you don't believe the F. B. I. technicians or whatever, that they don't do their work properly, at a reasonable level of reliability?

A. Is it true that I believe that the F. B. I. --

Q. Yes?

A. No, that's not --

Q. How much faith do you have in the F. B. I., the F. B. I.'s technicians and the way they do their work?

A. I have a lot of faith in them.

Q. You have a lot of faith in them?

A. Yes.

Q. So you don't think that they do theirs improperly?

A. No.

Q. Is it also important that any test procedure used by the laboratory possesses a high degree of accuracy and reproducibility?

A. Yes.

Q. What would you call a high degree?

A. Well, again, you'd like a test to do what it's designed to do and you'd like it to do it in a reproducible manner. The degree of accuracy would depend on the test.

Q. We have discussed the degree of accuracy with the -- I believe the matching window reflects your degree of accuracy, is that right?

A. They are related issues, yes, they're related issues.

Q. Now, for scientific purposes and what would be accepted in the general scientific community, what would be a high degree of reproducibility, percentage wise?

A. You want to express it relative to these tests?

Q. Well, you keep telling me that everything is -- all your procedures and studies and tests are all accepted in the scientific community and I assume that that is based upon the degree of accuracy and reproducibility?

A. No.

Q. What degree of accuracy and reproducibility are we talking about?

A. Again, we're talking reproducibility and accuracy

within a system. If I analyzed samples that I knew to be from the same person over and over again, I expect to get the same answer. That's a reproducible test. That's relevant to what we're talking about here.

Q. If you don't get the same answer every time, do you abort the test? Or would you say it's not reliable and chuck it out the window, or do you try to explain it away?

A. If all of a sudden I found out that RFLP procedures gave different answers on different days when I analyzed the same sample, if that were to happen, I probably would publish a paper saying, look, the medical field and the world has been using a technique that's flawed.

Q. It's what?

A. Flawed. If I can't get the same answer two days in a row, there's something wrong here. But it's not the case.

Q. Would you try to draw any conclusions on the results that you did get, or would you just abort it?

A. In science if you have a test that will not give you -- will not produce results in a reproducible manner and will not do what it's designed to do, you can't use that test.

Q. You can't use that test? Okay. But that wouldn't stop you from using the same procedure and doing tests on, say, other cases or other experiments using the same procedure over again?

A. Why would you use a test if it doesn't work?

Q. Good question.

A. It's a great question. As a scientist you wouldn't use the test, and it's a relevant question when it comes to this issue, I believe.

Q. When you set up the lab at the R. C. M. P. in, what, 1988 and 1989?

A. Yes.

Q. When was it ready for conducting tests for forensic purposes?

A. Accepting cases?

Q. Yes?

A. It was ready when we accepted our first case.

Q. What did you do to get ready for that?

A. Get ready, we put all the components of the test together, all the developmental work in deciding which probes we would use, which enzymes we would use, how variable these probes were , how they perform on casework samples. All that data was done in conjunction with -- was derived in conjunction with the working group and our own lab.

Q. And who established the protocol?

A. The written protocol?

Q. Yes?

A. The first one? I wrote it.

Q. You wrote it up? With the assistance of anybody else or just yourself?

A. With the input of other individuals. You rarely write anything in science by yourself. The adage of two minds being better than one, or a half a dozen minds being better than one.

Q. Would it be safe to say you copied much of it off, say, the F. B. I.'s protocols?

A. I certainly didn't -- I didn't invent the RFLP

procedure. I certainly didn't copy anything from the F. B. I. They didn't invent the procedure either. The techniques have been in existence for years and it's a simple matter to sit down and write down what you know. You're not inventing anything.

Q. No, but what I understood from your direct testimony was that the R. C. M. P. and the F. B. I. are attempting to establish a common system so that they're almost interchangeable?

A. They have to be conceptually compatible so we can compare results at the end. The means to obtain that end product, though, the protocol, the order -- not the order of events, but the ingredients or how you mix it up or the time that you might do this, et cetera, et cetera, those types of fine details along the way, those were put into protocols and those were done independently in both labs and those protocols are different.

Q. So could you take your, if you wanted to run samples, could you run to the F. B. I. lab and conduct your tests as the way the F. B. I. is set up and you're set up?

A. Could I go to their facility and use my protocol?

Q. Use your protocol?

A. Sure.

Q. Or if you were halfway through a test and for some reason or you weren't able to finish it, or you weren't able to run all the probes in your test, you could take your -- I don't know what you call it, the gel or the --

A. Membrane.

Q. Membrane? You could take that to the F. B. I. lab and finish your test?

A. If I had reason to go down to Quantico and finish a test, sure, I could go to Quantico. I could go anywhere that had the proper facilities and do the experiments.

Q. How long was the R. C. M. P. lab in Ottawa under construction?

A. Again, there was a lab in existence when I first came to the R. C. M. P. and I worked in that lab until I left. After I left, there was extensive renovations, essentially an expansion of that lab. We kept the existing space that I worked in while I was there and we expanded and had more space added to the lab and some renovations done.

Q. When did you leave the lab?

A. January 15, 1990.

Q. So you were there when the tests were conducted for this case?

A. If they were conducted before that time, I was there.

Q. What were the conditions of the lab at that time, still under construction?

A. When I was there?

Q. Yes?

A. There was no construction going on when I was there.

Q. And you say you left when, in January of 1990?

A. January 15, 1990.

Q. Were any of your experiments that you conducted and you wrote up the articles for peer review, had any of that been published at the time you -- before you left the R. C. M. P. laboratory?

A. Any of those articles before I left?

Q. Yes? Either on --

A. You usually remember when an article was submitted and when an article was accepted, those are the relevant dates for a scientist, when something gets accepted for publication, then depending on the journal, you can wait anywhere between six months or a year, even more, before it actually comes out in a form that gets distributed in libraries, et cetera.

Q. I'm just wondering what kind of publications or peer review that your work had before the tests, the lab conducted any test in Mr. Legere's case?

A. Again, other than one of those papers that was entered, they were all written while I was at the R. C. M. P., submitted while I was at the R. C. M. P., peer reviewed while I was at the R. C. M. P. and accepted for publication while I was still with the R. C. M. P. There was a paper on casework examples and quantification that's coming out in the Journal of Forensic Sciences that I wrote while I was at the R. C. M. P. but submitted shortly after I left the R. C. M. P., and it was accepted and will be published, naturally, after I've left there.

Q. During your protocols of 1989, I notice there's nothing in the protocols which helps or assists or acts anyway as a guideline for the interpretation of the autorads, is there?

A. No. Actually, I'd have to look at it. It's been a long time since I've looked at that protocol. I don't --

Q. But you wrote it up. You should remember it.

A. I write a lot of things and I can't even remember the

date it was written, 1989, a long time ago. I don't believe there's a long section on interpreting autorads.

Q. And there's nothing in that protocol which is directed towards the extraction of DNA from human hairs?

A. Again, I'd have to look at it. There's extraction protocols in that, extraction methods in that protocol that you could use to extract from hair and I'd be surprised if the word hair is not mentioned in it as a preface to any of those extractions. But I'd have to look at it again. Could I look at it?

(Document 42 passed to the witness.)

Q. I show you Exhibit VD-42 which is the forensic protocol dated October, 1989. Would you check to see if there's anything about the extraction of DNA from hair samples?

A. Again, the protocol on page 7 and 8 is used to extract from hair.

Q. Is there any mention of hair?

A. Not that I can see. There's no mention of bone marrow, dental pulp, or any other thing that would work with this procedure as well. It's a cookbook for the forensic scientist.

Q. So you use the same procedure whether you're extracting DNA from hair, semen, or blood tissue or skin tissue?

A. This protocol would work on those tissues.

Q. It would work on it, but how effectively would it work on it?

A. Those are things that went into the formulation of the protocol. If you try various methods on various things, you demonstrate that it works, and you

write the protocol. It does work on those tissues. It does work on hair. I've done it. I've done it on my own hair.

Q. I show you Exhibit VD-44 which is the DNA typing protocol of the R. C. M. P. dated January, 1991. Did you have anything to do with the drawing up of that protocol?

A. Other than the fact that it's an extension of the first one I did, I didn't write this one myself, no.

Q. Is there anything in that protocol about the extraction of DNA from hair samples?

A. Yes, I think I explained before that as these protocols, volumes went from volume one to volume three, the audience that the protocols were addressing changed more from the experienced person who developed the protocols just to have something written down that they can refer to and exchange with other people. It's more a teaching guide, and as they did that, they elaborated a little more on each step. Now, there is a subtitle here, Recovery From Hair Roots.

Q. I will again show you Exhibit VD-42 which you said there is a section there on how you generally extract DNA from hair samples. Can you tell the Court whether there was any difference?

A. One has got a specific title, Recovery From Hair Roots. The other is the generic protocol they could use, again, from dental pulp to bone marrow to hair roots, so the first sentence is definitely different. One says cut the stained material, or whatever, into small pieces and put it in the tube. The other is a little more specific. It tells you which part of the

hair to cut off, what would happen if the hair was mounted, handle it with fine tweezers, apply it to the tube, et cetera, et cetera. One is a little more specific than the other and one deals directly with hair. That's pretty much what I said before, I think.

Q. Is there any difference in using, say, different chemicals or different amounts in extractions, different amounts of chemicals in extraction of DNA from hair roots?

A. "Place root sheath in a 1.5 milliliter micro centrifuge tube." "Place stained material in a 1.5 milliliter centrifuge tube." Those are the same, a micro centrifuge tube and a centrifuge tube, and it's the same volume so we're okay there. "Add 400 microliters of stain extraction buffer and ten microliters of proteinaseK." Back to the hair; "Incubate overnight (16 to 18 hours)." "Incubate 6 to 18 hours." It goes on, "Purify by organic extraction, ethanol precipitation as described in section 1," which is the previous section which is adapted from here, and I don't think there's any need to read through what they tell you to do for both procedures. Steps, would be 4 through 11 in a generic protocol.

Q. So there's no difference. It's --

A. Well, of course, I just pointed out a lot of differences. One is talking about a specific issue and one is trying to express things in a general way.

Q. But as far as for the extraction, there is no difference for the chemicals used or --

A. If you give me a situation where it's not -- the

starting material is not comparable to a hair root/ Like, in the past I've been given things like an entire belt, a man's belt, and extracting DNA from that. Well, I obviously can't take the same approach as extracting from a hair root. But conceptually, I'm going to use the same chemicals, the same procedure. I'm going to break the cells, I'm going to get the DNA out, and I'm going to purify it and it'll end up in a little tube like this, with a small volume. I can't get that belt in that 1.5 milliliter microfuge tube so there's got to be some differences.

Q. If you use the same volume of DNA from a hair root, and the same volume of DNA from a blood stain and the same volume of DNA from a semen stain, should you end up with the same band intensity when you run the test?

A. Volume has very little to do with band intensity. Volume is tied fairly closely with concentration. You can have a large volume of a very unconcentrated DNA solution and you don't end up with that much DNA. You can have a small volume of a very concentrated DNA solution and you have more DNA. Volume itself is not the parameter you want to look at. It's one of the parameters.

Q. That's why you run the test first for, what, quantification and then volume?

A. You want to know how much DNA. You're really not interested -- you want to know how much the volume is, but you can control the volume. You purify the DNA and you add buffer to it. So whether I purify DNA from the belt or the hair root, at the end of the procedure I will add a predetermined amount of solution to it and bring it to the same volume.

That doesn't mean I have the same amount of DNA extracted from both of those exhibits.

Q. Is there any way that you can tell beforehand, before you run the test, as to the intensity of the bands that you might expect?

A. Well, you quantitate the DNA so you -- if I know at the beginning of the test it had very little DNA, that gives me a formal expectation of how well the test will perform and how intense those bands will be at the end.

Q. How much do you need to get, say, good intensity bands?

A. Good intensity bands, that's fairly subjective phrasing.

Q. I've seen a lot of subjectivity in your tests.

A. The point is what I would call good, intense bands, you might claim not to be able to see. But if we could come to an agreement that, say, those bands, you know, that's a schematic, but if I had bands that I could look across the room up against a white background like that and see clearly from this sort of distance, then nobody would argue that they're there, et cetera, et cetera. If they were obtained in a reasonable amount of exposure time on the X-ray, I would know that I wasn't dealing with one nanogram of starting material. I would know the range of starting material that I had. I probably had over a microgram to start, or near a microgram, which is a thousandth nanograms. It's all experience.

Q. Did you set up any standards that would be used by the R. C. M. P. lab to determine a match?

A. Standards to determine a match. I'm having a hard time figuring what you want.

Q. Well, it seems to be a big topic of discussion in the OTA report. A lot of scientists feel that standards should be set in determining matches. Did you set up any standards when you set up your protocol?

A. When I was working at the R. C. M. P., did I set standards for what I would call a match?

Q. Yes?

A. Yes.

Q. What was it?

A. We've gone through what my match criteria would be when we were looking at things. I went through numerous examples yesterday.

Q. Okay, if you could see a visual difference, you would say it was inconclusive or an exclusion. That was one. Any others?

A. Present me with a scenario. Present me with all the different, would you call this a match, wouldn't you call this a match.

Q. If you're going to ask me to present you with the scenarios and to conjure everything up, I suspect, Dr. Waye, we're going to be here a month. My question is either you're -- I feel that if you're going to cooperate, we can get this over early. If you're going to drag it on --

MR. WALSH: Objection, My Lord. Mr. Furlotte is entitled, My Lord, to ask any questions he wishes that's relevant provided he provides an adequate foundation for the witness to answer.

COURT: Well, Mr. Furlotte, you know that. Let's continue.

WITNESS: Again, you look at the patterns --

MR. FURLOTTE: You're the expert. You set up the protocol --

WITNESS: -- I look at the patterns --

MR. FURLOTTE: -- and you set the standards. Can you give me any other examples of standards you've set aside from the one of the visual recognition of an exclusion or an inconclusive match?

A. That's fairly all-encompassing. You look at the patterns, you assess whether they're a match, an exclusion, or an inconclusive. That's done both visually and it's done with computer-assisted measurements. That's fairly clear and that's standard.

Q. So that's the only standard matching criteria that you formulated?

A. That's several standards and that all goes into a match criteria.

Q. Okay, there's no other ones? That's it?

A. There's not multiple-match criterias? There's multiple things that go into the formulation and I just listed them, and I listed them many times yesterday, and that all comes into what we call the match criteria, and I don't have multiple match criterias, one for one day and one for the next if that's what you're getting at.

Q. Did you set up any standards as to how many probes would be necessary before you would call a match which you would consider sufficient enough to establish identity?

A. Again, that's something that -- that's not my concern whether somebody is going to take my conclusions and "establish identity". I'm going to provide them with information as to how common or rare this profile is. If I conduct two tests and I'm incapable of doing other tests, then I report that

something is at a frequency of one in 1,000, well, then, that's all I can do and it's the job of other people to interpret what that means. Obviously it doesn't mean that it's so rare that it couldn't occur in another individual, but obviously it doesn't mean that everyone in this room is going to look like that as well.

Q. You did not, then, set up the standard that it would take at least three probes to establish identity?

A. Again, I'm not concerned with how people are going to interpret --

Q. Just answer the question, Dr. Waye. Did you or did you not set up a standard --

A. No.

Q. -- that it would take at least three probes, a match on three probes before you could establish identity?

A. No.

Q. Are there any benefits derived in using either a what would call maybe a short gel or a long gel; a 15 centimeter gel or a 20 centimeter gel, or a 30 centimeter gel?

A. Depending on what you want to -- what you want the gel to do, how you want it to perform. There's different gel sizes. There's gels that are as small as five centimeters long we call the minigels, and there are gels that are enormous, as you mentioned, 30 centimeters long, depending on what you want the gel to do. If you want the gel to separate fragments that are 20,000 versus 21,000, those size ranges, you're required to run long gels. That's just the science. If you're only interested in resolving very small fragments, you can run shorter

gels. It depends on what you want to use the gel for, sir.

Q. Did I understand correctly that when you say you run shorter gels, it's not so much the length, the length of the gel itself, but the time that you have it under electrophoresis?

A. Well, they're related issues. I can use a short gel if I'm interested in the example I gave before. Do I want to tell the differences between 21,000 base pair fragment and a 20,000 base pair fragment. I can use a short gel and run it for a long period of time. I won't be able to derive information about fragments, say, that are less than 10,000 base pairs because I run the gel a long time and all those fragments will be outside, they'll have run through the gel. But if I'm only interested in looking at these very large fragments and discriminating between them, I can use a short gel to accomplish what a long gel would have done.

If I wanted to have the information from beginning to end, from 2,000 to 21,000, I'd have to run for that same period of time, but I'd have to use the long gel, so from 2,000 to 10,000 wouldn't be run through the gel. So it depends on what you want to do with the gel.

Q. Now, in Caldwell at page 486, the judge found that all of the four probes -- or I'm sorry, at page 440, the judge found that all the four probes used by Lifecode in this case produced an average of two dark bands on a white column. Now I understood that it only produces two bands, sometimes one, but sometimes two. Is that correct or is there an average of two? Can it sometimes produce three or

four, or would that be due to contamination or mixed samples?

A. Again, he's probably ill phrased the statement, but with these types of probes you have one or two bands per individual. He said on average you get two bands, it's not that you get three or one fifty percent of the time and the average is two. He's saying most often you get two bands.

Q. Okay, so you never get either three and one to average two; like here on the average most often you would get two bands?

A. Again, you want to talk absolutes and there are probes and there are enzyme combinations that you can analzye these very loci and obtain three bands in a single individual. You can't generalize across the world like that.

Q. There are probes where you can get three bands? I thought one band came from each parent, one band from the mother and one from the father?

A. Correct.

Q. Or am I mistaken here, am I misunderstanding?

A. No, you've understood that part of it fine.

Q. You can develop probes to get three banded patterns, is that what you're saying?

A. There are situations at loci like this where you could get a three-banded pattern, so that's why I say you can't generalize. What we're saying is the system is designed to use probe and enzyme combinations where you're going to get one or two band patterns. There are instances, if I can give you an example, it's not because it's flawed and people have three chromosomes, two inherited from --

Q. That's not it? Okay.

A. If I picked -- if I had the wrong enzyme or the wrong probe combination, or I built the system incorrectly, you could find an example where you either -- where you don't get one band on each chromosome. Sometimes you get two bands on one chromosome, so you can inherit two from your father and one from your mother and what you see in the gel is three, and that's just due because you have a restriction site within the locus. Normally you're measuring -- you have two sites outside the locus and the probe recognizes that fragment. If you had the same fragment recognized by the probe and there was a site within there, well now the probe is going to recognize two fragments at that locus.

Q. Even though it's not the same site, or is it the same site?

A. It is the same site. It's the same spot on the chromosome but it'll pick up two fragments just because there's a restriction site in the middle. You pick your probe and your enzyme combination so you're not inviting that to happen. You want to have formal expectations of what the results will look like for a single donor.

Q. So Lifecode's probes wouldn't do that, or your probes wouldn't do that?

A. Would never give a three-banded pattern?

Q. Yes.

A. I just described an example, and that's a restriction site polymorphism, again, another genetic variability where you're going to get a three-banded pattern. It's not the usual occurrence, that's why the system was set up like this.

Q. No, but I understood you to say that the only way that that could happen is if you picked an enzyme that would do that or you picked a probe that would do that, or you had an improper system? You know, you hadn't built your system up properly. That's the way you would get three-banded patterns?

A. No, you design the system so the formal expectation is that you get one or two band patterns. I told you if you have a restriction site polymorphism -- remember, these polymorphisms are in the genome and you have many thousands of restriction site polymorphisms and I have many thousand restriction site polymorphisms. If one of these restriction site polymorphisms is in the middle of a probe binding region, you can get three bands in a person, and it doesn't matter how you design a system, people are polymorphic and there's chances that you could get a three-banded pattern. In fact, it has been observed.

Q. With your probes or within your system?

A. With one of the probes in a very small percentage of the population there is a polymorphism that could generate a three-banded pattern.

Q. Were you able to distinguish whether that problem is with the design or your system, the enzyme you used, or the probe you used?

A. I told you, you design a system, both probe and enzyme, that will give you one or two bands per individual, the exceptions being when you have a polymorphism that creates a restriction site within the probe binding region. People are polymorphic. You don't pick a system that, say, 50 percent of the people in the population will have a polymorphism so 50 percent of the people will have a three-banded

pattern. You wouldn't pick that system. You'd pick a system where it's a rare event or it doesn't occur at all.

Q. You also told me that in science if you develop a system and you don't get the expected results, and you find out there's something wrong with the system when you don't get what you expect, you don't use it any more because it's not scientific?

A. That's not an unexpected result. I just told you that people are polymorphic. If people were all the same, and they should all have a two-banded pattern, they were all the same and I got a three-banded pattern, something is wrong. People are polymorphic. That's not an unexpected result, it's not a wrong result, it's not a flawed result. It's an explainable result, a publishable result. In fact, we did publish that result.

Q. Yes, I have a copy of it. Maybe I'll put it into evidence now. Is it titled "Identification of complex DNA polymorphisms based on variable number of tandem repeats (VNTR) and restriction site polymorphism?

A. Yes.

Q. Produced by John Waye and Ron M. Fourney?

A. Yes.

Q. Do you mind if I put that into evidence?

MR. WALSH: Oh, certainly not. In fact, I would have on redirect if he hadn't.

COURT: Do you want to see it and identify it any better? (to the witness). Perhaps you should show the witness.

(Document handed to the witness.)

Q. That is your copy of your article?

A. Yes.

COURT: That will be VD-52.

(DOCUMENT MARKED AS EXHIBIT VD-52)

Q. I understand in that research you found that there were three or more fragment lengths found when you used the probes D4S139, is that right?

A. In the examples on that paper?

Q. As an example in that paper.

A. We found on rare occasions individuals that did not produce the general and the more common expectation of one or two. That included three and more, yes.

Q. Three and more and that was on the D4 -- one of the probes was D4S139?

A. That was the probe.

Q. That was the probe, and any other probes besides that one you found that one?

A. I think there's one other probe mentioned in there with a different enzyme, so it's not --

Q. With a different enzyme, so that doesn't have anything to do with your system or any test conducted in this case?

A. No, generally when you make an observation and you publish it, it's probably mentioned in the discussion part of that paper, but you try to draw parallel situations either from the literature or from past work and that was from work that we did in selecting our probe and enzyme combinations.

Q. I also understood you to testify that it's only good science that when you find a problem either with an enzyme, a probe, or design in the system, then you abandon it?

A. No, I --

Q. Did you say that, or did you not?

A. That may have been what happened to that statement when it went into your mind and it came back out. That's certainly not what I said. I said if you demonstrate that the system that you have isn't reliable, that you don't use it, you don't have a reliable test. What we published there is an observation, a reliable observation, a scientifically valid observation, an expected observation.

Q. So you're saying it's still reliable, then, to use that probe?

A. For forensics?

Q. For forensic.

A. Or any other purpose? Sure.

Q. Are you aware that the F. B. I. abandonned that probe?

A. Not to my knowledge.

Q. So you don't know whether or not the F. B. I. abandonned the probe, 4DS139 (sic) because of that problem?

A. As I said, not to my knowledge. I haven't talked to Bruce Budowle in quite some time. I'm not aware of that.

Q. Are you and Bruce Budowle still on good terms?

A. I said I haven't --

Q. Or is he upset at you because you assisted defence in the Yee case?

A. Not at all.

Q. Not at all?

A. Not to my knowledge.

COURT: I wonder if we could take a break here.

(Accused escorted from courtroom.)

(Court recessed 10:55 a.m. to 11:15 a.m.)

(Accused Present.)

COURT: We were going to finish with Dr. Waye or stand him aside this morning. Did counsel have any suggestions on what time we conclude before lunch?

MR. WALSH: My Lord, after breaking yesterday at suppertime, we meant to mention this to you before we started this morning, when we broke yesterday at supper, Mr. Firlotte and I had a discussion as to what would be logistically best and for the flow of the evidence and we reached an agreement. We would hope that you would indulge us. We would ask that Mr. Furlotte be able to continue his cross-examination until when the Court considers the end of the day. At that point in time, the Crown would enter into evidence the autorads done in the particular case here along with a summary of the items and what lanes each of the items fit. There will be a written summary. That will be entered into evidence along with the report, a written report of Dr. John Bowen that he made as a result of the testing in this particular case. That would enable the Crown to have an adequate foundation to question Dr. Kidd, an adequate foundation, I would think, for my subsequent witnesses, and then Dr. Bowen next week at some point would take the stand and be subject to examination and direct examination with respect to the evidence that had already been entered in. We would consider that to be an appropriate course to take. That would enable --

COURT: I'm sorry, when Dr. Bowen came on he would --

MR. WALSH: Well, when Dr. Bowen takes the stand, and obviously he will be able to --

COURT: Yes, but you would have direct examination?

MR. WALSH: Yes, direct and cross on that, but at this point in time my main concern is to ensure that I have an adequate foundation for the witnesses that are to follow and if we get into a situation where we stand Dr. Waye aside at noon time and then put Dr. Bowen on and stand him aside, it becomes a problem for both the Crown in trying to present the evidence and in terms of me scheduling witnesses. We have found that this might be the most appropriate and reasonable way if you would agree.

COURT: How long would it take you to do that this afternoon?

MR. WALSH: To enter these things into evidence?

COURT: Yes?

MR. WALSH: Probably five minutes, and we could go to as far as the Court wishes to go today with the cross-examination of Dr. Waye, and then I can plan and have some reasonable way of telling Dr. Waye when he would be expected to have to come back here because Dr. Waye, as you are aware, runs a lab and children's hospital and it's very important that he not be away too often, or too long.

COURT: Maybe Mr. Furlotte will surprise us and finish this afternoon.

MR. FURLOTTE: Have I surprised you yet?

COURT: Yes. Agreeably. Everybody has.

MR. WALSH: Well, if the Court would indulge us, we would like to do that, My Lord, if you would agree.

COURT: This is agreeable to you, Mr. Furlotte? This pattern is?

MR. FURLOTTE: Yes, that's agreeable to me.

COURT: Well, let's go on now to until, let's say, half past twelve and then we'll start again about quarter to two or something like that and we'll finish this afternoon about four o'clock, perhaps.

Q. Dr. Waye, I show you Exhibit VD-52 which is an article entitled Identification of complex DNA polymorphisms based on variable number of tandem repeats and restriction site polymorphism. As I stated, that's prepared by yourself and Dr. Ron M. Fourney, is that correct?

A. Yes.

Q. Would you relay to the Court, please, just the significance of this finding?

A. The significance is that it was an event that we observed with a probe, a particular one relevant to our program as D4S139, and on occasion you can have three bands instead of a two band pattern. We devised experiments to demonstrate what was causing that.

Q. That's particularly in relation to probe D4S139?

A. The experiments and the strategy for scientifically showing what was happening to generate three bands rather than the usual expectation of one or two were done with that probe.

Q. And I understand from the paper that this occurred in about one percent of the individuals examined, seven out of 547, is that correct?

A. Yes, out of that sample size that's how many people we noticed this happening in.

Q. And I understand you ran the same test in the data base -- that was a Caucasian data base, Caucasians that you ran that sample, one percent?

A. Yes.

Q. Basically in what, Canadian Indians it ran at an average of about, was it eight percent?

A. Again, I'd have to find that section of the paper.

Q. Canadian Indian population, top of page 226?

A. It was higher, "about 8%, data not shown".

Q. And you state also at the top of page 226, it says, "In addition, the HaeIII polymorphism in the Native Indian population appears to be in genetic disequilibrium with the VNTR, since many of the individuals characterized by three-fragment phenotypes have a pair of fragment lengths in common."

A. Yes.

Q. "Thus, it is apparent that both the frequency of a given restriction site polymorphism and its degree of independence from the VNTR polymorphism may vary among different populations." Is that correct

A. Yes, you read that correctly.

Q. Now, if this happened to a high degree within any race, would that be an appropriate probe to continue using?

A. If I wanted to build a data base on a race and a high frequency of the people, as the example I gave before had three bands, we wouldn't -- that wouldn't be in our system.

Q. Would that create a problem if the frequency was too high? Would that create a problem with the interpretations within the established data base if you do have it?

A. It would make more work. What the purpose of this paper is if you ever do run into the occasion where, and again, one percent, of the people and the Caucasians that we serve, which is the relevant population that the system was built on for Caucasian, one percent, so it can happen if you ever do run across that case, we devised a strategy that you can define which two fragment lengths came from which chromosome. So you can actually define alleles and then go through and treat it as -- interpret it as a two banded pattern, which fragments come from which chromosome, define the alleles.

Q. So am I to understand that whenever you do come across a three banded pattern, that you don't even know which chromosome they come from?

A. Not without doing further tests, no. You can have -- you have three bands. There's a number of possibilities.

Q. So if you're running the D4S13 and it shows three bands, you can't say for certain which bands come from the number 4 chromosome?

A. That's not what I said again.

Q. That's not what you said, okay, I misunderstood you, then. Please explain it again.

A. When I say defining which bands belong to which chromosome, I'm talking chromosomes number 4. Which two bands -- obviously we've worked out the mechanism that this occurs. It's because you have a polymorphism on one of those chromosome fours at that locus, a D4S139 locus, so you'll have two bands generated from one of those chromosomes number 4 at that locus. Now you want to find out which of those two bands go together.

Q. Okay, so all three bands, then, actually would come from number 4 chromosome?

A. The two number 4 chromosomes. You want to find out which two come from which chromosome.

Q. The thing is, you're saying you should have had two bands but one of the bands broke in two for some reason or other?

A. A polymorphism, not some reason or another. The experiment clearly shows that.

Q. Now that's in one percent of the Caucasians that you've identified that in, a little over one percent?

A. Yes, it would be fair to say it's a little more than one percent, yes, 7 out of 547. It's approximately one percent.

Q. Could that actually occur more often or to greater frequency if some of the third bands were so short that they run off the gel?

A. Well, the gel's system is designed to include all the fragments of DNA not to run fragments off the gel. So if the system is applied the way it's designed, all the fragments that you cut will be contained on the gel. They're not run off the gel.

Q. Well, I've read in a lot of cases where different labs they admit that their gels are too short and therefore the real short fragments run off the end and are not accounted for. Are you saying the R. C. M. P. system has avoided that problem?

A. Again, the R. C. M. P. system was designed to contain all of the fragments that we wanted to analyze, which was all of the fragments. You're talking about -- if you're talking about other labs, you're probably talking about Cellmark Labs. They

analyze fragments from 2,000 base pairs and up, their system's design. Everything 2,000 base pairs and down is run off the gel, so certainly if you have a third band that's below 2,000 in their system, you can't detect it. It's less than 2,000, it's in the drink every time.

Q. So your system is designed to pick up bands of 200, 300 base pair lengths?

A. Those pieces of DNA are contained on the gel.

Q. And your system is designed to identify them?

A. Yes.

Q. And if they were there, they would be identified? Is that what you're saying?

A. Again, if there's sufficient DNA for detection -- remember, you can run into problems where you don't have enough DNA to detect it. I can't tell you that I can detect them if there's not enough material to detect them. We can't speak in absolutes.

Q. Is it possible that -- or does it work that the smaller the DNA fragment size, maybe the more difficult it will be to identify it on an autorad?

A. Yes, with these particular probes, the sensitivity of detection is dependant on their length. It's easier to detect larger ones than smaller ones.

Q. So if you had a weak signal and you picked up two very thin large bands, it could very well be that there is a third fragment there somewhere which is too faint to pick up?

A. I think I gave that example to the Court yesterday.

Q. I just wanted to substantiate that.

A. Yes.

Q. So you could actually have a lot more three banded patterns than what you realize?

A. That would not be my opinion. We're not dealing, when we analyzed these 547 individuals, we weren't --

Q. Is it possible --

A. -- we weren't --

MR. WALSH: My Lord, let him finish his answer.

A. We weren't dealing with amounts of DNA that were a sub of them. We loaded enough DNA that we could analyze what's on. In my opinion, no, that would probably be a very accurate indication of the frequency with which small bands, third band or otherwise, would occur.

Q. So you're saying in the R. C. M. P. system that that is not possible?

A. Again, you want to talk absolutes in a world where nothing is absolute. I just explained to you that the system and the way the data base was compiled, we weren't dealing with a situation that would give the result that you are trying to project.

Q. So in the R. C. M. P. system you're saying it's not probable?

A. We're talking about this data here and we're talking about the incidence of a small molecular weight third band. Dealing with that situation, I just told you it's my opinion that that's an accurate frequency of that event occurring.

Q. Do you suspect there's other scientists out there in the field that would have a different opinion, then?

A. No.

Q. They'd all have the same opinion?

A. Perhaps not that it would be seven out of 547. They have their own empirical data, but I'm --

Q. No, I'm talking about is it possible that there's

other scientists out there who would conduct the same experiments and, relying on your data, would be of the opinion that it's possible that where you identified two same bands, that there are third bands that are not being identified because they are--

A. Other scientists that would allow that possibility?

Q. Yes?

A. Again, I brought up that possibility yesterday and I'm a scientist. I brought that to your attention, that if I was at the limit of detection and I had a small fragment, it's possible that I could only detect the top two because of this factor of more sensitive detection at the top. I also explained that that would generate an exclusion.

Q. So when I get back and ask you the question again, is it possible that there are a greater number of three banded patterns in your data base that you have not been able to recognize because of the small band that would be faint and undetectable?

A. Again, I give you the same answer as the last time, the amounts of DNA that we analyzed building that data base were sufficient to detect fragments, small or otherwise, and I have full confidence in those numbers.

Q. So you're saying if the amount is sufficient to detect two bands, it should have been sufficient to detect three bands?

A. The number of the bands isn't at issue here. Whether you only have one band, it's the size of that one band, can I detect a band of that size? I ran enough DNA that I could detect a band of that size, third band, second band or first band, it really doesn't matter. It's the size of the band.

Q. You state also at the bottom of page 226, it says, "Moreover, the fact that this type of polymorphism was detected in a limited study focusing on only ten VNTR loci suggests that its occurrence is not infrequent." Do you still maintain that, that conclusion?

A. I'm not talking about the frequency of a three banded pattern at -- no, no --

Q. At one site, I realize that.

A. Let's put it into context here at the one site. Oh, you did have it right.

Q. Yes.

A. What I'm talking about -- I'm sorry -- what I'm talking about is if I looked at 100 VNTR loci, would I stumble upon this observation at another loci, and again, I've explained people are polymorphic and my expectation is that you could find polymorphisms at VNTR loci with particular enzymes in other systems. I saw it in two of ten, as you mentioned.

Q. You also state on the top of page 226, you say, "... it is precisely in situations where an 'unexpected' three-fragment phenotype is observed that the interpretation of DNA typing results may prove confusing. In forensic studies, for example, DNA standards of unknown origin are often compared without benefit of known standards. In such cases, the investigator has no formal basis for establishing the number of individuals that may have contributed DNA to a particular sample."

A. Yes, this is the justification for, in fact, publishing the observation. It's to point out something to other scientists that you can't think,

"I'm goi ; to see one band, two bands, and that's it." So that ıen somebody does come across the situation, I see th :e bands, I conclude there's more than one person. I'm laying out the logic for even expressing this.

Q. Okay, now you state, "In such cases allelic fragments may be identified using alternative restrict on endonucleases," or restriction enzymes. So are y. ı suggesting there what you should be doing is using a different enzyme rather than HaeIII?

A. No, I'm suggesting we do -- that if you had a forensic case where your client's DNA matched the blood sample but it wasn't two bands, it was three bands, that's still a match. If you wanted to put a frequency to that, you'd have to use an alternative restriction endonuclease and follow a scheme similar to the one describe in this paper where we applied HaeIII in combination or by itself with HinfI, MboI, AluI or RsaI, different enzymes, and sort out which fragments around which chromosome 4. That's all I'm saying.

Q. Is it possible, I'll go back to questions I asked yesterday, because of this study, and I admit you don't know anything absolutely, but is it possible that HaeIII actually cuts where it's not supposed to?

A. And generates this --

Q. And generates this phenomenon?

A. In my opinion, no.

Q. If, in due course, you were to find out that you end up with this phenomenon is the use of all the probes the R. C. M. P. use, what would your conclusion be then? The same thing? Would it change anything?

A. Again, you're stating something that's contradictory to what I know to be true, but if we can backtrack and reinvent the wheel and make it square rather than round, all the probes in the R. C. M. P. system gave not one, not two, but four, we'd still be developing the system.

Q. Would you be able to use the same data base?

A. We wouldn't have developed the data base without this part. Part of the initial plan was to find locus enzyme combinations where the majority of the time, and I submit that 99 percent is a fair use of the word majority, that expectation would be realized, and with the other probes, three-banded patterns or even more were, in fact unobserved for some of these loci. So you build a system that fits those criteria and we did build that system and we proceeded to build a data bases. Had with each of those loci enzyme combinations 50 percent of the people given three-banded patterns, we'd be moving onto another five loci for development.

Q. Okay, I'll go back to one of your statements originally, that if you're getting a three-banded pattern then there's something wrong with the design of your system, something wrong with your probe, or something wrong with the enzymes you're using, or now we have something wrong with the DNA or polymorphic segment?

A. We're having a real problem understanding each other. We developed the system --

Q. I told you we'd have that when we began, doctor.

A. We're meeting the formal expectation, then. Speaking of formal expectations, we develop a system,

we pick enzymes, probes; we evaluate them, we know in the population in question the formal expectation will be one or two bands. We've demonstrated that. We also know that, because people are different, you can have polymorphisms and, as I've demonstrated and published, one percent of the time you've got a three or more banded pattern at this particular locus due to a polymorphism. I have a formal expectation that this can happen. I publish a method to sort it out.

Q. But when you developed your system, this was totally unexpected, was it not?

A. As a matter of fact, the first time I saw it, it was a matter of perhaps two days that all the experiments to explain it were done. It wasn't totally unexpected.

Q. Well, when you first saw it. Before you even developed the system for the R. C. M. P., did you expect something like this?

A. It had been observed in numerous other systems. In fact, the reviewers, when they reviewed this paper, cited all sorts of examples in the literature where a VNTR locus with a particular enzyme gave more than two bands, and if I didn't expect the possibility that you could have polymorphism on the inside, I wouldn't have gone through the arduous task of evaluating ten such loci and numerous different enzymes, myself and the F. B. I. and other investigators. I didn't do that myself.

Q. So you're saying that this is nothing new? This has been known for years?

A. Restriction site polymorphisms?

Q. Yes?

A. I didn't invent them. They've been around a long time.

Q. I'm not saying you invented them, I'm talking about somebody finding it, that it's there.

A. Finding a restriction site polymorphism?

Q. You don't invent problems with people's DNA.

A. It's not a problem, it's a polymorphism.

Q. You don't invent them, do you?

A. No.

Q. Any more than you invent chromosomes?

A. Correct, they're there.

Q. And how long has this phenomenon been identified?

A. Restriction site polymorphisms?

Q. Yes? As three-banded patterns

A. As three-banded patterns, or two or more banded patterns.

Q. Yes?

A. I'd actually have to hit the literature myself and start looking at pictures, but I'm sure I could go back five or six years and find pictures in journals that have more than two bands at a VNTR locus.

Q. And when you developed your system, did you try to avoid something like this?

A. As I said, we looked at various loci and various enzyme combinations and that was one of the criteria for picking a locus, that it gave, predominantly, one or two bands.

Q. But you're finding out you didn't get your expected results?

A. No, polymorphism is an expected result. What we're looking at is we don't want that polymorphism to be frequent. As I said before, if 50 percent of the

people you look had three-banded patterns, that's not an appropriate probe locus combination to be using and it wouldn't be used. We're dealing with something one percent.

Q. If you found that it was too infrequent, which would be what in your book?

A. Too infrequent?

Q. Yes, for the use within your system that you have established, with the data base that you have established at this time?

A. I don't think it could ever be too infrequent. What you want is for it to be one or two bands. Too frequent, I just gave you the example of 50 percent. That would be something that's just unacceptably frequent.

Q. You state in the middle of page 226, you say, "We have analyzed this locus in HaeIII-digested genomic DNA and find that about 25% of Caucasians tested ... have three-fragment hybridization phenotypes (data not shown). This not only confirms the existence of an internal HaeIII site within the D7S22 locus, but also demonstrates that it is polymorphic in the population." And that's in what, 25 percent of the Caucasians, that D7S22?

A. Where the sample size was 100. I'm stating that this can't happen at another VNTR locus with another enzyme. Was it another -- no, it was that enzyme, and that rather than it being one percent, which is rare, it was 25 percent. You'll not that a cloned g3 and locus D7S22 probably won't be heard of again. It certainly won't come on a case specific evidence because that was deemed to be inappropriate.

Q. What I'm concerned about, Doctor, and this paper is that you have a look at Exhibit VD-45 in lanes B and C where we have a visual match, okay? Now, let's take, for instance, that this is the known sample, B. C is the unknown sample. These bands are extremely light in both lanes. These two bands are extremely light in both lanes. Now, I think you've already admitted that it's possible that there would be a third band but because it's -- the intensity is not there, we can't recognize it on the autorad. Is that right? That it's possible?

A. That's a scenario.

Q. That's a scenario. Is it also possible that two persons could share one band -- maybe this is a legitimate band. Two persons also share two bands with broken fragments and both would have third bands with broken fragments, but the third ones are definitely distinguishable in length and would not create a match, but we can't observe it? It's possible the third fragments would be a different length?

A. Again, you're dealing with a test that didn't work. You didn't detect the fragments.

Q. Right, but is that scenario possible?

A. Yes.

Q. So if you are dealing with a sub-group or sub-population something like the Canadian Indians, Canadian Natives, where there's a high frequency of this, three-banded patterns and you can only detect two of the banded patterns, you may have the situation where you're assuming a lot of high frequency situations where you're creating matches where actually matches do not occur?

A. You picked a bad example with the Native Indians.

Q. Do you have a better one? It's the only thing I have to work with, I'm sorry.

A. Well, the Native Indians that the third band would be in the same position. I mentioned -- you read --

Q. That's because you were working with pristine samples.

A. No, you read --

Q. We're not dealing with pristine samples in this case.

A. You read about disequilibrium, that the bands were in disequilibrium, polymorphisms were disequilibrium.

Q. Well, that only tells you that you have to have a separate data base for Canadian Indians.

A. Which we knew from the beginning.

Q. Which we knew from the beginning, right. But say we were dealing with this problem in the Native Indians and you weren't dealing with pristine samples, you were dealing with contaminated samples and samples that give very weak signals, very light bends in the autorads, where there's a lot of three-banded people, patterns, and you get, because they are very light, you can only detect two of those bands but you know there's a good possibility there's third ones lying down here which could either exclude a person or include a person, would you interpret a match under those situations?

A. This is something I went through yesterday in my presentation with one and two-banded patterns. There's nothing peculiar to three-banded patterns. You can do this exact same example with two-banded patterns and I did it yesterday, and I showed you that that was an example of something that has

been interpreted improperly and if it is interpreted that way, you've called a match that's not correct for one probe. You go to the next probe and you'll have an exclusion if they're from different people.

Q. I have no problem with that, even if there's three or four-banded patterns a person, if you can match them, you can match them, if you can find them. First of all you've got to find them, and what I'm saying is there's a possibility when you have light bands in a population that may produce three-banded patterns, there's a good possibility that when you only can come up with very light bands, that you're not identifying the third band and, therefore, you are not identifying a profile.

A. You've got a --

Q. It's inconclusive?

A. First off, you're dealing with something -- I went through the example of top and bottom band, and you can stick a third band in there if you're stuck on a third band example, two bands at the top that'll match and third bands whose sensitivity of detection are different because you're dealing with small amounts in your evidence sample and large amounts in your blood sample. You'll have a partial match and that's not even score. That's an inconclusive. That's not used against your client or for your client. How would you like it to be used?

Q. It would be inconclusive?

A. I just said it's inconclusive.

Q. Right, so you could have a lot of situations where you're calling matches, where if you knew whether or not the third band -- but because you don't know if there's a third band or not, I'm saying it should be ruled inconclusive where you have a population

where this situation could occur a lot, where you can't rely on just two bands in very faint band situations.

A. You're creating, if I understand you correctly, and I'm not -- that's a big assumption here. You're creating a situation where I'm given something that I can observe and you're going to counter by saying, "Well, I think there's things that you can't observe since you can't prove that they don't exist, these things that this one's here and this one's here, we shouldn't be doing this test." You have to deal with what you see.

Q. Maybe that might be the bottom line, we shouldn't be doing the test.

MR. WALSH: Again, I don't know if that is a question or a statement or was meant to be -- If he would please just ask his questions of the doctor.

Q. If you can't account and explain all the anomalies that you uncover during testing, should you continue on with the tests?

A. Which anomaly?

Q. I would call a three-banded pattern an anomaly.

A. And they're explainable.

Q. They're explainable?

A. There's methods to explain them.

Q. But you admit you can't not always detect the three-banded patterns if the --

A. I freely admit that if I'm given insufficient sample, that's the determining factor whether I'll detect . mething. It's not a DNA-specific problem. If I wa to detect a latent fingerprint, I have to put my nger on the table to leave it.

Q. In the case of Caldwell at page 440, the judge states, "It is highly unlikely that the probe will find its complementary code on fragments of equal length in the specimens of two people." Now, that is, again, an understatement or over evaluation of your system, is it not, when you're talking about high unlikely for one probe to find its complementary base pair among two different people? All you have to do to explain that situation or to find a frequency, I believe, is you stated yesterday was to use a Hardy Weinberg formula? Hardy Weinberg formula?

A. Okay, I'm not trying to stall or anything but could you read the whole sentence to me again? I want to get the coulds and the mays and the woulds straight in my head what the man was saying.

MR. WALSH: Perhaps it might be appropriate, My Lord, if he read the whole paragraph to get it in context.

COURT: I wonder if it wouldn't be better, perhaps, have you got that on a loose sheet there, Mr. Furlotte? Perhaps you could give it to the witness and let him just peruse it. It's a rather complex statement.

MR. WALSH: I have it here, My Lord.

COURT: Well, you give the witness one copy from somewhere, anyway.

(Document passed to the witness.)

COURT: Just give the witness a chance to read it first. Take your time.

Q. Again, as the information in that case, does it say that it's highly unlikely that the probe will find its complementary code on fragments of equal length in the specimens of two people?

A. This is what the judge or lawyer, whoever wrote this says, yes.

Q. And he's talking about one probe here, not variable probes?

A. That is what he has written.

Q. But in truth, it's rarely common for two people to share both bands on the same -- with the probing of individual probes?

A. Yes.

Q. On one probe?

A. If what he said was absolutely true, which is what we always seem to be hearkening back to, a judge saying something and it has to be absolutely true in all instances. If what he is saying is absolutely true, we'd do one probing, call a match, and go home for the day. We've got our match.

Q. Right.

A. We built a data base because we know fragments of the same length can occur in different individuals. That's why we say, "is consistent with coming from this individual," and not, "did come from this individual."

Q. And again in Caldwell, the judge, as in the other case as stated, says that, "It's highly unlikely that a probe will find its complementary code on fragments of equal length in the specimens of two people." So it seems to me that the expert witnesses in the States, at least, they're coming before the courts and they are not fully explaining --

MR. WALSH: Objection. We don't have the transcript. Now, I have not been objecting to Mr. Furlotte's use of cases to question expert witnesses. There is a danger associated that I expect the Court is well aware of, but we don't want to inhibit his cross-examination. But to make a statement like that

without the transcript is not founded.

COURT: Well, this is merely, of course, the judge's interpretation of what the effect of the evidence is and he may be correct or he may not have been correct. He's not a scientist and he's not, presumably, expressing himself in scientific terms necessarily.

MR. WALSH: But, and I was prepared to leave it at that, but Mr. Furlotte now is asking the Doctor questions to the effect that perhaps the experts in those cases were misleading the Court.

MR. FURLOTTE: My Lord, the only purpose of this cross-examination is I want to make absolutely certain that you come out with a better understanding of this so-called expertise than the judges in the United States who ruled that this stuff was admissible. It's --

COURT: Well, the judge in the Caldwell case may have been trying to play scientist. I suppose it's a natural --

MR. FURLOTTE: Well, I don't know whether he's trying to play scientist --

COURT: It's a natural tendency for judges, I suppose, on this type of case, but when I read their decisions I realize that they don't have a scientific background. They are expressing these things in laymen's terms. Well, anyway, what is your question to this witness, Mr. Furlotte, on this point?

Q. My question of this witness, it is not highly unlikely, is it, that two people can share both bands with one same probe?

A. Again, you have to define what "highly unlikely" is. Apparently with this judge, highly unlikely was the appropriate word. It, again, depends on the probe

and how variable it attacks. Some loci, a great number of people in this room, if you analyzed them, a high proportion, a significant number of people in this room would have fragments of the same length. Other probes, you could analyze my DNA and the phrase "highly unlikely" would be appropriate because you probably wouldn't find somebody in this room with the same pattern. It depends on the situation. He's given, I think, one interpretation that could apply to one situation and when we're dealing with hyper-variable probes, it's not an inaccurate reflection of what happens. He didn't give every qualifier for every event that could happen in every situation. It would be a long legal task.

Q. Now, could errors in matches be declared, I think one because of mislabelling, that would be one way?

A. Mislabelling at various steps. Obviously some --

Q. And it could end up with a false positive by mislabelling?

A. If somebody draws blood from a person and labels it John Waye when, in fact, it came from my wife, I'm analyzing something that's been misidentified.

Q. You could also have a false positive through the cross-mixing of samples?

A. If somebody took tube A and tube B, switched them around and mislabelled and proceeded on with the test, he attribute the patterns in a converse manner depending on how he mixes up the tubes.

Q. Bacterial contamination?

A. Would do the same thing.

Q. Or you could get a false positive through interpretation of bacterial contamination?

A. Not if the test is done properly and interpreted properly.

Q. Not if it's done properly?

A. We've gone down that road --

Q. Not in the R. C. M. P. lab, anyway?

A. I'm speaking personally. I'm testifying for myself.

Q. How about less than perfect chemical preparation?

A. Less than perfect chemical preparation, making buffers, the likes of that, PAG materials? Not in my opinion. There's things you certainly can do preparing a chemical that will either enhance the ability of the technique to work or it will detract from it and those are all things, protocol modifications, some protocols work better than the other. It's not going to change the result.

Q. What about something that might enhance the ability of the fragment to migrate?

A. We've already talked about Ethidium Bromide and the likes. That's a lab reagent.

Q. That would slow it down?

A. We have a study that shows that that alters mobility.

Q. What about certain chemicals that might speed the migration up of the fragment, just like long distance swimmers grease themselves to swim through the water to reduce the friction?

A. If there's chemicals that will slow it down, as a scientist, I'd have to allow for the possibility of you coming up with a chemical that might speed it up.

Q. Or different amounts of certain chemicals might change the so-called friction or resistance or the ability to speed it up?

A. It's a good rationale for having controls for that sort of thing, and they're built into the system.

Q. Can there be partial digestion by a restricted enzyme? It might give you a false positive?

A. No, because the test, the enzyme hasn't worked, the test hasn't worked. There's no result. The enzyme doesn't --

Q. No, no, I'm saying partial digestion, I'm not saying not digestion at all or --

A. Again, partial digestion, first off you'd have controls to detect it. If you detected it, the test hasn't worked as you designed it to work and you don't derive any information from it. If you interpret the test incorrectly, you could get the wrong answer.

Q. A lot would go on the interpretation as to whether or not the digestion process did work. Again you have to interpret an autorad?

A. No, you design a test -- That's the end result, you interpret the autorad, but if you design a test with checks all along the way that look at these possibilities, these things don't come out at random. This could or couldn't happen, we can or can't recognize it happening. We have controls all along the way. You can evaluate these things. Ask the question, did it work, didn't it work.

Q. And you can also get a false positive through sample degradation if you weren't aware?

A. You're saying also. I haven't agreed to any of the previous. Sample degradation falls into the same matter. If your sample is degraded, there's nothing to analyze.

Q. Band shifting?

A. Again we --

Q. Again, you deny you can get a false positive through band shifting, if the tests are done right?

A. We're getting the terminology, if the tests are done right and interpreted properly, I'll recognize that it's band shifting and, as I said yesterday, I can, as a scientist, demonstrate that that is a band shift and as a good scientist, I'd be well within my rights to call it a match. You bend over backwards to throw out that and call it inconclusive.

Q. I believe you've already established yesterday that the samples of the same person will not run exactly the same speed every time?

A. Yes, I can analyze my DNA on a number of occasions.

Q. And some of the causes on that might be the buffer that the agarose gel and the salt solutions are all manufactured and can vary between one batch and the next? Would those be some of the reasons why the same DNA would not run at the same speed all the time?

A. You'd be talking about different gels running things on different days if I changed all my reagents, a variability and a reagent could cause all the samples on one gel to run differently from all the samples the previous day or something like that does form a possibility. Again, you have controls for all that.

Q. You have controls for all that, that's right.

A. Certainly.

Q. Your marker lanes, your monomorphic probes?

A. Marker lanes, monomorphic probes, those are controls, yes, control DNAs.

Q. And again, the gel might not be absolutely consistent across its length? That might cause a variation in different runs in different lanes?

A. Consistent across its length? It would be incorrect dimensions? I'm not understanding, are you talking dimensions of the gel?

Q. How deep is your gel when you run your samples? How thick is it?

A. The exact thickness, I've never taken a micrometer to a gel. What you do is --

Q. Very thin.

A. No, when you make a gel, it's liquid. You make a volume of that gel, you put it in a form which is just like pouring concrete. You pour it in, set volume, same volume every time, the same form every time, and it'll come to the same level every time. When the gel solidifies, just like concrete, it'll be roughly the same depth every time.

Q. It's just like concrete when it solidifies?

A. No, or I would have brought one in to demonstrate to the Court what they look like. It's actually like Jello, but it's not liquid any more. I can pick it up and I could -- I couldn't throw it to you. It would probably break when you caught it, but I could pass it to you.

Q. Well, maybe it doesn't solidify evenly across the gel. Maybe this would be these expert witnesses you're talking about?

A. Doesn't solidify so liquid in one part, half gooey in the other part, solid in the other part?

Q. Well, maybe not right from liquid to gel, but slight variations?

A. I'd love to read the text of th experts trying to explain that. It's not a reality. You boil it, you cool it, you pour it, it solidi ies.

Q. I understand, what, you shouldn't allow a gel to get too dry either? It should --

A. Well, the gel is made with wate , so I boiled it, cooled it, poured it, it solidi ied; then I went on holidays for a week, water comes out of the gel, the gel decreases in thickness, it dries up. That wouldn't be good practice. Th .'s not in the protocol.

Q. No, it's not in the protocol.

A. You make up a gel and you use it.

Q. So it depends how long you allow a gel to sit to solidify that, depending on how easy the fragments are going to move through it. That would have some bearing on it, would it not?

A. No. If you're talking reasonable use. The use I just gave was a poor use of gel, leaving it on your bench for a week. If I left it on the bench for six minutes versus an hour it will have no effect. If I leave it on six minutes versus four hours, it'll have no effect.

Q. The thing I'm considering is maybe the gel would not solidify or dry up, to use another term, evenly across it and therefore you could get variations in lengths?

A. And I just tried to explain that that's a function of reasonable time, and I'm sure that there's more water a ms that leave the surface of the gel over a period of one hour versus five minutes. I'm telling you that its effect on migration of DNA is not measurable.

Q. What if the lanes into which the samples are poured can have variations and imperfections that can affect the speed from one lane to the next, which would be called band shifting. Do you deny that this could happen also?

A. Again, the lanes aren't cast individually. If you cast your lanes individually, then lined them all up and ran your sample, certainly a difference from lane one to lane two because they were cast independently. It would make a difference. Remember, you boil up the gel material for the whole gel, swish it around, you've got a homogeneous solution. You pour it, it's still a homogeneous solution. It solidifies and it's a homogeneous slab. The lanes weren't formed as lane one, lane two, lane three, lane four. They are formed as a pool and solidifies.

Q. Maybe I'll use an example of, maybe, you know, a person tanning, or I've used epoxy, I've used body fill before, and sometimes the body fill will harden in one area a lot quicker than the other area where I put it on the car, and maybe because I didn't mix it up well enough. Could this happen with a gel?

A. I imagine when you're --

Q. And cause band shifting?

A. I don't have much experience with bondo or any of that material, but I imagine the part that dried quickly was a thin layer of it and the part that dried quickly was a nice rust spot you were trying to fix. You're not dealing with a situation that's even comparable to pouring a gel.

Q. I'm going to show you Exhibit VD-37 which I believe was used to explain the migration of the band fragments through the gel. Was that meant to show

that the larger it is, then the more resistance it has to go through the gel, and therefore travel at a slower rate?

A. Yes, it was a cartoon. It's not even -- when I explained this yesterday, it's not meant to depict the exact mechanism by which this works. It's meant to depict the result of the larger ones will have a harder time going through than the smaller ones. It's meant to depict what we're trying to do. That's in no way to apply a mechanism here.

Q. Right, I understand that, but just for clarity's sake for everybody else, let's take, for instance, my pen is a DNA fragment, say that top fragment or the middle one. It doesn't matter, okay? Now, when that travels through the gel, does it travel sideways like that, or would it travel -- or would you know?

A. It's not a rigid molecule.

Q. Would you know how it travels, whether it travels that way or this way? (Indicating).

A. Again, I've been asked this question before, and really, the way you have to answer it is to get your electron microscope video camera and follow one of these molecules through. People haven't done that, not to my knowledge. The current theories about how DNA molecules migrate through a gel is much like a snake. It weaves its way through the pores and through this maze from end to end.

Q. But nobody knows for sure? Is that what you're saying, it's just a theory yet?

A. The actual mechanism by which DNA molecules migrate agarose gels, there are several theories about it.

What we do know is that they do migrate through and they do migrate through as a function of size. The mechanism, really, most molecular biologists would agree it's really unimportant.

Q. What would be important, though, would be if that theory is correct. What would be important is as to how fragile or flexible a DNA molecule fragment is so that it could work its way like a snake through the pores. If it was as straight as my pen and as rigid as my pen, it would have a hard time to work its way through, would it not?

A. It's not straight and rigid, though.

Q. Is there --

A. People can look at DNA molecules.

Q. Is there anything that can cause DNA molecule fragments to become rigid as from contamination of any kinds?

A. To increase their rigidity?

Q. To increase their rigidity?

A. Ethidium bromide increases the rigidity of the DNA -- it doesn't make it like your pen. Rather than having a floppy piece of spagetti, you might have a little more resistance to --

Q. Which causes band shifting, right?

A. It's one of the theories of why a dye like ethidium bromide would retard the migration of something through the gel.

Q. Now, you know through experiments that ethidium bromide causes band shifting?

A. Can cause band shifting in some systems.

Q. There could be a lot of other actors out there that could cause DNA to be rigid that we don't know about?

A. That could alter rigidity.

Q. Alter rigidity, yes?

A. Yes.

Q. I'm thinking of the latest guy -- I'm concerned because of the latest news from England where the Birmingham Six spent sixteen years in prison because of scientific evidence. Do you know about that case?

A. And that would be a DNA typing case?

Q. No, it wasn't DNA typing, it was just a common occurrence where scientific evidence was over estimated.

COURT: Mr. Furlotte, just a minute --

MR. WALSH: My Lord, I'm wondering, again, without -- believe me, I'm not attempting to restrict his cross-examination, but at some points I would like to know what the relevance of the Birmingham Six in England, other than I understand that some DNA lawyer in the States used it to colour up a magazine article that he wrote, I don't know what relevance it has to the issues we have here, and that's my only reason for standing up and --

COURT: There was no DNA involved in that?

MR. FURLOTTE: No, it's just the issue as the precautions we have to take when we are ready to declare how reliable tests are.

COURT: Isn't that more an argument you should be using on a stupid judge rather than incorporating it in a question to a scientist witness?

MR. FURLOTTE: Does that mean you want to step down from the bench, My Lord, so I can find a stupid judge?

COURT: Judges are supposed to be stupid. You're getting a little too far afield there.

Q. Dr. Waye, is there any way we know how thick DNA fragments are? Are they common, or are some fragments bigger than others in, say, thickness-wise? We know you're comparing length, okay, of the fragments, but say a DNA molecule of that band, or these strands, in different people could they be different sizes?

A. You'd be attacking the Nobel Prize winning work of Watson and Crick. DNA structure was defined in 1953. It hasn't changed since then and it's been looked at pretty closely. That's a pretty set dimension. It's a small dimention, it's not -- it's measured in angstroms.

Q. Yes, but has it ever -- I don't know. I'm totally ignorant on this.

A. The answer is no.

Q. The answer is no? How are we able to determine that, do you know, that the answer is no? That the thickness, aside from the base pairs and the number of base pairs and the length, how do we know that this is not thicker in one person than another? People are different sizes.

A. That does not differ in any one person versus your pet dog. DNA is DNA. It has the same chemical structure. It's going to have the same dimensions.

Q. I'm not talking about chemical structure.

A. Yes, you are when you're talking about configuration. That's part of the chemistry. That's the double helix.

Q. That's the double helix? So there's no way you would have the different size, a different size snake going through the --

A. Length is the parameter you're looking at.

Q. Length is the parameter you people are looking at. What I want to know -- I want to look at it from another view, and is it possible that you have a different size snake running through the gel?

A. Fat DNA, skinny DNA, no.

Q. You know that for a fact, do you, or you're guessing?

A. That's something that I have -- again, I haven't looked at 5 billion DNA from 5 billion people at that level of relying on the Nobel Prize winning work of Watson and Krick and how it's held up pretty much to peer review over the last almost forty years. It's fact.

Q. And as yesterday, just the fact that people get old and stiff and move a lot slower, that doesn't mean their DNA becomes a little more rigid along with their bodies? There's no micro-causing, macro-causing analogy here, is there?

A. Well, there's a lot of theories into aging. If that were a theory you wanted to put forth, you do like all scientists. You put forth a theory, you write a grant proposal and you try to get money to investigate that.

MR. FURLOTTE: My Lord, this might be an appropriate time to break for lunch, and if you want to come back a little early if we're quitting earlier than you had expected.

COURT: Can we start again at half past one?

MR. FURLOTTE: That's fine with me.

COURT: I just want to wind up this morning with a question which is perhaps a stupid one. Would jello work as a gel? What do you mix with water to make a gel?

A. I don't really know what gel is made of. It's made of a polymer-like, all of these jelly-like things are long molecules. Some of them are carbohydrates and you can add water to them and they will solidify. It's not a bad analogy, but I don't know exactly what Jello is composed of. Consistency-wise, it varies.

(Accused escorted from courtroom.)

(Court Recessed 12:20 p.m. to 1:30 p.m.)

(Accused Present)

(Cross Examination of Dr. Waye by Mr. Furlotte continues.)

Q. Dr. Waye, is it true that in the DNA structure the order of the different band sequences, even in your polymorphics, probably determine some characteristics which will be expressed in an individual's physical or mental make-up?

A. The order of the bases?

Q. Yes?

A. Yes, it's a genetic code, and yes, it does code for feature that make you human and --

Q. Different characteristic traits?

A. Yes.

Q. And even in some of the polymorphisms that, I suppose, have been identified, but yet no specific purpose has been attributed to them, it could have to do with certain diseases and/or mental traits?

A. There are undoubtedly genes or regions of DNA in the human body that will influence mental traits. That's a matter of fact.

Q. I think maybe even schizophrenia is being examined as one possible polymorphism?

A. There is evidence that certain genes located on certain regions on chromosomes are involved in schizophrenia.

Q. So just to understand the DNA structure a little greater, there's a possibility that some of the sites that these probes have been examining throughout the R. C. M. P. experiments and data base, that some of these probes maybe examined at sites that could be attributable to specific diseases yet to be discovered?

A. There's no evidence to support that. The sequence of the bases and the structure of these loci are not such that these loci code for proteins. That's the function of DNA. There's no mystery to how these order of the bases will code for a feature. The bases will specify a code which makes a protein. You can simply read a sequence into a computer and the computer will tell you the amino acid or the protein sequence that that piece of DNA can code for. These are non-coding regions of DNA. They don't code for proteins.

Q. You say they're non coding?

A. That's exactly what I said, non coding.

Q. I'm just wondering if, just so I can get a better understanding of this, that, say, for any specific probe that there would be people who fit into bin 8 or bin 12, they may share not just common fragment length, but they share a common physical or mental trait that is really -- we really can't detect what it is, but yet, they still share some physical or mental trait because of that specific fragment length?

A. Well, they're not genes that code for functions like that. They're non coding.

Q. They're non coding? As far as you know, or is it possible for them to be coded?

A. The VNTR region that we look at are comprised of tandem repeats and it's non-coding DNA. It's not making a protein, the absence or presence of which makes your eyes green or makes you have schizophrenia.

Q. Now, when you say they're non coding, is that just because we haven't been able to recognize a purpose to them, or is it because they would absolutely have no purpose?

A. No, it's a statement of fact, a piece of DNA is either coding or non-coding. It has the potential to make a protein or it doesn't. That's, again, shortly after the structure of this molecule was determined in the early fifties. Shortly after that people who discovered the structure and published those works deciphered the code, deciphered the method of how we can read through the sequence of base pairs to find out what, if any, protein it could code for.

Q. I understand from case law that at Cellmark their protocol was that one scientist would ordinarily work on a sample from start to finish. He'd then analyze a sample between known and unknown samples and I assume that's the same procedure taken at the R. C. M. P?

A. One person would analyze --

Q. One person would begin working on it from start to finish and then he would analyze it?

A. In my experience, and I did several cases, not

hundreds, but several cases, and that's the way I did it. I accepted the exhibits and I proceeded through the testing beginning to end and I testified in court. That's the way I did it.

Q. Now, I believe at Cellmark also that the second scientist would make a completely independent assessment of the match before any report was made? Is this also a procedure that's followed by the R. C. M. P.?

A. Again, when I worked at the R. C. M. P., when I did case work at the R. C. M. P., it wasn't a formal written policy that your results be reviewed and agreed on by everyone else, but as you generated the results, they were certainly shown to other people in the lab and your conclusions were certainly presented to people in the lab.

Q. Before they did the assessment, before the second person did the assessment, would your conclusions be known to them?

A. They'd draw the same conclusions as me.

Q. That's not what I asked. If they drew the same conclusions as you did, were they aware of your conclusions before they did their individual assessment?

A. You're asking if we had a blind assessment, if I handed them the results at the end, left the room, came back and saw if we agreed. On the cases I did, we didn't do that, no.

Q. Have you done any studies in the overestimation of homozygotes in data basis or in your -- the experiments that you've conducted?

A. When we did the data base, you analyze, as I said

before, analyze, observed events and compared them to predicted events, yes. That was done with the data bases as well.

Q. One argument about the overestimation of homozygotes because then what's expected is because small alleles may migrate to the outer edge of the gel and thus are not displayed on the gel. Have you conducted any research into this?

A. Specific research into that?

Q. Yes?

A. I think we covered that again before. When you build the data base, the running off the gel, you say it happens, I described to you why it doesn't happen, the gel system is not designed for alleles to run off the gel. So is that point covered?

Q. Yes, I believe it was covered. So in the R. C. M. P. data base there should be no excuse for an excess of homozygotes?

A. That's not a correct statement, no.

Q. If some labs are using the excuse that their data base may contain an excess of homozygotes which is normally expected in data base because, in a great number of cases, what's determined to be a homozygote, the short band runs off the end of the gel. You're aware that some labs are explaining their problem with that explanation?

A. That's one possibility that's put forth. I take a little exception with the word great, with your arguments about frequencies and great differences and this happens an awful lot. I analyze large number of individuals and when you compare predicted and observed events, you're looking at numbers like

I saw. I saw 11, I predicted 15.

Q. Right.

A. That's not an enormous difference.

Q. Say when you predict for the data base being Hardy Weinberg equilibrium, they assume, that is, many scientists do, that there should only be a certain degree of frequency of homozygotes. Is that correct?

A. The frequency of homozygotes and Hardy Weinberg, you are trying to link those two together?

Q. Yes.

A. Looking at homozygotes, that's not a very good test for Hardy-Weinberg equilibrium, no, it's not. It's not recognized as a good test for that.

Q. It's not recognized by anybody as such, or just yourself?

A. That's certainly my opinion and since we like to deal with court transcripts and court record, it's a number of experts and a number of judges' rulings that that's not an appropriate test for evaluating Hardy Weinberg equilibrium.

Q. But you are aware that some labs were concerned about the criticism of the excess amount of homozygotes observed in their data base?

A. Concerned about criticism?

Q. Yes?

A. Well, if you're being criticized, rightly or wrongly, the scientist, yes, you certainly get concerned about these things.

MR. WALSH: My Lord, Mr. Furlotte is asking me if he wants Dr. Waye, to refer Dr. Waye to an article. In fact, I just asked Constable Charlebois to get it for me from my office. I have a court copy, one for introduction in court, plus an extra copy for use.

Mr. Furlotte doesn't have a Court copy, but the Crown has no problem if Mr. Furlotte wants to agree with consent entering it.

MR. FURLOTTE: No, I agree with consent because I have had notice that the Crown was going to enter this as an exhibit.

MR. WALSH: Or refer to it, in any event.

COURT: What is the name of it?

MR. WALSH: I'm sorry, My Lord, it's No Excess of Homozygosity at Loci Used for DNA Fingerprinting. The authors of the article, their last names, are Devlin, Risch, and Roeder, and the date of this article in the Science, September 21, 1990.

COURT: We'll call that VD-53.

(DOCUMENT MARKED AS EXHIBIT VD-53)

MR. WALSH: I gave Dr. Waye a copy to facilitate cross-examination.

COURT: If you're going to ask the witness about this, Mr. Furlotte, perhaps you should ask him if he's familiar with it or if he wants an opportunity to familiarize himself further with it.

MR. FURLOTTE: I would want him to be familiar with it. Had you been familiar with this document beforehand, Dr. Waye?

A. I read the article when it was published.

Q. You read the article? Just take a few minutes and familiarize yourself with the article.

COURT: What questions were you going to ask? Is there any particular --

MR. FURLOTTE: I thought I'd just give him a few minutes to familiarize himself with the article.

COURT: Yes, but with what purpose in mind? I mean, are

there certain propositions in this article which you're going to question the witness about?

MR. FURLOTTE: Yes, My Lord, there's certain propositions which are counter to his opinion.

MR. WALSH: Objection.

MR. FURLOTTE: I believe, which I --

COURT: Well, that's what you want to find out?

MR. FURLOTTE: That's what I want to find out.

COURT: Well, okay, but how many pages are there in this article? It's quite a long article, it seems.

WITNESS: Four full pages.

COURT: I would have thought, Mr. Furlotte, perhaps that if you were going to cross-examine him on an article like that, you might have given the witness a little advance notice or the other side, so that he could prepare for it.

MR. FURLOTTE: I'm sorry, My Lord, but I probably falsely assumed that because the Crown had given me notice that they were going to introduce -- probably introduce this article as evidence for the Crown, I just falsely assumed that their expert witnesses were already aware of it.

MR. WALSH: My Lord, perhaps so we don't have any misconceptions here, I've got five witnesses. Some witnesses, obviously, have read certain items, rely on certain documents, and some wouldn't. Perhaps it would be appropriate, My Lord, and I know you're aware of the authority, but perhaps the Crown could state its position with respect to the use of these types of documents or in the manner in which Mr. Furlotte is cross-examining. If I may be allowed to refer to a case on this particular topic, My Lord?

COURT: Well, let's give Mr. Walsh an opportunity to state his position.

MR. WALSH: My Lord, one of the earliest -- as you are probably aware, one of the earliest decisions in Canada with respect to the use of textbooks and authorities on expert witnesses is a case known as the Queen versus Anderson, and it's reported (1914) 16 D. L. R. at 203, a decision of the Alberta Court of Appeal. In that particular decision at page 206 and 207, the justices were dealing with how examinations and cross-examinations of experts with the use of texts were to be dealt with, and they make the point that:

> "On cross-examination the Judge should be careful to see that an improper use is not made of text-books, practically to give in evidence opinions of absent authors at variance with those of the witness. It is quite apparent that if the witness is asked about a text-book and he expresses ignorance of it, or denies its authority, no further use of it can be made by reading extracts from it, for that would be in effect making it evidence..."

But if the witness, and I'm paraphrasing,

> "...admits its authority, he then in a sense confirms it by his own testimony, and then may be quite properly asked for explanation of any apparent differences between its opinion and that stated by him."

My understanding is, and again, considering the nature of this hearing, my understanding is that strictly speaking, and I don't know whether Dr. Waye is going to accept the authority of that or not accept the authority of it, but strictly speaking, a witness can't be asked to comment on something that either if he's ignorant of it in the sense that he hasn't made himself aware of it, or does not accept the authority of it. But we recognize

some -- the Crown recognizes that some leeway must be given to Mr. Furlotte in as much as if the Court was to decide that this was a Frye hearing, then the Court is entitled to look at other relevant scientific literature in the field.

COURT: Yes, well as the Anderson case, isn't it, points out, the mere incorporation of a statement from another article in a question and saying do you agree or not, doesn't make that proposition included in the statement evidence. I quite agree with the Anderson case.

MR. WALSH: I felt it necessary to make the statement now because of Mr. Furlotte's just recent comment that was almost to the effect of that he's putting -- he's making statements that there are people out there who disagree with Dr. Waye to that effect. I don't want any misconceptions about that.

COURT: Well, that is what Mr. Furlotte says and that isn't evidence, of course. But I would give Mr. Furlotte a fair freedom to put propositions up to a witness and say, "Do you agree with this," even though it may be incorporated in some article that the witness isn't aware of. I think that goes a little beyond the Anderson decision, but --

MR. WALSH: Yes, and we recognize that some leeway is required because if the Court were to consider this to be a Frye type hearing, yes, noting that there is other literature is an important thing. Whether it's authority, or accepted as authority, is something for the Court to make a decision of later. I just wanted to clarify that from the Crown's point of view at this point.

COURT: I want to make myself clear on one thing, and that is a lot of these articles that have been marked as exhibits here on the voir dire and the fact that they're accepted as exhibits doesn't mean that everything in those articles is proven or is evidence before the Court. A lot of these articles have been accepted through this witness, in particular, merely to show that he was familiar enough with the subject -- this is as I take it, in any event -- merely to show that he was familiar enough with the subject that he could prepare a scientific paper which was peer approved and published in an article or something. But you can be sure that for the purpose of this voir dire, I'm not going to read through every one of those articles and say how does this statement balance with that statement. There must be all sorts of divergent statements.

MR. FURLOTTE: No, My Lord, and the only purpose that I would want something like this into evidence is not to try and disprove that Dr. Waye's opinion -- or to prove that his opinion is wrong, definitely. The only reason is to show that there is controversy out there about his opinion and about the subject matter that the Court has to rule on.

COURT: Well, when you say that, though, you're asking the Court to accept that these other views have some authenticity and I'm not sure that that's really warranted. You're asking him for his opinion. Now, you may get the witness to admit that, yes, that is a view of a certain portion of the scientific community or you may, through your own witness, establish that a view of a portion of the scientific

community is so and so as contained in that article. But the mere fact you incorporate it in a question doesn't --

MR. FURLOTTE: In all fairness, Dr. Waye's opinion might very well be right. As I understand, and I've been reading through the materials, that other people have different opinions and I would like his expertise to be able to explain why these people may be wrong and he's right.

COURT: Well, one of your purposes on cross-examination, presumably, would be to endeavour to get the witness to acknowledge that there is a certain divergence of opinion on a certain field and perhaps he's prepared to do that, perhaps he isn't, depending on the area you're talking about.

MR. FURLOTTE: My Lord, as I think I explained myself when we started this voir dire that the only thing I am after is trying to search out what the truth is about the reliability of DNA testing and this is the only way I know how to do it.

COURT: We are sort of going a little beyond the conventional wisdom in cross-examining expert witnesses. The traditional advocacy policy as I've known it has always been that in cross-examining an expert, you keep it as short and as confined as you can in your cross-examination. You examine on areas where you know he is the weakest and you try to make your point to show that there's a weakness in his theory or his opinions or you try to cross him up in those few fields where you can be sure you're going to get the right answer. If you keep plugging away at things where there isn't any weakness shown, it sometimes only serves to strengthen the evidence

that the witness has given and perhaps to confirm that he is a true expert.

MR. FURLOTTE: And that's the gamble I must take, My Lord.

COURT: I am not trying to tell you how to run your cross-examination, but I think normally one would -- I think that traditional wisdom or conventional wisdom and advocacy is probably to put more reliance in establishing your case through your own witness or witnesses than it is to try to break down some other expert. I suppose the reason is that lawyers take on something more than a match when they take on experts. This applies to counsel on the other side as much as to --

MR. FURLOTTE: Oh, I concede that fact, My Lord.

COURT: Well, having said that, I won't say anything more.

Q. Dr. Waye, in the opening paragraph of that article it states, "One criticism of DNA fingerprinting is that the VNTR loci used for the fingerprints violate the assumption of Hardy-Weinberg equilibrium (H-W), making it difficult to calculate the probability of observing a genotype in the population." That would be starting at the third line down.

A. Yes, you read that correctly.

Q. And what is your position on that, that there are criticisms out there and whether or not they are valid?

A. There are criticisms. This article deals whether they are valid or not. If you go through this article and get to the meat of the paper and how they actually analyzed data and draw their conclusions, it's a very statistical paper dealing with formulas too long to state and involving Greek letters that I can' pronounce. I'm not a

statistician, so when I read a paper like this, and I probably do the same thing as you, I read the beginning, the premise for doing the paper, and try to read the introduction, and I read their conclusions, then I present the study if it hasn't already been presented by somebody who understands the statistical method and can give me an opinion on whether they formed their conclusions properly or not. We have statisticians that oversee the analyses similar like this at the R. C. M. P. I mentioned him yesterday, Dr. George Carmody, although yesterday I had him a faculty member at the University of Ottawa. It's come back to my mind that he's actually at Carleton. I clarify that for his sake. But this particular paper just takes that -- I'll use their word, controversy, their criticism. They're just stating a fact, that one criticism. Later on they state the source for that criticism and I think that's important. This is a scientific paper that actually asks a question, designs an experiment and answers that question.

The criticisms that they refer to, and they reference, are listed in the reference section. The articles they're referring to references 6 through 8, an article by Eric Lander. That's an article after he was an expert in Castro for the defence. He put together all his opinions about that case and published them as a commentary in that journal. That's not a paper of this sort where you deal with a problem, you design an experiment, and you actually analyze data and you generate a conclusion.

The next paper that they cite are a number of

commentaries made by lawyer-scientist teams that do nothing but fight DNA, and again, they're commentaries. That's why I'm trying to stress that these are commentaries, in scientific articles, but commentaries.

The next one, again, is a statistical paper --

Q. Also one article there, reference is made to number 3, Mr. Baird and -- is it Balazs?

A. Ivan Balazs, Dr. Ivan Balazs.

Q. Balazs, and they work for whom?

A. Lifecodes. That's not pointing out or criticizing a system, that's just pointing out an observation. They're citing the observation, not relaying the criticism.

The next article that brings up this criticism is a paper by Dr. Joel Cohen and it's published in the American Journal of Human Genetics where he laid out what he thought the problem was, he picked the examples, he chose and he dealt with issues and formed his own conclusions, and that article dealt pretty much from beginning to end with Alex Jeffreys' multi-locus DNA fingerprinting methods, so I'm not sure its applicability to this. On the examples he showed, although we want to deal with that paper specifically he said weren't contrived, he couldn't have picked a worse example to demonstrate his point for the defence.

What I'm getting at is those papers really weren't proper scientific papers in that you present a problem, you design an experiment, you conduct the experiment, you analyze the experiment, you draw a conclusion. They're commentaries.

This paper actually does the proper scientific method. Again, I'm not a statistician and at some point you'll hear from people who are statisticians and I'm sure that they can talk for days about what these formulas mean and how these formulas actually do that. The gist of the paper, if all of this material is applied correctly, is simple. These arguments that you have excess homozytes -- homozygosity and that nullifies your ability to do anything with this data are unsubstantiated.

Q. Do you agree with that opinion that the excess of homozygosity would invalidate the procedure of predicting probabilities?

A. No.

Q. Do the writers of this paper disagree with you, that it might invalidate the procedure, excess homozygosity, take you out of Hardy-Weinberg equilibrium?

A. If excess homozygosity was an actual reflection of a system that was grossly deviated from Hardy-Weinberg equilibrium, adjustments might have to be made to compensate.

Q. Right, and that's what this paper was about, is that right?

A. Making adjustments? No, it was addressing that issue.

Q. Correcting that appearance?

A. No, it's asking the question, do we have a problem or don't we.

Q. So basically, I may be wrong, but I assess this paper as the writers of this paper and the experiments assume that there is an appearance

out there that the appearance of excess homozygosity is not a fact, it's only -- it's only an appearance and not a reality and they attempt to bring it back into reality, or bring the rate of homozygosity back into an acceptable level. Would that be a fair assessment of this paper?

A. I don't think so.

Q. What would be a fair assessment of this paper?

A. Again, they took the situation where scientists working with these types of probes have noticed that when you compare expected versus absurd, you have excess single band patterns. They want to know if that is an actual reflection of deviation from Hardy-Weinberg or if that's due to our inability to define bases -- define fragment to the base pair. It's an obvious question. They came to the answer that this has nothing to do, or the length between excess homozygosity and Hardy-Weinberg equilibrium is not substantiated by their data. It's no --

Q. But basically -- no, it's not substantiated by their data. That's the excess.

A. They explain it, yes.

Q. And they explain it, and in their explanation, they attempt to explain that the excess frequency in homozygosity which is expected, say, in the Causasian race, is due to the fact that in a lot of cases where they report homozygote bands, that they are actually heterozygote and the short bands run off the end of the gel.

A. Who said that?

Q. Is that what they're saying in this paper?

A. That's not my understanding of what they're saying, no.

COURT: Perhaps we could leave this topic of the cross examination until Dr. Waye comes back, and perhaps in the meantime he would have had an opporutnity to read through this article in some detail. Would you be agreeable to that, Mr. Furlotte? Or perhaps you are through with this. Are you through with this article now?

MR. FURLOTTE: I wasn't through with it, no.

COURT: Perhaps you would be prepared to be through with it?

MR. FURLOTTE: No, I'm not prepared to be through with it, either.

COURT: All I'm getting at is I don't think we should be wasting time with taking ten minutes here to have to study up these things.

MR. FURLOTTE: Well, My Lord, I advised --

COURT: The witness hasn't --

MR. FURLOTTE: I still advised this Court that when I asked for the adjournment that I was not totally prepared and that it would take an extra length of time in handling the cross-examination because I wasn't prepared, and the Court wanted to proceed with this matter at an expedient rate and --

COURT: Five months after the trial started, as I said yesterday.

MR. FURLOTTE: Yes, and you want to proceed, and that's your decision, but I think --

COURT: Well, let's leave this. Can we leave this particular article? Will you continue with this on Wednesday of next week or whatever? Why don't you make a note in your book there that you will continue this.

MR. FURLOTTE: Fine.

COURT: But surely it could be wound up then with two or three well-phrased questions?

MR. FURLOTTE: I would hope so, My Lord.

COURT: We'll leave a copy of the article with Dr. Waye and beseech him in the meantime to look it over.

MR. FURLOTTE: I'm sure the Crown will provide him with a copy.

COURT: I might just add at this time that if, Mr. Furlotte, there are other particular articles that you propose to examine Dr. Waye on, perhaps you could let him know about that before we -- or when we disband this afternoon and he would have a chance to look them over and be prepared, or perhaps you could even indicate with counsel meeting together, perhaps you could even indicate to him what type of examination you might be --

MR. FURLOTTE: Well, the problem with that is, My Lord, I never know which article I'm going to have to get into until I know which answers I'm going to get out of Dr. Waye on my questions, and some articles it's probably not necessary to get into because he will be basically agreeing with me. But where he disagrees, and I think there are articles out there that will express a different opinion, scientific opinion, then I will have to get into it. So it's very difficult to judge.

COURT: The most basic rule in examining any witness is you never ask a question unless you know what the answer is going to be.

MR. FURLOTTE: I've never followed the basic rule, My Lord.

COURT: And that applies just as much to expert witnesses as it does to any witness. Well, anyway, if there

are articles that you propose to examine the witness on at length or that you know now you're going to examine him on, please let him know this afternoon so that he can equip himself with copies of them and perhaps before Wednesday or whenever he reappears, he'll have a chance to look them over. It would save time.

MR. FURLOTTE: Any articles that I had intended, or at least most of them that I had intended on presenting into court, I had provided the Crown with a copy of them so they are available to his expert witnesses, and I've been provided with copies of documents that the Crown intends to introduce, so like I say, maybe I falsely assume that his expert witnesses were familiar with all those articles, and in particular, all his expert witnesses were familiar with those articles.

COURT: Well, the Crown, as I pointed out earlier, some of these articles in for a totally different purpose, or at least I gather they have. I have said what weight I attach to them, or what importance I attach to them earlier, they merely prove that in Dr. Waye's case that he's published certain of these papers. But I'm not going to look at their content, necessarily, as evidence. I shouldn't say necessarily, I'm not going to look at them at all for content. But the ones that you have provided, perhaps you intend for a different purpose. Perhaps you intend to cross-examine on their content, cross-examine various Crown witnesses, Crown experts. Have you indicated to the Crown what use you intend to make of those?

MR. FURLOTTE: Of their documents?

COURT: No, of the documents you've provided them with.

MR. FURLOTTE: I've never told the Crown what intention I have, what areas I wish to make use of them, no, any more than he's advised me as to which areas of these documents that he intends to make use of.

COURT: Am I underrating, Mr. Walsh, the importance of some of the articles and exhibits you've put in?

MR. WALSH: Well, My Lord, as you're aware, at the outset the Crown's position was that we believe that -- or the position we're taking is that the test for the Court on this particular hearing before the Jury would be actually entitled to hear this, the test that we're submitting is a test of reasonable reliability, if we can establish on balance that what is here, what we have here is evidence that's reasonably reliable so that it can be assessed by a Jury and then weight can be placed on it by the Jury. In that particular regard, it was our opinion, the Crown's opinion, that the introduction of these particular documents through -- most of the documents are through their actual authors -- go towards indicators of reliability. It's an indicator that they have published with respect to the steps in the test. It's an indicator for the Court that what is in fact being shown the Court or offered to the Court is reliable. It's one indicator of reliability.

At the same time, we recognize that this Court may rule that in fact what we have here is a Frye hearing and we must show on balance that what is involved -- we must show acceptance in the general scientific community.

Again, this documentation --

COURT: Well, it serves that purpose, again.

MR. WALSH: -- is an indicator of general acceptance in the scientific community. That is the purpose behind the introduction of some of these documents. There are some that Dr. Wade did not author, but he actually adopted in his testimony. I remember the documents dealing with the testing with respect to environmental concerns, things of that nature, and he indicated that there was testing out there. My understanding is he adopted and relied on those types of reports in which he -- to develop the technique at the R. C. M. P. lab, or one of the developers.

So this evidence certainly goes to all those indicators. That's why, in Mr. Furlotte's case, I haven't taken a strict position in terms of what he introduces or doesn't introduce. But I do accept what the Court has said, that you cannot simply drop a document into the courtroom and expect the Court to say that this is authority for what is actually said there, unless there's some weight or a foundation can be given to it.

COURT: Can we leave this business of what preparation Dr. Waye or any of the other experts should have insofar as other documents on which they're going to be examined is concerned. Can I leave that to the two counsel involved to discuss this after we adjourn this afternoon and if you can do something to speed it up or make it so that the witnesses will be a little better prepared or have some advance notice of the fields of examination that could be done? Okay, so why don't you go on, Mr. Furlotte?

Q. Would you agree, Dr. Waye, that the forensic setting is much more demanding than the diagnostic and experimental utilization of this procedure?

A. No.

Q. Is the fact that you're dealing with, maybe, contaminated DNA samples or degraded DNA samples does not make it any more demanding?

A. It doesn't make it any more difficult to analyze the samples, to my mind, and again, we're speaking personally. The comparison between clinical and forensics if you're just talking about the RFLP procedure, its demand or its difficulty lies in the end use, the decisions, the consequences of your using that technique and the ramifications of using that technique, and clinically I would argue that the weight or significance that's put on the result is much more -- has much more importance than forensically. The decisions that you base on it are life and death.

Q. Would you agree that the failure of molecular weight markers to align properly on a gel indicates a malfunction of the electrophoresic process?

A. If the markers didn't work?

Q. If the markers didn't align properly?

A. It means that the markers didn't align properly. It means that they didn't migrate through the gel properly.

Q. If such an event occurred, should you attempt to interpret the autorad?

A. Well, if the markers don't work in one lane or they didn't run properly in one lane, the computer itself is not going to have any base. It's not going to have a ruler with which to size those fragments.

Without the sizing data, we again made it pretty clear yesterday without the sizing data, regardless of what we see visually, we don't have a ruler or anything to make --

Q. What you're saying is it would not be scientific to attempt to form a match on such an autorad?

A. In a forensic setting. In a clinical setting, you don't have to go very far to find scientists -- perhaps not clinically, but in a research environment you don't have to go far to find scientists who wouldn't even run these types of markers around their samples. They rely solely on watching where the bands go and experience.

Q. Okay, but in the forensic setting, if the marker lanes did not line up properly, you would not attempt to draw any conclusions on that test?

A. Again, I have to crawl into your mind and find out what the word properly means.

Q. If the marker lanes don't line up properly, would you consider that test to have failed?

A. Could you describe to me what you mean by line up properly?

Q. Well, I guess that, again, is a subjective basis. Would it not be depending on how far out of line they were?

A. In line, out of line, you have to describe what you mean. If I ran one set of markers and say there's four bands there, the positions of my four fingers, and that's how far they are from the top of the gel, and I ran four here, is that aligned properly to you? (Witness indicates.)

Q. From here?

A. I'm just trying to get a definition.

Q. They don't look too bad from here.

A. If it was like this (witness indicates) what would that mean to you?

Q. I would say they're out of line.

A. Okay, I just want to know where we're standing here. The computer is capable of analyzing that type of data.

Q. It can analyze that type of data?

A. Yes, the computer can. It's not optimal data, but the computer is quite capable of handling that. What the computer does, it'll scan back and forth between flanking markers, which is why we always flank our sample lanes that we're analyzing by markers. They're flanking markers. The computer will bounce back between its reference points and analyze things in between. It's capable of doing that. That's well within the computer program.

Q. Okay, but I believe you told me earlier that you discounted computers whenever you could see a visual misalignment?

A. We weren't talking about markers at that point.

Q. What's the difference when you're talking about markers or you're talking about DNA fragments of polymorphic nature?

A. I'm sure there's no difference to you, but in my opinion, you're talking different things. One, I know the markers are the same.

Q. What if you have missing markers in certain lanes?

A. The marker lanes missing? Somebody forgot to load it or it didn't get detected?

Q. Yes?

A. Then I don't have a ruler. Again, I can't make that comparison.

Q. What if you have four marker lanes across your gel and maybe we've got some missing in lane one or lane two, three or four, would you just ignore the missing ones and try to establish a sizing off the remaining ones?

A. You're talking about lanes of markers being missing or the rungs on the ladders, individual ladder, ladder wrungs in the sizing?

Q. The individual ladder wrungs?

A. They're called a sizing ladder so I draw the analogy to a ladder.

Q. Okay, ladder wrungs.

A. If you're missing a wrung?

Q. If you're missing a couple of wrungs in a lane or two?

A. Well, if you're running a marker that should have ten bands in the wrung and you're running it as three, I can't understand why that would happen but that shows you something did go wrong and you couldn't really use that marker to base your size estimates.

Q. So you would use the other markers to --

A. If you had other marker lanes flanking your sample, yes.

Q. Okay?

A. If you didn't, you wouldn't draw your conclusion because you can't size it.

Q. What about your cell line markers? I believe that's an allele of a known number of base pairs, known polymorphic probe? Did the R. C. M. P. run that as a control, a human cell?

A. What you just described wasn't a cell line --

Q. When you run a human cell as a control? Is that a nonpolymorphic site for the known individual or was it a polymorphic?

A. The controls you're describing are human DNA samples.

Q. Human DNA samples? Right, and are they monomorphic or polymorphic?

A. Well, they're human DNA samples.

Q. It's just a human DNA sample and it's the same probe that's run down?

A. I'm just trying to straighten out -- you just can't say is a human DNA sample polymorphic. It's a human DNA sample. Polymorphism means could it be different from something else. It's a human DNA sample so it will be different from another human DNA sample.

Q. When you run the tests, I believe you run -- at the R. C. M. P. they run a couple of known individuals as the DNA of Nancy Monteith?

A. When I was doing case work, the DNA from Miss Nancy Monteith was run.

Q. And there's DNA also from another known male?

A. It's from a cell line which is just cells from an individual and they're immortalized and you grow them in culture.

Q. So when you run those, you know what the base pairs ought to be or --

A. For a given probe?

Q. Yes?

A. A VNTR probe?

Q. Before you do any probes -- let's take, for instance, the lane for Nancy Monteith. Now, I assume you run her with all tests or just -- if you're testing other cases also, you run her as a control in all of them?

A. Well, when I was at the R. C. M. P. doing case work and doing population work, that particular sample, I'll depersonalize it, make it NM, was run on all of gels, population and case work.

Q. So you would basically known roughly the base pairs that NM would have for each probe beforehand?

A. Roughly, and I say that because we've analyzed NM's pattern for each of the probes hundreds of times but we haven't, as I mentioned yesterday, gone to the task of pulling out her bands and actually counting them up. So I know that, you know, remember we don't measure the precise base pairs. I know that when I run her sample with D1S7, I'll get a two-banded pattern and it'll be of this size and this size plus or minus whatever I have observed.

Q. Did you ever check for the variation that you might get with her known DNA being run each time?

A. That's why it's put on the gel, because you have a formal expectation of the result, and again, if somebody sends you the wrong probe or you, for one reason or another, working with a probe and it's actually not the probe you think you're working with, you won't get the expected result. It's an immediate indication that I thought I was working with D1S7, I got the pattern for D2S44. Something's wrong. That's why you put it on there.

Q. Okay, it's to assure you're working with the correct probe?

A. It's a control.

Q. It's a control. Now, in all the different tests you would run with NM, would any observation be taken as to how far, for the same probe, how far the sizings may vary in the different tests run?

A. Yes, those bands are sized.

Q. How much would they vary? Do you know? What was the greatest variation that you can remember of hers?

A. You'd have to pick a locus and a band, and I'd have to, again, get on an airplane, go home, and start rummaging around in my notes.

Q. What would be the normal to expect, the maximum?

A. Again, I can't answer that question with the date in front of me, without it in front of me. It's a lot like the numbers we were talking about yesterday with the monomorphic.

Q. With the monomorphic, up to six percent variation?

A. Well, no, yesterday we were talking zero to five and values in between there. When you're looking at these other bands, you'd be talking the same sorts of range. It's not a 45 percent or 95 percent as you might like it.

Q. Have you done any studies or testing as to how cancer could affect the mobility of DNA, the migration rate?

A. Cancer?

Q. Yes?

A. Somebody having cancer?

Q. Cancer cells, yes, cancer cells of DNA?

A. Just last week I was working with tumors.

Q. Pardon?

A. Just last week I had the occasion to DNA type with some of these probes a tumor.

Q. Could you expect a different migration rate if the cell was infected with cancer?

A. In this particular case, the example I had, no there wasn't. Not a surprising result. Cancer is probably due to a mutation in a gene that's critical to growth. I was analyzing the tumor with a probe that doesn't code. It's non-coding, recognized as a non-coding region and probably not involved in tumor growth.

Q. So you're saying cancer mutation would not affect the sites that you search for in the DNA testing for forensic purposes?

A. Cancer is a big word. There's a lot of different mutations that cause cancer. There's a lot of different types of cancer. There's a lot of different mutations that you can have in a genome that will give rise to various types of cancer. All I'm telling you is that those mutations are in genes that have critical functions in the cell regulating cell growth and keeping cells growing in a controlled manner. These regions that we look at do not code for protein functions.

Q. If a person's DNA, and maybe for the lack of a better word, I'll say was infected with cancer, would that be considered a contamination or a mutation?

A. Infected with cancer. It's not really something you catch, it's something that happens and the cells that are derived after it have that change in it, and it's a mutation.

You alter the DNA in one particular cell. If the alteration that you make in that cell is at a region of DNA that's critical to controlling cell growth, the cells that result from divisions of that mutated cell can develop into a tumor because they are now programmed for uncontrolled growth.

Q. Let me put the question another way. If DNA has been known to have been infected with cancer, any type of cancer, and that infected DNA was being run through the gel, could the migration rate change because cancer has attacked that DNA cell? In other words, could it create band shift?

A. It's really a difficult question to answer because you're not talking about something that's real, you're talking about cancer being an infection, you know, something that one cell can either give to another cell or one human being can give to another human being. What it actually is is something that happens to one particular cell. That cell continues to divide and, naturally, the cells afterward have that mutation as well. Again, if --

Q. Okay, one cell. What about the cancer that affects or destroys the white blood cells?

A. Leukemia, or something like that.

Q. Leukemia, yes.

A. Or alters.

Q. So if you were going to run a DNA sample on somebody who had leukemia, would that DNA run or migrate at the same rate as that person if you tested them before they had leukemia?

A. Yes, it would. It would migrate at the same rate.

Q. It would migrate at the same rate?

A. Yes.

Q. No change? So it would not create band shift?

A. No.

Q. Would any experts disagree with you on that fact that you know of?

A. I'm sure you probably could find somebody that might give that opinion. I can't imagine somebody having an opinion that having extracted DNA from a tumor cell, that DNA as a whole would band shift because it was from a tumor cell, or from a cancer, but Lord knows, somebody might come up with that opinion. It's not my opinion.

Q. Do you consider yourself qualified to make that opinion?

A. I've analyzed DNA from tumor cells and done exactly what you said, as I said, last week, taken a tumor and compared it to the blood and a skin biopsy all from the same individual, analyzed it.

Q. And you didn't find any band shifting last week?

A. Nor did I expect band shifting.

Q. Nor did you expect it, and you would have never, ever found band shifting in such a situation?

A. That's not an experiment I'd repeat over and over again. There's no purpose to it.

Q. Are you aware of Dr. D'Eustachio's study on the validation of environmental insults?

A. A published study on environmental insults?

Q. No, not published.

A. I have spoken with the gentleman.

Q. You have spoken with the gentleman?

A. On the telephone.

Q. About his concerns with him?

A. I spoke with him about other concerns. We've talked on the telephone. I believe he has expert reports being disseminated all over the world. Is that what you're referring to, an expert report for the defence?

Q. Criticizing the validation studies on environmental insults that you rely on? Is that the basic gesture of his expert reports, that he circulated around the world?

A. I think you're stretching that a little bit. You're assuming that everything he's criticizing is everything that I base all my opinions on. Am I mentioned in that article?

Q. No, you're not mentioned in the article. You know which reports, I believe, have been introduced into evidence here that you were relying on, the environmental studies? I believe there were two separate ones?

A. Again --

Q. You have relied on or that you were aware of?

A. Again, I'm aware of those studies. Those are studies amongst others that I take into consideration when I consider the effects of the environment. Those aren't things I look at and say I don't have to think about this any more.

Q. So you didn't concern yourself about the effects that the environment might have on DNA samples for forensic purposes because you relied on those studies to show that they were inconsequential?

A. I find no evidence that environmental insult can take samples that don't match, make them match, and my test procedure be unable to distinguish that. I am confident, and I think all the data out there will back me on this, or I'm confident it backs me

on this, that although environment can influence the way DNA migrates, as we've talked about band shifting and stuff, I recognize and I always have recognized that things can cause slight variations in how things migrate. You control for that and you build in as part of your system ways to check for that, and ways that will help you interpret whether or not that happened.

Q. Are you aware of Dr. E'Eustachio's criticisms about these environmental studies?

A. I read through that --

Q. Expert report?

A. Once, not recently. Could I see it?

Q. Dr. D'Eustachio found that multiple gels were scored as successes even though the relevant positive control tracks failed. Did you research this data yourself to see if that was in fact true?

MR. WALSH: My Lord, I think first of all --

WITNESS: You're referring to the wrong data.

MR. WALSH: Excuse me, My Lord, first of all I would make an objection. My objection is twofold. One, the Doctor just said it. Mr. Furlotte has to at least give some clear foundation of where that opinion fits into relation of whose data, but more importantly, I would think, if Mr. Furlotte, before he can start having Dr. D'Eustachio testify, he has to establish that Dr. Waye accepts that report as authority. That's my understanding. But that is the two bases that I formulate my objection, My Lord.

COURT: Where does this report come from? It's not in evidence now?

MR. FURLOTTE: It's an expert report that was submitted in the Yee case

COURT: Well, again, may I suggest that if you want to ask questions on this report of this witness, why not let this witness have a copy of the report and then ask your questions next Wednesday. Is that fair enough? Dr. Waye has said that he, on some occasion, has read a report which presumably is the report to which you are referring, Mr. Furlotte, but --

MR. FURLOTTE: The expert report.

COURT: But it's obviously not clear in his mind. He may have read it incidentally or something some time ago, I don't know.

MR. WALSH: I pointed out to Mr. Furlotte, My Lord, that I would not consent to a wholesale introduction of experts reports that are written for other cases in other countries or in other States because it's a very difficult and deceptive type of practice. I'm familiar with one letter, in fact, written by one expert with respect to a report that he had filed for one case and it got bootlegged and used in other cases and he filed a pretty strong objection to it.

I pointed out to Mr. Furlotte that as far as expert reports go, I'm not going to take a liberal attitude. I must object. He can put statements, obviously, to Dr. Waye. If Dr. Waye doesn't accept them as authority, I don't know how he could have those people testify.

COURT: What do you mean when you say you don't know, he could have those people testify? You mean testify through the reports?

MR. WALSH: Through the report in this courtroom is what I meant.

COURT: Well, I think your position is correct, Mr. Walsh. As I said earlier, if Mr. Furlotte wants to extract statements or pronouncements or findings of the report and put those to an expert witness and say, "Look, do you agree with this or don't you," okay, that opinion can be solicited or that evidence can be solicited. It doesn't amount to introducing that pronouncement or that opinion as evidence in this courtroom.

MR. FURLOTTE: As again, it's just hearsay evidence and it's not offered to this witness or to the court to prove the facts of this hearsay evidence.

COURT: It's merely a question that you're putting to him.

MR. FURLOTTE: It's offered for the purpose to show that there is stong opposition against the validity of these validating articles and that there is opposing opinions --

COURT: You're getting into some detail here with this article of Dr. D'Eustachio, is it?

MR. FURLOTTE: D-'-E-u-s-t-a-c-h-i-o.

COURT: I think we could save time if perhaps a copy were provided the witness and he had a chance to look it over and then you put whatever few questions you have to him.

MR. FURLOTTE: I don't have a copy of the expert report. Dr. Waye has admitted that he has read the expert report as to the criticisms of these validation articles.

MR. WALSH: My Lord, again, there is the very point I'm making. Mr. Furlotte doesn't even have the report with him. He's reading some comment. We don't know the context in which it was made. He's certainly entitled to all kinds of liberty to cross-examine, but not in this fashion.

COURT: What does he say, or what is it you're trying to ask Dr. Waye?

MR. FURLOTTE: I'm reading from case law in Yee which was reported at page 28 of the Yee decision at which the trial judge made findings of fact about the criticisms that were in the expert report.

COURT: If you want to quote from the trial judge re Yee and ask this witness if he agrees with some statement, ask him in the same way you did. But why bring D'Eustachio into it? Have you got something from Yee that you want to ask?

MR. FURLOTTE: Yes.

Q. Dr. Waye, I assume that you relied on these so-called validation articles on the effects of environmental insults on DNA?

A. I was doing case work before those articles were even published. Those articles merely confirm what I already knew.

Q. Had you done any tests yourself?

A. Environmental insult tests?

Q. Yes?

A. Yes.

Q. Did you attempt to get your studies and your experiments validated?

A. Validated?

Q. For peer review?

A. Attempt to publish?

Q. Yes?

A. Isolation of DNA off corduroy, et cetera, no, I didn't. Those are embarrassing scientific questions.

Q. What environmental studies did you do yourself?

A. Effects of isolating DNA off various materials, et cetera.

Q. Just off materials?

A. I, from the beginning of my forensic experience, have taken the approach that in a laboratory you can't replicate everything that happens in the environment and there's not much point in even trying. I, scientifically, took the approach that you build a system that if the environment were affecting the way a band was migrating, you would be able to detect it. I've always recognized that the environment can cause a band shift. Nobody has ever demonstrated that the environment will take a two-banded pattern and turn it into a twenty-two banded pattern. It's not a good scientific premise.

Q. Now, the validation studies that you read here, I suppose in some sense are relying on, they were conducted where, the F. B. I. lab, and where else?

A. Again, those studies that were entered there had nothing to do with the F. B. I.

Q. Nothing to do with the F. B. I.?

A. Which is --

Q. Who conducted --

A. -- Peter D'Eustachio's criticisms of F. B. I. results. Those results were done, involved scientists from Academia and scientists from Lifecodes.

Q. And scientists from Lifecodes. Now, I understood your testimony when I was discussing about ethidium bromide contaminating DNA samples, that the explanation was by the F. B. I., and you agreed with them, that you conducted your test on ethidium bromide and how it affected band shifting. Since that was done in your lab and your design system that that might not hold true for the labs performed by other companies because ethidium bromide does not affect the F. B. I. and their process.

A. That's not, again, quite what I said unless my memory is diminished. What I said is that we demonstrated that in our system it had an effect. I think the onus goes on the F. B. I., then, since they were using it, to document its effect in their lab, and simple bake-off experiments were done with both procedures at the F. B. I. to show if it did or didn't have an effect in their system and what, if any, that effect was.

Q. And their experiment, I understood you to say, that it didn't have that great effect at the F. B. I. lab that it had in the R. C. M. P. lab?

A. That, again, was their conclusions, that they didn't feel it was -- I didn't do those studies myself. Those are all stuff that I've heard.

Q. So how can you depend on environmental insult studies done in somebody else's lab?

A. I told you I didn't depend on it. I was doing case work before those were ever published.

Q. And the only thing you attempted on environmental insult was how the DNA was affected by certain materials, on corduroy or something?

A. I was quite well aware of all sorts of other studies done mixing -- these were done at the R. C. M. P. before I even came -- mixing DNA with all sorts of different contaminants.

Q. So your lab has not conducted any studies on how DNA might be affected by heat, humidity, soil, smoke?

A. I have to go back and go through the bookshelves and the old notes to see whether each or every one of those things that you've looked at has been done, and I'm sure if they were all done, you'd come up with something else, turpentine, or gasoline, unleaded, leaded. That's the argument about these environmental studies. If I say I mixed blood with gasoline and showed it had no problem with VNTRs, you'd say leaded, and then we'd go to octane, and from a scientific point of view, you're really dealing with an issue that's a no-win situation, I'm going to continue to show that things have no effect, when in fact I have a system that will measure if I do stumble across something that does have an effect on band shifting.

Q. So are you saying maybe it's something based -- one of the reasons why you don't do your own -- or the R. C. M. P. doesn't do its own validation studies in the effect of environmental insults is because maybe the same reason you use HaeIII, the cost, it's cheap?

A. That was one of the reasons. I didn't say the R. C. M. P. doesn't do validation studies, or doesn't do its own validation studies, and Hae III, it was frutuitous -- it was nice that it was cost effective, using it. I think if HaeIII had cost

50 percent more we'd still be using HaeIII. It was nice that it happened to be one of the cheaper ones.

Q. At least according to some of the experts in the field of DNA evidence, you do admit that they contend that environmental insult studies are not the same for every lab? In other words, in the R. C. M. P. lab where you got band shifting and serious -- what you thought might be serious band shifting, caused problems with interpretation of autorads, that the F. B. I. does not obtain the same results of band shifting because of ethidium bromide contamination. And it could work vice versa that, although the F. B. I. doesn't obtain any notable band shifting because of environmental insults in their lab, if you run that same process through your lab, you, again, may obtain serious band shifting?

Did you understand all that?

A. I understand. I have a very difficult time following the logic. I feel that -- and I hope I've made this clear -- that the environmental factors can cause band shifting. Maybe I'll underline 'can' again, can cause band shifting. So what the experts are saying, that the environment can influence the way a DNA molecule migrates, I agree. I agree with all those experts, always have, that's why I designed a system so if there were band shifts, I could detect them and I could deal with them. Don't you think that's a nice direct way of doing it rather than saying unleaded gasoline causes band shifting, does leaded gasoline. Go home and say, "Honey, what else could I mix DNA with to ask this question." It's not scientifically logic or

practical way to address the question.

Q. So what you're saying is because you developed the monomorphic probe to detect band shifting, you couldn't care less what effect the environment has on DNA?

A. I applied the monomorphic probe precisely because I do care what effect it has. I want to ensure if I have a match like that, that that's not a match that was forced because of a band shift, either up or down.

Q. Yes, but your monomorphic probe is going to tell you that.

A. That's exactly why I --

Q. That's what you're saying?

A. -- why -- and I didn't develop the probe for that specific purpose. I pulled it out of the freezer, it was for another purpose, but that's precisely why we took that logic in building that in as part of our system.

Q. So now, band shifting caused by environmental insults is absolutely no concern of yours because you now have the monomorphic probe to tell you whether or not it has occurred and how much? It that a safe assumption?

A. Other than the wording, I'd probably agree with you. These aren't things that I frivolously write up, I don't care about the environment, I don't care what shape that DNA is. We go through a lot of tests to define what type, what shape the DNA is, or how it's endured the environment right from beginning to end, and I think the critical test is the end product, asking the question, well, how did it migrate.

Q. Are there also criticisms out there against your paper that you wrote about the ability of a monomorphic probe to detect all band shifting? Are there any criticisms against that theory of yours?

A. I've drawn that criticism myself in my own publications as a scientist in the discussions of papers, that it's theoretically possible that you could have a band shift in one area of the gel and not a band shift in another area of the gel.

Q. And monomorphic probe would not pick it up?

A. Obviously if the monomorphic probe is in the area of the gel that doesn't have the band shift and the other does, it didn't pick it up, and what you have there, if it's a visual band shift, you take a piece of data that was an inclusion and you call it inconclusive. Again, you've said nothing about where that sample came from. It didn't come from your client. You're not making a conclusion that it did or didn't come from your client. You're saying, "I can't call it."

Q. If there's a band shift, would it always be in the same direction?

A. Not necessarily.

Q. I'll say for each individual sample being run, you might have one sample shifting in one direction and the other sample shifting in the opposite direction. But, say, for one sample in lane four, are you going to get the band shift for every fragment in the same direction, or will even these fragments shift all over?

A. In general with band shifts, if you have a shift of one fragment in one direction, the other

fragment will shift in the same direction.

Q. In the same direction?

A. At one point it was thought that that -- That's my general experience whenever you try to replicate band shifts in the lab, either with the ethidium bromide or other ways, or if you actually observe them dealing with samples that you know came from the same individual, like comparing vaginal swab, DNA back to blood, and you know if you have a shift there. These are from the same individual, so you know if you have a shift. But generally one band will shift in one direction, the other will shift in the same direction. That's an observation of fact.

Q. That's an observation of fact?

A. I don't exclude the possibility that one band could shift up and the other band, if it was in a remote distance, quite some distance away from it and difference in size, could shift in the other direction. In fact, I'm aware of examples like that. It's very rare. It's a very infrequent observation. You really have to show it to people to illustrate that it can happen.

Q. But are we talking, or are you talking now about the two bands for the same probe, or are you talking about the bands for the different probes running the same length?

A. My example there was one probe.

Q. One probe?

A. Samples that I knew came from the same individual. One was in the environment and one was from the body, so the samples I know came from the same individual.

Q. So for example, say --

A. One band shift is slightly up and one band shift is slightly down. The patterns were still visually -- you look at them and you go, that's consistent with coming from that individual and you know it came from that individual anyway. That's how you know you had a band shift, because I know the origin of both samples.

Q. All right, we have a fair idea that contamination or degradation might cause band shifting in one direction for both bands. What could cause band shifting in opposite directions?

A. Again, I have no idea what would do this.

Q. Is this what you would call an anomaly which is unexplainable?

A. The phrase electrophoretic anomaly has been brought into play. The point is that you take things that should look identical, they shift a little bit, you look at it, it's inconclusive. At one point people called those -- would call that --

Q. At one time you would call that a match?

A. Not myself.

Q. Not yourself?

A. I'm aware of people who have.

Q. Would that be proper?

A. Not in my opinion.

Q. Not in your opinion? I see also in the Yee case, page 129, that Dr. D'Eustachio appeared to be concerned that choosing a match window that exceeds an acceptable level of risk, that there is the risk of false positives, having too big a match window? Is that possible?

A. Well, it's raising the concern that if I allow for bands to be 20 percent apart and still call them a match, that I am running the risk of false positives. Certainly, if you have a huge match window the largest extreme would be let's consider the whole gel, our match criteria, everything from top to bottom, everyone is going to be a match. That's the extreme.

Q. So would you admit that if there was too big a match window, that you could end up with false positives?

A. I just said that, everyone on that -- if that whole surface was your match window, everyone on there is a match.

Q. But if you use that same criteria in formulating your data base, I understood that that was going to correct that.

A. If I used that same formulation for building my data base, we'd have one bin and everyone would be the same, so I'd analyze the DNA and I'd say he's a type one. I'd analyze the next person and I'd say, hmmm, he's a type one, too, and we'd go on that way. We'd accomplish nothing.

Q. So in order to have the best discriminate powers that you can get, the smaller the matching window, the better.

A. Obviously it's ludicrous to consider the whole thing a match window. It's somewhere between, and remember you have to realize that you don't have base pair resolution, so it's ludicrous at one level to have a match criteria be the entire gel. It's ludicrous at the other level to have your match criteria to be exact base pair matching as you were suggesting yesterday. Those are the two extremes. Somewhere

in between there you've got to look at your data and say, "What reflects reality?"

Q. And I believe he was also critical in that he found that a laboratory should meet, you know, matching criteria and that you should understand the factors that alter band migration which you agreed with him that because you can't understand the band shifting in different areas in lanes --

COURT: Who was critical, Mr. Furlotte, and who agreed with the criticisms?

MR. FURLOTTE: No, I just said. I'm saying that Dr. D'Eustachio --

COURT: You're saying somebody -- was critical? How do we know that?

MR. FURLOTTE: In the Yee case -- no, Dr. D'Eustachio, in the Yee case --

COURT: We don't know what he said in the Yee case. Some judge says what, that -- what does the Yee case say?

MR. FURLOTTE: It's just in the judge discussing the evidence given by Dr. D'Eustachio and his criticisms, in a sense, not criticisms, but his opinion as to what the match criteria should be before --

COURT: It wasn't criticism, then?

MR. FURLOTTE: Well, I believe D'Eustachio agreed with Dr. Waye that if you don't understand the factors that alter band migration, then you should declare--

COURT: Where are you telling this witness that he criticized? No, you were telling Dr. Waye that D'Eustachio criticized something and now you say he didn't criticize. We get back to this --

MR. FURLOTTE: Well, okay.

COURT: If you could put some precise statement to the witness.

Q. Do you agree? You agreed awhile ago that some scientists were calling matches when they had band shifting in opposite directions?

A. No, I don't agree with that.

Q. No, you don't agree with that, right.

A. I didn't say that, either. I gave that example.

Q. You gave that example, and I thought you said that you would not call that a match?

A. I said at one point people were calling matches like that, and I said I wouldn't.

Q. You wouldn't, you said? Because you said you didn't think that was proper, and I believe Dr. D'Eustachio says that one of the factors in making match criteria is that you must understand the factors that alter band migration and, because we don't understand why there's band shifting in opposite directions, he would not call the match either. Am I safe to assume that you agree on that aspect of it?

A. If that's actually what Peter D'Eustachio is saying and not your interpretation or paraphrasing or reworking of what he's saying, I'd have fault with his logic. What he's saying is that if I observe something a million times, I have to understand exactly why it's happening in order for it to be real, and I've had this discussion during one of the breaks wondering about why a fish would hit on a fly when you're fishing. You can catch fish with a fly but you don't have to understand what's in the salmon's mind when he goes after the fly. He's obviously not feeding. They don't feed then, is my understanding.

MR. FURLOTTE: My Lord, I wonder if it's time for a break?

COURT: Yes, we could have a break now. I sort of thought we might have a long session, a short period, or something. We had talked about stopping about four o'clock.

MR. FURLOTTE: Oh, well, it depends, how late did you intend to go today?

COURT: Well, I'm not going to go as late as we did yesterday, for sure, but why don't we take fifteen minute break and then we'll come back for about half an hour or so.

(Accused escorted from courtroom.)

(Court recessed 3:15 p.m. to 3:30 p.m.)

(Accused present.)

Q. Dr. Waye, did I understand from your direct testimony that Dr. Hagerman had consulted with you before he gave expert testimony in the Yee case?

A. I believe it was prior to him either issuing a report or testifying in person in that case.

Q. Yes, and did that have to do with the studies that you did with the ethidium bromide?

A. That was part of our conversation. We talked about a lot of different things.

Q. Did you know that he was consulting with you in order to prepare his report and his testimony in the Yee case?

A. Yes, prior to speaking with Paul Hagerman, Dr. Hagerman, I was called by Barry Scheck. I think he was defence counsel in that case.

Q. Yes, he was.

A. He talked to me about various things dealing with forensic DNA typing. At the end of that conversation he asked my permission if his expert could call me and again talk about various things. I saw no reason to deny that permission and I talked to Paul Hagerman after that.

Q. So did you basically know the crux of Dr. Hagerman's testimony, that he was going to give in the Yee case?

A. No, we talked as two scientists would talk. He asked me questions, I tried to answer them.

Q. In the Yee case, Dr. Hagerman was critical about the F. B. I.'s use of ethidium bromide. You were aware that he was going to attack the reliability on those grounds, were you not?

A. He asked me questions about the studies. My recollections are that his own studies, and his own considerations of the theory of electrophoresis et cetera, were borne out in our actual practical experiments that were published there.

Q. And if Dr. Hagerman's criticisms were valid in the Yee case, for instance, the effects that the ethidium bromide, that it might seriously compromise not only the reliability of the tests performed in that case, but also the reliability of their data base? Would that hold true also for the data base if it held true for the individual test in that case?

A. Was that his criticism?

Q. Would that be a valid criticism?

A. Yes, I think that's a point almost read out of the paper that I wrote, that if ethidium bromide in your system is causing a problem with how things

migrate, obviously that will affect data base samples as well as evidence samples.

Q. That would mean the data base would not be reliable either?

A. That would mean that the samples that you ran in the data base and the data that you derived from the data base would have diminished accuracy, if the ethidium bromide was having an effect on migration in your system.

Q. I assume the R. C. M. P. run the full data base with the use of ethidium bromide?

A. With the use of ethidium bromide?

Q. Yes?

A. No.

Q. They did not use the ethidium bromide in all their tests in conducting or in formulating their data base?

A. You just said the R. C. M. P. Did you mean the F. B. I.?

Q. I'm sorry I meant the R. C. M. P. -- I meant the F. B. I. I told you, it's getting late.

A. All these acronyms for law enforcement agencies. The F. B. I., their protocol, and again, I didn't build their data base, but the logical assumption would be that their protocol they followed for running data base samples is the same as case work samples, so they probably did build their data base in the same manner, with ethidium bromide.

Q. If Dr. Hagerman was correct that ethidium bromide would have affected the F. B. I. tests the way they affected the tests that you conducted, if it would have been the same for the F. B. I. when they used

ethidium bromide as when you used it, would that make the test and the data base unreliable? I know the F. B. I. claims that it doesn't affect their tests the way it affected yours, but if it did, would it make their tests and the data base unreliable?

A. I think you'd have diminished reliability in both if you could demonstrate that what you're trying to accomplish, you weren't accomplishing. If band shifting was occurring all the time as a result of ethidium bromide both in your data base and in your case work, well, that's essentially the message of the paper we published.

Q. Would it have been diminished sufficient that you really shouldn't rely on results? That you should draw inconclusiveness on the test rather than exclusions or inclusions?

A. Again, if it was causing band shifts such that you couldn't make calls as matches and you couldn't reproduce your data base or reproduce the patterns in two different samples, those are factors that affect the numbers and the data base, and those are factors that can affect calling a match. It's all a matter of degrees.

Q. In your tests with the ethidium bromide, what degree of shifts did they actually cause, the highest, up to what? What percentage of shifts?

A. In those particular experiments, again, those experiments were designed to demonstrate band shifting in the lab under controlled situations where we could monitor all the variables and find out exactly what's happening here. Those experiments

showed shifts of up to six percent.

Q. Up to six percent, and shifts to that significance would, I assume, be definitely unreliable if they had occurred?

A. Shifts of those magnitudes demonstrate that you are affecting the mobility. Again, I'd have to actually -- these are percentage shifts, right? I explained yesterday that I could look at patterns that are visually indistinguishable and sometimes the computer will tell me they're six percent off. Well --

Q. Okay, let me put it this way, a shift of six percent undetected would be sufficient to create a false positive, would it not?

A. Again, if you interpreted a test incorrectly, we keep harping back to this, you're --

Q. No, no, I'm talking about without a monomorphic probe. I'm not talking about your system. If the shift is undetected, a shift with the magnitude of six percent would be sufficient to create a false positive?

A. So you're sitting there blind with the blind assumption that everything ran perfectly --

Q. And nothing shifts.

A. But you got -- but everything, in reality, is shifting all over the place?

Q. Yes?

A. Could you ever -- well, you've already handled the data improperly to begin with. I think we've already dealt with it if you interpret things incorrectly and you use the system incorrectly, you run the risk of drawing the wrong conclusion,

and I'm not saying it happens all the time if you have ethidium bromide, and I think that --

Q. Which could create a false positive? If you're going to draw the wrong conclusions, you could draw the wrong conclusions --

A. If you want to misinterpret a test four different ways and then at the end say it's a false match, well, choose to do so. I don't even like to think about using science improperly, interpreting science improperly, coming up with the wrong answer and saying, "We get the wrong answer all the time." I just said that if you do all those things, you run the risk of getting it. Now, you want me to say how often would you get it.

Q. No, I'm not asking you to say how often you would get the false positive. I'm just saying that if there was a band shift to a degree of six percent, and that band shift went undetected, that you didn't know there was a band shift caused by either ethidium bromide or some other cause, that would be sufficient for you to interpret out of that test a false positive?

A. Again, a band shift -- we got into this yesterday -- a band shift is something visual. So saying a band shift of six percent -- the sizing --

Q. Well, basically if one lane shifted --

A. -- and then you went on to say that it's undetectable. See, band shift is something that you can see visually and then you say it's undetectable and I'm having a real problem figuring out what you're trying to get me to say. I've already said if you misinterpret the test as you want me to, could you get the wrong answer.

Q. Maybe I misunderstand what a band shift is. I understand a band shift to be something of which it ought not to be. In other words, if there was no contamination, the band would be here, but because there is contamination, then it has shifted and run down here, or it has shifted but it hasn't run far enough?

A. Now, if you look at that, would you call that undetectable or detectable?

Q. I take it it's undetectable because you don't know why it is here or here when it ought to be here.

A. And we're not using a monomorph?

Q. We're not using a monomorphic probe, and that's why we can't detect it.

A. So -- you're putting me in a place that I don't normally work in. You're putting me in someone else's lab using someone else's protocols, someone else's system, and you're misinterpretations and you're saying, "How would it work out?"

Q. How did you find out that there was band shifting ethidium bromide? What control did you use?

A. How did I find out? It's something that's been known for years.

Q. Tell me?

A. Well, like anything else that's been known for years, it's been documented in the literature. You open up the books, you use your eyes to start reading and it's something someone else has observed.

Q. How did you measure it, your degree of band shifting where sometimes you got up to six percent? What method did you use?

A. What was the precise experiment?

Q. Yes?

A. Took a DNA sample from an individual. We took increasing amounts of DNA from that same individual. We ran the test on that individual, both in the presence of ethidium bromide and without ethidium bromide. In the presence of ethidium bromide, band shifting was apparent and it was the magnitudes of the shift depended on the quantity of DNA you were analyzing. Remember, we analyzed a gradient of DNA from the same individuals. It's a visual shifts. And the adjacent analysis where ethidium bromide wasn't included, the bands had different intensities because you were analyzing different amounts of DNA, but there was no visual shift. You then took those autorads, you went to the computer, and you sized them. The ones that didn't have shifts, you determined the size for them. The ones that did have shifts, you determined the magnitude of the shift. Six percent was the number quoted in the paper. I can't remember the exact base pair numbers, but that's what's in the paper. That's what was published, and that's the end of the experiment.

Q. Okay, now, if we run a sample as you did with your experiment, say you run without ethidium bromide in lane B, if I understand you correctly, and you run one with ethidium bromide in lane C. You could expect, maybe, a six percent variation in these two lanes with the same DNA?

A. No, the experiment wasn't done that way. You ran all the samples in one gel. The gel itself contained the ethidium bromide and the buffer that the gel is immersed in, so the samples in lane A and lane B had ethidium bromide.

Q. That's right. I have no problem with that. What I was trying to do is try and show a description of how a contaminated DNA fragment might comigrate with a fragment of identical sources, identical DNA from identical -- from the same person. How you deliberately contaminate one fragment with ethidium bromide and the other lane of DNA from the same person you don't contaminate it with ethidium bromide. If you run it, you could expect to have a shift, a variation of band sizing, by about six percent?

A. No.

Q. Is that a fair assessment?

A. No, that's a total misrepresnetation of the data. What you'd have to do is have a look at the pictures that are in the article and maybe you'll be able to see what I mean. I can only comment on the experiments we published and did, and you're describing something that -- I'm thinking about it but I think it would be technically difficult to even comment or replicate what you're describing there. That's certainly something I haven't done.

Q. All I'm saying, Doctor, is if in your tests results you deliberately -- if you took DNA from myself and you extracted it and you had your two samples of DNA and you deliberately contaminated one of the samples with ethidium bromide, okay, is that fair so far?

A. Okay, we're doing something I haven't done, so --

Q. Well, yes, but we don't have to do everything one way. So if we have one sample that is not contaminated with ethidium bromide, we put it in

lane B. If we have the other sample that is contaminated with ethidium bromide, we put it in lane C, and we run our gel. You could expect that the two bands will not line up and they will vary by as much as six percent?

A. I couldn't put a number to that.

Q. But from your own experience you said you could get band shifting up to six percent like you did?

A. Band shifting of six percent was as a function of DNA concentration on the same gel run on the same conditions. You're describing two lanes that are run in different conditions, one with ethidium, one without, right?

Q. Yes?

A. You describe an experiment that has nothing to do with the empirical data that I published.

Q. No, I'm not saying --

A. What I will tell you from there is that it wouldn't be unusual, and it wouldn't be unexpected, to have a band shift there, and I think I've been more than clear in admitting that things you do to DNA can alter their mobility in gel, and adding ethidium bromide to sample B is adding something to DNA that can alter its mobility.

Q. Now, how I understand your monomorphic probe to work is that after you've run your gel and you've run your different probes, at the end you run your monomorphic probe -- say we're still using these two lanes, okay -- and if your monomorphic probe will tell you the degree of variation that maybe contamination caused, or will it just tell you that there is a band shift?

A. I can size the monomorphic probe and tell you that the monomorphic probe is shifted five percent of its weight.

Q. If a monomorphic probe shifted five percent of its weight from lane B to lane C, then would you expect the other probes to have shifted the same degree, five percent?

A. That wouldn't be a valid assumption, no.

Q. It would not be a valid assumption?

A. No, there's actual studies in there showing that -- showing, at least with respect to ethidium bromide, that the magnitude of the shift is dependant on the size of the fragments; limited data; it was actually requested by one of the reviewers, a question of that, and we did the experiment to address that.

Q. And there is no way that you can prorate it, depending on the size of the fragment?

A. Well, these are precisely the type of studies that Dr. Hagerman wanted to do in his lab. I wouldn't do them in my lab because we're not forensically dealing with prime samples that are laced with ethidium bromide. It's not a common thing that you find out in the environment. So you'd be understanding and doing all these theoretical studies, the effects of ethidium bromide. You don't use it in your system. I have a heck of a time trying to figure out the relevancy to spending a lifetime monitoring its effects on lane migration if you don't use it.

Q. It also seems that some scientists appear to be concerned that there's a problem with the persistent

interpretation of autorads with over-exposed bands. Do you see that as a problem in interpreting autorads?

A. The darkness of the band is a function of a lot of things, how much DNA you analyze, and how long you left the autorad on film, and the conditions you left it on film and the type of film, et cetera. If you leave something -- if you analyzed a lot of material and you left it on film for a long period of time, you have a couple of things that can blur your picture or make it difficult to analyze the picture. One thing is your band is going to not only increase in intensity, but it's going to increase in thickness.

Q. Increase in what?

A. In thickness. The band starts off with a very sharp line of radioactivity. The longer you leave it on exposing, the radioactivity not only goes straight up, it goes into various directions. The longer you leave it on, the wider the band becomes, so something that starts off as a thin slit, upon long exposures will eventually come to look like a football, or a blob, types of words that have been used in transit, blobs and dots, et cetera, rather than slit. That's all a function of both how much DNA you analyze and how long you chose to leave it one film.

Now, the problem comes in when you don't have very much control over how much sample to analyze, when you're limited in your evidence samples, when you have to analyze four pieces of evidence samples and they all have different amounts of DNA in them,

et cetera, and you can't get exactly the same amount of DNA in each lane, so the band intensities are going to vary in each lane. So obviously, if you have to leave something on film for, say, a week in order to see your bands in the sample that didn't have much DNA, during that week's time, your lane that had a lot of DNA, say, from your blood sample is going to go from being a thin, discrete band to something more like a football in shape.

Now you're asking the computer, find me the center of those bands. Well, when it looks at the slit, it has no problem finding that. When it looks at the blob, it'll find what it thinks is the center, but it obviously has a little more leeway to find the center of that, right? It's not a slit.

Q. There's no guarantee that the exposure will, I suppose, shift or the dark band will travel as much in one direction as the other?

A. The radioactivity will be expelled downward rather than upward?

Q. Yes?

A. No.

Q. There's no way, so it's not a question of the computer picking the center of the big black blob?

A. No, the slit will expand in both directions.

Q. It'll expand in both directions evenly, is that what you said?

A. Depending on where the radioactivity is in the slit. If it's evenly dispersed along the length of the slit, it --

Q. So are you saying the computer could pick out the center of the big black blob, but it would just be a little more difficult because of the size of it?

A. Oh, it does find what it thinks is the center of it.

Q. That's where it marks the band?

A. Correct, you ask it to look at the density of the image along the length of the lane and it will look at the density and it will draw peaks where the density is, and then you ask it where the bands are and it finds that peak and centers around that point.

Q. So while some scientists believe that you shouldn't interpret autorads with the big black blobs, you would have no hesitation in drawing conclusions on it?

A. Which scientists would say that?

Q. I don't know, I'm just -- I'm reading at page 32 of the Yee case. It's probably Dr. Hagerman. It would be Dr. Hagerman.

COURT: What does he say precisely, or what does the judge say that Hagerman said?

MR. FURLOTTE: The judge says -- I'll have to start at the first paragraph on page 31. It says:

> "Dr. Hagerman also described and analyzed the band shifting effects of ethidium bromide. He asserted that the F. B. I. did not adequately understand the ethidium bromide caused band shift problems. He stated that among other causes, the most serious problem with ethidium related band shifting and a cause that makes the problem of addressing the ethidium bromide band shift problem difficult, if not impossible, is the inability of the F. B. I. to accurately determine DNA concentration. He also cited other sources of error in the F. B. I.'s own ethidium bromide experiments including loading mass inaccuracy, the unnecessary use of increased amounts of restriction endonuclease and a persistent interpretation of autorads that displayed heavily overexposed bands."

MR. WALSH: My Lord, perhaps if we're going to get it within context, we should also, if we're going to read into the record for the purpose of the Doctor addressing a question as to the interpretation a judge has of the doctor's opinion in that case, if we could also read into the record the judge's opinion as to the effect of that particular testimony or his assessment of that testimony, I think would be appropriate, and if that was the case, I would ask the Court if we could refer to page 108.

COURT: Of the same judgment?

MR. WALSH: The same judgment, My Lord, yes. I think it puts it in the proper context because on one hand, Mr. Furlotte wishes the Court to know what Dr. Hagerman -- what the judge says Dr. Hagerman says, so I think it's important to know what the judge said of Dr. Hagerman's conclusion, and that's set out at 108.

COURT: Well, perhaps we can ask one or other of you two to read what he did say, what the judge did say. Actually, we're concerned here only with the very last little phrase or clause of what Mr. Furlotte read out dealing with the blobs, or the overexposure to radioactivity which would create a blobbish mark. Wasn't that the -- that was the point that you were asking this witness about.

MR. FURLOTTE: That was the point. It's not the conclusions.

COURT: What were those words, those last ten words of what you read?

MR. FURLOTTE: It says Dr. Hagerman also criticized..

> "...the unnecessary use of increased amounts of restriction endonuclease and a persistent interpretation of autorads that displayed heavily overexposed bands."

That's the only issue that I wanted to get out.

COURT: Yes, but let's hear first what the judge said about -- did he make a finding on that?

MR. WALSH: Well, he made a finding -- Mr. Furlotte read not only that last statement, he read all the causes of ethidium bromide. That's why I felt it was necessary to actually put it in the right context.

MR. FURLOTTE: I didn't want to be accused of taking it out of context.

MR. WALSH: Well, I just want to add --

COURT: No, but if the judge says, "No, I don't accept a single opinion of Hagerman's," that presumably puts an end to the whole thing, doesn't it?

MR. WALSH: One of the conclusions, at least, I can direct to the Court at this time, and you have the case, My Lord, but one of the conclusions I would ask to read into the record to put the Doctor's opinion in context if he's going to be asked one here, is the judge stated at page 108:

> "With regard to the testimony of Dr. Hagerman about the effects of ethidium bromide, I find that there can be little doubt that there is a likelihood of band shifting that can result from the use of ethidium bromide just as the defects in the validation, mixed body fluid, environmental insult studies suggest that band shifts can occur from other causes. However, even accepting the likelihood of band shifting in some instances, I find that the likelihood of multiple shifts resulting in a match to be so slight as to be a matter of weight and not admissibility."

MR. FURLOTTE: My Lord, that has nothing to do with the issue at hand here.

MR. WALSH: Well, why did he read it?

COURT: Well, this is sort of going back to the earlier part, it has a bearing on that, I think. Does he go on and talk about all this overexposure to radioactivity?

MR. FURLOTTE: No, he doesn't.

MR. WALSH: I don't have it right at that point, no. It does go on to say in accordance with what Mr. Furlotte had read, part of what he had read, he went on to say at the same page,

"Like the F. B. I.'s selection of -- "

MR. FURLOTTE: Well, My Lord, again, that has nothing to do with the interpretation of autorads that display heavily overexposed bands.

MR. WALSH: Well, why did he -- I'm sorry, My Lord.

COURT: All right, well, what do you want to ask this witness now, Mr. Furlotte?

MR. FURLOTTE: This witness asked me -- I told this witness that some experts out there in the field believe that a problem is that there is a persistent interpretation of autorads that display heavily exposed bands and that this is improper, and this witness asked me who said that, so I just brought back as to what the judge said that Dr. Hagerman said.

COURT: Well, he's not allowed to ask you a question, so that puts an end to that.

Now, you haven't got any further questions you want to ask him?

MR. FURLOTTE: I have nothing more on that.

COURT: So let's stop right there, then, for today and --

MR. FURLOTTE: Well, there's just one other question that I -- do you think that there could be a problem on attempting to interpret overexposed bands?

A. Not if you do your analysis properly.

COURT: All right, we'll stand this witness aside until whenever you can agree, counsel can agree, to call him back.

MR. WALSH: We'd like to have the Court's direction on a matter, My Lord. At this point in time I had originally asked the Court's permission to stand Dr. Waye aside until I could recall him after Dr. Bowen's testimony. Mr. Furlotte had elected to cross-examine Dr. Waye on the testimony he had presented up until that point. Now, when Dr. Waye comes back, will he be, when he comes back and is put back on the stand, will he be subject to further cross-examination by Mr. Furlotte on the issues he had previously testified to before I go into my recall direct examination? Or would he be recalled and I would start my direct examination and then Mr. Furlotte would continue with his?

COURT: Well, Mr. Furlotte had indicated earlier he would like to complete the cross-examination on this section first, am I right in that?

MR. FURLOTTE: On what he's testified to in direct evidence, I want to cross-examine him on that first before he's recalled to testify on the other matters, and that I want to finish that cross-examination of Dr. Waye as soon as Dr. Kidd is finished.

MR. WALSH: No, you're not going to get that right.

COURT: What we will do is we will finish the cross-examination of Dr. Waye when he resumes the stand, then we'll have your re-examination on this portion. Then, assuming you go on with his testimony, we'll have your direct examination on the second phase, then cross-examination on that phase then your re-examination.

MR. WALSH: Fine, that clarifies my position. Thank you, My Lord.

COURT: I want to --

MR. FURLOTTE: Just to make that clear again, what did you say?

COURT: I said we'll have --

MR. FURLOTTE: When do I get to -- when is Dr. Waye going to be recalled back on this matter?

COURT: Well, I haven't any -- have you agreed on this?

MR. WALSH: Here is the other point.

COURT: What I've said has no bearing on when he is called back. I'm saying that when he is called back, that will be the sequence, but, and I want to warn you in that regard, I'm not going to permit, when he's cross-examined on the new phase, I'm not going to permit either examination or cross-examination that extends way back into this first phase. This first phase, you're finished with it.

MR. FURLOTTE: I would agree with that, My Lord.

MR. WALSH: I understand that, My Lord.

COURT: If I hear the words ethidium bromide mentioned again, I'll scream, on the second phase.

MR. WALSH: That's fine, My Lord. No, I understood that, and I fully intend to abide by that. I just want direction as to what happens when he does come back.

COURT: On Monday, you're going to put in some other evidence by agreement now. Then on Monday you're going to call Dr. Kidd?

MR. WALSH: That's correct, and here's the situation as I see it as of four o'clock today. Dr. Kidd will be testifying Monday morning, subject to -- we have some evidence to enter right now, but as far as Dr. Waye, in having seen Mr. Furlotte's cross-examination, the length it's taking, and now having an assessment -- I'm not being critical, I'm just having an assessment of where we're going. Dr. Kidd will be testifying, then when he concludes, it's my intention to call Dr. Carmody who flights are booked, scheduled to come in after Dr. Kidd, then I will call Dr. Bowen, and I expect at that point to have used up all of next week, considering the direct and cross-examination if this week is any indication. I do not expect with any reasonable likelihood that I would be in a position, or we would be in a position, to have reached Dr. Waye or Dr. Fourney, and I expect that we would probably have to use a couple of days of the following week.

That would, in essence, balance out in terms of the fact, My Lord, that we moved it two days in the -- we went from Monday to Wednesday and I'd have to gain that two days back at the other end. I'm looking to the future, but I think that realistically it looks now that we will be extending past next week into a few days of the following week.

COURT: Well, perhaps Mr. Furlotte, I don't know whether he's going to say he'd like --

MR. FURLOTTE: Yes, My Lord, when I agreed to cooperate with the Crown the other day to accommodate him in getting Dr. Kidd in here as scheduled, I agreed that I would forego my continued cross-examination of Dr. Waye in this matter, have him set aside, and allow Dr. Kidd to testify, and then put Dr. Waye back on so I could finish my cross-examination.

Now, it's my understanding that I did not have to agree to that, but since I have been so generous, I feel the Crown is now attempting to take further advantage of my good nature and allow him to recall Dr. Waye at his convenience rather than mine, and I would object to his format.

COURT: Well, I suppose it could be said that the Crown are fairly generous in making Dr. Waye available for cross-examination for eight hours, I think it is, up until now, and probably when the Crown made that arrangement, they probably assumed his cross-examination could be completed in far shorter period than that, as the Court would have assumed.

MR. WALSH: I'd like, you know, what --

MR. FURLOTTE: I guess the Crown is guilty of making a lot of false assumptions as I have, My Lord.

MR. WALSH: I would like to know what the purpose would be behind the necessity and how it would affect his cross-examination to have Dr. Waye testify immediately after Dr. Kidd, other than to disrupt the Crown's schedule. I would like to know what purpose, or what advantage he would gain by cross-examining Dr. Waye immediately after Dr. Kidd, or at a more appropriate time for Dr. Waye the week

after. What practical legal advantage?

COURT: I can answer for Mr. Furlotte, probably, it would be a little fresher in his mind. Would that be your answer, is it?

MR. FURLOTTE: It would definitely be fresher in my mind as to the testimony that Dr. Waye has given, not only in direct examination, but also the answers that's he already given on cross-examination. If this is put off for another week or two, I'm probably going to end up asking him the same questions all over again.

MR. WALSH: Well, we can accommodate Mr. Furlotte there. We have the Court Stenographers who are dutifully typing as quickly as they can the evidence. I would expect -- I can't speak for the Court Stenographers or Your Lordship, but I would expect we should be in a position to have a transcript of Dr. Waye's by next Monday. I would hope that they could have it done in a week. Maybe I'm wrong.

COURT: The Court Reporters have the advantage. They only have to sit and listen to this for one day at a time.

MR. WALSH: If that's his problem, perhaps we could have a transcript, My Lord?

MR. FURLOTTE: Well, My Lord, that's not the only problem. The problem is that I am pressed for time as it is in preparing for cross-examination, and preparation of my own, and if I have to spend all that time that I may be able to make use of in re-reading evidence that is fresh in my memory now in order to refresh it in a couple of weeks from now, then again, that is taking away from my ability to provide Mr. Legere with full answer and defence.

COURT: Well, what has been said today is probably going to be available in transcript form by the end of the next week, within a week, I would imagine -- yes, and before that, in fact, and consequently, if Dr. Waye were to go over until Monday, a week from next Monday as you are suggesting --

MR. WALSH: Yes.

COURT: -- then you would have had a chance, Mr. Furlotte, to review, even if briefly, the transcript of what he has said to date. You're going to be no further behind. You're going to be ahead of the game, actually, because you know what you've covered.

MR. FURLOTTE: Well, does that mean if we finish with Dr. Kidd on Tuesday or Wednesday, then we have the rest of the week off?

MR. WALSH: We have Dr. Carmody and then you're going to have Dr. Bowen.

COURT: No, all week long, go on with Carmody and Bowen. That's not going to work any hardship on you or anyone.

MR. FURLOTTE: I believe it is, My Lord.

COURT: Well, I don't accept that. I don't think, really, you'll find that it does either when you get into it because, as I say, you're going to have the benefit of your transcript. You can remind yourself of what you have said. I would think it would work to your benefit, really.

MR. FURLOTTE: My Lord, it's going to take me as long to read that transcript as it has to go through it here in the first place.

COURT: Oh, you could skip every 99 pages, read every 100th page and you'll get the gist of it, won't you?

I'm exaggerating when I say that, but I think you could go through it pretty quickly. You can tell the topics that you've covered, anyway.

What we are going to do is we are standing this witness aside. Does it make a big difference --

MR. FURLOTTE: My Lord, I can only say for the record, had I known that this was going to evolve, this procedure, I would never have agreed to standing Dr. Waye aside in order to accommodate Dr. Kidd. Let it be said.

MR. WALSH: Well, My Lord, I've just about hit the end of my patience and my rope. I'm scared to say anything here because of the smoke screen that Mr. Furlotte has been throwing up all week on hiding behind this full answer and defence. He's got 16 volumes stacked up behind him and the way we're going, we're going through each one. This appears to be the scorched earth policy that some would -- anyway, I'd better not say any more.

The point remains, My Lord, I could if you give me five minutes, I'll talk to Dr. Waye about arrangements. I'll do whatever I can to facilitate Mr. Furlotte and the defence of his client. What we have here is a situation, My Lord, that Dr. Waye runs or is in charge of a lab. He's dealing with a hospital. He has very, very important reponsibilities. That is not to detract from Mr. Legere's defence, and what we were simply asking is an accommodation where the man was entitled to go back to his lab and come back and be subject to further cross-examination.

Apparently, Mr. Furlotte can't agree to that and if you give me five minutes, I'll discuss the matter with Dr. Waye. I'll discuss the matter with the coordination team as to what we can do with Dr. Carmody, and if I can accommodate him, I will. If I can't, I'll come back and make my same position, My Lord.

COURT: All right, we'll take five minutes to give you a chance to do that.

MR. FURLOTTE: My Lord, I have one last point to make. The Crown had prepared Dr. Waye to finish testifying at the end of next week because the Crown was going to complete its case next week. Now, I can't see how come that Dr. Waye all of a sudden is not available next week. It just flies in the face of logic.

MR. WALSH: Dr. Waye has been subjected to cross-examination here, My Lord, that is extensively long, at least at this point in time. What I have here is another situation where I have another doctor, Dr. Carmody, flying in scheduled to testify after Dr. Kidd. As a result of my understanding, or now seeing the length of the cross-examination, I recognize that there's no way Dr. Waye is going to be able to get on next week, and as a result, what am I going to do with Dr. Carmody? I've got him flying in.

MR. FURLOTTE: Well, it only disrupts with the order that Mr. Walsh would like to present his witnesses.

COURT: Well, I feel that the length of the cross-examination has had quite -- is quite a contributing factor in throwing the schedule out of arrangement, and I suggest we have the recess now. Mr. Walsh, you talk

to Dr. Waye to see what is convenient to him. I feel that we've got to try to accommodate, all of us, on both sides and myself, we've got to try to accommodate these expert witnesses. They have other responsibilities and they can't just be at our beck and call all the time.

I am prepared to have Dr. Kidd go on on Monday and Tuesday followed by Dr. Carmody followed by your other man.

MR. WALSH: Dr. Bowen.

COURT: Use up next week. I want to see every day used, then Dr. Waye come back on the Monday, and you have someone else after that?

MR. WALSH: Then I'd have Dr. Fourney and Dr. Waye again. The other thing that just occurred to me, the other thing if Dr. Waye were to testify after Dr. Kidd, -- well, I won't --

COURT: If you can work it out after you've consulted with your colleagues and Dr. Waye and Mr. Furlotte, if you can work it out that Dr. Waye comes back next week and we get finished with him. I don't know how -- can you give any estimate, Mr. Furlotte, of how much time you might require him for in cross-examination?

MR. FURLOTTE: My Lord, as I go through my notes and material that were concerns of mine in cross-examination, I noticed that a lot of it I've already covered because when I would ask one question earlier, it led onto different matters which I intended to cover later. How much of that I've actually covered so far, there's no way I can guess at that.

MR. WALSH: What is so frustrating for the Crown, My Lord, is that I can appreciate Mr. Furlotte's point and his need to cross-examine on valid points. There are times I get the impression that he's cross-examining on the evolution of mankind since biology. He's carpeting the thing, but that's his choice. I'm just saying I didn't expect that type of attack so it has disrupted my schedule somewhat. I want to discuss it with Dr. Waye.

COURT: I said earlier, you know, the conventional wisdom I think was the expression I used, advocacy and in the cross-examination of expert witnesses, you ask a few questions in areas where you know you're going to win and if you don't, you're only improving the evidence of the expert witness. I think, Mr. Furlotte, without trying to tell you how to conduct your defence, I think you must keep that in mind and, you know, when you give the impression to the Court or to anybody listening that you're just grasping for straws in a variety of 100 different fields, it's not really improving one's case very much.

However, take your five minutes, then you're going to come back. We'll decide this point. We'll decide the schedule, and you'll also put in --

MR. WALSH: I could do that right now.

COURT: All right, let's do that.

MR. WALSH: I have here, My Lord, I made an agreement with Mr. Furlotte so we can proceed. I have the report of Dr. John Bowen, the R. C. M. P. forensic laboratory. It's dated December 4, 1990. It consists of six pages.

COURT: That's VD-54.

(DOCUMENT MARKED AS EXHIBIT VD-54)

COURT: And this is a report pertaining to what?

MR. WALSH: It's pertaining to the case of the Queen versus Allan Joseph Legere, the DNA diagnostic report relating to this case. I shouldn't say diagnostic, a DNA forensic report.

COURT: Copies of this have gone to the other side?

MR. WALSH: Yes. This would also be necessary, in any event, if Dr. Waye has to come back next week. It would be necessary to have this in anyway.

Also, My Lord, I have here a binder containing duplicate autorads, duplicate of original autorads generated in the case of the Queen versus Allan Joseph Legere. The book of autorads is divided in the following fashion. It has two pages of paper typing on which are listed the lane numbers and the items contained in the lanes followed by 14 duplicate original autorads, followed by one sheet of paper of typing that lists the lane numbers and items contained within the lanes, followed by nine autorads. It was divided for convenience purposes. If I could ask to have that marked as one item?

COURT: VD-55. Are the lanes described or what they relate to?

MR. WALSH: Yes, My Lord.

COURT: These are not subject to depreciation --

MR. WALSH: No, My Lord, they will not in any way deteriorate.

COURT: Deterioration is the word I meant.

(DOCUMENT MARKED AS EXHIBIT VD-55)

COURT: And again, copies of these have been given to Mr. Furlotte?

MR. WALSH: Yes, My Lord. The next item is a booklet containing autorads. The booklet is divided in the following fashion. The first part, the first is a single page with typing. It references lane numbers with the items contained in each lane identified, followed by ten duplicate original autorads, duplicate of the originals, followed by a single sheet of paper headed 'miscellaneous known sample', followed by ten duplicate original autorads.

COURT: Another ten?

MR. WALSH: Yes, My Lord, ten autorads after that single sheet of paper, and I would ask that that be marked as a single item.

COURT: The whole thing?

MR. WALSH: Yes.

COURT: VD-56.

(DOCUMENT MARKED AS EXHIBIT VD-56)

COURT: Is the origin of these autorads agreed to, where it comes from? Is that material to your further evidence of Dr. Kidd?

MR. WALSH: Yes, this evidence was the evidence that we agreed to enter so that Dr. Kidd could talke about the case specific evidence in this case. These duplicate originals, or duplicates of originals were prepared by Dr. Bowen who will testify later next week.

COURT: They are referred to in Dr. Bowen's report?

MR. WALSH: That's correct. They relate to the report, My Lord.

They relate to the report, item 54.

COURT: Is that everything?

MR. WALSH: If I could just have a moment, My Lord. Item VD-55 for the record, My Lord, so it's easier to follow, at least on the voir dire, VD-55, the two sheets of paper that begin the book refer to gel number 1, or the first membrane. It lists 22 lanes and sets out there what is contained in each lane. I said the duplicate original of the autorads exposing those lanes are immediately following. Then the sheet of paper mentioned that follow those autorads refers to gel number two, or the second membrane, and it lists six lanes, and it sets out the items that are contained within those lanes followed by the autorads that expose those lanes.

COURT: Fourteen, did you say, or something of that nature.

MR. WALSH: I can't remember, that number is not as high. Nine, fourteen and nine, and booklet VD-56 starts with the first sheet of paper mentioned refers to gel number three, the third membrane. It lists twelve lanes and it sets out the items contained in those lanes followed by the appropriate number of autorads as I mentioned earlier. I believe it was ten, then it's followed by the single sheet of paper headed miscellaneous known sample, followed by another ten autorads, duplicate original autorads. I would suggest, perhaps, My Lord, if I may, the advantage to filing them on Friday as opposed to Monday morning would, if the Court wishes to take advantage of it, the Court may want to take the opportunity to review those and familiarize yourself with them. It may facilitate the hearing next week.

COURT: Do you think I should do it on Saturday or Sunday?

MR. WALSH: I wouldn't dare suggest, My Lord.

COURT: You don't have duplicate copies of those, spare duplicate copies that I could -- it doesn't make any difference, I can use the court copies except I don't like taking court exhibits home with me.

MR. WALSH: We will have Monday morning a lightbox if any of these items have to be referred to by any of the scientists. They can actually put the item on a lightbox and you will be able to see it from -- hopefully we'll be able to see it from the side and the witness box.

COURT: Now you want five minutes?

MR. WALSH: Please, My Lord.

COURT: Why don't we all, to save Mr. Legere having to be taken back, he can stay here and his counsel can stay here and the rest of us get out. Is that fair enough?

MR. RYAN: Yes, My Lord.

(Court recessed 4:25 p.m. to 4:35 p.m.)

(Accused remained in courtroom during recess.)

MR. FURLOTTE: Maybe Dr. Waye should be instructed that he can't speak about this case again to anybody while he's --

COURT: You can appreciate that, Dr. Waye?

DR. WAYE: Yes, sir.

COURT: You can't even talk in your sleep about it.

DR. WAYE: I'll try not to.

MR. WALSH: My Lord, I have discussed the matter with Mr. Furlotte and with Dr. Waye in terms of the actually scheduling and that, and the support people, and Mr. Furlotte can't come up with

an alternative suggestion and neither can we. Unfortunately, it's important and it's necessary that Dr. Waye be brought back the week after next, hopefully the early part of the week after next.

COURT: Well, I think, unless you want to say anything further, I think I will, as I indicated earlier, I have got to prescribe something here and I think the proper thing is for Dr. Kidd to come on at 9:30 Monday morning. We'll have two days for him which you say should, about a half day, perhaps?

MR. WALSH: I would hope I will be done in a half day, and may I suggest to the Court that as I indicated to you, Dr. Kidd, obviously he's under limited time constraints as well, but we have two days for Dr. Kidd. I'm not sure if Mr. Furlotte, the extent of his cross-examination. I've spoken to Mr. Furlotte. He would be agreeable to having a long day on Monday. For example, if the Court wished, we could start at nine, run -- say we got near suppertime on Monday and we could take a break for a short time and then perhaps go through to seven, something, an extended day because the last thing in the world I would need is to have Dr. Kidd stuck here on Tuesday night. It would be impossible.

COURT: Yes, do you agree with that? You don't see any great difficulty in getting --

MR. FURLOTTE: No, I'll extend my day Monday to accommodate the Crown.

COURT: You don't see any reason why you shouldn't be through with him, why we shouldn't be through with him?

MR. FURLOTTE: Oh, I have no idea how long my cross-examination is going to take of Dr. Kidd either.

COURT: Well, we'll plan on going to, say, seven. We'll have a new Court Reporter, I guess, on Monday. Are you in communication with them before Monday.

COURT STENOGRAPHER: I will be.

COURT: Would you point out to, perhaps, Miss Peterson, to make a point of pointing out to them that we may have a long day. They might want to have somebody do the morning and somebody spell off in the afternoon because it makes a pretty long day from nine to seven for one reporter.

Dr. Kidd, and then you follow with Dr. Carmody?

MR. WALSH: Dr. Carmody, My Lord.

COURT: And would Dr. Carmody be here on Tuesday so that if Dr. Kidd did finish earlier?

MR. WALSH: Yes, he will be. Dr. Carmody, in fact, is scheduled to fly in this weekend and he will be available as soon as Dr. Kidd finishes his testimony.

COURT: And then?

MR. WALSH: Dr. Bowen, and I expect that that should take up the week.

COURT: And then you're talking about Dr. Waye comes back on?

MR. WALSH: The following week, and Dr. Fourney.

COURT: And Dr. Fourney.

MR. WALSH: But Dr. Waye would be the only one that had been subject to recall. The others will testify and finish their testimony all at one time.

(Court adjourned 4:40 p.m. to May 6 at 9:00 a.m.)

(Accused escorted from courtroom.)

IN THE COURT OF QUEEN'S BENCH OF NEW BRUNSWICK

TRIAL DIVISION

JUDICIAL DISTRICT OF FREDERICTON

B E T W E E N:

HER MAJESTY THE QUEEN

-and-

ALLAN JOSEPH LEGERE

AFFIDAVIT

I, Bonita DesRoches, of the City of Fredericton, County of York, Province of New Brunswick, MAKE OATH AND SAY AS FOLLOWS:

1. THAT I am a Stenographer duly appointed under the Recording of Evidence by Sound Recording Machine Act.

2. THAT this transcript is a true and correct transcription of the record of these proceedings made under Section 2 and certified pursuant to Section 3 of the Act, to the best of my ability.

3. THAT a true copy of the certificate made pursuant to Section 3(1) of the Act and accompanying the record at the time of its transcription is appended hereto as Schedule "A" to this affidavit.

SWORN TO at the City of Fredericton)
in the Province of New Brunswick)
this 9th day of May, A. D. 1991)
BEFORE ME:)

(Verna Peterson))
A COMMISSIONER OF OATHS)

Bonita DesRoches
(Bonita DesRoches)

MY COMMISSION EXPIRES
DECEMBER 31, 1994

SCHEDULE A

RECORDING OF EVIDENCE BY SOUND RECORDING MACHINE ACT

CERTIFICATE

I, Bonita DesRoches, of the City of Fredericton, County of York and Province of New Brunswick, certify that the sound recording tapes labelled R vs. Legere, initialled by me and enclosed in this envelope, are the record of the evidence recorded on a sound recording machine pursuant to Section 2 of the Recording of Evidence by Sound Recording Machine Act at the Voir Dire Trial held in the above proceeding on May 3, 1991, at the Burton Courthouse, Burton, New Brunswick, and that I was the person in charge of the sound recording machine at the time the evidence and proceedings were recorded.

Dated at Fredericton, New Brunswick, this 3rd day of May, 1991.

Bonita DesRoches