IN THE COURT OF QUEEN'S BENCH OF NEW BRUNSWICK

TRIAL DIVISION

JUDICIAL DISTRICT OF FREDERICTON

BETWEEN:

HER MAJESTY THE QUEEN

- and -

ALLAN JOSEPH LEGERE

VOIR DIRE PROCEEDINGS held before Honourable Mr. Justice David M. Dickson at Burton, New Brunswick, on the 6th and 7th days of June, A. D. 1991.

APPEARANCES:

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

Weldon J. Furlotte, Esq., and) for the Accused.
Michael A. A. Ryan, Esq.,)

VERNA PETERSON
DOLORES BREWER

(COURT RESUMES AT 9:30 a.m., JUNE 6, 1991.)

(ACCUSED IN DOCK.)

THE COURT: Now, this is a resumption of the voir dire hearing. The evidence in the DNA phase of the voir dire was completed the last time we sat and then we were going to hear argument today. On the completion of argument there may be two or three other matters pertaining to the trial generally that we'll want to discuss briefly. The representation here is the same as last time except Mr.Sleeth is back, I see.

Now, Mr. Walsh, do you want to go ahead with your argument on the DNA aspect?

MR. WALSH: Yes, My Lord, thank you. My Lord, at the outset I would point out the manner in which the Crown wishes to proceed. I forwarded by courier on Tuesday a brief with respect to the case law associated with the DNA typing generally. I've provided - I understand from Mr. Furlotte that he received his yesterday as well as I only completed it on Tuesday. I headed it Part 1 because you will note in that particular brief and because after looking at the extent of the material and the complexity of the matter I felt it important to prepare a more extensive brief on the population genetic aspects, and that is being typed at the present time and will be filed, I would expect, some time next week. The Crown felt that that would be - it's important that that particular aspect be completely canvassed, and I will, as I say, file it with the Court and provide a copy to Mr. Furlotte and he indicates that he has Part 1 of the brief at

this point in time, he received it yesterday. As a result of filing the brief with the Court, at least this Part 1 of the brief, it won't be necessary for the Crown to go into any great detail with respect to its interpretation of the case law applicable to this area, and more importantly, how it applies to the evidence of this particular case.

You will note, My Lord, that the Crown decided to approach the matter from the point of view of all the DNA cases in which the courts have ruled for one reason or another that certain aspects of DNA should not be admitted on that particular case, and what we chose to do in that particular brief was to address all those particular cases as opposed to the hundreds of cases that - and I've noted in the brief that it has been admitted in hundreds of cases in the United States, apparently it's routinely admitted in Great Britain, and on at least the two published decisions in Canada it has been largely admitted. The brief approaches it from looking at the cases in which it wasn't admitted and why it wasn't admitted and how relevant it is to this particular matter.

As a result the Crown set out there - it referred to Castro, Shwarz, Pennel, Caldwell, two appeal decisions, United States versus Two Bulls and Massachusetts v. Curnin, Arizona and Despain, Illinois and Fleming and Watson, and Vermont v. Passino, and what the Crown did is to look at each of these cases and determine the application to the case of The Queen v. Allan Joseph Legere, and the conclusion that the Crown drew from that is that those cases by and large have no application or they

would not apply so as the Court could use that as a precedent for actual exclusion of the evidence in this particular case, either DNA typing generally or the case specific evidence here.

You will note that Castro in fact, and although Castro is touted as being the case in which DNA was excluded, when the case is looked at closely you can see that the judge, the learned trial judge, was endorsing DNA typing and interpretation and in fact what he ruled there, and quite rightly, the Crown certainly would agree with his conclusions, that the test results in that case were so poorly done that it would be unsafe to put the evidence of the inclusions before the jury, but in arriving at the conclusion the trial judge endorsed DNA typing.

Shwartz dealt with the particular lab, Cellmark in that case, the manner in which they were actually conducting tests at that time, but again, that was not a decision in which they did not accept DNA typing, in fact they endorsed it. The point they were making there is that the test results in that case would be unsafe to put before the jury. The Crown has pointed out in its pre-hearing brief that it was of the opinion that Shwartz had trespassed into the right of the jury to assess the evidence, to weigh the evidence when they looked at it, that in Castro it was a case where the evidence was completely and totally unreliable, therefore was irrelevant, but in the particular Cellmark case it was not totally and completely unreliable and that the criticisms of the particular case specific evidence were matters in the Crown's opinion that it could rightly put before the jury. In any event,

they endorsed DNA typing generally.

Delaware v. Pennel, again another decision in which the case specific evidence, in that particular case the probability frequencies, they accepted Cellmarks matching technique, they would not accept the frequency calculations because there was a failure to show the validity of their data base and whether the binning had been correctly done, and in fact there was an adjournment to allow the scientists in that particular case to revise some of their calculations, but again there was no - there was an endorsement of DNA typing.

Georgia v. Caldwell -

THE COURT: I haven't actually read the Pennel case but was it from that case that Mr. Justice Flanagan in the Bourguignon case got the idea of putting the restriction on the evidence with regard to frequency that he did?

MR. WALSH: No, Shwartz, he got that from Minnesota and Shwartz, and in fact, My Lord, this morning when I go into the population genetics I'm going to address that particular topic as it's considered very important from the Crown's point of view and I intend to address that at some length when we get into that aspect, but it was the Shwartz case which Mr. Justice Flanagan referred to.

The decision in Georgia v. Caldwell, again the DNA typing was accepted. What they pointed out there, they would not allow the genotype scoring, the type of scoring, the type of probability figures that are being proposed here, simply because there was no proof that they were valid. There was no proof with

respect to the Hardy-Weinberg equilibrium and those particular aspects. They found that there was a lack of testing in that particular case but the case again highlights the fact that the technique and interpretation is valid if properly done.

The two decisions, United States v. Two Bulls and Massachusetts v. Curnin, are two appeal decisions Now, neither of those cases rejected DNA typing offhand. What they pointed out and what we've attempted to point out in our brief is that they were giving directions to the trial courts as to how to conduct these particular hearings, that you can't as in Two Bulls and in Curnin go on those Frye tests, and that's the tests they had to meet in each of those jurisdictions - go there with one witness, and in one particular case a witness who really did not have an expertise in the population genetic aspect, and one witness was not prepared and didn't believe - the witness didn't believe that he or she could actually provide an opinion as to the general acceptance in the scientific community, and what those particular cases really do is point out how these hearings are to be conducted, not that DNA typing and interpretation is to be rejected. In those cases the courts of appeal found that it hadn't been proven at trial, not that there had been evidence that it was not acceptable, and in fact, we've pointed out and we've stated in our brief at Page 5, My Lord, that the Crown's position is that "Two Bulls and Curnin are correctly decided in that if the test for their jurisdiction is Frye, then general acceptance cannot be proven by one witness (particularly where that witness cannot provide

opinions on all aspects) nor can the test be met without reference to the manner in which the tests were conducted for that case.".

The next decision is Arizona v. Despain, and in looking at that particular case in our brief we juxtaposed Frye - or excuse me, we juxtaposed Yee which I would suggest is the most extensive hearing held in North America yet with respect to the admissibility of DNA typing, and we juxtaposed the manner in which Despain and Yee approached the particular evidence. Now, in Despain the trial judge had the transcripts from Yee, the transcripts of evidence, and from Anderson in a Mexico case in which the FBI's data was accepted, but he went on and the basic RFLP procedures he accepted, but he said that the issue was its application to forensics and he found that the burden of proof was greater than a preponderance of evidence but even with that lower standand he did not find DNA typing and interpretation generally acceptable because of the "legitimate controversy". We pointed out that there was a superficial treatment of the evidence by the trial judge, and he just raised population genetics as an example of this controversy.

The most important aspect of Despain, My Lord, is that he refused to weigh the evidence. In Yee, as you will note, the magistrate - and his decision, as you're aware, has been endorsed by the trial court, but in the major distinction between Yee and Despain is that in Despain the trial judge would not weigh the relative merits of the opinions that were being provided to him. He said there's a controversy, and therefore under the Frye test, in his interpretation

of Frye, he was not going to admit the particular evidence, it wasn't for him to resolve the controversy, in his opinion.

In Yee, the magistrate weighed the evidence. He did what is required in any other decision made by a court on any aspect, and that is weigh evidence, it's necessary to weigh evidence, and in Yee they did that and they arrived at the conclusion that it certainly was scientifically acceptable. We state in our brief at Page 6: "In Despain the judge was content to note there was controversy while in Yee the court weighed the relative merits of the opinions to arrive at its conclusion. While in Despain the court reasons that it should not be resolving scientific controversy, Yee refuses to allow the dispute to govern its acceptance solely on the basis of 'scientific nose counting'. The latter view, it is submitted, is to be preferred.".

In fact, I would suggest, My Lord, and we've pointed out again in our brief, that one of the disabilities that the trial judge was operating under in the Arizona case of Despain is the fact that he only had the transcripts of of those experts who testified in Yee and in Anderson. He did not have the benefit of their testimony viva voce and he was at a very great disadvantage even if he had attempted to weigh the relative merits of the opinion based on the transcripts only.

We go on to the decision of Illinois v. Fleming and Watson, and that was a serious challenge to the FBI's black data base in the United States, and again I won't read verbatim from our brief, we've outlined the points that were made in that particular

case. They ruled that the typing technique, including the autorad interpretation, met the Frye standard, but concluded that there was a sincere and significant disagreement about the population genetic issues and, as in Despain, refused to weigh the opinions, that is, the divergent views. The frequency estimates were not admitted. Without the estimates they would not allow the evidence of the match.

The evidence on the population genetic concerns regarding the black data base I've set out in the particular brief, the concerns they had, and then I've pointed out that in this particular case the positive evidence in the case of The Queen v. Allan Joseph Legere, the positive evidence would actually account for all the concerns the trial judge had with respect to the black data base, and the fact that it was a black data base is a very critical distinction and that distinction has been made recently in another Illinois decision, My Lord, and that is the Illinois v. Robert Stremmel II, and that is a decision that has been recently provided to Mr. Furlotte and to yourself, and that is another concurrent jurisdiction in Illinois, that one dealing with the Caucasian data base, and the trial judge specifically stated: "In this case, we are dealing with the FBI's Caucasian data base, not the black data base as concerned the court in Fleming. I find this to be of critical importance", and then he goes on.

There's a decision, a recent decision, Vermont v. Streich, a decision in which Dr. Shields had testified, and in that particular case in Vermont the

court found it would allow under both Frye and the reasonable reliability test - would allow DNA typing and interpretation into evidence.

Daniel Vanderbogart, a New Hampshire case, in fact it's the decision that the affidavit that Dr. Shields filed in that particular case that was commented on in this particular case. He filed that affidavit in Daniel Vanderbogart for the purpose of surrebuttal. The conclusions that the trial judge drew there endorsed DNA typing and the population genetics aspect.

The one decision that the Crown decided to a actually go into in great detail, and it was because of two factors, one, it's very recent - and in fact Streich, Stremmel, and Vanderbogart and Passino are between April 30th and May 13th, or decisions that came out in that particular time frame, but more importantly, Passino is a decision in which Dr. Shields testified and which the trial judge noted that his evidence was non-refuted, or had not been refuted, and so the Crown chose to look at that particular case in extensive detail to determine why the judge ruled as he did, what Dr. Shields' opinions were, and what if any application they had in this particular case and whether or not his evidence was refuted by the Crown evidence in this particular case, and again, My Lord, it's not necessary to - we have it in writing, but at Pages 9 and 10 we have set out the reasoning, the reasons of the trial judge, and he specifically said that, "Unfortunately this court did not have the benefit of testimony from witnesses like Drs. Budowle and Kidd ... The failure of the state to effectively refute Dr. Shields'

testimony in this case is a substantial factor in our conclusion that the state has failed to establish the admissibility of the DNA results in this case", and this is with respect to the population genetic aspects. In that particular case the RFLP technique and autorad interpretation was not challenged.

The other important aspect to note about that case is the trial judge said even if, even though Dr. Shields' testimony was not refuted, and the trial judge had in essence wished he had the benefit of someone's testimony like Dr. Kidd, even so he points out, My Lord, at Page 18 of that judgment that: "If the admissibility of the DNA profiling in evidence in this case turns solely on the question of whether the FBI had adequately compensated for the possible existence of sub-populations the Court might be inclined to admit the evidence", but one of the major factors for not doing so is they didn't know what data base would be applicable, in essence there was positive evidence in that case that Mr. Passino had a very mixed ethnic background, including a mixture of several races, and particularly native American Indian, and the mixed ancestry was again a very distinguishing feature to the judge, but the point to be made there is that even with Dr. Shields' testimony not being refuted he would have been inclined to allow the population genetics aspect to go before the jury.

We've set out, My Lord, the reasons why we feel the positive evidence in addition to what we've just pointed out, we've also set out the reasons why we feel in this particular case the Crown evidence, the positive evidence of the Crown, refutes Dr. Shields'

testimony and the essence of his testimony in Passino, we've listed them. Both Dr. Waye, Dr. Carmody, Dr. Fourney, their evidence actually refutes the points that were actually made by Dr. Shields and would actually address the concerns the trial judge had in Passino. To make a note of them, particularly Dr. Kidd's comments, but at Page 10 we've set them out: The fact that the AmerIndian, American Native Indian and Canadian Native Indian data did not affect the conclusions that the Canadian Caucasian population is not affected by any significant substructuring; you have all Dr. Carmody's teats; you have the fact that Dr. Shields fails to differentiate between statistically significant differences and forensically significant difference. the fact that he hadn't differentiated as to what in fact is a statistically significant difference, we've outlined that; the fixed bin method, the trial judge had pointed out that there was no evidence as to the effect that the fixed bin method would have with respect to any of these issues, we have positive evidence in that particular case, and pointed out at the top of Page 11, and again since it's in writing it won't be necessary for the Crown to read verbatim.

We go on to point out, My Lord, that in hundreds of cases under admissibility tests ranging from Frye to reasonable reliability, including a myriad of state generated variations such as verifiable certainty and simple relevancy, DNA typing, interpretation and frequency estimation have been accepted throughout the States. This acceptance by the courts in the United States is particularly important since many of those cases involved the most

serious and extensive challenges yet to DNA typing, particularly the Caucasian, as it applies to the Caucasian data base. Castro, Anderson, Jakobetz and Yee would be considered some of the most extensive, and Yee in fact would be considered the most extensive hearing on DNA typing and interpretation, and in all those cases, either on the reasonable reliability test, under a Frye test, DNA typing, interpretation, frequency estimation, have been accepted.

We point out that, "The sum total of these cases is that forensic DNA evidence is relevant, it's probative, it's reasonably reliable and generally accepted in the scientific community and is evidence that should go before the jury".

We go on, My Lord, with respect to the Canadian case law. That was a review of those decisions, of the American decisions, in which for one reason or another it was excluded, keeping in mind that we have a case book that's been filed of American decisions that we haven't actually touched on in this particula brief in which it has been admitted. The Canadian case law, we point out that there's two unreported but published decisions. The Keenan and Hunt decision, apparently from my reading of that particular case the trial judge was admitting DNA typing, interpretation and frequency expression under the Frye standard, although I must admit it was not an extensive hearing, it did not appear to be an extensive hearing, and the other one is the most recent one - that last one, Keenan and Hunt, was a decision December 17, 1990, in the Ontario General Division Court, and the most recent one is

R. v. Claude Bourguignon decided January 14, 1991, the voir dire decision. In that particular case Mr. Justice Flanagan would not admit the statistics. Now, under the Frye standard he would admit DNA typing, he would admit the interpretation of the autorads, and he would admit the - and from my reading of that particular decision he found the statistical expression to be valid, the data base to be valid, but looking at Shwarz he was of the opinion - now, Shwarz is somewhat different, Shwarz would, from my reading, not allow any kind of expression associated with the existence of a match because of the fear of prejudicing the jury, but in Mr. Justice Flanagan's case he put a twist on it, so to speak. He altered the Shwarz decision slightly to allow a qualitative statement to be made by the forensic experts, but without reference to the statistical figures, and for reasons that I will address when we go into the particular field of population genetics I'll explain the Crown's position and why we feel that Mr. Justice Flanagan's decision, with all due respect, is incorrect and not supported in Canadian law, or for that matter, apart from Shwarz, in American law.

With respect to the case of Allan Joseph Legere the case specific evidence that's been called here, we've addressed it again at Pages 12 through to 14 to the end. We reiterate and set out in detail that the two tests that would appear to be applicable - we don't know, with the state of our Canadian law now the Crown cannot and does not know with any certainty or any surety as to what test it must meet, whether it's the Frye standard of general

acceptance in the scientific community, whether it's reasonable reliability as set out in Jakobetz, we just don't know. As a result, the Crown has attempted to meet the highest standard. We've set out in detail summaries of both those standards, and they're contained at Pages 12 and 13, but what we do know, My Lord, and what the Crown would suggest is the evidence, is that whatever admissibility test this Court determines to be the appropriate one for a Canadian jurisdiction, we do now from the Crown evidence, the viva voce evidence, the exhibits, the numerous exhibits that have been filed, that the evidence is overwhelming that the R.C.M.P. forensic application of DNA typing and its interpretation is acceptable, it meets both of those tests, and we would suggest, respectfully suggest, that the evidence is generally accepted and reasonably reliable and that the evidence is overwhelming on that particular fact and I would refer you generally to Dr. Waye, Dr. Fourney, Dr. Kidd, and in fact, even Dr. Shields in cross-examination would apparently endorse that particular aspect of DNA evidence, and that is the molecular biology and the interpretation part of that particular aspect. Each step, at least the Crown had hoped that it has shown and we believe that we have shown that each step of the procedure has been scientifically validated, it's been documented, and it's been controlled for.

You would only need, My Lord, to look at the list of the exhibits kept in this particular matter. Just to look at some of the actual articles that have been written, some of the procedures that have been adopted. Remember the evidence with respect to

the slot blot quantification that was developed in fact by Dr. Waye and Dr. Fourney, the evidence with respect to the publications as to why they're using a particular digestion enzyme, Hae III, that's in a particular paper. All the steps have been adopted.

I would ask the Court to remember the quality assurance program that's in place, to look at and remember the protocols that have been set up by the particular R.C.M.P., to remember how they went about determining the match window, the measurement imprecision associated with this particular type of evidence, to remember the issues of band shifting that Mr. Furlotte made much of during his cross-examination and the opinions, particularly of Dr. Kidd, that the chance of - over a multiple loci, the chance of band shifting causing a false positive is to be almost impossible or vanishingly small or exceedingly rare, and he explained why that was; to remember the evidence with respect to the impact of environmental effects, that the major impact it has is that it produces no results, that they can't extract DNA, that if it is extracted and is degraded they have controls to determine throughout the testing procedure or the extent of the degradation and what effect it's having on the mobility in the gels and things of that particular nature. These are all steps that have been documented in detail by the R.C.M.P. forensic system, in conjunction with the FBI, in conjunction with other forensic laboratories, to attempt to actually provide for our community a reliable source of probative evidence on which to determine the existence not of guilt or innocence, which again Mr. Furlotte made much of in

cross-examination, but to determine the source of a forensic sample as one piece of evidence to be taken into consideration, albeit a very powerful piece of evidence to be taken into consideration in conjunction with the evidence of the whole trial.

The evidence in this case itself, the actual typing that's been conducted by Dr. Bowen, has not been challenged in terms of the defence evidence. Mr. Furlotte cross-examined on it, Dr. Bowen explained the particular - why the interpretations he made, how he performed the particular procedure, how it was documented. You must remember Dr. Waye endorsed those particular tests, Dr. Kidd endorsed those particular tests, Dr. Carmody endorsed those particular tests, and when you're looking at the particular endorsement you must look at their qualifications and their skill and their experience with respect to forensic DNA typing or DNA typing generally. Those are all aspects that are extremely important. In fact, if you remember Dr. Shields, he had made a statement in Bourguignon that the actual test results performed by Dr. Waye in that case were some of the best he's ever seen. He considered Dr. Waye to be a very skilled person and that he would value his opinion, and Dr. Waye's opinion is that those are the results. There's no doubt in any of those particular eminent scientists' minds that those sources of evidence match.

Finally, My Lord, on this particular aspect of the brief, the Crown has submitted and it maintains that it has proven the validity of the R.C.M.P. Caucasian data base and the reliability of the estimates of probability generated for forensic

purposes, and particularly as it's applied to the case of The Queen versus Allan Joseph Legere. However, we've pointed out here, My Lord, and we've set out, that in the Crown's view since the population genetic aspects of DNA typing are sufficiently complex and the defence evidence mounted a challenge on that particular issue the Crown feels it would be prudent and necessary to address this particular area at some further length, and I have pointed out and I would like to do that at this particular time, My Lord, and as I had pointed out earlier that once I have completed the actual brief on that particular aspect I will file it with Mr. Furlotte and with this particular Court.

THE COURT: I'll let you decide when you want to have a break this morning.

MR. WALSH: Well, I'm good for a while, My Lord. I hope I don't go on overly long but I'm good for a while if you permit me.

THE COURT: Yes, but just to sort of plan the morning, you anticipate finishing this morning?

MR. WALSH: Yes, My Lord.

THE COURT: And you'd take what, perhaps an hour on this aspect?

MR. WALSH: Or perhaps longer. If you could allow me at least until noon time. I may be done before that, it's hard for me to judge, My Lord.

THE COURT: Yes, well, you will want to break after a little while so you decide when you want the break yourself.

MR. WALSH: O.K., fine. I will depend on my co-counsel, they get bored easily and they'll indicate that.

With respect to the population genetics aspect there's been various definitions offered throughout

this trial, you read any of the case law, the case book in this particular instance, you will find numerous definitions, and the one that from the Crown's point of view is the most succinct was made in Castro. In fact, the trial judge in Castro, from the Crown's view, did an excellent job. In most of the - the judge in Yee, for example, and Jakobetz, they did excellent jobs, and in Fleming and Watson, in actually setting out the evidence in detail and their reasoning in this particular case, so the Crown looks to these particular cases and the detailed analysis and the reasons and adopts some of them to make points with respect to this case.

In any event, Castro points out that - at Page 992, and again I may - if you may permit me, My Lord, I won't refer to actualy page numbers, etc., since they will actually be included in the brief unless you wish them at this point.

THE COURT: No, that's all right.

MR. WALSH: Anyway, one of the quotes was that: "The population geneticist determines the frequency with which a specific allele occurs within a given human racial group. The information obviously is critical since a necessary part of any forensic DNA typing is to put a meaning to the matches that are declared". In fact, it's been clear from the decisions a probability estimate is generally considered to be the essential prerequisite to the admissibility of evidence because without some form of expression to the match the jury are left with no meaning as to what - for example, in this particular case, what the existence of the matches as shown on this particular exhibit, VD-88, what that means. I remembe

reading in one of the particular cases the judge used the expression, "whether it's as common as the nose on your face or as rare as the smile of the Mona Lisa". That was an expression without that the jury doesn't know, no one knows what that means. As a result it's an extremely important area and the reasons for going into it.

The method, and I've broken it down in actual sub-parts so you can see where I'm going in this. The first aspect I'd like to deal with, My Lord, is the method of frequency calculation. What the evidence shows and what the case law points out and what the exhibits will demonstrate is that the first thing that must be done to actually calculate a frequency is to find the individual allele frequency the individual band frequency, and they do that by using the bin frequencies obtained by a method of binning. It's a method that has been adopted and adapted by the R.C.M.P. and FBI. They've set it out in the Exhibit VD-49, 49A, those particular exhibits, it's documented there as to what the procedure involves. It's also - if you would refer to VD-87, it's a statement of the working group on statistical standards for DNA analysis, and again the binning method and the reasons for its adoption are explained in that particular document, and again, it has been explained throughout the case law in Jakobetz, Yee, Fleming and Watson, all of those decisions have detailed it, but I think it's important that the Crown summarizes its view on what that binning method - what is actually being done, because it gives an insight as to the statements that are later made that they overestimate the frequency associated

with any particular allele or any particular band, and it's important to understand why they make those statements.

I've summarized the binning method, there's several steps, but first of all, it's clear and obvious that by this time that using the size markers that they use in the forensic testing they set up bins, collections of groups of alleles in which alleles are placed in these particular groups as a result of that, and as a result of these groupings of alleles it overestimates the frequency for any one particular because it covers a wide range of band sizes.

The other thing that is done is that the bins the width of the bins, and if you can think of a series of boxes, the width is greater than the actual window of measurement precision or imprecision that has been determined by the forensic labs. In the R.C.M.P. case it's 5.2% plus or minus 2.6, but you'll remember the evidence of Dr. Carmody that the R.C.M.P. bin sizes range from 5.7 to 15% with the average of 10%, so you're collecting a greater span of alleles than you would ever - greater span than is actually used in the actual testing result in terms of the match window, and the average is in fact almost twice as much.

The match window, the measurement imprecision, the 2.6 or 5.2%, is also used in actually placing an allele in one of the boxes, one of the bins, and if you are going to in essence put a band into a particular - I use the analogy, box, but by applying measurement precision, 2.6 or 5.2 or 2.6 one way or the other, it would go into another box that has

more alleles and therefore a higher frequency, it will go into that particular box, another case where it supports the opinion that it overestimates the particular frequency.

The other aspect, My Lord, is when they finish and they see that one of the boxes has three bands in it or four bands in it or five bands in it, they take that box and dump it into the next one that has more bands, they collapse, and the purpose is again to put the rare bands, and they would be considered to be rare because there's very few of them in this particular bin - they dump them in bins with more alleles, higher frequency, thereby making the rare alleles that much more frequent, another example of why and how they go about the binning method, and then there's a simple mathematical calculation that the judge in Yee quoted by saying that they divide the total number of bands located in the bin by the total number of bands generated from all the data base samples tested for that probe and they come up with a bin frequency associated with each of the particular bins, and then on occasion it's necessary, as the R.C.M.P. have done, to rebin their data because of additional samples coming in and their conservative binning methods, and in fact Exhibit VD-64 is the rebin population distribution table that was used to actually calculate the frequencies in this particular case, in the case of The Queen versus Allan Joseph Legere.

Dr. Kidd, when asked his opinion with respect to the fixed bin method, he states that: "It vastly extravagantly overestimates the frequency of the band pattern".

The next step, as Your Lordship is aware, in arriving at the final figure is the Hardy-Weinberg equation, to be differentiated from the Hardy-Weinberg equilibrium that we will be addressing shortly. The Hardy-Weinberg equation is simply a very basic kind of a - I've heard the expression used, and I can't remember exactly in what case but it's the genetic expression of the product rule, and it's that fancy 2PQ, or if you're talking about a single band, P^2, and Dr. Waye had the best way of explaining it from a layman's point of view so that we can understand it, and he said in his testimony:

> "What that formula does is simply says I saw this band, P, in X number of people, and band Q in X number of people, and using the Hardy-Weinberg equation I predict Y number of people will have both bands together."

Very simple expression, very simple way, it's a Hardy-Weinberg equation, and what they do is they simply, using that equation, take the bin frequencies that you would associate for each band, multiply them, and that comes up with your frequency at any particular probe, as a result of any particular probe, and the next particular step that's taken, and the last step, or I should say close to the last step, is the use of the product rule, and the product rule has been described, it was described in Yee as one of the most rudimentary principles of probability theory, and that is the multiplication of independent events, and when they apply the product rule they're simply taking the probe frequency by the probe frequency by the probe frequency by the - as a result of the number of times the particular lab is able to actually use a

probe that actually expresses a match like in this particular case set out in VD-88. In one particular case with respect to Exhibit 1J we have a four-locus match, a four-probe match, and with respect to 135 we have a five-probe match. It's the multiplication of each of those events that arrives at that final particular conclusion.

I intend to say later - to address the confidence intervals. At this point I simply point out that the product rule, the end result, generates the best estimate. The evidence is clear that what they're generating is the best estimate, and I will address the whole concept of confidence intervals applied to that best estimate and I will do that near the end of my summation.

It's obvious, My Lord, from the evidence, and if you look at the OTA Report, which is Exhibit 24 in this particular case, that the more probes that you're able to apply and thereby multiply, the higher or lower, depending on how you view frequencies, the frequency will be associated with respect to the matches. In this particular case is a prime example. With Exhibit 1J compared with the source purported to be coming from Allan Joseph Legere, the probability of a four-probe match was one in 5.2 million male Caucasians. With respect to 135, which was a five-probe match, applying the product rule was one in 310 million male Caucasians. It's a prime application of the application of the product rule by the number of probe sites.

Now, obviously, in order to do these mathematical or these standard and very basic - they're very basic calculations - it requires a data base, a data

base that will entitle the Court and the forensic scientists to rely on the reasonability of the estimates that they're producing, and in this particular case the Crown has sought and we would respectfully suggest and submit that we've proven the reliability and the validity of the R.C.M.P. Caucasian data base. The evidence would indicate, My Lord, that a Caucasian data base in a country like Canada, and particularly in a province like New Brunswick, is the or one of the most relevant to be applied, that there are over - I believe the evidence is that there's over 95% of the Canadian population or close to 95% of the Canadian population is Caucasian, that in New Brunswick which the evidence is there's 700,000 people, 25 million in Canada, I believe, and 700,000, approximately, in New Brunswick that again over 95% of that particular population is Caucasian.

There was evidence with respect to how the data bases were compiled, to the extent that the argument may be that the data base was deficient because of geographical limitations, there being no direct sampling done in Atlantic Canada. I would suggest and the Crown would submit that it's been overcome by the selection of blood donors on CFB Kingston, and in fact, My Lord, if you look at the exhibits associated with respect to the profile of CFB Kingston, they're Exhibits VD-59 through to 63, it's almost like a miniature Canada in terms of its expression of the genetic make-up, or I should say the diversity in Canada, except for Western Canada and particularly B.C., which seems to be somewhat under-represented, but that's made up of the fact

that there's a separate data base that was compiled from Vancouver. The question then becomes whether or not the data base is truly representative of the Canadian Caucasian population in terms of ethnic sub-groups, and the evidence is that the Canadian Caucasian population is largely comprised of British and French ancestry and their distribution in Canada is reflected in New Brunswick, and I refer you particularly to Exhibit VD-97 and the evidence of particularly Dr. Carmody and Dr. Fourney. In other words, New Brunswick's ethnic diversity is reflected in the data base, and what is important here is a statement made by Dr. Kidd when he was testifying, and he was asked the question, "What about ethnic diversity, Doctor", and we were examining him with respect to the R.C.M.P. data base. His answer was, and this is very important, he said:

> "Well, certainly I mentioned major ethnic groups. If we're talking about within Caucasians the Canadian white population or Caucasian population is of mixed European ancestry. It's a higher proportion of English ancestry than we have in the United States but it is a mixed European ancestry, so one would want some representation of that but that's almost going to happen automatically because the population is fairly randomly distributed in terms of any of the major groups. One does not need geographic representation because there is no strong indication that the population is subdivided geographically in a population like Canada or the U.S. whereas in Africa one might very well want some geographic representation because of gene frequency differences."

So even without, I would submit, the evidence of the CFB Kingston representation, even without that, it's clear from Dr. Kidd's opinions that you do not need geographic representation because of the

purposes for which the data base are being put, in his opinion.

The size of the R.C.M.P. Caucasian data base, I think if you look at the number of decisions, Andrews, Wesley, Cobey, Spencer, there's a Chinese decision, Shi Fu Huang, out of New York, that's in your case book, Pennel, Jakobetz and Yee, all of those decisions would indicate that the R.C.M.P. Caucasian data base is more than a sufficient size. In fact, Dr. Kidd's comment in Jakobetz was that, "Once it is determined that the alleles are randomly occurring throughout a targeted population sample size can decrease to as little as one hundred individuals", and in fact the evidence is that it is one of the largest in the world for forensic purposes.

The other aspect of the Canadian Caucasian data base with respect to whether or not there was an appropriate sampling theory applied, whether they actually went about getting those particular samples in an appropriately - scientifically appropriate manner, he goes on. The bottom line for Dr. Kidd's opinions with respect to the data base was that they had been selected using an appropriate sampling theory to obtain a random selection, that it was large in terms of its size and was representative of Canada by province and in terms of ethnic diversity. That was the bottom line for Dr. Kidd's, in the Crown's view. He concluded in his testimony, and the Crown obviously relies heavily on Dr. Kidd's opinions because of his expertise, his unique position in relation to human population genetics, and his experience in demography, human demography,

and again there will be a point made later, but he concludes, and this is where the Court is required, My Lord, to weigh evidence, and you will be required to weigh evidence in relation to the population genetic aspect, it's an absolute necessity. It's an absolute necessity, as I pointed out earlier, in any case, but he says in relation - the question was asked:

> "Doctor, in your opinion, to what extent do the frequencies generated from the data base, the R.C.M.P. data base, reflect the Canadian Caucasian population as a whole and New Brunswick for VNTR purposes",

and his answer was:

> "I think they are very representative. It would be hard for me to imagine creating a better, more representative sample than the one that has been assembled."

In the Crown's view that is an opinion that must be given great weight.

Part of Dr. Kidd's opinions obviously was related to the actual testing that Dr. Carmody did. Now, one of the tests he did, and it's in the evidence, is that he did Chi^2 and likelihood ratio tests. In fact, he looked at the Vancouver, Ottawa and CFB Kingston data, did this test to determine whether there was any bin frequency differences. Same kind of test Dr. Shields did when he compared the R.C.M.P. with the FBI with the other Caucasian data in the United States, same kinds of test. He wanted to determine whether or not there was any bin frequency differences, and he concluded that there wasn't, and Dr. Carmody testified that - when he made that conclusion he said:

> "I conclude with great assurance that as we get greater samples and as we increase sample sizes from other areas of Canada that it is very unlikely that they will show differences from the existing samples that we have",

and he goes on to testify:

> "Our studies on the Canadian Caucasian data base drawn from these three samples in Canada would indicate that in fact there was no local geographic genetic differentiation that is present in our Caucasian population or at least none that is statistically significant enough to be seen in our samples and that would mean that the calculations that I did using the data in the R.C.M.P. data base would hold whether we were making the inference about British Columbia, Ontario, or the Maritimes."

That's his conclusions with respect to - and that's the conclusions of Dr. Carmody, that's the conclusions of Dr. Kidd, and the prescriptions that have been provided by Dr. Fourney and the conclusions of Dr. Waye. They are satisfied in their expert opinion with the R.C.M.P. Caucasian data base. Bottom line, that's the take-home, to adopt a phrase of a colleague of mine. That's the take-home message with respect to that particular aspect, and My Lord, I now wish to proceed into the question of substructure which will be obviously of some length and perhaps it might be appropriate now to have a break?

THE COURT: O.K., we'll take 15 minutes or so now.

(BRIEF RECESS - RESUMED AT 11:00 a.m.)

(ACCUSED IN DOCK.)

THE COURT: Mr. Walsh?

MR. WALSH: Thank you, My Lord. The issue I wish to deal with now, My Lord, having left the question of the

data base, is the issue, the general umbrella, of substructure, population subdivision, structuring, and all these phrases, inbreeding in its widest sense, all phrases associated with whether or not the frequency of particular bands, the frequency you may see a particular band may vary on the basis of ethnic ancestry of the particular sub-population or a result of some regional variation, and Yee is a case in which the trial judge set out in quite good detail the whole concept of substructure, and it was addressed by Dr. Carmody, Dr. Kidd, and Dr. Waye, but the purpose of assessing whether or not and the existence and the effect of substructure is to determine whether or not your data base can produce reasonably reliable results or estimates of frequencies.

The evidence, My Lord, would be that there appears to be a consensus among scientists that structuring does occur within the North American Caucasian population caused by many different phenomena. The difference of opinion that exists you read from the case law and as you can see by juxtaposing the evidence of Dr. Kidd and Dr. Carmody and Dr. Waye and Dr. Fourney with Dr. Shields is that the difference of opinion that exists is over the extent of substructuring and its effect on the ability of the forensic scientist to make reasonably accurate frequency estimates. The assessment of whether substructuring exists and its extent and effect involves a number of different assessments, and the Crown's point of view for the purposes of argument in the brief it's to file, we would identify four assessments that generally should be made in

addressing the question of assuming substructuring, what is its extent and more importantly what's its effect in relation to forensic testing, and the four areas that the Crown has identified, My Lord, is first of all the existence of Hardy-Weinberg equilibrium, really, simply put, whether or not the two bands that you see at a particular probe site, whether they are randomly associated with each other. The next question is linkage equilibrium, whether the bands at one probe site are connected physically or genetically to the bands at another probe site. That's the first two issues.

Now, the importance of those two issues was pointed out in a number of cases but it has also been pointed out in Exhibit 24, VD-24, the OTA Report, and at Page 67 they state:

> "Essentially the population must be one where individuals randomly mate and reproduce so that distinct subgroups are absent. In such freely mixed population there will be no correlation between the alleles, the maternal and paternal chromosomes (Hardy-Weinberg equilibrium) and no correlation between the alleles at different loci (linkage equilibrium)."

So the first two assessments are particularly important.

The third assessment that the Crown would identify is whether there is any geographical bin frequency, what are the results of geographical bin frequency comparisons, whether significant variation is caused to these frequencies by various effects such as by geographical differences, and the fourth area that the Crown would identify is the empirical observations on data worldwide, whether significant variation is caused because of geography, race, inbreeding in the widest sense, etc.

Now, those are the four areas that in the Crown's view should be looked at, and in fact the evidence would actually point to the fact that it has been looked at in relation to the R.C.M.P. Caucasian data base and in this particular case.

Now, as far as the phenomena of Hardy-Weinberg equilibrium, one of the first aspects that we must have clear in our mind is with respect to some of the earlier cases that you will see; for example, Wesley and Castro, Pennel and Caldwell. They dealt with what has been termed in the scientific literature Wahlund's effect, and the Wahlund's effect or principle would be that excess homozygosity an excess of single bands in a population over the expected, would be an indicator of disequilibrium, and that principle was established, I believe, in the 20's, or at least mentioned in the 20's.

When they initially started doing testing for forensic purposes those cases dealt with whether or not such a test had been done and what were the results of the test, but since that time scientific advancement has shown that that is not an appropriate test because of the nature of the systems that are actually being used here. You only have to look at Jakobetz and Yee and Fleming and Watson to show that that is not now considered to be an appropriate test because there's a number of conclusions that can be drawn from such a test, and one of the conclusions is that in fact it's an artifact; that is, that you're seeing so many one-band patterns it's because it's an artifact of the process that you cannot distinguish between bands or alleles that are very close together, and I would refer the

Court to the Promega paper, Exhibit VD-50, particularly at Page 136, and an article that was mentioned quite often during the hearing, and that's the Exhibit VD-53, it's entitled "No Excess of Homozygosity at Loci Used for DNA Fingerprinting", and really that article, where it fits into this whole scheme of what we're talking about is the article by these Yale scientists, showed exactly what was suspected all along is that using Wahlund's effect because of the nature of the system you're using you're almost always going to see excess of homozygosity, but it's not according to them a true indicator, because when they apply the test that they developed and they looked at Lifecodes data they didn't find the excess of one-band patterns that was originally believed existed.

In fact, during the hearing we quoted from the Jakobetz case at the footnote 20 in which the trial judge says that in light of the consensus that this really isn't an appropriate test, the consensus of the scientists, the Court said that, "It's unnecessary and this Court happily declines to blaze a trial through this thicket of true homozygosity versus single bands", so that is an aspect or, to use a phrase, a red herring that one must at least keep in mind is that in some of the earlier decisions, like Caldwell, for example, where it commented on the fact of the question of a certain test hadn't been done; Pennel, in which these kinds of tests were thought to be appropriate at the time, but the major decisions since that and this particular article would appear to be, the consensus would appear to be that that is not an appropriate test, and one should

not become bogged down in the question of whether that is in fact the test and what application these cases had to it.

What Dr. Carmody's evidence is, and again we must distinguish between the use of the Hardy-Weinberg equation from whether or not we can show Hardy-Weinberg equilibrium - now, what Dr. Carmody has done, and his evidence is clear on that particular case, is that he tested for Hardy-Weinberg equilibrium not using Wahlund's effect because of its inappropriateness but using what he called the non-parametric median test, and he found that there was no sufficiently high correlation, and that is at the individual probe sites used by the R.C.M.P. in their data base, that would affect the use of the Hardy-Weinberg equation. That was his bottom line conclusion with respect to that statistical test. He poined out that it would not allow to determine low correlations but his evidence was that low correlations would not - even a minor disequilibrium, Hardy-Weinberg disequilibrium, or low correlations would not affect the uses to which it were put. He was more concerned with the high correlation and this test has ruled that out statistically. The other test that you look at to determine whether or not this whole issue of sub-structure is this linkage disequilibrium or linkage equilibrium, and two aspects of that is whether the probe sites are physically linked or statistically linked. Now, the view now, and there is really no question on that, is that if you're using different probes on different chromosomes then there's no risk that you're going to have physical

linkage, and the evidence is clear that the R.C.M.P., their probes are all on different chromosomes, D2S44, D1S7, they're all on different chromosomes, 2, 1, 4, 17, 16, 10, so there's not that risk of physical linkage, and what they're simply addressing there is whether or not you can use the product rule and that is whether they're independent events.

The other aspect that must be addressed is whether or not there's statistical linkage between these particular probe sites, and again Dr. Carmody applied a non-parametric median test, and what's important, it's a test recommended by Seymour Geiser, an actual statistician that Mr. Furlotte has referred to in cross-examination on a number of occasions, and what Dr. Carmody did was actually adopt or take one of his recommendations and apply this test to determine whether or not there was any statistical linkage between the probe sites.

The statistical test, one is trying to determine if the existence of two bands revealed by one probe will give information that will allow a statistical prediction what two other bands will be revealed by another probe, and his non-parametric median test, the variant of that that he used as recommended by Seymour Geiser, was that there was no strong correlations between the probes that would affect the use of the product rule, another example of looking at an aspect of this question of sub-structure, and this opinion was supported by Dr. Kidd that in fact statistically and physically in both their opinions there was no linkage, there's no disequilibrium, so that they're in fact independent

events so that you can actually multiply one probe by one probe by one probe by one probe and allow the product rule to apply.

The next aspect that the Crown had identified, My Lord, with respect to an area that should be looked at in terms of the question of substructure, is geographical bin frequency comparisons, and if you remember, Dr. Shields had done that, he set it out in the affidavit that he had filed in Vanderbogart, and he had also testified with respect to the types of testing that he did in comparing the FBI with the R.C.M.P. Well, Dr. Carmody did the same kinds of test as I pointed out earlier this morning in determining whether or not he could match fit his data bases, Vancouver, Ottawa, and CFB together, and he concluded that he could because there was no statistical bin frequency differences. The other conclusion that can be drawn from that, and the evidence is clear, is that since there's no statistical bin frequency differences there is no evidence of substructure, and I'll quote, and so I won't misquote, from both Dr. Kidd and Dr. Carmody as to the conclusions that can be drawn from that kind of testing, and that is that Chi^2 likelihood ratio testing, and Dr. Kidd said: "It tells me, one, that there is unlikely to be any substantial substructuring in the Canadian population that is relevant to these loci because even if there were substructure if the frequencies are the same it's irrelevant". Dr. Carmody's opinion was that: "I have been able to show for the Canadian data base that we used for these calculations that there was absolutely no evidence of what we call substructuring; that is,

that we could treat the Canadian Caucasian population as a homogeneous unit", and in fact, to hark back on, that is in fact the kind of test that Dr. Shields used to try and draw the conclusions that he drew. It's important to remember this in terms of substructure is that there will be and there has been evidence with respect to comparisons between the Canadian Caucasian population and the American Caucasian population, but the first thing we must remember before we even make that comparison so we can determine whether there's a North American Caucasian substructure is that the statistical evidence is that there is no substructuring occurring in the Canadian Caucasian population or any sub-structuring to the extent that it would have any effect on the calculations that are finally generated by the R.C.M.P. That is the take-home message and the bottom line with respect to those particular tests, and if we keep that in mind, then we can look at the comparisons that were made to other North American Caucasian populations to see what that tells us, so that's in fact what Dr. Carmody did do, and he actually looked at some of the work Dr. Shields did and drew the same conclusions Dr. - drew the same calculations that Dr. Shields did that there were statistically significant bin frequency differences between the Canadian Caucasian data base and the FBI and Dade County in Florida and in Fort Worth in Texas and in Minnesota, although Minnesota was much closer to the Canadian, but the distinguishing feature there is that he concluded that although there were these statistically significant bin frequency differences,

same conclusion that Dr. Shields had made, there was no forensic difference in terms of the manner in which these particular calculations, these figures are put, and that's where he differs greatly from Dr. Shields, and the explanation for the differentiation between a statistical and forensic or meaningful difference is that there is no likelihood that the degree of substructuring would not have the reported frequency and the actual but unknown frequency average out or be unfavourable to the accused, and that's from Yee. "The evidence is that any danger caused by substructuring would self-correct over multiple loci. That is, if the frequency for one probe is understated because of substructure it will be overstated for the other loci", and the best example, and one need only pick up Exhibit VD-65, and it's the calculations that Dr. Carmody did in relation to the statistics derived with respect to Mr. Legere's evidence, the evidence applying to Mr. Legere, the comparisons he made and how it would compare with all these other Caucasian data base whereas Dr. Shields left it hanging. In one example, in filing the affidavit he says there's a difference between 9.6 million and in the other one it's 5.2. If you put Mr. Legere's data through the FBI it's one in 9.6 million. He didn't explain, first of all, that there is no real statistical difference because it's only a difference of a factor of two, but he doesn't accept the proposition that when you multiply them out, the end result, there's no forensic difference, and that's been demonstrated, and both Dr. Kidd and Dr. Fourney maintain that, it was maintained in Yee, and accepted by the judge in Yee.

Dr. Kidd explained, indeed, that by pointing out that for every allele in the sub-population that is less frequent there will have to be an allele that is more frequent because frequencies must sum to one, and when you do the calculations as Dr. Carmody did, it's self-evident.

The fourth area that the Crown would identify that one must address when you're looking at in terms of the question of substructure is empirical observations worldwide. What does the evidence worldwide - what kind of conclusions can you draw from that particular types of aspects, and what we must do is look at those scientists and look to those scientists who study human populations and have looked at worldwide human populations, and obviously the Crown is relying heavily on Dr. Kidd's opinions because it's the very reason he's testified, why the Crown brought him to New Brunswick, is that he is a pre-eminent human population geneticist with experience worldwide, human populations worldwide. This was not simply the Crown's word for it, it was something that was accepted in Jakobetz, for example, the judge in Jakobetz, the federal judge, stated that: "Dr. Kidd testified that he has looked at data from many sub-groups including Italians, Swedes, Irish, Amish, mixed Europeans, and all have very small differences in allele frequencies. Furthe Dr. Kidd stated that the differences in sub-groups are absolutely insignificant to the method in which the FBI uses its Caucasian data base".

The trial judge pointed out that - he states that Dr. Kidd is at that time, "the Director of the Human Gene Mapping Library, an nternational

organization that aintains a computer data base containing information on gene locations". We had evidence of that at this particular hearing. Dr. Kidd, "He oversees a DNA committee, a sub-committee of that particular library, through which he has observed data on the frequencies and character of all known DNA polymorphisms occurring within differen populations throughout the world".

The experts identify the fact that all populations maintain high variability. They recognize, however, that there are differences, substantial differences, in bin frequencies and differences in allele frequencies between races, and that's why they have separate data bases. That's not disputed, and the evidence at this particular hearing and set out in exhibits like the fixed bin paper, Exhibit VD-49, 49A, is that that's recognized, and probably the best comparison is that it's one that's been used numerous times both in the OTA Report, in Kirby's textbook on DNA fingerprinting, by Dr. Shields, is they use the comparison between the probe D2S44 between the R.C.M.P. Caucasian data base and the Canadian Native Indian data and to show the differences, but that is again a self-evident feature of something that the scientists know is that you will find differences of a degree between races that require separate data bases, and the issue for us is the degree within a race and what effect that has, the substructuring point, and it's necessary because to look at Dr. Kidd's testimony with respect to his observations over 25 years at looking at human populations, to look again, and I will with the Court's permission in a very summary

way review Dr. Kidd's unique qualifications, his unique position in relation to these issues. In a summary way, if I may, My Lord, I just want to highlight some of the aspects here as to why there should be so much weight, substantial weight, placed on his opinions with respect to his observations worldwide. He has been studying human populations for 25 years. His post-doctoral work and early professorial work was under one of the foremost experts in human population genetics, this Cavalli-Sforza. He continues to corroborate with him on his work. His areas of specialty are human evolution and human population genetics and medically oriented human genetics. His research is almost entirely related to molecular biology and human population genetics. His lab's major areas of research interest are genetic linkage mapping, putting together the genetic map of homo sapiens using DNA polymorphisms as the markers, attempting to identify genes that cause complex human disorders like for example the particular form of cancer that they're attempting to identify the defect for. They're doing detailed molecular study of a particular region of a particular chromosome in relation to early human development, and they're doing human population studies, and I'm quoting from Dr. Kidd, "where we have continued to collaborate with Cavalli-Sforza and his lab and my lab have now assembled a collection of something in excess of 800 cell lines established on humans from around the world and we are studying those samples for dozens to a hundred or more different DNA markers". He has been or he's associated with scientific organizations of the

highest prestige in the scientific community, some of which you can only enter as a result of election. The Human Genome Organization, 300 scientists worldwide, he's been elected to that particular body. He was instrumental in running the human gene mapping library, I don't need to repeat that, because that was highlighted in the Jakobetz case. His testimony has been accepted over some very prestigious scientists in the most major cases in the United States in DNA typing.

He has extensive experience in human demography. He has taught human demography, he explained why and how human demography applies to these questions of substructure. He pointed out that human populations obviously have unique problems and require specialized study as related to other organisms. I have in the brief that I wish to file with the Court, My Lord, I have and I will set out at some length excerpts from the transcripts to repeat some of the opinions that Dr. Kidd has provided with respect to his observations worldwide, because they're extremely important, they can't be discounted, and I don't intend to do it here because as I say, I'll be filing it in writing. However, throughout this particular - when I went through to identify his opinions he has looked at very tightly - in his opinion the most inbred populations in the world. He has looked at Mennonites, he's looked at the Mennonite community in Saskatchewan and Alberta. He's looked at Amazon tribes, one particular tribe that is the most tightly inbred population he's ever seen in the world. He's looked at the Middle

East. His conclusions are that all of these frequencies have high variability and that they would not have any effect, significant, meaningful - there is no significant meaningful substructure as applies to the Caucasians in North America that would affect the frequencies of this particular case. That is again the take-home message, the bottom line, from his particular opinions, because why it's so important is there's attempts to extrapolate information from other races to show that look, there is this difference, therefore we have to be concerned, we have to be cautious with respect to the Caucasian population, but he's looked at these populations and he's given opinions that clearly point out without hesitation that we can use these forensic numbers.

The only possible conclusion, it is respectfully submitted, My Lord, that can be arrived at on the Crown evidence in relation to substructure is that it is not evident in the largely homogeneous Canadian Caucasian population, and although evident in the United States Caucasian population from a North American perspective, would have an insignificant forensic effect. This has been confirmed by Dr. Carmody, and again I will in my brief highlight excerpts from Dr. Carmody's opinion with respect to his conclusions on whether substructure exists, the extent, and its effect, and these opinions are very, very important because Dr. Carmody is actually doing the work, the statistical work on the Canadian Caucasian data base, for the R.C.M.P. but not as a member of the R.C.M.P., as he clearly pointed out.

Mr. Furlotte quite rightly during the cross-examination of the Crown experts, particularly in the population genetic field, was highlighting from the case law that there is an obvious disagreement on certain population genetics issues, particularly over substructuring. If you remember from Dr. Shields' testimony in his cross-examination, it's that in his opinion we do not know enough yet, that he's not comfortable enough yet, and Dr. Carmody when he was under cross-examination by Mr. Furlotte provided a summarization of really what the Crown considered to be an appropriate summarization of the divergent views, so to speak, and he said in Volume VIII of the transcript, "Well, I would characterize it by saying that there are some people" - and this is characterizing the differences of opinion - "that there are some people in population genetics and in the scientific community outside of population genetics who are expressing cautions about the use of data like this and that are saying there is great potential in it but perhaps we should delay a bit until we have more samples and have looked further and have been able to do better statistical tests on larger data base sets. On the other hand, there is a significant component in the population genetics community who are proceeding to do tests, design tests, look at the data as it is to try and show and corroborate what the procedures are that are being applied by the people in the forensic area", and the question was put, "But I assume it's your position that it's still safe to use the product rule even though the scientific community cannot decide on the issue yet".

Answer: "I feel that it's safe to use it because I feel that there is enough justification and the people whose opinion I regard with equal value with Doctors LeWontin, Hartl, and Lander indicate that there really isn't any significant deviations that we're going to ever find in these particular populations. I would say that even if we were to find ultimately in five, ten years from now that there were some small amounts of deviation from Hardy-Weinberg equilibrium and some small amounts of deviation from linkage equilibrium that the effects I think we already know are going to be in the sum effect on the third or fourth decimal place in these calculations. They are not going to have any significance in terms of forensic implications." That would be - so I think that in fact the technology and the statistical techniques are mature enough to actually apply at this time."

And that is, I would suggest, My Lord, a fair summary of the divergent views or at least a very fair summary of the divergent views with respect to these issues and Dr. Carmody's own opinions with respect to them.

To leave substructuring one must hearken back to what was mentioned at the very beginning and that is the binning method. Now, the binning method obviously is a particular process or an adaptation for the purposes of actually trying to arrive at individual band frequency, but it also has an effect, according to the experts, of over-compensating for a number of things, and one is it compensates for frequency variation caused by substructuring, so if there is any that they haven't been able to

detect this, and I went through in detail at the outset of the argument this morning as to all the conservative groupings of alleles that are done to over-compensate, and so that is an effect that binning does have. There's a disagreement, mind you Dr. Shields doesn't appear to agree, but the Crown experts certainly do, and there is self-evident a conservative grouping of alleles. The method accounts for sampling, and this was pointed out in Jakobetz and Yee. "The method accounts for sampling error caused by such matters as for example the inclusion of an unrelated individual or the mixing of a sampling from another racial group", and incorrect allele sizing because of limitations in the technology. "The method allows for limited sample populations which could result in finding only a very few particular alleles when in fact they are more frequent." For example, if we have a small population size and you find in one of the bins less than five alleles you're going to collapse that, and part of the reason for collapsing it is that it takes a rare - what we consider to be a rare allele makes it more frequent, but it also recognizes that perhaps because of limited population size that those alleles are in fact somewhat more frequent, and this was put to Dr. Waye to give his explanation with respect to his statement, and I'll repeat the statement.

> "The FBI and R.C.M.P. scientists in the conclusion to the fixed bin paper, VD-49A",

because I'm quoting from Page 29:

> "They state a conservative statistical approach was developed to compensate for the possibilities of sampling error and differences in racial sub-groups, limited population size and

limitations in agarose submarine gel electrophoresis and Southern blotting technology."

So the binning method is over and above the findings as to the existence of substructuring, or the limited existence according to the Crown evidence with respect to the Canadian Caucasian populations.

The Crown maintains, My Lord, that the end result, and again a take-home message, to adopt a phrase, derived from assessing the evidence is that the frequency estimates generated by the R.C.M.P. DNA lab generally and in this particular case are biased in favour of the accused because they over-estimate the true frequency, and that comment and that question was put to the Crown experts and they all were of the same opinion, that that is in fact the system maintained by the R.C.M.P. and by the other forensic labs, bias in favour of the accused by overestimating at every step the true allele frequencies.

Now, My Lord, with respect to probability estimation, we've addressed or the Crown has attempted to address the issues of the data base and the validity of the data base and as a result the whole question of substructure and all those sub-questions, Hardy-Weinberg, linkage, etc., so we have a figure that comes out at the end in this particular case for two of the matches, one in 5.2 million male Caucasians and one in 310 million male Caucasians, and one thing that must be made clear is the reason why we looked at those issues is that United States law, English law, Canadian law, all require that they not be speculative, that they be based on data that has been compiled and reasonably and rationally

looked at. You can't simply pick numbers and figures from the air. It must be shown that - the quote I would use from Yee is that, "The probability evidence must be shown to be based on empirical scientific data rather than unsubstantiated estimates", and as a result we've had the prior discussion this morning with respect to these particular assessments.

The basis for the conclusions drawn by the Court in Yee, and I indicated earlier that I will take statements from that, but in Yee the Crown would adopt one of the statements with respect to statistical frequency figures that we're offering at this trial because one of the statements made we adopted because it's critical to the understanding of the Crown's submission, and it's at Page 117, and the trial judge said:

> "Limitations on the state of our understanding of the presence and effect of ethnic-dependent variations among VNTR is, I conclude, a matter relating to certainty and not a circumstance that caused the FBI's data base to produce probability estimates on the basis of speculation."

And we would adopt that particular statement in relation to the R.C.M.P.'s particular Caucasian data base.

The Crown experts called in this case, My Lord, have made it clear that the intent of making any statistical statement associated with these particular forensic samples is to say one thing, and that is that they're rare. Dr. Carmody in attempting to teach me the relevance of these particular estimates says there's three things you have to know about the statistical expression associated with these forensic matches, they're rare, they're rare, and they're rare, and you must keep that in mind. That is what they're

attempting to express, they're not common. If there is a match across these loci, three, four, five, it's an expression of rarity, and as a result the question that I addressed earlier, whether and what is statistically significant difference, and the evidence at this hearing with respect to the 9.6 million versus the 5.2 million, and Dr. Shields seemed to kind of leave that hanging in the air and allow people to draw their own conclusion as to, well, that is a big difference. That's the only conclusion one can draw from what Dr. Shields did without explaining. He did admit on cross-examination and after Dr. Carmody and Dr. Kidd commented on the fact, that's not statistically different. If we think of it in terms of well, we have a pile of money, 9.6 million, and a pile of money, 5.2 million, yes, it is, but when we're dealing with these statistical powers it only differs by a factor of two, and all of these particular aspects bear on that question of rarity, that when you're dealing with 9.6 million, 5.2 million, there's no real difference there in terms of the way that you're calculating. They express, again to be redundant, rare events.

Dr. Kidd used the lottery, if I buy one ticket, two tickets or three tickets, statistically my chances are no different, and the chances are that it would be extremely rare that I would ever win, and that was one analogy he used to explain why we mean - what they mean when they talk about statistical differences.

Now, this comes to a very important feature to address here and this is confidence intervals, the application of confidence intervals on the best

estimate frequency generated. Dr. Carmody went into it at some length. In VD-65 it sets out why confiden intervals will do what is very hard to explain to anyone, and that is why we say there's really no forensic difference, and that is the confidence intervals, and in this particular case Dr. Carmody is using 99% confidence intervals, and when you apply confidence intervals to show the highest and lowest frequency around the best estimate it provides a scale for anyone, scientist or layman, to weigh the significance of such a match. It provides a means to explain when they talk about rarity that we're providing a best estimate to you. We're not providin you the exact figure, we can't do that at these sample sizes, but we can tell you that this is our best estimate, the lowest it could possibly be is this, the highest it could possibly be is this, you weigh it, there's your scale, and VD-65 clearly sets that out, and when they apply confidence intervals to the FBI data and the other Caucasian data it becomes self-evident what they mean when they talk about a best estimate, and Dr. Carmody defined the confidence interval and the purpose of it. He said that - or Dr. Kidd said that: "Adding a confidence interval conveys also the degree of certainty one should associate with the estimate and one can therefore form an individual opinion of how much weight to give to the estimate itself".

As applied to this case, My Lord, you will remember the evidence is that the match declared between what purports to be the hair of Allan Joseph Legere and the forensic sample from Nina Flam can be expressed as a best estimate probability of - the R.C.M.P. have expressed it, of a coincidence of one

in 5.2 male Caucasians, and when they apply the 99% confidence interval, and I believe Dr. Shields' evidence if you use three standard deviations that would be 99.7% in actuality, that it would be no lower, when Dr. Carmody did the test, than one in 3.1 million male Caucasians or higher, depending on how you interpret it, or no higher or lower than one in 17 million male Caucasians. There's your scale. That way the layman, particularly the jury, has a way of understanding what they mean by a best estimate and what they mean by 99% confidence, the scientists are saying with 99% or greater confidence that we know it's going to be somewhere in between there and our best estimate is this, and when you apply it to the one in 310 million male Caucasians with respect to what purports to be the hair of Allan Joseph Legere and the forensic sample taken from Linda Daughney, it goes from as low or as high as one in 175 million male Caucasians, a probability of that being - seeing that particular pattern again to the high of one in 1.3 billion male Caucasians, but again the jury gets to weigh it, put it on the scale. The scientists are saying with 99% or greater confidence we can tell you that it's in there, and our best estimate is one in 310 million, and the reason, I suggest, My Lord, that the confidence intervals are so important is that we lead to the next issue, and that is that whether the statistical probabilities should be put to the jury even though a court was to find that they're valid, that they've been shown to be valid calculations, that the data base is valid, that the jury should be entitled to weigh, and this is where we come to the

Shwarz decision and we come to the Bourguignon decision. In the pre-hearing brief that I filed I pointed out in that brief at Pages 24 and 25 that:

> "In Shwarz the Minnesota Supreme Court would not allow statistical probability testimony because of the risk of prejudice to the accused, essentially because of the risk that juries would equate the probability of guilt with the evidence of the high probability that two samples came from the same source."

As I noted in that brief, My Lord, "That decision has been completely rejected everywhere in the United States", and in fact one of the dissenting judges in Shwarz put in another plea that they reverse themselves on that particular issue. The Minnesota legislature has in essence overruled them because the Minnesota legislature would allow it by statute.

THE COURT: What page was -

MR. WALSH: In my pre-hearing, not post-hearing. Now, in Canada, and you've alluded to this at the very outset this morning, in Bourguignon, and you noted that Mr. Justice Flanagan would only permit a qualitative statement to be made. He looked at Shwarz and then he said that he would allow a qualitative statement to be made without any statistical figures because he was afraid of the risk of prejudice; that is, the jury equating the high probability of two samples matching with the probability of guilt.

Now, My Lord, with all due respect to Mr. Justice Flanagan, it's the Crown's opinion that he has in fact abdicated one of his essential roles as a trial judge on a jury trial, and that is to

provide direction to the jury as to the use that can be made of any piece of evidence to be presented to them, but it can be made and it's made every day and with respect to all kinds of evidence. Direction are given to the jury as to the use that can be made of them, and if we use the term prejudice in the sens that it's very, very probative, that's the very purpose why we consider it so relevant is that it is very probative evidence in giving the jury an opportunity to weigh and determine whether or not in their opinion two forensic samples match and how that match fits into the other evidence that's called on the particular trial.

There's two major problems, My Lord, with what Mr. Justice Flanagan has done, or did in that particular case, is he's allowed the expert to express a qualitative statement that it's vanishingly small or exceedingly rare, or rare, the possibility of that coming from anybody else other than the two matching sources, the two sources that match, and the two problems in this, #1, if you would only permit a qualitative statement to be made by the expert it prohibits the defence from getting at the weight of the expert's opinion in front of the jury. Simple example: Doctor, what is your opinion as to the existence of that particular match? In my opinion that reveals that the match is extremely rare, exceedingly rare, or the possibility it could come from anyone else is vanishingly small. The Crown sits down, the defence says what is the basis of your opinion, Doctor. The judge has in his pre-hearing effectively stopped him from answering. He has, in essence, prohibited the defence from

attacking the basis for the particular opinion, the qualitative statement.

The converse, from the Crown's point of view, it prohibits the expert from explaining to the jury the basis for his opinion and why he says that. It prohibits him from explaining the range and whether or not we're dealing with precision or accuracy. That is the concern with respect to Mr. Justice Flanagan's comments.

In the pre-hearing brief I referred to Martinez, which was a case in Florida in which the same argument was made by the defence. They wanted them to apply Shwarz and say, oh, don't give the figures to the jury, just let them know that it matches, and the judge pointed out down there that that is an extremely illogical conclusion because at the very point where the evidence has the highest probative value would be the point that you would remove it from the jury. In fact, to apply that logic, the Court pointed out, you would apply it to fingerprint evidence. Fingerprint evidence, they say no two people in the world have the same fingerprint. That doesn't mean that the accused without question committed the crime. A court and judge would point out to the jury that you use that particular piece of evidence in conjunction with the case, so with respect to DNA forensic evidence, certainly it has high probative value, and that's the very reason why the Crown is submitting that it should be used, but to prevent the jury from weighing that particular evidence I would suggest, My Lord, with all due respect again to Mr. Justice Flanagan because I know that his purpose was to protect the

jury and to protect the accused, but in doing so he has effectively limited the prosecution and the defence and taken the scale away from the jury upon which they could weigh that particular evidence, and the Crown would submit the following, My Lord, with respect to what we would suggest would be the - if the Court concludes that the data base and the calculations are valid and they are a matter of weight for the jury, we would suggest that the expert be entitled to give his qualitative statement it's done every day for all kinds of forensic cases in Canada, hair comparisons, etc., etc., etc., his qualitative opinions as to what that means to the expert, that's important evidence for the jury to weigh, what does that mean to you, you deal with it every day, what does that mean. He's going to give a qualitative statement, It's subjective, and as you remember Dr. Carmody saying where do you draw the line between rare, very rare, etc., but when you back it up, and again our suggestion is that the expert be entitled to provide the statistical basis for that particular statement and the confidence interval around that particular statement. That allows the jury to get the full benefit of the evidence. The obligation is on the trial judge to ensure that the evidence is used properly, and again being redundant, the confidence interval is an extremely important aspect there because it allows anyone to weigh the opinions. They do not have to accept and take as gospel the opinion of an expert, they can look at it themselves, and the expert says this is rare. What's the scale, I know it's this low and this high, yes, I agree, and they can weigh

as well how they came about that particular calculation.

In fact, Dr. Kidd stated during the hearing, he said, "I would be much happier if the jury were presented with that variation of numbers, let them make their own choice", and he's made that statement in relation to the confidence intervals.

My Lord, at this point I wish to refer to the defence evidence because I have touched on it briefly but it's important at this particular juncture to actually address this, and when I finish addressing the defence evidence I will be quickly concluding, and I wish to put it in here because this is right at the point where we have made the suggestion as to the validity of the data base, as to the validity of the frequency calculations, and as to the Crown's submission as to what should be given to the jury in relation to the trial to be held this fall, so it's important then to look at, O.K., what is the defence evidence in relation to these aspects.

The defence evidence is represented by Dr. William Shields. It's apparent, My Lord, that the only challenge he made was on the probability figures and in fact it was clear that he endorsed the DNA forensic typing generally and the case specific typing and interpretation made by Dr. Bowen; at least he took no exception to it, and I will allude to this at the conclusion and the relevance of that to this particular matter, but a number of aspects, My Lord, of his testimony were contained in Exhibit VD-136. VD-136 was the affidavit that was filed in the New Hampshire v. Vanderbogart, a murder

case in the United States, and in that as we've alluded to before and in the hearing, Dr. Shields filed that affidavit to try and obtain surrebuttal, to get back on the stand, and in doing so he took the data that Mr. Furlotte provided him in relation t the case of The Queen versus Allan Joseph Legere and he ran it through the FBI computer. Now, whereas the R.C.M.P. came out with a best estimate of one in 5.2 million with respect to the one particular match the R.C.M.P. computer came out with 9.6 million, and he left it hanging. In part of his affidavit, and I've stated this earlier - in his affidavit he simply said, look at the difference, but Doctors Carmody and Kidd have pointed out there's no statistical difference there. It's a difference by a factor of two, something that Dr. Shields admitted finally, so that I would suggest was a presentation to back up his opinion that was in fact misleading, whether intentionally so or not is not the issue, the fact is it was misleading, that it was not made clear in that affidavit and it would not be made clear to anyone reading that, particularl a layman, what in fact was the significance of that difference when in fact the experts' opinions would show that there was no statistical difference.

Nor, My Lord, did he raise in his affidavit the concepts of confidence intervals to explain why there is an estimation and why there is variation but no significant variation. That was not raised in his affidavit. He mentioned it in his own testimony but only after Dr. Carmody, Dr. Kidd mentioned it, and the question I have is why was that not mentioned in his affidavit. I had the advantage from the affidavit of kind of a pre-emptive

strike in the sense that I had a forewarning of what he was going to say because he filed it, but he filed it under oath as an affidavit to convince the Court he should be entitled to testify, and I raised the question and I leave it with you, why did he not mention the confidence intervals in the affidavit when he was comparing the R.C.M.P. and FBI data, because it's something that he was fully cognizant of. In fact, in cross-examination he admitted that he uses confidence intervals in his own work because of the small sample populations. That is another aspect that concern the Crown, why was that not mentioned.

Another fault that the Crown would suggest in Dr. Shields' concerns is that he did not acknowledge the concept of statistical versus forensic difference He would not acknowledge that multiplying across loci, even though you may have statistical variation in your bin frequencies, multiplying them out they would sum to one; in essence the figures would not be forensically different, and he would not acknowledge that. Perhaps part of the reason why, he never applied the confidence intervals there in the affidavit. Dr. Carmody and Dr. Kidd made it very clear that there is a difference between statistical significance and forensic significance, and which is clear to anybody once you apply a confidence interval to it, again another self-evident feature.

In fact, one of Dr. Shields' statements in that affidavit was that by looking at running Vanderbogart's evidence through the FBI computer and then through the R.C.M.P. data base and vice versa

with Legere he points out in his affidavit, Dr. Shields does, that there is an indication that the adoption of the wrong data base could have drastic consequences, and the strongest comment on this aspect of this forensic difference when he says that he doesn't acknowledge this is when he says that that is an indication that using a different data base is going to have serious consequences. The strongest comment came from Dr. Kidd, and when I put the question to him on that particular aspect of that affidavit, what is your opinion, and he said, marginal comment was in capital letters, NO, with an exclamation point. That's an absolutely incorrect statement", and I'll leave that particular aspect because I've dealt with it earlier, but that is an important aspect, and when in fact most of - if I could just hearken back when he was talking about the difference between 9.6 million and 5.2 million being a real difference and Dr. Kidd when he was asked to comment on that and to comment on the analogy Dr. Shields used that if I was told by a doctor whether it's 50,000 or 100,000 chances of dying whether I would adopt a particular procedure, Dr. Kidd's comment was that's nonsense, and that's the words he used in the court room, that it's a nonsensical statement in relation to what we're actually dealing with, and again your confidence intervals will show that, a self-evident feature.

The affidavit generally, which contained a lot of the same evidence that he presented to you orally, both to Dr. Carmody and Dr. Kidd, they were asked to address that affidavit, the concerns that

were raised in that affidavit and how it affected their opinions, and Dr. Kidd stated:

> "This affidavit has raised no concerns that I have not long been aware of. These are the kinds of arguments that are being raised by the defence in many cases. I've thought a lot about them. I completely reject it, and in fact, I find some of these statements are clear misstatements of fact or using wording that I think gives a very incorrect impression of what the method is really doing and it raises no concerns in my mind about what the R.C.M.P. are doing."

I had mentioned earlier, My Lord, that Dr. Carmody had done comparisons between - the same kind of comparisons that were done by Dr. Shields, and he confirmed Dr. Shields' calculations. I mentioned the comparisons that he did with respect to the Canadian Caucasian data base Dr. Carmody did, and I was struck by his evidence that Dr. Shields did not appear to be fully cognizant of a lot of these particular tests, statistical tests of Hardy-Weinberg linkage equilibrium, except in the most general way, and he finally admitted on cross-examination that all of these particular tests, they at least - they provide - the best I could get from him is that they provide some evidence of the absence of structuring, these various tests, one of them in fact one of the same tests that he was using to make the points he did in the affidavit.

The other thing I was struck by, at least from the evidence, and it certainly is for the Court to weigh, is that he did not appear to be cognizant of how representative the data base, the Canadian Caucasian data base, was. He understood generally where it was selected from, but if you remember, the Crown put to him the CFB Kingston data, and if you

look at the particular aspects of that questioning he still was not prepared to say that that was representative, although when he made his initial statements he was not even aware of that particular aspect. There was another incidence of him testifying with respect to a point although he had not taken in all the data.

All these concerns that Dr. Shields raised appear to have been influenced by two main propositions that he was putting forth, or two main observations, and one was that the evidence of substructuring in the Amerindian and the Canadian Native populations and some of the black comparisons in the States was that they were indicative of substructuring in the Caucasian population, or concern for the Caucasian population, an attempt to extrapolate from that concerns - and that was reflected in the Passino decision in which he referred to that kind of data, and again in his own testimony.

The other concern or influence is that he performed the test that he called background band sharing, and according to him it revealed extremely high levels of inbreeding in the widest terms. Now, when you look at both these propositions that he was making, first of all the Amerindian and the Canadian Native population data, Dr. Kidd compiled the Amerindian data and Dr. Carmody worked on and is working on the Canadian Native Indian population, and both those doctors pointed out that that kind of data did not affect or did not reveal - that data reflects substructuring, something that hasn't been denied, and to a significant extent in some of that

data, but it did not affect their opinions with respect to the Canadian or North American Caucasian populations.

In the written brief that I'll be filing, My Lord, I'll refer to Dr. Carmody's opinions. He was asked, what effect is this Canadian Native Indian population that you see, and the bin frequencies have on the Caucasian population. He gives an opinion it had no effect.

Dr. Kidd, his Amerindian data, he points out it's the kind of thing - and I will set out the quotes - it's the kind of things that he would expect because of what he knows of human demography.

These opinions, My Lord, we would suggest, substantially weaken Dr. Shields' version or his view of that kind of evidence because he hasn't taken Dr. Kidd's own opinions into consideration, and there was much made on the cross-examination, maybe too much so in terms of hammering the point at him, but he had not considered Dr. Kidd's own opinions on his own data. He said no, the data itself is all I need, but I reiterated earlier Dr. Kidd's unique position with respect to human population genetics, that Dr. Shields himself admitted that he was not a human - he would not be of the calibre in terms of human demography of Dr. Kidd, although we had some trouble during cross-examination, I would suggest, and it will be for you to judge that, as to whether Dr. Shields appeared to be very reluctant to acknowledge that Dr. Kidd was in fact a pre-eminent or an eminent human population geneticist, and it would seem to me, My Lord, that that actually detracts from the

weight that should be given to Dr. Shields's testimony when he made a very clear statement, I'm not concerned with Dr. Kidd's - in essence what he was saying is I'm not concerned with his opinions, and from a simplistic point of view it would seem to the Crown that someone like Dr. Kidd and his credentials and the fact that he's been accepted in some of the most major cases in the United States, his opinions should be taken into consideration before one makes their own opinions, and in fact, it's something that he did not do, he made no bones of it, and to do so - I made a comment in the Crown's opinion not to have taken those actual opinions into consideration before arriving at his own conclusions is, in the Crown's opinion, at very worst foolhardy, and at best it seriously weakens the foundation for Dr. Shields for his own opinions.

The concept of background band sharing that he used, according to Dr. Shields, his sample population of five to ten people based on the forensic samples that were obtained in the Miramichi area was that it was indicative of inbreeding in the wider sense, and in essence a level of inbreeding equivalent to the highest ever seen in Europe, and that's the bottom line from the direct and the cross-examination. This would mean that he has arrived at a coefficient of inbreeding higher than has ever been seen in any Canadian Caucasian population.' Dr. Carmody gave an opinion with respect to that, I believe, on his redirect, with respect to Quebec, and the highest level ever seen in Canada was, I believe, .003, and Dr. Shields's opinions, keeping in mind he's not a human population geneticist, keeping in mind that he

does not have the extensive experience with human demography, that he arrives at these conclusions. The conclusions are startling, and the Crown maintains that the unlikeliness of the correctness of those conclusions is borne out by several considerations, and that is first of all his qualifications, and if you remember, on cross-examination I asked him if he was aware of the Canadian - any opinions with respect to generally the Canadian Caucasian population and inbreeding and his answer was something to the effect, I doubt whether that would ever have been done, and we referred to Kirby, Dr. Kirby's text on DNA fingerprinting in which it was set out there what the non-isolated Canadian Caucasian population, what the coefficient of inbreeding was, which was extremely low, and then he made it a point on cross-examination, well, how do we know that's referring to VNTR's, etc. However, Dr. Kirby is a text that he did admit is an authority at least he treats it as an authority, so that another example of weakness, he wasn't aware of that kind of studies. His findings fly in the face of extensive empirical studies as to inbreeding. Dr. Kidd has clearly pointed out - he was asked a question on cross-examination about are you aware if some people are concerned about inbreeding in small isolated populations, and he said yes, but it's not a concern - I'm aware that they are studying them but it's not a concern of his because he has studied some of the most inbred populations, in the narrow sense, in the world.

In addition, the other reason that we would put for being so unlikely that his conclusions are

correct is that Dr. Carmody did not believe that band sharing was evidence of inbreeding. In cross-examination by Mr. Furlotte he made much of the fact that a true indicator would be excess of homozygosity, an excess of true one-band, not band sharing, and the other opinion of Dr. Carmody, and if you remember, My Lord, Mr. Furlotte had him do a number of band sharing type tests, and the one thing that he kept coming back to and the word that he used that I remember, was he said that your samples are pathetically small, and that's the exact point we would make with respect to the calculations that Dr. Shields was making is that his samples were pathetically small to allow that kind of determinati

The final aspect of Dr. Shields's testimony related to the Nichols and Balding correction factor, and that's that famous article in England in which they apply a correction factor to allow for sub-structure, and the evidence was clear on that, and it's contained in an article entitled, "Effects of Population Structure on DNA Fingerprint Analysis in Forensic Science".

Before I go into that, there was one other thing that struck the Crown that may affect the weight to be given to Dr. Shields's opinion is that Dr. Shields was insistent that one did not have to be concerned about the causes of substructure in order to ascertain the extent or the effect of substructure. Now, from a simplistic view that to me seems to be illogical, particularly when his own opinion was that we don't have enough information yet, he's not comfortable, we need more information, and it would seem to me to know something about the

causes as for example would be associated with studying human demography would be important in knowing what the extent and the effect of something is.

In any event, the Nichols and Balding correctio factor, My Lord, he applied a correction factor in his direct testimony and it's set out in Exhibit VD-121, and he pointed out on direct testimony it was to allow for substructure, and I made a point at the very outset of my cross-examination to ask him what was the purpose of this correction factor, before we even dealt with it, and it was to allow for substructure, for inbreeding, and then I left it and we went back to it and he said yes, he applied it to allow for - to correct for any existence of inbreeding or substructure. Then when it was pointed out to him that the coefficient or the statistic he used was .05, which was equivalent to the highest levels of inbreeding ever seen in the world, consistent with, according to the authors of that paper, a society of uncle-niece marriages, he applied that as a factor to reduce the numbers to the level he had, and when that was pointed out to him that that paper also said that .005 is the highest ever seen in Europe and that .0005 is typical of inbreeding in society and that the evidence shows that when you apply those lower coefficients of inbreeding that the numbers are remarkably similar, and you remember the transcript and you commented on the fact that Dr. Shields read well, and actually the point was to show that when you apply lower coefficients of - realistic coefficients of inbreeding, you come up with numbers

that are remarkably similar, but anyway, when that was pointed to him he mentions for the first time, oh, but yes, we're also including and the article says that the reason we're using .05 is that we're allowing for measurement error in the process. That's the first time he's raised it. He wasn't able to account for the statement, I would suggest the transcript will reveal. The statement in there, the authors of that paper said that, "Hence the value of 5% appears to be very conservative for any large population and smaller values would be appropriate in cases where extreme inbreeding is known not to occur", and he wasn't able to, in the Crown's opinion, explain what the authors meant by that, but he was adamanant that you could use .05 because they were allowing for measurement error, and I asked him on cross-examination, well, wouldn't the fixed bin method - doesn't that account for and allow for measurement error, and he was more familiar It would, he said, if the bins were wide enough, and he said he was more familiar with the FBI, but he did make the statement in there, and I interpret it or it can be interpreted - he made the statement that if your bin sizes had to be at least twice the size of your match window in order to have the effect, and the evidence of Dr. Carmody was that the R.C.M.P. bin sizes in fact average 10%, which is twice or approximately twice the match window. They go from as low as 5.7 to 16 per cent. In any event, the point to be made, My Lord, is that although he added this additional factor to account for using the highest coefficient of inbreeding ever seen in the world he had failed to take into consideration,

he didn't seem to have enough knowledge to take into consideration, the effect the fixed bin method would have in relation to that, the point being that if he wants to use Nichols and Balding as a correction factor which the Crown suggests has no application based on the methods the R.C.M.P. use and the evident of substructure, but if he did want to do that, then he should at least be using statistics that are realistic in terms of correcting. You can over-correct to zero, but we're attempting to try to infuse some common sense and realism associated with these particular samples, and in fact when he applied what he thought was the correct band sharing of inbreeding for the area from which the samples come, the Miramichi area, or from New Brunswick, he went from 226,000 on a four-locus match which he had testified to on direct examination all the way up to one in 404,000, four-locus match, instead of the one in 5.2 million that the R.C.M.P., but the other thing he did was that Nichols and Balding, using the highest coefficient of inbreeding ever seen in the world, the match between Exhibit 135 and 56A and 69A went from one in 310 million to one in 5.9 million. The point to be made, My Lord, is that when Dr. Shields's testimony was given closer examination it revealed, we would suggest, that he had been taking worst case scenarios to make his point, and it's not something that is new for Dr. Shields because in the Streich, Todd Streich, the Vermont case, that was one of the judge's comments, that in one of these statistical works that he was doing he had actually taken the most extreme example, and I would suggest that closer examination here revealed that Dr. Shields

performed unrealistic calculation devoid of any practicality in relation to that, and I wish to quickly conclude, My Lord.

We apply the reverse, even the defence evidence, even the defence evidence, put in its most favourable light by applying the Nichols and Balding correction factor associated with the degree of band sharing that he says occurs; accepting all that, which the Crown certainly doesn't, but to allow for all that, to even allow for a correction factor highest ever seen in the world, even the defence expert himself said that the bottom line for the figures that he's generated is that they're rare, that they were extremely rare figures, and that is putting the defence evidence in its best light, and we go back to what Dr. Carmody said we must remember about these numbers, they're rare, they're rare, they're rare.

To even go one step further, to put the Crown's case in the worst possible light, even Dr. Shields was prepared to suggest, and he did suggest, that you could still use the phenotype scoring. That is actually instead of using these calculations and these formulas you could still, and this was putting the Crown's case in the absolute worst possible light, I've tried to put the defence case in the best possible light - we would still be entitled to use statistics. Mind you, very low statistics, according to him, you could look and say in an 800 sample - if for example eight or nine hundred data base sample, and you could according to him use a phenotype scoring where you say O.K., I saw that one time in 900 so I'll tell the jury that the

frequency is one in 900 for that probe, or if I haven't seen it, it's less than one in 800, and it's a purely rudimentary principle, but the point to be made is that even in that case statistics could be used, and that's the point. The defence evidence, the bottom line is that statistics can still be used you can still give a mathematical expression to the existence of the match. That has not in fact been refuted. It's just what mathematical expression can I give, and when you apply confidence intervals to these - to go back to Nichols and Balding and to the R.C.M.P. and FBI calculations - you can still give and you can still weigh this particular evidence and the jury can weigh it, the jury can look at it.

In determining the appropriate method, and at this point I hope the Crown's position is clear as to the method that we suggest we've proven to be reliable, it's been used in hundreds of cases, it is of extreme importance that the Court distinguishes between the weight, the province of the jury, and admissibility, the domain of the judge. If the process and method of calculation of probability figures is generally accepted, in your opinion, and/or reasonably reliable, that is from the Crown's perspective, the use of a data base, the use of the Hardy-Weinberg equation, the use of the product rule, the use of confidence intervals, then any disagreement My Lord, over the correctness of the sum result is one of weight, weight for the jury to look at how these calculations were made and why they were made. That is the point that's made in Yee, made in many of the cases.

The issues of substructure and its effect on the validity of the probability estimates are matters of weight for the jury only, and that kind of conclusion has been drawn historically. The American cases obviously have greater experience with respect to many of these forensic techniques. Prior to DNA there was electophoresis testing of blood, etc., etc., but historically courts have pointed out that these matters are essentially matters of weight, and if we remember one of the very first cases that we've ever seen is Castro, in which the trial judge pointed out that when it's totally unreliable, if we agree, then it's irrelevant, and it's purely prejudicial and it should be removed from the jury, but once the Court concludes that it's reasonably reliable, then any question over the correctness of the final sum total is a question of weight for the jury, and again I come back to the conclusion, and that is that these expressions of what these matches mean between the samples purporting to be of Allan Joseph Legere and these found at these crime scenes is that they're very rare, very rare events, very rare calculations.

The final thing I'd like to do, My Lord, is somewhat unusual but I think it would have an effect, and it goes back to putting the defence case in its best possible light, and that is when you look at the defence evidence in association to what they consider to be their opinion related to these particular issues. In relation to what Dr. Bowen did Dr. Shields apparently took no exception, and in fact I put the question to him as to where - if anything that Mr. Furlotte referred you to, whether or not you disagreed with any of the calls of Dr.

Bowen, and he said no. He agreed - and we quoted statements from him from Bourguignon, that the application - the question put to him was, "And the application in DNA typing forensic, is it generally accepted", and the answer was, "The application of DNA typing, the molecular portion of it, the running of the gels done carefully is acceptable to determine if there is a match or not and I would suspect that almost all molecular geneticists would say that as well". He goes on to say when he was asked for his opinion with respect to the general acceptance in the scientific community, he says, "My personal perspective is that DNA typing is reasonable, relevant, and when it is done right, even an exciting tool to allow for the exclusion and inclusion of evidence", and that was another statement that he agreed with that he previously made.

When it came to the question of the correctness of the figures used to express the match he pointed out that - the question was put to him on cross-examination, "Well, I'm kind of a simplistic-type person. I'm not - one in 5.9 million, would you consider that to be common, rare? Would you consider that to be almost proof of the same source if the two forensic samples came from the same source". "No" - and this is a figure he generated using the Nichols and Balding correction factor - "No, I would consider it to be rare, exceedingly rare". That is the defence evidence associated with respect to the issues that we have, and I would suggest, My Lord, that this is evidence that should and must go before the jury. Thank you.

THE COURT: Thank you very much. It's half-past twelve. Mr. Furlotte, you're going to argue this, I presume, for the defence.

MR. FURLOTTE: Yes.

THE COURT: What time do you want to start? Quarter to two?

MR. FURLOTTE: I think quarter to two. I'd like to try to finish up today, quarter to two.

THE COURT: Yes. I don't want to rush things at all but I have to get away from here by five o'clock at least. Otherwise we'd have to go over till tomorrow morning. I'm ready to go over till tomorrow morning but if you felt you were going to push that too close we could even perhaps start a little earlier.

MR. FURLOTTE: Well, I suppose by four o'clock I might have an idea if I can finish by five. If I can't finish by five, then we may as well finish at four-thirty.

THE COURT: Yes, I don't want to push it but I do have an unavoidable obligation this afternoon at half-past five. Well, quarter to two, then.

(LUNCH RECESS - RESUMED AT 1:45 p.m.)

(ACCUSED IN DOCK.)

THE COURT: Now, Mr. Furlotte, you were going to make your representations?

MR. FURLOTTE: Yes, My Lord. My Lord, before I begin my representation I would like to put on the record that I would object to Mr. Walsh submitting any further written brief to this Court which I would not have had the opportunity to address. I know he has stated he expects his written brief to cover the area he's already addressed in court but I also notice when he was giving his oral argument that he

was saying, well, this further will be addressed in my written brief, and I would submit, My Lord, that if the Crown Prosecutor is allowed to present a further written brief to this Court in argument that I would then not have had the opportunity to rebut or address any such arguments that he proposes in his written brief or any areas of the transcript that he wants to bring to your attention that I might on the other hand have been able to, if not explain away, at least qualify statements that are made therein, and I thought maybe I'd be better putting this on the record before I start rather than after in case Mr. Walsh would like to comment on it.

THE COURT: Yes. Let me say this, I understood from what Mr. Walsh said that any brief - and he can correct me if I'm wrong - any brief he put in would be reiterating or would be summarizing or would be reflecting what he has said here this morning in respect of the population genetics aspect of the argument without introducing anything new, with the exception of certain - you said you would have quotes in it which you didn't -

MR. WALSH: Yes, the purpose behind the written brief is to address the issues of population genetics. In my oral argument this morning I followed the brief that I intend to file, albeit the written brief would certainly be more extensive in the sense that there are quotations and references to the evidence that will be included in the written brief that I didn't actually address in my oral argument other than alluding to it. It follows essentially the oral argument that I made, but in detail with references to the evidence and references to the case law.

I don't know what Mr. Furlotte indicates that he's not going to have an opportunity to respond to it. Whereas he's now having an opportunity to respond to any oral argument I've made which is essentially most of the written brief, he would in addition have the opportunity when I file my written brief to respond to the written brief by his own particular brief in writing, and from the Crown's point of view, My Lord -

THE COURT: I wonder if that wouldn't be a solution to the problem. Mr. Walsh has given his oral presentation on the assumption that he would be able, I presume, to file a written brief reiterating the same arguments but enlarging on them insofar as quotations and so on from evidence are concerned. Why would it not be a satisfactory solution to say that if he doesn't - how long would you require to do that, a week, or -

MR. WALSH: I've been working on it - I was trying to get it ready for today but the typing is still in the works. I would hope to have it filed next week.

THE COURT: Suppose a time limit were put on and then we said what, ten days after that you would have the privilege -

MR. FURLOTTE: I think the key words you said, My Lord, was a solution to the problem, and that's what I wanted to address. Our last day in court it was my understanding that the directions from the bench was that you did not want written briefs, you did not mind a short outline of the issues and short comments on it, and I had no objections to that. The advantage again, and disadvantage to Mr. Legere, is that Mr. Walsh is, I suppose, blessed with the ability

to spend all his time on DNA evidence. Unfortunately I do not have, I'll say that advantage or - and in that sense immediately, as soon as this -

THE COURT: You may be becoming a millionaire and he may be just coasting along on his usual salary.

MR. FURLOTTE: Well, I'm just trying to provide the best defence for my client in the - I suppose in the time period that I have to do it, and Mr. Walsh can spend all his time on DNA evidence. As soon as this hearing is over I have to go and prepare for all the other 250 or so witnesses for trial, and Mr. Walsh doesn't have to do that, and he can dilly-dally all summer on this if he wants and spend all the time to prepare the best argument he can, I would admit he needs a darned good argument, but I don't think he should be given all summer to do it.

MR. WALSH: My Lord, I've got only two points to make here. One is that in the last two days I have had the total of probably eight hours sleep trying to get ready for this. Now, Mr. Furlotte's work ethics may be such that he doesn't want to respond in writing, but he's going to be given the opportunity to respond in writing and have the last word. Now, he has mentioned time and time again over the last few weeks he wants the truth, and what I'm attempting to do is provide as much detail as reasonably possible so that when the Court arrives at its final conclusion it will have all the information taken into consideration. Mr. Furlotte is going to have the opportunity to respond and the last word, and I won't even comment on this aspect of I have nothing else to do because I have to -

THE COURT: Let me make just two comments here very briefly, and one is I think it's regrettable, Mr. Furlotte, if you were going to raise this point that you didn't do it before Mr. Walsh at least spoke this morning, you know, because he's been going along here and he hasn't quoted sections from the evidence on the assumption that he could put that in his brief, and he -

MR. FURLOTTE: I discussed this at noon-hour with co-counsel and we have no problem if Mr. Walsh wants to get up and finish his oral argument that he feels that he may not have done because he was going to do it in a written brief. Before I start, I have no objection to him doing that.

THE COURT: Well, the second thing I want to do is this, we'll resolve it in this way. You will have a week, Mr. Walsh, within which to put in your written brief on this second part of the thing. I would expect you to confine yourself to the points you made this morning, enlarging as necessary in references to the - as you indicated you would in your oral presentation, referring to the excerpts or pages, whatever, in the printed transcript. Then you will have ten days, Mr. Furlotte, to reply to that. You will have the advantage then of having what he has said here this morning by way of argument on paper. It should be - if there are particular points arise, you may feel that you've covered it adequately this afternoon. If after having read his material you feel there are other points you want to add, then you can go ahead and add it, and you'll have ten days from the time that he puts in his brief to do that. I'll say this, that I'm going to be

largely guided by what has been presented here in oral argument today and not by the briefs, but I do want to have the advantage of having the page numbers of the quotations that he was referring to here this morning when he was referring to Carmody and Kidd and so on. This isn't going to work a hardship on anybody, really.

MR. FURLOTTE: My Lord, one other problem I have with that is this trial has started properly. Mr. Legere, although he did not enter a not guilty plea, you entered it for him and on his behalf, and this is not just -

THE COURT: By operation of law.

MR. FURLOTTE: By operation of law, yes, and by operation of law the trial for Mr. Legere has started, and Mr. Legere has the right to be present for all arguments that may have any effect on the outcome of his trial and any written brief would - my position is any written brief would have to be submitted to the Court in open public before Mr. Legere. Otherwise there's things going on behind Mr. Legere and outside of Mr. Legere and he doesn't know what's going on, and there's no doubt that the decision, your decision in this case, is going to have a great effect on the outcome of his trial, and I think these should be done in open court and not by written briefs either by the Crown Prosecutor or by myself.

THE COURT: Well, we'll do this, we'll provide for the briefs in the way that I've outlined earlier. If counsel feel that a further oral argument is desirable or necessary, I'll hear your joint representations together on that and we can decide whether you want to

I think we're sort of talking about nothing here. I doubt very much if you'll feel that there should be oral argument and I doubt if Mr. Walsh will feel, because I think the thing has been so totally and completely canvassed in the five weeks that we've devoted to it, but - however, we'll leave it open in that way, and I would expect those representations to be made within a few days - I'll specify later, before we adjourn today, within a few days of the close of the ten-day period which expires after - ten da after Mr. Walsh has filed his brief. O.K.? I totall agree with you that if - Mr. Furlotte, I want this understood, that if you feel there should be an oral argument on anything that has been filed with me out of open court I think you should have and the Crown should equally have the right to that, opportunity for that argument, if necessary.

MR. FURLOTTE: All right. My Lord, the only objection, I guess, and one basic objection against just presenting written briefs is a written brief if I just submitted, that's basically the end of it, and as has been going on in this court between myself and expert witnesses and sometimes between myself and yourself and myself and the Crown Prosecutor, there is a lack of communication, that we don't communicate on the same level or that we say something that the other one takes a different meaning from what we're saying, and if I'm presenting my argument orally to you and you don't quite understand it, then I'm available for you to ask questions. If I just submit a written brief to you, then I don't have that advantage of clarifying any point that you may not quite understand.

THE COURT: I completely agree with you on that. I will say this, too, that if Mr. Walsh provides a brief which goes far beyond what he says it does today I'm going to send it back to him and I'm going to tell you, look, don't bother putting in a brief in reply. O.K.

MR. FURLOTTE: O.K., My Lord. My Lord, I believe I have about maybe ten issues or sub-titles here to deal with in court, and the first one, of course, would be the relevant law in Canada, what legal standards should the courts apply. I'd submit, My Lord, that the Court could use the Frye Plus standard which is applied in New Mexico, and in that citation, State v. Bell, and it's reported at 90 N. M., which would be New Mexico Reports, at Page 134, and also at Volume 560P, 2nd Edition, Page 925, a 1977 case. There the Frye standard plus reliability plus relevance, and relevance meaning probative value weighted against prejudicial effect, so in that case they use not just the Frye standard but they also use the relative reliability and the relevance test which is used throughout the United States. Or again the Court could use -

THE COURT: I'm sorry, the name of that case, Mr. Furlotte?

MR. FURLOTTE: State v. Bell.

THE COURT: New Mexico?

MR. FURLOTTE: New Mexico, or again, My Lord, the Court could use the reasonable reliability test as advocated by the Crown. However, and I may qualify that to this particular case, I believe common sense ought to prevail, whichever test this Court decides to use or whichever test any court in Canada decides to use. Under the Frye test the purpose of a Frye hearing is to determine whether the proponent of

novel scientific evidence can meet its burden of proving that the evidence is accepted as reliable by the scientific community. In this case the Court must decide two Frye issues: one, whether the Crown has carried its burden of proving that the R.C.M.P. DNA test is generally accepted as reliable by the scientific community; and two, whether the Crown has carried its burden of proving that the procedure for computing the statistical frequencies of DNA prints is generally accepted as reliable by the scientific community. The critical question facing the Court is whether a general scientific consensus has been achieved. It is not the Court's responsibility to decide which party to a scientific dispute is correct and which is incorrect. The Court need only decide whether scientists generally agree or disagree concerning the reliability of a new technique. If the Crown did not prove that there is general acceptance by the scientific community in the relevant fields that the novel techniques are reliable, then the Frye burden has not been met and the novel scientific evidence must be excluded.

In a Frye hearing the Court has to confront two questions: one, would the R.C.M.P.'s method for declaring matches be generally accepted as reliable by the scientific community if that community had all the information that was available to the Court, and again, two, would the R.C.M.P.'s method for calculating the statistical probabilities of a match be generally accepted as reliable by the scientific community if that community had all the information that was available to the Court. Before the evidence can be admitted both questions have to be answered

in favour of the Crown.

Under the issue of reasonable reliability: The Court should not be asked to resolve questions of reliability that the scientific community are unable to answer. Unlike the Frye test where the Crown need only prove general acceptance of reliability by the relevant scientific community, the reasonable reliability test requires only evidence that, in fact, the novel technique is reliable, without the need for general acceptance by the scientific community. It would not be necessary for the Crown to show that there is no disagreement as to reliability within the scientific community. However if there is evidence of disagreement within the scientific community as to the reliability of the novel technique, then the burden on the Crown is to prove that the disagreement is not substantial, not founded, not warranted, and irrelevant. The Court must look at the degree of resistance by the scientific community in accepting the novel technique as reliable. If evidence shows that the concerns by the reputable scientists are valid and that the issues are yet unresolved by the scientific community, the Crown would be hard pressed to ask the Court to resolve those issues and declare the novel technique as proven to be reliable.

It has long been recognized that scientific techniques that are reliable for one purpose in a particular field may not be reliable when applied to forensic case work. Representative of this phenomena is hypnosis and voice prints.

Hypnosis is reliably used in the diagnosis and treatment of mental disorders. Courts, however, have

held that the forensic use of hypnosis to be inadmissible. Similarly, voice printing can be reliably used by speech scientists, psychologists, and engineers, but the technique has not distinguished itself in forensic applications.

The principal issues in contention of DNA analysis are not matters of weight. They turn on this Court's assessment of what relevant scientific communities would generally accept as reliable. If the R.C.M.P.'s predicate experiments would not be generally accepted as reliable, reproducible or valid, then the DNA evidence should not go to the jury. Reproducibility in particular is a very discrete issue. Similarly, if the R.C.M.P.'s method for calculating a statistical probability is not generally accepted as reliable in the population genetics community, then the jury should not be asked to resolve that controversy.

Under the heading of relevance: Separate and apart from the issue of general acceptance and reliability a court ought to require that scientific evidence be admitted only if its probative value outweighs its prejudicial effect.

The potential prejudice is huge. If the jury is told that a DNA match is made from the probabilitie of a random match in one in ten thousand or any other similar number, the evidence's effect on the jury will be powerful. There is prejudicial effect to the frequencies listed of one in 1,000 to one in one million. Those numbers, on the face of it, can easily be equated with beyond a reasonable doubt, and there is prejudicial effect to that when there is not significant scientific agreement or consensus

on the precise number that we're dealing with.

The power of these statistics is particularly alluring to a jury confronted with the complexity of DNA RFLP analysis. After hearing such a large number the average person would find it difficult to be disinterested, or even patient, in trying to decipher and analyze such evidence. How long will a jury be able to pay attention to questions about band shifting, matches, substructure and population genetics theory? It is too easy to jump on the numbers and not have to grapple with the theories and what they're actually saying. The probative value of illustrating with numbers the points that each side is entitled to make is outweighed by the prejudicial effect of the numbers and the conclusions which could be drawn from them.

Beyond the heavy weight given to scientific testimony by jurors, media attention given to DNA testing, regardless of type, has given it an aura of infallibility such that jurors are unlikely to suspend belief in defective results, even when technical errors in the testing procedure leading to unreliable results are pointed out. Thus, such evidence is likely to be far more prejudicial than probative.

Consequently, the Court, to assure that an accused will not be unfairly prejudiced, must be convinced of the reliability of the evidence to a very high degree of certainty.

If this Court is unsure whether the R.C.M.P.'s methods are generally accepted as reliable by the scientific community or if this Court finds that issues are being debated by the scientific community

and that more research is necessary before a reliability decision can be made or if this Court finds that more time is needed for the scientific community to examine the R.C.M.P.'s studies and data the evidence cannot be admitted.

Under the heading burden of proof and prejudice The Crown bears the burden of proving that the DNA evidence is admissible. That burden ought to increas as the potential prejudice from the scientific evidence increases. In this case, given the enhanced aura of special reliability that surrounds DNA fingerprinting, the burden must be a heavy one requiring a very high degree of certainty.

Since DNA evidence has the apparent power to prove essential elements of a case, that being identity, beyond a reasonable doubt, it follows that the burden on the proponent of such evidence to prove that the method is generally accepted as reliable by the scientific community should approach beyond a reasonable doubt, or certainly be some unit of measurement greater than a preponderance of the evidence.

In People v. Reilly, a 1987 case reported at 196 California Appeals, 3rd Edition, Page 1127, and at Page 1148, quoting from People v. Brown, 1985, 40 California, 3rd Edition, Page 512 at 533; it stated:

> "Kelly/Frye hearing does not demand judicial absorption of all the relevant literature, nor does it require a decision once and for all whether a particular kind of scientific evidence is reliable. The Court need only conduct a fair overview of the subject, sufficient to disclose whether scientists significant either in number or expertise publicly oppose (a technique) as reliable."

In discussing the admissibility of electrophoretic testing, the court in Reilly citing People v. Shirley, a 1982 case reported at 31 California, 3rd Edition, Page 18 at Page 55 stated:

> "A further finding that the technique is in fact reliable was beyond the trial court's realm. Its duty was not to decide whether the technique is reliable as a matter of "scientific fact", but simply whether it is generally accepted as reliable by the relevant scientific community."

That's Reilly (supra), at Page 1135 and 1152. It continues:

> "The needed consensus is that of scientists, not courts."

Quoting Reilly again, (supra), at Page 1135:

> "Judicial notice in the context of Kelly/Frye hearing has been taken of judicial decisions, People v. Kelly, (1975), 17 California, 3rd Edition, Page 24, and again, People v. Palmer, 80 California Appeals, 3rd Edition, Page 239, as well as testimony in other trials (Brown, supra, at Page 535), and scientific and legal arguments (Kelly, supra, at Page 35."

The quote from Kelly:

> "The courts view such writings as evidence, not of the actual reliability of the new scientific technique, but of its aceeptance (vel non) in the scientific community."

Sorry, that's quoted from Shirley (supra), Page 56.

The fact that recent court opinions are split is yet another indication that there is a split of opinion on the merits of the procedure and method and therefore something less than the general acceptance required by Frye.

A review of expert testimony in other cases supports the claim that there is general disagreement among scientists rather than the Crown's claim that there is general agreement. How the Crown can hope to prove the R.C.M.P.'s novel techniques as reasonabl

reliable when there exists general disagreement within the scientific community is beyond comprehension, or at least beyond common sense.

Expert witnesses as opponents to the claims made by the R.C.M.P. far exceed expert witnesses as proponents of the novel technique, both in number and stature. This is admitted by the Crown's own witnesses.

In the face of a weak effort by the Crown to show scientific acceptance or reliability, it is unmistakably clear that the R.C.M.P.'s matching, binning, and calculation of frequencies have not been accepted by the scientific community and is not considered as reliable by the only people qualified to make that decision.

Before I go on to my oral argument, My Lord, I would like to, I suppose, take the position of Dr. Shields and advise this Court that I'm not concerned with Mr. Walsh's opinions, I'm not concerned with the scientific opinions, and neither should the Court be concerned with my opinion as to the reliabilityness of this evidence in whatever context the Court so desires, but as Dr. Shields, I got the opinions or I have the data as which is stated in the evidence by the expert witnesses.

Just a brief touching on quality control which an exhibit put into evidence by the Crown, VD-92, "Quality Assurance", dated February 18, 1991, that quality assurance manual which was accepted by the R.C.M.P.'s laboratory, one of the conditions or standards was that they have on Page 20 of that exhibit, VD-92, which calls for open proficiency tests, and on Page 21 it calls for blind proficiency

tests. The evidence of Dr. John Waye, on cross-examination, Volume 5, Page 355, I've put a question to him, I said:

> "Did you do any proficiency testing on Dr. Bowen in '89?
>
> A. I didn't, no, I didn't.
>
> Q. Did anybody do any proficiency testing on Dr. Bowen in '89?
>
> A. I can't recall, Dr. Bowen was trained and Dr. Bowen analyzed a number of samples. I did proficiency tests and certainly that was part of the procedure, I'd be outside of my knowledge if I talked to you about proficiency results or when he was tested or how many samples he was tested or who tested him."

Again cross-examination of Dr. Waye in Volume VI, Page 28 and this was setting up standards:

> "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."

Again Volume VI, Page 73, cross-examination of Dr. Waye, his answer:

> "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."

Volume XI, cross-examination of Dr. Waye, Page 94, the question to Dr. Waye:

"Q. In fact, Doctor, there was no standards set for proficiency testing while you were at the R.C.M.P. lab, was there?

A. Proficiency tests were being conducted. They weren't being designed, set up and scored and arranged by myself.

Q. Are you talking about proficiency testing when you were training Dr. Bowen to do these tests?

A. Again I didn't train Dr. Bowen."

Just as a point of interest, My Lord, when I asked Dr. Waye in Volume VI at Page 92:

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

His answer is "No", but when I asked Dr. Kidd basically the same question in Volume XII, Page 34, I put the question:

"Q. In the Yee case, Dr. Gilliam concluded at page 33 again, "-- that the proponents of the forensic application of DNA technology are, in using a quasi-continuous allele system, taking DNA electrophoresis methods about as far as they can go, and stated that it was a 'very technically demanding problem'." Would you agree with that, that it's much more technically demanding than in medicine in your lab?

A. Yes, by and large I think it's more technically demanding."

My Lord, just for a brief argument, the quality assurance, as I understand it, is to keep tabs and to inform the technicians that they are going to at least have some kind of control and examination over their tests and the results to keep the quality of those tests up. I believe in the OTA report it was well documented that many mistakes in samplings are made in laboratories and that high degree of

probability, at least a small degree of probability is existent in almost every lab that conducts either these kind of tests or any other kind of forensic tests, and in some laboratories and in some tests the error rate is quite high. However, I believe that there is no evidence to show and I believe the evidence does show that the possibility of errors in mixing up samples, that is not calculated into the probability factors when you're calculating the frequencies of the binning and the matchings in a data base. There is, I suppose, no justifiable way that one could try to calculate that possibility into the end product, and unfortunately that's a problem which the scientists have to deal with and I suppose a problem which would have to be addressed to a jury as a matter of weight, but again, since no proficiency tests were being conducted, at least there's none into evidence by the R.C.M.P. lab when this case specific evidence was taken, there's no way that this Court can even guess as to what their rate of error may have been. In the end result, when the Court is questioning itself as to whether or not this evidence is reliable to put before a jury, I just submit, My Lord, that this is one of the issues that you would have to bear in mind as to whether or not the probative weight would be greater than the prejudicial effect on the accused.

My Lord, back to the reliability tests, whether we use the Frye or the reasonable reliability tests, again I'm just going to refer to evidence which was given on the hearing, and particularly by the Crown's own witnesses. On cross-examination of Dr. Waye, Volume VI, Page 90, Mr. Walsh stated:

> "...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."

I would submit, My Lord, that Mr. Walsh was submitting at that time that the test for this Court to decide is whether it's reasonably reliable and then if this Court decides it's reasonably reliable, then it can be assessed by a jury and then the weight can be placed on it by the jury, but I understand the Crown from stating this morning, and again it may be a misunderstanding of mine, is that the Crown wishes you to put everything before the jury as a matter of weight and not even decide whether it's reasonably reliable, to allow the jury to make that decision.

At the bottom of Page 90 Mr. Walsh states:

> "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."

My comment to that, My Lord, is I submit that that is not a proper test for a Frye hearing. It's not whether they must show acceptance in the general scientific community, a Frye hearing must show acceptance by the general scientific community and not just some member in the scientific community.

We only have to go back to the Shirley case or the Reilly case which I had quoted earlier. It says:

> "A further finding that the technique is in fact reliable was beyond the trial court's realm. Its duty was

> not to decide whether the technique is reliable as a matter of "scientific fact", but simply whether it is generally accepted as reliable by the relevant scientific community."

When Dr. Waye was on direct examination, Volume V, Page 315, he's discussing peer review and publications, and he was speaking about publications, he said:

> "..there's many, many more commentaries from people who either have an interest for or against the technology. And a lot of times there are published in peer reviewed journals as fact when in fact they're rolling commentaries either for or against DNA typing. And there's been exaggerations both ways. DNA typing has been started up on a pedestal doing much more than it is ever capable of doing and it slid down as far as being incapable of doing nothing properly."

Again on Page 316 where Dr. Waye stated:

> "Some of the best accounts, some of the best descriptions I've ever had of the whole procedure have come from judge's rulings at the end of long hearings."

And yourself asks:

> "No, but they hardly amount to peer reviews?"

And Dr. Waye stated:

> "No, but they're understandable and they have a way of getting away from all the complex scientific jargon and putting it into simple terms and they're actually a nice place to start."

I would submit, My Lord, at this time that the courts are probably the best place to end up deciding whether or not these techniques are reliable. They are not a good place to start.

In Volume IV, Page 66, cross-examination of Dr. Waye, I asked Dr. Waye:

> "Q. But the court is a form of peer review, is it not, in actual practice?
>
> A. I think scientists would disagree with that....that the court is

an appropriate peer review for scientific method or a scientific application."

At the bottom of the page he says:

"...I think it's a nice place to start when you have a large case where a large number of people testify, generally the judge's ruling on it will be quite extensive and will summarize the views of all the various scientists. So it's a nice place to start. The next place you would probably go if you actually wanted to take all those views into consideration is the telephone and actually phone some of these people and then conduct in a proper scientific manner",

and I put emphasis,

"and then conduct in a proper scientific manner, talk to the person who has a dissenting view, talk to him about what his concerns are, ask how you might address them, scientist to scientist rather than actually going to his full transcript and reading through all the -- ".

When I asked on Page 67 of Volume IV, I asked Dr. Waye about expert's reports, I said:

"You will read them. So you will admit that some of these experts called by the defence do have interesting reports?"

His answer:

"A. They're good reading, some of them, yes.

Q. Some of them have valid criticisms?

A. Some valid points are made."

In Volume XI, Page 106, cross-examination of Dr. Waye:

"Q. ...when you say it's your opinion it's accepted in the general scientific community do you just dispell and again ignore all the opposition to the methods and its application?

A. No, you don't ignore criticism, not as a scientist. You evaluate the criticism, you evaluate both where it's coming from and what

its substance is and what relevance it has.

Q. How much opposition would you need before you say, well, it's not generally accepted?

A. I'm not sure that's my decision to make. Again I'm not a pollster and nor am I involved in setting standards as to whether something is admissible or not admissible in a court of law."

Volume XI, Page 101, cross-examination of Dr. Waye:

"Q. Is the forensic application of RFLP analysis generally accepted in the scientific community as being reliable enough for the purpose of which forensic application is using it?

A. In my opinion, yes.

Q. The general scientific community out there accepts your opinion, that's what you're saying?

A. No."

Page 102 of Volume XI I stated to the Court:

"I'm trying to establish what the doctor means when he says it is generally accepted in the scientific community."

And the answer by Dr. Waye:

"A. Yes, that's the key part of that, what one person views as generally accepted. I've already said that I'm hesitant to say that my views are shared by everyone else, I know they're not.

Q. I'm not saying everyone. When we say generally, what do you mean by generally?

A. Amongst rational, thinking human beings who have a base of knowledge to -"

Q. You mean by a majority?

A. Not necessarily.

Q. Not necessarily. Do you mean by 40%?

A. Again, people who have a basis upon which to form a relevant opinion. Certainly if I walked down the street and if I walked into a bowling alley and queried the people you'd get very different answers from

> if - you may get different answers than if you queried a scientific audience who has a knowledge upon which to base that opinion. In my opinion it's generally acceptable if you ask people who are properly informed and experienced in this particular application."

I take it from that evidence, My Lord, that Dr. Waye considers this to be generally accepted in the scientific community by asking so long as one person or two or three people in the general scientific community accept this as being generally reliable. That I would submit, My Lord, is far from the test as set out in Frye.

On cross-examining Dr. Kidd on the reliability Volume XII, Page 32:

> "Q. Well, it seems that a lot of criticisms are coming from scientists who are not in the forensic field, say, Dr. Gilliam considering what the match criteria and windows - in forensics do you believe that the scientists in your field, you don't deal in forensics yourself?
>
> A. Not in the strict sense.
>
> Q. Do you feel it's one of your responsibilities to kind of be an overseer in what's going on in the forensic labs?
>
> A. Quite frankly, yes, because I have many years of expertise and experience in exactly the techniques that are now being applied in forensic laboratories by individuals who do not have that same level of expertise. And I certainly feel that there is incumbent upon me as a member of society to make that experience known and available."

At Page 59 of Volume XII, cross-examination of Dr. Kidd, I put the question:

> "Q. The point is, Dr. Kidd, is that there's a good many scientists out there in the general community who will agree with Dr. Lewontin, is there not?

A. Certainly there have been quite a few people who have testified in court cases to very similar opinions and have advanced them in other settings.

Q. As Dr. Lewontin? As Lewontin's opinion or as your own?

A. As Dr. Lewontin's opinion, that's correct. Not all of those people are, in my opinion, very well qualified to deal with these issues. Dr. Lewontin is eminently qualified in this area. I am not going to in any way challenge him. I have reached a different conclusion.

Q. Your opinion is not generally accepted -- how should I put that?

And Mr. Walsh said, "Carefully", so I put the question:

"Q. We've been playing with words here for a couple of weeks now, a slight of tongue can cause a lot of damage. Your opinion, doctor, would be hardly accepted by a majority of the scientists who would be qualified to give an opinion?

A. I have no good way of answering that. I can give a counter response, I don't know who would be qualified, I know many colleagues that I consider well qualified will agree with me. I know there are others who will not. There is room for scientific disagreement. In one court case I was presented with two lists by a defence attorney, people who had testified in much the same way I had and a list considerably longer about four times as many names who had testified against the admission of DNA ...".

Still in Volume XII, cross-examination of Dr. Kidd, I put the question; Page 113:

"Q. Would you call it a form of peer review?

A. What, a form?

Q. The fact" -

And I believe this was a form of peer review, actually, the expert witnesses coming to court.

"Q. The fact that witnesses for the defence go to court, provide expert reports as opponents to the reliability of RFLP --

A. In fact I think it's a complete breakdown of rational and proper presentation of evidence into the court system, because virtually all of the people I know that I consider highly qualified experts are refusing to testify because it's too great an imposition. And some of the people who are regularly testifying have no credentials that I think are acceptable at all, and not all of them certainly but some. And I think it is far easier for the defence to get witnesses than it is for the prosecution. I should qualify --

Q. Doctor, some of the witnesses for the defence have contributed their time voluntarily --

And yourself, My Lord, said:

"You didn't finish your answer, Doctor. Let the doctor, the witness finish his answer."

"A. I was going to say that I -- that my statement might seem prejudicial against the defence. I was thinking of the majority of context that I've been involved in. I should have more properly said, pro DNA is harder to find witnesses to testify than anti DNA.

Then on Page 114 it continues:

"Q. And in that respect it makes it easier for defence lawyers to get expert witnesses, that's the context you meant it in?

A. That's correct."

Volume XII, Page 143, cross-examination of Dr. Kidd:

"Q. Would you admit, doctor, that there is a general disagreement as to -- in the scientific community as to the reliability of these standards and results of these tests and the conclusions to be based upon the results? Will you admit that there is general disagreement in the scientific

community over the reasonable reliability?

A. The way you have phrased the question I will not admit that.

Q. Would you admit, doctor, that the product rule cannot be applied to identifying characteristics unless a valid foundation is first laid for the probability assigned to each of the characteristics and unless mutual independence of each of the characteristics is established?

A. That sounds very good and I would generally agree to that except that I think what you are going to mean by some of the words in that statement will be different from what I would mean by them. So I will --"

And he left it there. On cross-examination of Dr. Carmody, Volume VIII, Page 190, when I asked him about the qualifications of Dr. Lewontin that does not agree with the questions of reliability:

"A. He gave examples in this report that shows that he does not agree with that until there's more empirical data, yes."

And also when asked about Dr. Lander:

"A. Dr. Lander also is saying that we need more empirical data."

And I asked him about Dr. Hartl:

"A. He's saying that we need more empirical data."

I asked him about Dr. Ron Acton:

"A. I don't know exactly what I said earlier that I haven't actually read things that he has written. I am guessing that he's saying that we need to address the question of population subdivision and get more empirical data but I have not actually read what he's written.

Q. So there are a considerable number of eminent scientists in that field of population genetics that disagrees with those people in the scientific community who accepts it?

A. Yes."

At the bottom he says:

"In my opinion and in my judgment there are also pre-eminent people who feel as I do that the empirical evidence that we now have is strong enough to support using the Hardy-Weinberg equation and the product rule."

Continuing on cross-examination of Dr. Fourney in

Volume X, Page 154, Dr. Carmody (sic) states:

"A. Yes, I think it's important to know the population that you're dealing with.

Q. And also it is an issue, a bonafide scientific issue is the validity of the statistical methods used to assess the significance of RFLP inclusions. That is also a bonafide scientific issue?

A. Yes, I would say the statistical issue involved with frequency would be an issue that is a concern in the general population of scientists. But once again, you have controversies on both sides, and without it I don't think we'd have any science."

Continuation of cross-examination of Dr. Fourney,

Volume X, Page 132:

"Q. Are you saying there are no more controversies?

A. I think the controversies associated with forensic application are primarily dealing with aspects of the population genetics, for instance. The actual application of the technology is valid and has been well recognized. The office of technology assessment makes that very clear."

Again while I was cross-examining Dr. Waye, Volume V,

Page 289, I put the question:

"Q. And again, is this particular type of procedure of multiplying one band pattern by another band pattern by another band pattern, do you have an opinion as to its acceptability in the scientific community?

A. Again, if applied properly it is scientifically accepted."

And I would submit, My Lord, that he put the qualifier, "if applied properly", and the basic issues before the Court is whether or not they are applying the Hardy-Weinberg formula and the product rule properly.

My Lord, in VD-24 which is the Office of Technology Report, at Page 95, I would like to read in part of a paragraph. It says:

> "On the other hand, the Frye test has been criticized for being difficult to apply and for relying on the theory of general acceptance that may not equate with scientific reliability and validity. Some commentators note that workers in a novel area sharing a common goal can develop a technique that furthers their professional aim and they can generally accept it regardless of its scientific reliability."

Which I submit, My Lord, is what the forensic labs are doing in this case, they themselves are accepting it as generally reliable for their purpose but it is not being accepted as being generally reliable by the relevant scientific community.

If I may continue on, My Lord, under the topic, I believe, of Ø6, and I have it headed, "The R.C.M.P.': Novel Statistical Methods Have Not Been Subjected to Adequate Scientific Scrutiny".

The statistics are based on assumptions which have not been verified. Tests for independence which could be run have not been run. Additional studies which could answer troubling questions have not been done or completed and the procedures themselves, having just been published, have not undergone sufficient scientific scrutiny in view of all the opposition to the claims amde by the R.C.M.P. All

of this argues powerfully against the Crown's contention that these procedures are reliable and are generally accepted.

The Crown introduced into evidence the Office of Technology Assessment Report, VD-24, but the report does little to salvage the R.C.M.P.'s methods for calculating frequencies or the probability of a match. The report acknowledges the extensive debate among scientists on the fundamental questions concerning population genetics (VD-24 at Pages 66 to 68). Of far greater concern to this Court, because it will be a statement of scientists rather than of government, is the soon to be completed findings of the forensic DNA analysis committee of the National Academy of Sciences.

THE COURT: I'm sorry, what were you reading from there?

MR. FURLOTTE: These are my own notes.

THE COURT: Yes, but I thought you were quoting something?

MR. FURLOTTE: No, I wasn't quoting anything. I would quote from VD-24 at Page 66. It states:

> "Debate over population frequencies and RFLP analysis takes several forms, Pages 16, 17, 29, 57 and 69. General agreement exists that any potential bias that could result from calculating population frequencies be conservative, i.e., favour defendant. Nevertheless, questions are raised about whether existing population data bases are properly applied and whether they adequately support calculations of inclusions as currently practiced."

Again on Page 67, bottom of the first column:

> "One critical factor, these basic calculations are only valid when applied to populations in which the DNA fragments are statistically independent. Otherwise the value calculated might greatly underestimate the true occurrence of the pattern in

> the general population, making a match seem rarer than it actually is. Essentially the population must be one where individuals randomly marry and reproduce so that distinct sub-groups are absent. In such freely mixed populations there will be no correlation between alleles on the maternal and paternal chromosomes, Hardy-Weinberg equilibrium, and no correlation between alleles at different loci, no linkage disequilibrium."

Top of Page 68:

> "If the population is not freely mixed, then correlations between alleles at two loci can exist, even if they lie on different chromosomes."

I'd like to point out, My Lord, that it says at Page 67 that: "These basic calculations are only valid when applied to populations in which the DNA fragments are statistically independent", and they are talking here about DNA fragments at different loci.

My Lord, under the heading of "Fixed Bin Approach": The R.C.M.P. has not done adequate research on the degree of measurement error in its tests to allow a likelihood ration to be computed. However, Crown witnesses admit that an upper confidence interval ought to be applied. Unfortunately, for matters of scientific certainty or probability, it is unknown whether it is proper to use a 95%, or a 99%, or something in between as an upper confidence interval. Since the use of a 95% upper confidence interval could change the probability factor from one in six million all the way down to one in one thousand, we are dealing with numbers and methods which are unreliable and unjustifiable. Again, Crown witnesses admit that R.C.M.P. ought to use upper confidence intervals to correct for the size of the data base and measurement error. However, an upper

confidence interval does not correct for substructure.

Under the heading of "Substructure": It is not valid to use the Hardy-Weinberg formula or the product rule unless the tests are run on a homogeneous population, a population which mates randomly and is well mixed. Evidence of substructure is evidence that populations do not mate randomly.

Evidence supporting the theory that there is substructure in the Caucasian population comes from data on the rate of homozygotes (individuals with single-band DNA patterns) in the Caucasian population. When genetically different sub-groups are pooled together in a single data base, one finds a greater number of homozygotes than would be expected under Hardy-Weinberg assumption. Substructure can also be proven to exist if one finds a statistical significant difference in bin frequencies of two populations so tested. There are also other ways to test for substructure, any one of which would prove it inappropriate to use the Hardy-Weinberg formula and the product rule in calculating frequencies.

The R.C.M.P.'s approach to computing statistics is neither valid nor accepted by the scientific community. The most serious problem is that the R.C.M.P.'s approach depends on the assumption that the Caucasian population has no substructure and is randomly mating, an assumption proven as blatantly wrong. It is a universally accepted principle that the existence of undetected population structure invalidates the use of the Hardy-Weinberg formula and the use of the product rule when computing the frequency of genetic characteristics. Examples given in court illustrate that computations relying

on assumptions lead to serious errors where there is undetected population structuring. Simply put, if there is a structure among Caucasians then the R.C.M.P.'s method of calculating statistics is totally erroneous.

The Crown has failed to prove that the R.C.M.P.'s computations are not erroneous. The Crown has not proven that the degree of substructure is not greater than that revealed by the defence. Since the degree of substructure revealed by defence is statistically significant the defence has shown there is at least substructure to a degree of statistical significance which invalidates the use of the Hardy-Weinberg formula and the product rule.

The R.C.M.P.'s approach to computing the frequency of DNA prints is seriously flawed, not only because of its failure to attempt to evaluate the degree of substructure but also because there is no attempt to validate the statistical independence on which the product rule depends. To use the product rule as the R.C.M.P. does without verifying statistical independence is not acceptable in the scientific community. The Crown is again relying on another assumption without justification.

A method which claims that North American whites constitute a single homogeneous reference population to which all forensic cases can be compared is, as a matter of science, invalid and unreliable. Not only is the reference population unreliable for estimating an allele's frequency at a single locus, the multiplication method across loci is equally invalid. You are merely multiplying your mistakes. If two sub-populations differ in

their allele frequencies for two loci, then the population as a whole is not in linkage equilibrium, as I previously quoted from VD-24.

The fact that frequencies have been calculated for two sub-groups, the FBI and the R.C.M.P. data bases, that does not justify the use of either one or an average of the two. This evidence merely tells scientists that substructure definitely exists within Caucasians. It does not give any indication as to what degree substructure exists or how many different sub-groups exist.

Substructure is a quantitative issue. Since we do not know how much substructure there is, and we do not know by what factor there may be an overestimate or an underestimate, it is impossible to render a scientific opinion on whether some particular method on correcting bins did or did not compensate for something of which we don't know.

Without the numbers which express the extent of genetic diversity due to substructure, as a matter of common sense, much less reasonable scientific certainty, no one can tell how much of a number is needed to compensate. Again, you can't put a number on that which you have not investigated. It is uncalculatable.

The issue here is not quantitative disagreement between experts on the extent to which the R.C.M.P.'s estimate is wrong. Rather, the issue is fundamentally one of foundation and admissibility There exists no underlying data nor a procedure from which one expert can, in a scientifically acceptable fashion, offer an opinion as to how far off the estimate is.

The R.C.M.P.'s estimate is an unacceptable estimate, with or without an upper confidence interval. When we don't know what the right answer is and we don't know how far we are from it, due to substructure, then any number is unacceptable scientifically. It's an unacceptable procedure in science to float numbers for which there is such uncertainty.

If the procedure itself is scientifically unacceptable, as opposed to an erroneous result arrived at using an acceptable procedure, then the threshold test for admissibility has plainly not been met. The issue here is not the numbers, but rather first principles.

Under the heading, "Reproducibility of Data Base". Even if the R.C.M.P.'s methods of multiplication within and across loci were valid, which it is not, and its reference population appropriate, the R.C.M.P.'s procedures would still be fatally flawed because the frequencies assigned to the bins have never been proven to be reliable.

Reproducibility is a must for scientific evidence to be accepted by the scientific community and the courts. In view of the FBI's problems in reproducibility of its data base, the Crown could hardly expect the Court to accept the bin frequencies proclaimed by the R.C.M.P. after only one attempt, and no attempt thereafter to verify or validate its accuracy.

Evidence from Dr. Shields shows that when he did comparisons between the FBI's old data base and the FBI's new data base with his client's profile -

and that's in the Vanderbogart case - the probability figures changed from one in 50,744 to one in 102,934. Using the R.C.M.P. data base, the probability figures would have been one in 200,107.

To clarify for the Court, in the Vanderbogart case probability figures that were submitted were one in 50,744. Had he used the other data base which was calculated - which was put together by the FBI, and that other data base was of the same agents, FBI agents, the figures would have come to 102,934, and by using the R.C.M.P. data base they would have been one in 200,107. Unlike when he did the comparisons in the Legere case, if he compared Legere's profile with the FBI data base it made it the probabilities were much less, they were one in nine million rather than one in five million, and here it just had the opposite effect in that client's case, the Vanderbogart case, where the R.C.M.P. data base would show that it would be more prejudicial to his client, Vanderbogart, and in Legere's case the FBI data base would have been more prejudiced, so it's not a question of which data base you're going to generate the greatest figures out of, or the least figures, it's you can't tell until you know the profile that you're running through either data base.

I might add that these figures were arrived at without the use of an upper confidence interval. Too bad for Mr. Vanderbogart that Dr. Kidd never advised the FBI it really should be using an upper confidence interval.

The FBI's test and retest data provided an excellent presentation of its laboratory's poor

quality control and absence of reproducibility. The FBI's tests and retests were performed on the same Caucasian data base that it relied upon to calculate frequencies in case work. If their frequencies are unreliable, then so, too, are the ultimate probabilities being offered in cases. The R.C.M.P. has not offered any proof of reproducibility of bin frequencies. In fact, evidence showed that the R.C.M.P. could not at times match the accused's own samples. The accused's DNA, as with FBI agents, would be fit into different bins upon different tests. It would appear from the evidence that the technique or system used by the R.C.M.P. is even less reliable than the system used by the FBI.

My Lord, it might be an appropriate time for a break.

THE COURT: O.K., fifteen minutes.

(BRIEF RECESS - RESUMED AT 3:30 p.m.)

(ACCUSED IN DOCK.)

THE COURT: Now, Mr. Furlotte?

MR. FURLOTTE: My Lord, further under the heading, I suppose, of the topic of frequencies and substructure, I wish to read into my argument the so-called data that I will be relying upon in my final argument, and I would submit that you would have to pay particular attention to.

On the cross-examination of Dr. John Waye in Volume IV, Page 80, I put the question to Dr. Waye. I said:

> "Now, as you apply that to the human population, what are you attempting to do?

A. You're attempting to ask questions about the frequencies with which these detectable differences occur on a population level.

Q. Now, I understand that there are differences within different ethnic groups?

A. There can be, yes.

Q. Can be and these would be identified as different, say, sub populations?

A. If they were contained within what you're calling a general population, then, yes, you'd preface that with sub, you'd call it a sub population. It's an identifiable sub group within the broader population."

Again at Page 82 of Volume IV:

"Q. No, I mean we know if they're white or black they're probably going to be statistically different, because they don't mate at random.

A. Correct."

At Page 88 of Volume IV, top of the page, the question to Dr. Waye

"Q. So if it doesn't overlap it would be significant?

A. They're not even in the same ballpark.

Q. That's one in twenty six or one in fifty, not even in the same ballpark?

A. Right. You've analyzed --

Q. One in forty would not overlap, it would have to come with about one in forty eight.

A. Again, you have to know those exact numbers to know that, whether those numbers would be significantly different. Those are basic statistic tests that I can do given the proper tables, so that I can take a large amount of data and give to a statistician."

Volume V, Page 254, on direct examination of Dr. John Waye, bottom of the page, Dr. Waye states:

> "I think it would be scientifically incorrect to start with the assumption that if I analyzed a hundred people in this particular town, that they'd look like a hundred people in Victoria, British Columbia. I think that would be a scientifically poor assumption to begin with. But I think when you've done these studies and you realized that I've sampled from, say, five areas in North America, from all over North America and I get the same answers when I go from place to place, it's reasonable to conclude that if I go to a place that I have already analyzed I'm probably going to get the same answer again."

He continues to state:

> "It would be a blind assumption and an incorrect assumption to analyze in one area and then extrapolate to the rest of the country or the rest of the world."

Volume V, Page 265, direct examination of Dr. Waye, he states:

> "Looking at all that data, geographically or regionally, there's no difference, no significant differences in their frequencies of the things that we are measuring."

And at the bottom of Page 265 he continues to state:

> "Again, knowing that the population of New Brunswick doesn't deviate significantly from the populations in Canada in general and knowing that for instance, data bases are predominantly English people, say, the Vancouver data base and predominantly French people in the Montreal data base are very similar, there's no basis to believe that New Brunswick would be different from any of those other data bases. So you could apply a data base from Vancouver to a case in New Brunswick."

I'd just like to bring to the Court's attention at this time, My Lord, that the evidence did show that there's no evidence that there's anybody from New Brunswick in the R.C.M.P. data base and while

Dr. Waye is mentioning that there's no difference between the English people and the French people in the Montreal data base are very similar, we do not have any evidence as to what the statistics were in the Montreal data base, nor did the Crown's expert witness care to reveal it.

Volume V at Page 267, again direct examination of Dr. Waye, Dr. Waye states:

> "Among the first studies that were ever done with these types of probes and with other types of probes are to use the conventional racial groups, blacks, whites, orientals and assess the frequencies in each of those populations for that precise purpose to determine if there are any differences between the races and it does occur."

Page 268, Volume V, Dr. Waye continues, and this is on direct, Mr. Walsh's question:

> "Q. And about that data, in your opinion, doctor, what, if any comparisons can be made and what conclusions have you drawn from looking at data below the border and in Europe?
>
> A. The frequencies that you derive from the data bases don't change because of political boundaries again, geography has very little to do with the frequencies that you find in the Caucasian population."

Page 301 of Volume V, still under direct examination, Dr. Waye states:

> "If I could give an example, if you took -- if you took black individuals, white individuals and treated that as one population. If the frequency of a given band was very rare in the blacks, very common in the whites and you treated that as one population, you'd derive frequencies that don't apply to either of those racial groups. So that would be an improper application of both the Hardy-Weinberg formula and the product rule. That's called sub populations and I believe we talked about that yesterday."

Mr. Walsh put the question:

"Q. Is there things that population geneticists do to actually determine whether or not there is subgrouping and the effect its having on the frequency calculations, is there general things that are being done, that have been done and are being done by population geneticists?

A. There's empirical things that you can do, yes.

Q. Empirically, meaning you actually look at data?

A. Correct.

Q. From other areas?

A. Yes.

Q. And what other things are done?

Page 302:

"A. There's statistical tests and again, these are based on empirical observations. What you actually do is, if you've analyzed five hundred people, you can actually look at those five hundred individuals and say - how -- and now, we're not talking about frequencies of individual bands, how often have I observed individuals in that five hundred person population sample, how often have I observed individuals that have both of those bands that were in the question sample. And you'll come up with an observation, a certain number of individuals in your population sample may have that exact patter, that exact two band pattern. Now, you've already derived a prediction using Hardy-Weinberg equation, right. You can now compare your observed events to your predicted events and there's statistical tests that you can run when you have all of your observed events. So you've taken all the combinations that you've seen in that population sample, and then you've derived the frequencies and you've come up with numbers of how often you'd predict."

I'd like to point out that Dr. Waye is referring here to an empirical test to - in a sense

to attempt to validate your data base, whether it's a proper data base and is a proper representative of the population and that you should check your data base to see how many people are sharing two bands, how many people are sharing four bands, how many people are sharing three, four, or rather than say maybe bands, probes might be a more realistic answer.

There's no evidence in this court, and I tried on cross-examination to find out in the R.C.M.P data base how many people shared two probes, how many people shared three probes, and what were these statistics based on their data base as to the probabilities of them finding two people to share two probes, two people to share three probes, and I could not get an answer out of the Crown's expert witnesses. However, I believe we'll find out later on as I read through that they did come across where five people shared five bands but they took them out, whether they were justifiable or not, that's for someone to decide.

On direct examination of Dr. Carmody, Volume VII, Page 40, Dr. Carmody states:

> "If that number does not change substantially then you say and you have confidence that the estimate that you derived on the sample that you had is a reliable indication of what it would be under all further samples."

At the bottom of the page he states:

> "My conclusions were that the R.C.M.P. data and my analysis of it was a true reflection of the occurrence of these variants in virtually any Caucasian population in North America. There were some slight differences for France for the two probes that I looked at there. It is difficult to say what the net effect of the differences between France and North America would be in terms of doing all the calculations

> because I don't have data on all five probes from France."

And I'd point out maybe at this time, My Lord, in VD-65 where Dr. Carmody submitted and did the calculation for France on two probes that the calculations for D2S44 was one in 59 for the R.C.M.P. one in 70 for the FBI, one in 73 for Florida, one in 48 for Minnesota, and one in 34 for France. On D10S28 it was 108 for the Canadian or the R.C.M.P. data base, one in 92 for the FBI, Florida did not have any, one in 143 for Minnesota, and one in 54 for France.

Looking at VD-65 under the R.C.M.P. data base Dr. Carmody calculated 99% upper confidence interval where for D2S44 the one in 59 could be dropped down to one in 44, which the frequency in France is still only one in 34. Now, there's evidence that they're always looking to give the benefit of any doubts and to be conservative in favour of an accused person. If you also put an upper confidence interval on France's calculation, one in 34, I don't know how far down it would drop. Same thing under D10S28, if you put an upper confidence interval on one in 54 we don't know how far down it would drop, so if Mr. Legere in any way could be compared to his ancestors from France, then there's no way that the R.C.M.P. or the courts or anyone could know what those frequencies would actually end up at.

At the bottom of Page 41, Volume VII, Dr. Carmody continues:

> "For some loci for some probes, particularly D2, D10 and in same cases D17, there were statistically significant differences between the bin frequencies in Florida and in Texas. Minnesota it turns out --

> perhaps not surprisingly -- is more like the profile of Canada than either Texas or Florida is. However, conceding that there are differences and statistically significant differences in bin frequencies still had virtually no effect on doing the forensic calculations as we have gone through for each locus and for the product rule of ultimately getting the forensic probability."

And I will discuss the difference between statistical significant differences and forensically significant differences later. Mr. Walsh put a question to Dr. Carmody on Page 42, Volume VII:

> "Q. I don't know if you mentioned the FBI. Did you notice any significant statistical difference in the bin frequencies between the FBI and the R.C.M.P.?
>
> A. There were I believe --- and I have the data that I can refer back to -- I believe for D2 and D10 there were some statistically significant differences."

At Page 45, Volume VII, Dr. Carmody continues under direct examination:

> "...what I mean by talking about the numbers at that very low infrequent level not being significantly different so I am saying that if a number is one in a hundred thousand, one in a million, one in two million, they are insignificantly different from one another. The precision of our estimates is not so great that we can say that it is exactly and precisely one in 1.1 million. We would have to give some kind of interval of that estimate to really reliably indicate where we thought the estimate actually was."

Bottom of the page he continues:

> "Statistically there would not be a test based on the sample sizes that are used in forensic work that could discriminate and that would say that one in five million is statistically different from one in ten million."

I would like to point out, My Lord, that Dr. Carmody is admitting that when the Crown is arguing no forensic differences that statistically there is no test that they can apply as to whether or not there is a difference. I'll later get into that but we're dealing with matters of feelings and purely subjective opinions when the Crown's witnesses are referring to no forensic differences.

Discussing about the 99% upper confidence interval Dr. Carmody states at the end of Page 46 of Volume VII:

> "A. There is not a great significance, and in fact in these intervals often when you have an estimate of one in five million you could not exclude in fact all the way up to one in ten billion, and on the low side that one in five billion could be as small as one in two million."

Again, not being a statistician I find it difficult that when you're applying an upper confidence interval, be it 99% or 95%, if the middle number you're working on is five million, how it can go all the way up to ten billion but can only drop down to one in two million. I would simply put, My Lord, did the Crown's expert witness explain that sufficiently for the Court.

On Page 47 of Dr. Carmody's testimony, Volume VII on direct examination he states:

> "A. I have seen in the studies I have done, I see, and I would expect no significant forensic difference in the implications and from the numbers that you would derive from any of those calculations."

On direct examination still at Page 62 of Volume VII, discussions about France from the Crown Prosecutor, Dr. Carmody says:

"A. I think it is trying to give a sense of how Caucasian populations might vary when we get larger samples taken worldwide.....show quite a bit of variation in bin frequencies. Statistically significant differences in bin frequencies from one Caucasian to the next and they are indicating that in fact these variants are much more common in France than they are in present-day Caucasian populations in North America."

And I would submit, My Lord, without clear evidence that there are French people in the R.C.M.P. data base who are descendants from France that it would be impossible to tell what the difference may be.

Again, Dr. Carmody at Page 63 of Volume VII, he states:

"...there is a good representation from the Province of Québec, for example".

There is no evidence before the Court that there is anybody from the Province of Quebec in the R.C.M.P. data base.

Volume VII, Page 67, Dr. Carmody's direct examination in relation to the work that Dr. Shields had done in the Vanderbogart case Mr. Walsh asked Dr. Carmody:

"Q. Do you have any opinions with respect to the work that he has done?

A. I think the work that he has done is correct. I think he has found statistically significant differences for some loci in the bin frequencies between the Canadian database and the R.C.M.P. database",

and I might think, My Lord, that between the Canadian database he probably meant the FBI data base and he continues to state:

"That in fact there is no forensically significant difference even though the bin frequencies are slightly and in fact statistically significantly different in the F.B.I. database than

in the database that we used in the Canadian calculations."

At Page 69 of Volume VII Dr. Carmody continues:

"It means that in fact by substructuring that one really has to be sensitive to the fact that within that geographic or demographic unit that one is studying that there are smaller components within which there might be some differences, and so one of the uses of substructuring is to indicate and try and convey the idea that the population that you are studying in toto is not a homogeneous unit and should not be treated statistically or mathematically as a homogeneous unit."

I would take from Dr. Carmody's statements here on direct examination he's stating that once we have evidence of substructuring it just states that we cannot treat it as a homogeneous unit, either statistically or mathematically, and that would be for computing frequencies also. He continues:

"The consequences of having smaller and substructuring in populations are that you can get deviations from the predictions of the Hardy-Weinberg equation, from the predictions of the product rule and so forth. That you would get perhaps an excess of homozygosity. That you would get gene frequency and bin frequency differences geographically. All of those could be a consequence of having substructuring in a population and they are very necessary to be aware of that possibility when studying human populations because as has been well documented human populations are not one homogeneous inter-breeding genetically uniform mixture like that. They indeed are made up of separate ethnic, geographic, socio-economic geographic units within which sometimes there is not complete random mating.

Question by Mr. Walsh:

"You accept that that does in fact go on in human populations.

"A. It certainly does and it has been well documented.

Q. Both in Canada and United States you accept that.

A. Both in Canada and United States and certainly in Europe and Asia."

Dr. Carmody continues to state at Page 70, Volum VII

"A. It does not invalidate them because I have been able to show, for the Canadian database that we use for these calculations, that there was absolutely no evidence of what we call substructuring."

And he goes on at the bottom to state:

"...between the F.B.I. and the R.C.M.P. database ...it tells me that there is some substructuring present. That is, that you do find these differences in bin frequencies..."

At Page 78 -

THE COURT: A few minutes ago, Mr. Furlotte, you said that there was no evidence that people from Quebec were included in the R.C.M.P. data base, but I thought you were relying on the fact that there were people from Quebec in it through whom French ancestors would contribute genes and so?

MR. FURLOTTE: I was never - I don't know whe e you got that idea, My Lord.

THE COURT: What idea?

MR. FURLOTTE: That there was people from Quebec in the R.C.M.P. data base.

THE COURT: You say there isn't.

MR. FURLOTTE: I'm saying we have no proof that there is.

THE COURT: There's no proof, no.

MR. FURLOTTE: Just as the - I forget which expert witness -

THE COURT: But I thought your argument might be that there were people from Quebec in it to establish that there was French ancestry reflected which would change the frequency figures.

MR. FURLOTTE: My Lord, even if there was people from Quebec in the R.C.M.P. data base and the data base represents both the French and British and God knows what else, it would be irrelevant if there is a statistical significant difference between the French and the English because if the data base is representing two sub-populations which we might know of that there is two, it's not a proper representatio of either one because there cannot be any statistical significant differences in two loci. That would make it linkage disequilibrium and therefore you could not use the Hardy-Weinberg formula and the product rule to obtain your degree of frequencies, but since we don't even know whether there's any French for certain; heck, we don't even know if there's any English because they've been randomly selected and not purposely chosen. Could be everybody out in B.C., they were all French who gave blood that day, I don't know.

Again at Page 78 of Volume VII, Dr. Carmody on cross-examination, I asked:

"Q. Now, whether or not the differences in the number of different databases throughout North America and Europe. whether or not the ones in North America are substantial enough to prove substructure or linkage equilibrium, would it be safe to say that the data that you were using and which you formed your opinion on, that that in itself has not went to the general scientific community yet to establish whether or not your opinion is correct or whether Dr. Shields' opinion is correct?

A. That is true. It has not been published in peer reviewed - in the peer reviewed literature at the present time.

Q. So you would admit that Dr. Shields' opinion may in the end be accepted in the general scientific community rather than your own?

A. That is possible."

Continuing on cross-examination on Page 82 Dr. Shields (sic) states:

> "A. ...there may be some forensic background where you can say, well, this was an isolated community and the alternative person that committed this crime is very, very likely to have come from that general geographic area. You can never rule out somebody having flown in from Europe or something like that, but the more reasonable assumption is that it was likely to be a suspect from that area. It makes more sense to use a database from that particular local area where you have that kind of subdivision."

I think what Dr. Carmody is saying here, trying to explain here, is that it might be asking too much of forensics to get data bases from all over the world and run the comparisons through every data base that has been established throughout the world, that probably the one in their own country and own local area would be sufficient for forensic purposes.

Again at Page 83, Volume VII, Dr. Carmody on cross-examination states, and this is in relation to the data he had from France:

> "...but then I only had two out of the five loci and if I had the other information and carried it through, it could well be insignificantly different from the Canadian sample even based on the French data."

Now, he says although it could be insignificantly different, but there is again no known evidence before this Court that it is not significantly different, and in fact, the data that is before this Court and before the scientists is that there's an extremely good chance that it is significantly different. Continuing from Dr. Carmody in Volume VII

at Page 88, I questioned Dr. Carmody:

> "Q. If you were doing your studies in a population to find out if there was linkage disequilibrium -- if I take my time with it I am okay -- what might be the first indication that you would come across for you to suspect that, gee, maybe there is?
>
> A. Well, the very first thing and the simplest thing to test would be these bin frequencies. If the bin frequencies were the same in the two places and there was no evidence of heterogeneity that way or population substructuring that way, I would say, well, we could still look for disequilibrium but we are less likely to find it. If you found some differences in gene frequencies and bin frequencies in the two places then you would say, hey, maybe we should pursue this further and maybe there will be some disequilibrium. Maybe there will be some deviations from Hardy-Weinberg equilibrium.

And this is a statement by Dr. Carmody, and then I put the question:

> "Q. Like you did with the Canadian Indians. That situation.
>
> A. That's right. In the case of the Caucasian populations we found no evidence of that when we compared the bin frequencies so we therefore said, well, -- and we are still going to pursue and look for linkage disequilibrium in those because we need bigger samples if we are going to continue to look for that. In the preliminary test that I did, which I admitted were not terribly strong, there was no evidence of strong disequilibrium being present. There might be some weak linkage disequilibrium, but it was below our power to resolve and see it."

And this is Dr. Carmody's answer in relation to his questioning or comparing the Caucasian populations.

At Page 96 of Volume VII I put the question to Dr. Carmody:

> "Q. Dr. Waye I believe also testified that it wouldn't have mattered even if you use the databases contained in England or in Europe, but now that you have received this information

about France you would have your doubts about that?

A. I would want to look at the data, yes. I would want to look at the data and I wouldn't feel safe in saying that it wouldn't make any difference. I would want to look at the data."

Continuing at Page 97, Volume VII, I put the question to Dr. Carmody:

"Q. From what you have found from France you might tend to disagree with that would you?"

I'm sorry, maybe I better go back. I stated:

"I will give Mr. Walsh that. I am not sure he stated as such, but where Dr. Waye stated that there was no significant difference in the allele frequencies between Caucasians in North America or Caucasians in England or Europe.

Q. From what you have found from France you might tend to disagree with that would you?

A. I would tend to disagree with that. I think that -- that is what I had heard up until about a month ago when I was given some data and when I was given Shields' information, that I actually did the tests, and my understanding up to that point was that there was no difference. I believe now that if you looked at these five loci some of them would show differences in different national populations."

Page 108 of Volume VII, I questioned Dr. Carmody:

"Q. But when you scientists in the field of population genetics are looking for a substructure it is almost on comparison with a public opinion poll in relation to say elections, how they are going to go. You think there might be some small community out there that is off the norm.

A. There is the same concern taken in terms of aggregating samples. That one has to be sure to try and pick up all of the local heterogeneity or substructure that might potentially be there and so you want to construct your sampling design -- when you sample any community that you try to draw statistical inferences about in

such a way that you feel it is as representative of the entity you are trying to describe as you possibly can get it. That means in the case of Canada, for example in this case, you want to get them as widespread geographically as you can and you want to be able to convince yourself statistically that there is no significant difference from one region of the country to the next.

Q. I will be getting on this later on but I believe there are some scientists who believe that population genetic study for the purpose of forensics that they should be doing population genetic studies for each area of the country.

A. That's correct. There are people -- and I would not say that that is incorrect. Certainly there is evidence in the aboriginal population in Canada, there is evidence in black populations, Hispanic populations in the United States, that there are very many differences from one local area to another local area."

He continues on:

"A. Within blacks and within Hispanics. In the case of Hispanics they are Caucasian. They are classified as Caucasian biologically."

I would submit, My Lord, that once tests are conducted you might find out that descendants from France would be like the Hispanics, they will need their own data base. He continues:

"It is strictly a linguistic category to put them into that category, Hispanics, obviously. There are significant differences and so it is natural, and I would support any proposal, to do further studies on a local scale, just to make absolutely and nail-down tight, the fact of whether there is local enough variation that we have to worry about the forensic implications or whether there isn't. In being a scientist I want to see the evidence. I don't like just going by what people's feelings are."

At Page 140 of Volume VII I questioned:

"Q. Did I understand you to say that the frequency of homozygotes in a database, Caucasians, would be roughly ten per cent?

A. That is the numbers for some loci and some loci it is even less than that, but it is on that order. There have been a few cases where you get higher than that. I think perhaps D17 might be higher than that. I don't remember exactly but it is on that order."

On Page 8 of Volume VIII, Dr. Carmody, I questioned him on cross:

"Q. Doctor, to get back to Exhibit VD65, I see like for the Canadian data base and figures you used the 99% upper confidence interval?

A. Yes.

Q. The R.C.M.P. does not normally use that, do they?

A. No, they don't. They don't."

And at the top of Page 9 of Volume VIII, Dr. Carmody:

"A. They may in future if I have any -",

and I guess I cut him off, but I assume he was going to say if he had anything to do with it they were going to use it. At Line 20 of Page 9, Volume VIII Dr. Carmody states:

"...unfortunately mathematically we don't have a good way of expressing that imprecision without using something that you call a standard error or a 99% confidence level or some other equivalent technique.

Q. I understand too that because there are some experts out there in the fields that they feel because of the large size of the matching window of the FBI and the R.C.M.P. that probably a better figure would be to use the 95% upper confidence level?

A. Possibly. I'd say that that almost comes down to a question of taste. I feel that I like the 99%."

But yet I point out on Page 109 of Volume VII Dr. Carmody says, "Being a scientist I want to see the evidence. I don't like just going by what people's feelings are", so I would submit, My Lord, that

whether it's proper to use a 99% upper confidence level or a 95% upper confidence, or anything in between, it's a pure question of feeling and not of scientific statistical importance. In discussing the effects of substructure with Dr. Carmody in Volume VIII, Page 35:

"Q. But you would admit that the scientists who excel in their fields of population genetics like Dr. Hartl, Dr. Landers, Dr. Lewontin, they would rank maybe the top three, would they not?

A. No, I wouldn't say that because I would say an equal number of illustrious outstanding population geneticists who feel quite comfortable with it.."

Again, "who feel comfortable with it", not who have statistically found that it is a valid proposition.

"Q. How do you think this issue should be properly resolved?

A. I think the scientific issue needs to be resolved by further experimentation, further work, further gathering of data, and by people actually designing new statistical approaches that heretofore have not been applied to data like this.

Q. Would you say that a judge or a jury of 12 common people are poor people to resolve this issue?

A. I would say a jury, certainly."

Still in Volume VIII, Page 82, question to Dr. Carmody:

"Q. And if Mr. Legere was compared to, say, ten or twenty people in the community and that same frequency kept occurring, would that kind of evidence suggest that we might be dealing with a substructuring?

A. If the people that you were comparing were all from the same family or were brothers and sisters it wouldn't necessarily mean that, but if they were randomly drawn from the community it would make me wonder about it, yes."

Page 86 of Volume VIII:

"Q. Now, if the sampling was taken from just one small Indian Reserve out on the West Coast and one small Indian Reserve in Northern Ontario you could possibly get even a greater significant difference?

A. You could possibly, yes."

And this is when I was questioning him about the Indian data base, that because out west they took it from a very, very broad area and in Northern Ontario they took it from a wide expanding area. If they just went to two separate tribes or reservations within that area these differences again may be much more significant.

"Q. And it would be improper to use the data base for one group when maybe the suspect comes from the other reserve?

A. That's right, and certainly as I expressed yesterday, if you had a suspect coming from still a third population that hadn't been sampled but you knew it was an aboriginal population I'd be very worried about which data base to use because I would not think it would be proper to even take an average of those two.

Q. Now, in the Caucasian population in Canada, because of those test results amongst Indians, wouldn't it be feasible and scientifically acceptable and necessary in order to show that we don't have a problem in the Caucasian data base or amongst Caucasians like we do amongst the Indians that maybe a sampling should be taken from some small isolated community amongst the Caucasians?

A. I would say that yes, I would support the idea that it would be good to have that information.

Q. And it's quite possible that if you conducted that test that you would find that if you did a small community say in Eastern Canada and a small community in Western Canada like you did with the Indians - or not yourself

personally but like was done with the Indians, that it's quite possible that we may end up with the same results as we had with the Indians?

A. I would say from just what I've seen so far with the Caucasian data base that it is very unlikely. I can't completely rule that out but I would say it's unlikely from virtue of the fact that not only do I now have information on the Canadian populations but I've seen some U.S. populations where there are some significant differences, statistically significant differences, at some of these probe lock between some populations at the bin level, the bin frequency level, but yet when you do the forensically relevant calculations they don't make any difference."

And again I point out he's saying forensically relevant calculations, which I will be arguing in the end is simply based on a matter of feeling, and not scientific feeling.

"Q. Yes, but let's try to stay out of the forensic field again.

A. All right.

Q. Because the forensic field as I understand is borrowing their theory and the product rule from the general scientific community?

A. That's correct.

Q. So I think we should stick with their criteria, would that be proper, for validating or invalidating the Hardy-Weinberg?

A. Fine.

Q. That would be a proper assessment?

A. Yes.

Q. Do you have any idea how long it would take to - as I suggested, to form a data base on a small scientific community - or not a scientific community but a small community in Eastern Canada and a small community in Western Canada amongst the Caucasians?

A. I would guess that that could be done given the money and manpower in three months, four months..."

At Page 89 of Volume VIII Dr. Carmody states:

> "...and you can do statistical tests on on that regardless of the size",

and I'd like to refer here to - referring back to the size of the tests that Dr. Shields did in his examination and his evidence before the Court, but the test, although it was a small size the test could statistically be done.

> "...but the problem with doing rigorous tests for deviations from Hardy-Weinberg equilibrium or deviations from linkage equilibrium are requiring large sample sizes, larger than the typical sort of small communities that we're thinking of and sort of sampling, so it becomes a practical problem then and I would say yes, it would be - and I think it would be for population genetics purposes interesting to sample some small Caucasian communities like that to see how much the bin frequencies vary."

At Page 91 of Volume VIII:

> "Q. Yes, I understand, and it's like you used Kamloops and some small community in Newfoundland, but if we use Kamloops - say the suspect was from Kamloops and the crime was committed in Newcastle and you had the population data base from each area and they were significantly different like the Indians, there's no way you could draw any conclusion as to what probability factor you could put on it, could you?
>
> A. Not unless you had the data base from each of those communities that were relevant, that's right.

At Page 130 of Volume VIII:

> "Q. So the best a scientist can hope for, then, is to either form a working model and a hypothesis and prove to the scientific community not that it's absolute but that it's workable?
>
> A. Yes.
>
> Q. And if you - basically if you can convince the general scientific community that it is workable and it's probably reliable for the purposes that you want to use it for, then that is sufficient and then generally accepted in the scientific community, is that right?

A. That would be, I think, a fair statement of how science proceeds, yes.

Q. So it's basically the scientific community says, yeah, it's probably a good working model?

A. Yes.

Q And once you've reached that state, then it's up, I suppose, to other scientists, what, to come to their peers and review committees to prove that it's not workable?

A. To prove by evidence, by objectively obtained and objectively supported evidence, that in fact disproves that generally acceptable conclusion.

Q. So what I understand to be happening in the field of forensic evidence here in relation to DNA analysis is that the forensic scientists are going to the general community and they're saying, look, we have a system here that's probably tenable and it's workable for our purpose and it suits our purpose for what we want to establish in court; now you prove we're wrong. Is that basically what's going on?

A. Well, I think there is evidence in support of the position of the forensic community, and they're saying that, show us evidence that we are wrong, so I guess I would agree with your conclusion.

Q. So in here basically what the scientific community does, they've put a reverse onus on the general community or the general scientific community?

A. Well, I'd say I'd characterize the state at present in the scientific community is that there are differences of opinion.

Q. And basically what it's boiled down to at this point - I guess I won't walk into that one - at the point is that the forensic field is stating to the general scientific community that, look, we have a working model and our product, the end result, our figures, tells us that it's reliable, now you prove that our theory is wrong; is that what they're doing?

A. I'd say that might be a slight caricature of it but I think basically that's the situation."

At Page 136 of Volume VIII:

"Q. Dr. Carmody, if in the Canadian Indians data bases you said that, you know, it's improper to use one or the other, but if you did use one rather than the other could you possibly convict an innocent person? Could that kind of information to a jury or a judge - could it possibly convict an innocent person?

A. Well, I think that the calculations that I've seen, even when you use in that extreme case of the native Indian populations, that could possibly change your net frequency of perhaps one in 50,000 to perhaps one in a million. If that degree of difference were going to make a difference of a conviction or non-conviction, then I would have a worry, I'm not - I don't really feel that it would. I mean my feeling is that even if you can show something - I'm expressing my opinion now - I think if you can show some forensic evidence that the probability of this match, getting a random match like this, is less than one in 10,000, to my mind that's low enough for me to call it beyond all reasonable doubt. Once you get up into the astronomical figures much greater than that it doesn't carry any more weight to me personally, and so my feeling is that once you've been able to establish that it's at least one in 10,000 -

Q. So you state that one in 10,000 would be beyond a reasonable doubt for yourself?

A. For myself. I mean, I would like to look - and the other thing about this is that I'm not aware of cases - there may well be them and I'm sure there have been some - where in fact it's only solely and exclusively the DNA evidence that is convicting somebody. It may carry a lot of weight in a particular case but I haven't seen instances where that is the only evidence that we have. I think it has to be corroborated by other evidence."

I would just like to bring to the Court's attention at this time that the evidence by Dr. Carmody, at least in my opinion, the Court would have to form it's own opinion, is that when Dr. Carmody is discussing that there's no forensic differences, no meaningful differences, he is basing that on his own feelings, as the other expert witnesses for the

Crown have, as to how much evidence it would take to convict a person or how much evidence it might take to convict a person. If a jury or a judge might convict on one in 10,000, one in 50,000, one in 2,000, then forensically there is no difference, and they are again expressing their own personal and subjective opinions without any scientific justification.

Volume VIII, Page 164, Dr. Carmody continues.

> "...so what they're pointing out here is that indeed, to have a precision in your bin frequency estimates where you need to have a precision pre-assigned and predetermined of one per cent, you would need these very large samples of 907; at least If you were willing to accept a 5% error margin in the frequency of the most frequent bin the prediction is that you would need 151 individuals, so in that context I think - I hope I haven't been too didactic here or whatever, but in this context it doesn't necessarily mean that if you had a sample as we had of 750 individuals that we couldn't make precise estimates. Our estimates are likely not to be within the one per cent range in each frequency but they're certainly within a 5% range of each frequency from this context."

And I expect it's based on those or that assessment of the R.C.M.P. data base that Dr. Carmody would use a 99% upper confidence interval, but again, it's what everyone feels comfortable with, according to Dr. Carmody. When I asked Dr. Carmody about the construction of data bases, Volume VIII, Page 174, Dr. Carmody says:

> "It may be that some of them are doing it right, it may be that none of them are doing it right."

Page 183, Volume VIII, Dr. Carmody, this was talking about Dr. Hartl's criticism of the FBI data base:

> "Q. And therefore that brought it down to a probability that the FBI was only right 16% of the time?
>
> A. Again I would -
>
> Q. That was his calculation?

A. That was his calculation. I think his calculation is right."

Page 203, Volume VIII:

"Q. So whether the scientific community - I assume if you're going to get together and decide whether or not it is valid to accept the Hardy-Weinberg formula statisticians as Dr. Geiser should be included in the panel?

A. And indeed they have.

Q. And this is an expertise that is being evolved?

A. Yes, it is, and in fact I can mention a number of statisticians around the world who are involved in this area. One of them in England is going to be visiting me in June, his name is Ian Eviett, and he's published extensively in this area and a highly well-regarded statistician.

Q. So you would admit that they would be a valuable asset in determining whether or not the Hardy-Weinberg formula and the product rule is valid in this case?

A. Yes, they are, and we would depend on their expertise quite heavily.

Q. And if a person such as Dr. Geiser was going to conclude that, you know, the computing these statistics the way the R.C.M.P. and the FBI does it is neither valid nor acceptable in the scientific community, then they should be listened to also?

A. Yes."

Bottom of Page 204, Volume VIII:

"Q. Dr. Carmody, would you agree that without the knowledge of the frequencies of certain alleles as represented by DNA fragment sizes in a population it is impossible to calculate the likelihood that a" -

I guess I didn't want him to answer. That finishes with Dr. Carmody's in this heading and it's 4:30, there's no way I can finish this afternoon, so whatever the Court's desire.

THE COURT: Any idea how long you would be?

MR. FURLOTTE: I would say I'm approximately halfway.

THE COURT: Well, we're talking about tomorrow. I mean we can't get through this afternoon regardless whether we leave it now or five o'clock, or later, so I think - what is the feeling of other counsel? Do you want to adjourn now and start again in the morning at nine-thirty?

MR. FURLOTTE: The Crown may be suggesting starting at nine. It doesn't make any difference to me.

THE COURT: Yes, well, nine o'clock is -

MR. FURLOTTE: I guess they're hoping I can finish by noon.

(ADJOURNED TO 9:00 a.m., JUNE 7, 1991.)

COURT RESUMES 9:30 A.M., JUNE 7, 1991

(Accused present in prisoner's dock.)

THE COURT: Now, Mr. Furlotte, you have something more to say.

MR. FURLOTTE: Yes, My Lord. My Lord I believe when I left off yesterday with Doctor Carmody I was discussing under the topic of the frequencies of binning and possibilities of substructures. This morning I wish to continue on with the evidence that I would like the Court to refer to in its deliberations now with Doctor Fourney which is direct evidence of Doctor Fourney in Volume 10, page 105. Doctor Fourney states:

> "I think it's important to recognize the fact that any DNA lab should have the population data base with respect to the area that it's going to do an analysis, so each country probably generates its own data base."

On cross-examination of Doctor Fourney, volume 10, page 162, I asked Doctor Fourney:

> "Q. What guarantees do you have that anybody from New Brunswick is in the R.C.M.P. population data base?
>
> A. None.
>
> Q. None whatsoever?
>
> A. But we have an equal guarantee that it doesn't exclude them, either.
>
> Q. No, right, that doesn't mean there's nobody there just because you don't know if there is.
>
> A. That's right, a negative conclusion is not worth much."

At page 166 of volume 10 Doctor Fourney states:

> "A. -- sadly enough, after talking to Leo Laverne at the Montreal lab which is actually compiling this type of statistics in a fairly large format,

> they have well over 500 samples now. We also are going to get an opportunity to look at those samples and, to date, from what I understand from George Carmody that the differences that are detected between the Caucasians and Leo Lavergne's data base and ours, in particular with D2S44, for instance was one of the probes you mentioned, are not truly significant."

I am just wondering why at that time Doctor Fourney did not address the other probes that the DNA labs were using. Why is he just saying that there is no particular difference for D2S44? If the Montreal lab had 500 samples to work with, that is under their own testimony a sample data base, and that data should have been made available to the Court and especially to the defence.

At page 58, volume 11, I was asking Doctor Fourney the possible difference between French and English, and Doctor Fourney said yes, this is with - what's his name - Leo Lavergne - Doctor Fourney says: "Yes, he's basically looking at a population base in the Quebec area and he's got a data base now from Montreal and I believe he's looking at other regions within Quebec." So it would appear that at least people in the Province of Quebec are doing it the way it has been suggested all along by most of the defence experts. It's not sufficient to get a data base for one big general area but in order to verify, I suppose, or validate that that data base is representative of a larger community then they must be conducting tests in smaller communities to make sure that there is no significant difference. The question of the defence is if Quebec can do it why can't the R.C.M.P.?

Again, in volume 11 at page 59 Doctor Fourney states:

> "I think the main rationale behind Mr. Lavergne's study would be the fact that there is one region in Quebec that he's particularly interested in north of Quebec City as a sort of a basis to draw a conclusion whether or not there's going to be any differences, say, between this region and the already established data base in Montreal."

At page 60, volume 11, I questioned him. I said:

> "Q. Does his preliminary findings show that there may be a difference between French and English Canadians?
>
> A. I'm trying to recall whether George Carmody might have looked at any of that data. He's been contacted and I think George found that with comparison in two probes, for instance, that Mr. Lavergne's data was very similar to ours in one region of the histogram, for instance, but there were a few bins that differed from the R.C.M.P. Caucasian data base and the differences seem to be possibly attributed to a technical problem that Mr. Lavergne had within his laboratory such that Mr. Lavergne is now repeating that data base and hopefully once it's repeated we'll be able to draw just and valid conclusions. At this stage, as I would suggest, that any data that we wished to compare with Mr. Lavergne would at best be very preliminary.
>
> Q. But it's possible we could end up with a situation like the Canadian Indians?
>
> A. Well, in reviewing the data from other labs, certainly in the Caucasian populations, we know that there are bin frequency differences but overall, I think forensically (and again he uses the word forensically) they'll have no significance."

Without the data from the Montreal data base and Quebec it's difficult to know what differences there are between the R.C.M.P. data base and the one in Montreal when Doctor Fourney, again, is talking

about forensic differences and not statistically significant differences.

Volume 11, page 111, cross-examination of Doctor Waye I was referring to VD-50 which was the Promega Paper and I questioned - I said:

"Q. I notice at page 150 of that paper, Table 4, the "Features of allele frequency population databases for a Caucasian population", and you have there the percentage of heterozygotes. Correct?

A. Yes.

Q. And what percentage is expected for heterozygotes, Caucasian data base?

A. Depends on the locus. Some of them are more polymorphic than others.

Q. Generally you expect it's about 90%?

A. Again, it would depend on the locus and how polymorphic it is."

And if you recall my previous questioning, Doctor Carmody, he said that the expected homozygotes would be about 10%.

"Q. Okay. The D1S7 was 89% heterozygotes?

A. Yes.

Q. And for the D2S44 it was 91%?

A. Yes.

Q. D4S139 it was 86%?

A. Yes.

Q. And the D16S85 there was 69%?

A. Yes.

Q. And for the D17S79 there was 68%?

A. Yes.

Q. And I see over on page 152 for your table 6 for your "Expected and observed frequencies of homozygotes and heterozygotes" and to find out whether or not they were in equilibrium, using the D1S7 you rejected equilibrium?"

And he stated:

"A. If you take this test as a test of equilibrium.

Q. Yes, which is what you were doing in this paper?

A. That's the way the table is arranged, yes."

Now, just as a point of interest, one of the tests which were submitted in the past as to whether or not there was Hardy-Weinberg equilibrium was one of these tests to see if there was the expected amount of heterozygotes in a population, and the expected is around 90% for each probe. And in this paper they found that four of their probes did not meet their expectations and in fact there was only one that met it, the D2S44, which was at 91% heterozygotes. Granted, two of them were just -- the D1S7 was 89% which was not much less but at least in their paper they said that it did not meet the test and it was invalid. Again, for the D4S139 it was a little under 90% at 86% and it was also found to be - not to meet the test and it was invalid. However, when you look at the D16S85 way down to 69%, and the D17 way down to 68%, that's a long way from the 90% expectancy.

The R.C.M.P., as I understand from the papers they presented, an exhibit, was they are trying to excuse that phenomena or data, as you want to call it, by the fact that well in their measuring system

it's hard to distinguish between two bands because two bands can exist but they are so close together that their system cannot distinguish them, therefore, that's an explanation. Just as some of the labs in the past have been using - and I will say the excuse - that well there was an overestimate of homozygotes in their population because their shorter bands were running off the gel. The R.C.M.P. on the other hand, their system, their short bands they state cannot run off the gel so therefore they are able to see all the bands. So they cannot use that excuse so therefore they use the reason that under certain circumstances they can't distinguish between two bands because they're so close together they appear to be one and through, I suppose, manipulating their exposed bands they can guess and some cases they can see well yes there is two bands but if you expose it a little longer then you just get the one band. And I believe in their documents that that was one of the tests they performed but while it explains some of the excess of homozygotes it didn't explain them all and, again, it was a very weak test, admitted in the paper. So I would submit My Lord that the R.C.M.P. has not proven that there is no excess of homozygotes in their general population data base.

Continuing at page 112 of volume 11 I questioned Doctor Waye:

> "Q. If it's not a test of equilibrium why would you put all this data in here and state which probes are accepted and which probes are rejected?

A. This was at that time a test that people were using as an indicator of equilibrium."

At volume 11, page 119, cross-examination of Doctor Waye, again, I was referring to VD-49B.

THE COURT: Is this Waye or Fourney?

MR. FURLOTTE: Doctor Waye. I was referring to VD-49B, the fixed-bin paper. 49B was the draft copy, I believe, of November, 1990 if I correct the date on that, which was submitted for peer review and later changed. I quoted a part from that paper.

"The application of the conventional formulation of the Hardy-Weinberg rule requires discrete alleles and no measurement imprecision."

"Is that correct?"

"A. That's what it says.

Q. And it says:

"Neither of these requirements exists for VNTR loci that are analyzed by agarose submarine gel electrophoresis and Southern blotting."

Is that correct?

A. Again, that's what it says.

Q. Do you agree with that?

A. That we don't have discrete alleles and that there is measurement imprecision?

Q. Yes.

A. I've been saying that all along.

Q. And you also agree that to apply the Hardy-Weinberg rule the Hardy-Weinberg rule requires discrete alleles and no measurement imprecision?

A. The Hardy-Weinberg principle has a lot of requirements tagged to it, none of which fit natural populations. It's a theoretical model. It doesn't fit any populations."

I'm not sure if I understand why they are using a theoretical model that doesn't fit any population but yet they see fit to use that theoretical model even though they realize it doesn't fit any population.

> "Q. But it would still require discrete alleles and no measurement imprecision before you could use the Hardy-Weinberg rule?
>
> A. If you follow the way those fellows wrote their paper and outlay their requirements at the beginning for an ideal situation I can't think of a population that would fit it, humans included."

And Doctor Waye was a co-author of that paper.

Volume 11, page 124, again continuing cross-examination of Doctor Waye I was referring to, again, VD-49B. I said:

> "Q. ... on page 29 of my copy of the draft copy dated November, 1990, and you will have to read it from here because I don't believe it's in the new one, it states:
>
> "Ultimately, it would be desirable to define alleles discretely to be correctly genotyping, not just phenotyping VNTR profiles, and to reduce measurement imprecision. Then it would be legitimate to apply the Hardy-Weinberg equilibrium."

Volume 11, page 183 on direct examination of Doctor Kidd Doctor Kidd states:

> "If one is dealing with Blacks or Caucasians one needs to define those because it's a well-known fact in human population genetics that allele frequencies, and hence bin frequencies, can vary among populations, so one should use a reasonably appropriate population."

At the bottom of the page he states:

> "... the Canadian white population or Caucasian population is of mixed European ancestry. It's a higher proportion of English ancestry than we have in the U.S.

> but it is a mixed European ancestry. So one would want some representation of that but that's almost going to happen automatically because the population is fairly randomly distributed in terms of any of the major groups."

Page 190 Doctor Hardy (sic) states:

> "... when Hardy-Weinberg is used and then the product rule is used you are always at every step using a frequency that you know to be an overestimate of the true frequency. You don't necessarily know how much of an overestimate but you know it's an overestimate ...."

So when the R.C.M.P. and the F.B.I. are claiming that their fixed-bin approach is conservative in favour of an accused person I guess myself at this point in time, and I'll say point in time, that I really can't disagree with that because I haven't found any reason yet to disagree with it and I would have to concede that my understanding and I believe most witnesses - expert witnesses - are of the understanding that it is conservative to a degree but, again, Doctor Kidd states you don't necessarily know how much of an overestimate it is. So there is no way of calculating how conservative the fixed-bin approach is, "but you know it's an overestimate and, therefore, the final number you get is designed to be an overestimate of the true frequency." But, again, that would be qualitative to the defence's point of position that the true frequency if it was a valid data base representing a homogeneous population.

Mr. Walsh questioned Doctor Kidd:

> "Q. What, Doctor, if any, conditions must be met or what assumptions must apply before the Hardy-Weinberg equation or the product rule can be used?"

I'm still on page 190.

> "A. There are a large number of formal assumptions underlying the Hardy-Weinberg rule. One of them is that there is no deviation from random mating. Clearly, we know non-random mating occurs for height, for amount of education, for socioeconomic status, but the Hardy-Weinberg rule says that kind of non-random mating is irrelevant unless this gene is related to those characteristics, and none of these genes ..."

Basically I stopped there. I don't have the other page. He's basically saying that none of these genes have anything to do with height or socio-economic status or any of the other things that he had mentioned. Basically, the defence would agree with that because the polymorphic sites that are being investigated have nothing to do with -- or at least not that we know of -- have nothing to do with the way we look or the way we act. But then, again, those are not the tests.

Volume 11, page 194, Doctor Kidd on direct states: "Statistical significance simply means that it is very likely that the difference is real ..."

Page 197, volume 11, Doctor Kidd states, in discussing about substructures, he says:

> "I am tempted to give a flippant answer that it's a red herring but I will try to be a little more specific.
>
> No human population has true random mating for all components.
>
> From a genetic point of view though, one has to say is any of this relevant to the genetic systems, the DNA variation being transmitted on the chromosomes. And for it to be relevant these differences have to be associated with different frequencies of some sort of alleles."

Page 200, volume 11, Doctor Kidd continues.

> "The frequencies will be different that one calculates because of these variations in allele frequencies but by and large they will all be small numbers. And I saw an affidavit by someone that talked about the difference between 1 in 50,000 and 1 in a 100,000. That's ridiculous. That sort of difference is not meaningful."

Again, Doctor Kidd here is not talking about statistical significant differences. He's just talking about meaningful differences in a forensic setting.

Page 14, volume 12, Doctor Kidd on direct when Mr. Walsh was asking him about the affidavit presented by Doctor Shields in the Vanderbogart case Doctor Kidd states:

> ""... I reiterate my earlier conclusion that a large number of population geneticists, working from both theoretical and human prospectives all agree that substructure must be investigated in order to validate the current FBI protocol for determining match probabilities." And my marginal comment was not possible. This is an argument that is being made, has been made in several cases in which I have testified, that one cannot assume there is no substructure, one must investigate it and demonstrate unequivocally that there is no substructure. And that is simply not possible in the human setting ..."

At page 15 Doctor Kidd continues to state:

> "The amount of information one would need to meet this standard that is being put up is simply horrendous and I simply reject the need to meet that level -- that standard. I have looked at a lot of data, I have examined a lot of human populations. It is impossible to say there is no substructure. What one can say is that there is no evidence of relevant substructure to the VNTR's as used in forensic settings."

The position of the defence, My Lord, on those comments is that in the end the Crown has to prove the basis for its experts' opinions and they cannot form their conclusions and their opinions on mere assumptions. They have to have proof that the basis for their opinions are in fact real. However, Doctor Kidd in these circumstances says that he admits there is substructure to a degree. He doesn't think there is significant substructure in a forensic setting because it would have no meaningful difference. He's rejecting the idea of the R.C.M.P. having to be put through the problem of proving that there is no substructure of meaningful difference and no significant difference because the chore would be too horrendous. I believe, as Doctor Carmody had testified, that to prove that there was no linkage disequilibrium that we take samples of about 50,000 people. The position of the defence is the mere fact that the job would be horrendous for these experts to prove the basis of their opinions that is not sufficient excuse in law not to do it and just to assume that it is there. We need the proof.

Still in volume 12, page 16, Doctor Kidd on direct discussing the Vanderbogart case states:

> "As the evidence already indicates the FBI reported that a random match to his genotype would occur with a chance of 1 in 51,744 using the C2(old) database. The new chance of a match is 1 in 102,934 using the FBI's C3 composite database and would be 1 in 200,107 using the R.C.M.P. database."

Again, using the three different data bases you come out with I would say substantially different figures which may or may not have a meaningful

difference to a jury and, again, which is not an indication of what the figures ought to be but just what they are when you are comparing that profile in the different data bases. It's not an indication of how much difference there ought to be but just how much difference there is using those data bases.

Again, at page 17 of volume 12 Doctor Kidd states:

> "The comment that I have then with respect to his calculations is that this is exactly the sort of variation I expect to find. It is part of the reason I like to see some sort of confidence intervals built into the reporting of these systems. None of those differences is significant and really meaningful in a forensic setting."

Again, Doctor Kidd is stating that he also would like to see the upper confidence intervals placed on these figures both probably by the R.C.M.P. and by the F.B.I. But the comment with respect to Doctor Kidd is that he states: "... this is exactly the sort of variation I expect to find." But that's something that Doctor Kidd, in my understanding, has never revealed to the court before, and with that now I understand why Doctor Kidd was not interested in reading the expert reports by Doctor Hartl or any of the experts who criticized his evidence in the Yee case. He wasn't interested in their reports because he knew they were right. He knew he could not object to their opinion because their opinions also were right. Doctor Kidd, if he's going to maintain that there is no meaningful difference in a forensic setting, then he can stand by those convictions til death do us part and there is nothing anybody can say about it but, again, those opinions that Doctor Kidd

is expressing are not opinions of a scientist. They are opinions of an ordinary man who is not basing those opinions on any scientific evidence. And when Doctor Kidd is testifying I suppose at the Yee case and especially in this case he's wearing two different hats.

Page 33, volume 12, on cross-examination of Doctor Kidd I asked him a question.

> "Q. It's quite possible in another year or two that even forensic labs will not be using this technology any more, that they will be using the PCR or going to discrete allele systems.
>
> A. It's entirely possible, there are many people working toward that with a variety of different techniques, simply to get around the problem presented by the absence of discrete alleles for these systems."

Now Doctor Kidd admits that there is a problem by not using the discrete allele systems and by using the quasi-continuous.

At page 49 I think we may get some ideas as to what Doctor Kidd's subjective feeling is on what is meaningful in a forensic setting. It says:

> "Three percent, I would consider chance is unlikely. Twenty percent chance is very likely."

That one chance in five is very likely.

> "But that's where in a trial jurors have to make their own decisions of what is meaningful. But by the time it was factored in, it turned out in my opinion to be not a meaningful difference."
>
> "Q. So as I understand, doctor, you in the case for Cellmark that you don't recall the name, you found maybe a difference between one in eight hundred million and one in two million but no significant difference?
>
> A. Correct, I'm sorry, no meaningful difference in a forensic application."

So we are distinguishing here what's the difference between a statistical significant difference and what's a meaningful difference in a forensic application. But as you go back up to the top of the page Doctor Kidd says three percent he would consider unlikely. "Twenty percent chance is very likely, but that's where in a trial jurors have to make their own decisions of what is meaningful.". So I would suspect that unless the defence can come into court when Doctor Kidd is a witness and prove that the chances are less than 20 percent Doctor Kidd is going to swear under oath that so long as there is a 20 percent chance that a person might be guilty that has a meaningul -- that would be meaningful in a forensic setting, and unless we could reduce the probabilities down to below that then he would support any kind of application by the R.C.M.P. or any kind of figures of probability that the R.C.M.P. would like to put before the Court.

I continued at page 49:

"Q. In a forensic application. As long as the one in two million would be sufficient to have meaning?

A. Correct, if the difference had been one in eight hundred million to one in ten, I would certainly say that was significant but one in two million is still a very rare event."

I would say that Doctor Kidd on this kind of testimony would be putting the onus on the accused to show that substructure would exist on that level, that it would have to reduce it down to 1 in 10. I would submit, My Lord, that that would be an extremely heavy onus on any accused person.

At volume 12, page 58, Doctor Kidd states:

> "... clearly allele frequencies at classical markers vary across European populations. It is undoubtedly going to be true that for some allele frequencies, even some bin there will be statistically significant difference between the bin frequency in Italians, say and Swedes. What I -- the reason I used the term red herring is because I have seen enough data to convince me that those differences will be numerically rather small and will be insignificant in the final conclusion that is reached from a multi-locus forensic application. These are not like conventional two allele systems that human population geneticists have dealt with for decades. All alleles are rare. It is not a situation where a frequency of an allele may go from five percent in one population to ninety percent in another. It may go from five percent to eight percent but not to ninety percent. And the situations that give rise to multi-locus disequilibrium require that there be large differences. So I don't -- I disagree with Dr. Lewontin's conclusion about the necessity of doing a lot more than what has already been done with these VNTR systems. I don't disagree at all with the premise that substructuring has been demonstrated with other genetic loci. That's clearly true."

Page 68 of volume 12 I questioned Doctor Kidd:

> "Did you see a need for having different data bases for the different races and ethnic groups?
>
> A. For the major races and ethnic groups, yes, I do.
>
> Q. Why would that be necessary?
>
> A. Because we know and have known for decades that the difference in gene frequencies between the major ethnic groups is far larger than the differences within the ethnic groups. The differences between any Caucasian and any African is greater than the differences found among Caucasians. And consequently, it is quite reasonable then to take account of this higher level of variation."

Page 70 of volume 12 he states:

> "... law enforcements have found it necessary to construct different data bases for the Blacks and the Whites, they must have found it necessary to find that they needed some degree of statistical difference between them to justify them or justify the necessity of different data bases. Now, do you know what that degree of differences is?
>
> A. It is my understanding that it was not a degree of statistical difference, it was a legal matter that it was legally important that there be different data bases."

Now, I submit, My Lord, that Doctor Kidd agreed that on page 68, volume 12, that it was necessary to have different data bases for the different major ethnic groups to come up with a reasonable and a reliable frequency calculation. I would also submit, My Lord, that under the test that Doctor Kidd and Doctor Carmody are submitting, if you run Mr. Legere's profile, if you run Mr. Vanderbogart's profile, or if you run anybody's profile through any data base that exists in this world you are going to come out with a frequency that would satisfy Doctor Kidd as being meaningful in a forensic setting which would be totally improper and unscientific because we would be just putting accused people up on a roulette wheel, taking a spin and saying take your pick, anyone is sufficient. It would be fixed and not fair.

At volume 12, page 74A, Doctor Kidd states:

> "I would not calculate a frequency, I would be tempted to calculate several different ones because of a lack of knowledge of which would be the most appropriate. There are actually two questions that are being confused at this point, and that is

> there are at least two different reasons for calculating a probability. One does not necessarily know the ethnic type of the criminal, the individual who left the forensic sample. Sometimes in a rape and the victim is alive there is an identification that it was a white man or a black man. But if the victim did not see the attacker or the victim can't testify, one doesn't know. There is then the suspect who is a different individual and there the ethnic identity is known. In one case one can calculate the probability of pattern observed which will only come up if there is a match, the probability of the pattern observed occurring by chance in the general population, if we don't know what the appropriate ethnic group of the criminal is, how common a pattern is this, the other is the probability of someone else in the ethnic group of the defendant, how likely is it that another person of the defendant's ethnic group has the same probability. And one does those calculations against different data bases ideally. So I know in some of the cases where the results are being reported, where the ethnic identity of the criminal is not known, rather than use only the ethnic identity of the defendant the calculations are reported if the criminal is Hispanic, if it's Black, if it's Caucasians, these are three probabilities that would be relevant. And then it's up to ultimately a jury to decide how to interpret those numbers."

In such cases in New Brunswick where the identity of an accused person is not known, and there's clear evidence that Indians differ substantially, both statistically and have meaningful differences, that Doctor Carmody has specified it wouldn't even be proper to use a data base from one group of Indians for an Indian from some other part of the country. New Brunswick and especially, I suppose, Newcastle, there are many Indians around there, we do not have a data base for Indians. We would not know what the probabilities of such a

match would be in an Indian population, especially in New Brunswick. We do not know what the probabilities would be for a data base of French Canadians. We do not know what the data base would be for English Canadians. All we have is a conglomerate of different individuals who may or may not be different and the indications are strong that there's going to be a big difference between French Canadians and English Canadians.

Volume 12, page 85, my question:

> "Doctor, what's the degree of probability that two siblings profiles might match?
>
> A. Twenty-five percent per locus.
>
> Q. Per locus, and how would that compare to somebody who wasn't related? We can't give a distinct figure but roughly?"

Page 86:

> "A. It depends on the system and the degree of discrimination but certainly one of these loci the numbers that were calculated that I saw per loci were on the order of one in fifty, one in seventy as opposed to one in four. So the probability of two unrelated people matching at a single locus is much lower than two full siblings."

I believe evidence showed by the defence that those probabilities - normal probabilities, existed much more at least in the Newcastle region and I suppose the Court expanded that to New Brunswick, but out of the evidence that we have given it seems that there would be - those figures do not hold true for the evidence that the defence has put forward.

Still on page 86:

"Q. If you were to assess, say, a group of samples who come from different people, maybe you would run an autorad of ten different people and you were to see these ten people sharing a lot of common bands, maybe the average of twenty-five percent, would you assume -- and using these probes, and using those probes, would you assume that maybe these people are related or would that be pure chance?

A. It certainly could happen by chance alone, if you've got a limited sample of ten people. Depending upon how many bands were shared, how few bands were represented, I would be -- the more bands shared among the people the more likely I would be to say, yes, it's more likely they're related, but it's a continuum of probabilities and any pattern is possible by chance alone, that's the nature of chance, any single pattern is extremely unlikely by chance alone.

Q. If you were to find a community who happened to show a lot of common bands, say, on the twenty-five percent level, would it be fair to assess somebody in that community with a general population data base, that maybe the FBI or the R.C.M.P. has?

A. It depends on the question you're asking because if you have no prior basis for saying the criminal comes from that small community, then it's by definition a small community, a very small part of the total population. So all of them are fairly rare. If you now want to say, here is an individual from that community, what's the probability that someone else in the community has the same band? Then you probably want the frequencies of that band in that very specific community, if you can show that they're different from the population at large.

Q. So it might be that that community ought to have their own population data base?

A. It might be, depending upon what it was."

Page 141, volume 12, still with Doctor Kidd.

"Q. Would you agree, doctor, that if one used a ninety-five percent upper confidence interval that in some cases the degree of probability could change from one in six million to one in one thousand?

A. I suppose it's possible, anything is possible, depending upon the particular data and that data base and the population frequencies, I -- of course, it's possible."

My Lord the defence would submit at this time that where Doctor Carmody testified that whether you use a 90% upper confidence interval - some prefer to use a 95% confidence interval, it's just a matter of feeling, there is no statistical way you can calculate which is the best to use, and whichever would be the best to use, as stated time and time again by the crown witnesses and the position of the R.C.M.P. and the F.B.I. is that they are always attempting to be conservative in favour of an accused person. That he should not be unduly prejudiced against. So any upper confidence interval should always be used in favour of an accused person, and those probabilities can change from one in six million to one in one thousand.

Page 143, volume 12.

"Q. Would you admit, Doctor, that the product rule cannot be applied to identifying characteristics unless a valid foundation is first laid for the probability assigned to each of the characteristics and unless mutual independence of each of the characteristics is established?

A. That sounds very good and I would generally agree to that except that I think what you are going to mean by some of the words in that statement will be different from what I would mean by them."

I believe, My Lord, I asked Doctor Kidd that question from what I found in VD-24 between pages 66 and 88 where I got that information.

Page 145, Doctor Kidd.

"Q. Would you agree, Doctor, that without the knowledge of frequencies of certain alleles as represented by DNA fragment sizes in a population it is impossible to calculate the likelihood that a match could arise simply by chance?

A. You can't calculate a probability without an estimate of the frequencies that go into the calculation."

My Lord as I have under item 10 of my own comments "Improper use of Hardy-Weinberg Formula and Product Rule", given the resolution limitation of its gel electrophoresis and the highly polymorphic nature of the VNTR's they employ, the R.C.M.P.'s system cannot distinguish where one allele begins and another ends. Unlike a discrete allele system, a quasi-continuous allele system cannot, in theory or practice, declare definitively that a known and unknown sample share the same discrete allele at a locus -- a real match. Nor can a quasi-continuous allele system identify a known and unknown sample as being the same length.

The R.C.M.P. claim that it is proper to use the Hardy-Weinberg formula and the product rule in calculating probabilities of a pattern because the use thereof is analogous to the use of it in calculating frequencies of blood types and protein markers within populations. Comparing DNA tests to traditional serology tests is like comparing apples and oranges. The major difference is that the

genetic markers used in traditional serology work are part of discrete allele systems which means that these systems have a limited number of distinct types. For example, in the ABO blood system everyone is either type A, B, O or AB; in the PGM protein system everyone can be typed through a finite combination of two of the following: +1, +2 or -1, -2. By contrast, the VNTR markers used by the R.C.M.P. are a continuous allele system which means that the system has lots of different alleles, rather than just a few. These alleles are difficult to tell apart and, more important, the number of alleles which exist for each loci is unknown.

The R.C.M.P. recognizes that its system is different in this respect from traditional discrete allele systems. Also, one of the published articles, VD-49A, by the R.C.M.P. recognizes the fact that its system is different from the system for which it was considered proper to use the Hardy-Weinberg formula and the product rule. The R.C.M.P. clearly recognizes the fact that it is not valid to use the Hardy-Weinberg formula and product rule, but on the other hand the R.C.M.P. assumes that the fixed bin approach is conservative enough to compensate for the invalid use of the Hardy-Weinberg. This is clearly a fallacious and invalid argument. The first principles are non-existent. The mathematical formulas cannot be used on either an open class of events or an unknown class of events.

The purpose of the fixed bin paper, VD-49A, is to describe and defend the novel procedures. It is these procedures which the defence has challenged as unreliable and inappropriate. It is these procedures which the Court must review under the FRYE test or any other test of reasonable reliability. The novel procedures used by the R.C.M.P. which are not used in traditional serology tests include the R.C.M.P.'s procedures for measurement of the position of bands, the use of matching rules, and the use of fixed bin analysis, as well as the assumption of Hardy-Weinberg equilibrium and the assumption of linkage equilibrium. The defence challenges the R.C.M.P. procedures for each of these innovations and assumptions.

The use of these novel procedures raises issues in this case which have never been raised with respect to other genetic tests. Concerns about measurement error and lack of precision in band measurement have never been raised with respect to traditional serology tests because for those tests precise measurement of bands on an agarose gel is not necessary. Similarly, concerns about matching rules, the need for upper confidence interval statistical estimates, and binning, which are central to the defence's challenge to the R.C.M.P.'s test, have never been issues with respect to traditional serological tests. The traditional tests do not employ these novel procedures.

It is not simple enough for the R.C.M.P. to now recognize that the fixed bin approach was not conservative enough to compensate for the invalid use of the Hardy-Weinberg formula and the product rule, and now propose that the use of an upper confidence interval will correct for its improper use. The evidence is clear that, in principle, it is improper to use the Hardy-Weinberg formula and product rule. It is improper to assume the conservative measures or correction factors proposed by the R.C.M.P. validate the use of a theory or proposition which is not supported by its first principles. Neither scientific assertions nor legal findings of fact are promoted upon mere assumptions.

The Crown's attempt to mix apples and oranges is also fatally flawed in a second respect. The Crown ignores the fact that the statistical procedures used in connection with traditional serology tests have received far more extensive scientific validation than the procedures used by the R.C.M.P. DNA analysis.

Courts have admitted statistics based on the product rule in connection with other genetic characteristics, such as ABO groups and PGM markers. However, the product rule may only be applied where certain foundational showings are made. I cite People V. Collins, 68 California (2d), 319; also reported at Volume 438, Pacific (2d), page 33; and also cite State V. Sneed, 76 N.M., 858; and also in Volume 414, Pacific (2d), page 858, a 1968 case and 1966 case respectively. Says specifically, and I quote:

> "The product rule cannot be applied to identifying characteristics unless a valid foundation is first laid for the probability assigned to each of the characteristics and unless the mutual independence of each of the characteristics is established."

Here I cite Jonakait titled "When Blood is Their Argument: Probabilities in Criminal Cases, Genetic Markers, and, once again, Bayes Theorem". That is cited 1983, University of Illinois, Law Review, page 369 at page 375.

> "Extensive research has been done to verify the frequency and independence of the genetic markers used in traditional serological tests."

Again, citing Jonakait at page 375-77.

> "This research has been published in peer review journals and is widely accepted by the scientific community. Thus, proponents of the evidence based on other genetic markers have passed the test which courts and the scientifc community have traditionally imposed before allowing the use of the product rule. They have proven that the markers to be multipled are accurately measured and independent. The proponents of DNA testing have not passed this test."

The Crown has not made the necessary showings that the VNTR markers used by the R.C.M.P. are statistically independent. The R.C.M.P. assumes these markers are independent because they assume that the Caucasian population is homogeneous and randomly mixing and is therefore in Hardy-Weinberg equilibrium and linkage equilibrium. But these assumptions are not generally accepted by the scientific community and have not even been tested in a scientifically appropriate manner. The Crown's position is that it is asking the Court to rely on all these assumptions because it would be too technically demanding upon them if it had to be proven.

In view of all the controversy, the defence's position is prove it.

In one respect, DNA evidence is no different from any other proffer of statistical evidence. To satisfy the threshold of reliability, the proponent must present a scientifically valid foundation for the assertion that the factors to be multiplied are independent. If a scientifically sound factual foundation for independence is not established, as it has not been in this case, then the statistical evidence is inadmissible. Use of the product rule is invalid as a matter of law and the ensuing probabilities are meaningless. And I believe I quoted that from Collins. Well, not quoted from Collins but basically you will get that information in the Collins case.

Not only does the R.C.M.P. use procedures which the scientific community does not accept, the Crown has clearly failed to meet the required showing that the R.C.M.P.'s statistical procedures are valid.

One cannot totally ignore the valid criticisms of the scientific community of population genetics, that a laboratory cannot compensate for unquantified substructure by taking conservative measures in the calculation of initial allele and genotype frequencies. One pertains to correlation; the other pertains to your estimate of individual facts, and you can't penalize yourself on bin frequencies to make up for a problem of estimates.

Mr. Walsh had submitted a pretrial brief on what the law may be in Canada - what a Canadian standard may end up, and on page 23 of that brief it states:

> "The evidentiary principles applied to this type of statistical evidence were fully addressed in Kansas V. Washington in relation to blood type characteristics as follows: expert testimony of mathematical probabilities that a certain combination of events will occur simultaneously is generally inadmissible when based on estimations rather than on established facts."

One of those cases he cites is State V. Sneed which I have also referred to.

He states, again, underlined, he says:

> "By contrast, population percentages on the possession of certain combinations of blood characteristics based on established facts are admissible as relevant to identification."

Again, just below the underlined portion, highlighted:

> "Attacks on validity of the underlying statistics go to weight of such evidence, not its admissibility."

But, My Lord, that is only whenever the Crown bases its statistical probabilities based on established facts and not on estimations.

My Lord I believe the Crown in this case, through its witnesses, Doctor Kidd and Doctor Carmody, the proponents of this type of evidence have finally admitted that there is need of upper confidence intervals because they cannot establish by facts that the frequencies would be one, in this case, seventy-eight, as listed on VD-65. By using the inner confidence interval they have to drop it down to well maybe it's one in fifty-six from somewhere up to one in one hundred and twenty-nine.

My Lord it's clear, I believe, finally, that what we are dealing with here are not established facts but rather estimates and, therefore, the first principles are now gone for the use of the Hardy-Weinberg formula and the product rule.

So to read again from the Crown's brief, at page 23 he says:

> "Expert testimony of mathematical probabilities that a certain combination of events will occur simultaneously is generally inadmissible when based on estimations."

What we are now dealing with in these cases are pure estimations.

Now, I will continue on with my own brief My Lord.

In science, the results of such testing are only accepted as reliable when others can reproduce them. As Sir Karl Popper, the distinguished philosopher of science has explained, and I will quote:

> "A scientist puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences he constructs hypotheses and tests them against experience by observations and experiment.
>
> What characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival.
>
> Only when certain events recur in accordance with rules or regularities, as in the case with repeatable experiments, can our observations be tested -- in principle -- by anyone. We do not take even our own observations, until we have repeated and tested them. Only

> by such repetitions can we convince ourselves that we are not dealing with a mere isolated "coincidence"."

That's from Karl Popper; the title of his book "The Logic of Scientific Discovery" at page 27, 42 and 45, printed in Harper Torchbook edition, 1965.

Also, and I quote again:

> "A scientist can come up with a hypothesis about the natural world through any process at all -- systematic study, inspired speculation, or fevered dreams, but that hypothesis must ultimately be subjected to controlled tests, reproducible by others. Only if the tests support the hypothesis can the hypothesis be accepted."

That comes from Goldberg titled "The Reluctant Embrace: Law and Science in America", and it is volume 75, Geo., which I would assume to be Georgia, Law Journal, page 1341, and that citation was at page 1342 - 43. That's the 1986 edition.

Back to my own comments, My Lord, science can only be considered reliable when there has been experimental validation that not only has been repeated but repeated by others. In view of the fact that the estimations of bin frequencies for the R.C.M.P. data base have not been repeated, no empirical tests have been performed to validate the hypothesis, and those tests performed by the defence tend to invalidate the hypothesis, the Crown experts should not be allowed to testify that the accused's DNA pattern either matches or is indistinguishable from evidence found at the scene. If this court should disagree, the crown's expert witnesses should certainly not be allowed to testify to any numerical number or other qualitative degree of probability that the pattern would or would not occur, as there

is no scientific agreement about how to find a proper number upon which there could be any reliable assessment.

My Lord it might be an appropriate time for a break because I have more parts of the transcript which I wish read in for the Court to consider.

THE COURT: All right, we will take 15 minutes.

(RECESS - 10:15 - 10:40 A.M.)

COURT RESUMES: (Accused present in prisoner's dock.)

MR. FURLOTTE: My Lord, again, I will read in some parts of the transcript that I would wish the Court to rely on. In volume 11, page 37, cross-examination of Doctor Fourney I put the question:

> "But Doctor as a scientist - and you are going on that model and that theory of the product rule - but if you are continually coming up - keep coming up with examples and circumstances which tends to prove your theory wrong, do you normally reject these circumstances and this empirical data coming in and close your eyes to it or should it cause you concern that you want to really study the issue first?
>
> A. You never prove a theory; you can only disprove a theory."

I submit, My Lord, that that would also go for the words 'any hypothesis or working model'.

Volume 7, page 66, Doctor Carmody on direct states:

> " ... -- the way this is stated is that if you were -- how large a sample would you typically need in order to find one of these would be 5.2 and it wouldn't mean that if I took one more in my sample that I would get of them."

I'm reading it.

> "The chance of getting another one would be 1 in 5.2 million so in fact the chance of getting two of them from two separate samples would in fact by the product of those. 1 in 5.2 million times 1 in 5.2 million."

This was on direct examination and a question from yourself. To Doctor Carmody from yourself My Lord, whether or not he would be multiplying those factors 5.2 million by 5.2 million, and he had agreed with you.

Again at page 143 of volume 7 under cross-examination I stated:

> "Q. I remember the judge asking you when you were doing your direct examination about one chance in five million. What did that mean? That only one chance in five million that there is somebody else out there with that band frequency, and he mentioned that the chance for there to be two people out there in five million then you would have to say the chance of two people coming together with that band frequency say in the population would be five million times five million and you would also multiply it by two again?
>
> A. No. In that case you don't multiply it by two. You are looking at the genotype frequencies and that is a frequency where you don't have any ambiguity about one being mother or father and so forth, and you just ask for a match on all five of those loci in that case and it would be one in five million times one in five million too.
>
> Q. So the chance of two people coming together at the same time and same place would be five million times five million?
>
> A. Yes."

Continuing with cross-examination of Doctor Carmody in volume 7, page 11, he's talking about rare possibilities and he said:

"And whether you come up with a number that says it is one in a million, one in 25 million, I think that that is telling me that it is a very rare occurrence that you are going to find somebody with that particular virtually unique genotype for those things we are able to get a snapshot of genetically.

Q. That is why you say there is no significant difference whenever -- depending on which database you use even though there is a significant difference in the bin frequencies, the end product there is no significant difference. So therefore it is valid.

A. In the cases that I have run that is true.

Q. The thing I have a problem about that with, Dr. Carmody, is are you sure you are not using the numbers to support the theory rather than using a valid scientific theory in order to obtain the numbers? Are you putting the cart before the horse?

A. Well, I would say that I am testing the theory in the sense that the theory says that there -- it makes a prediction. It says there won't be any differences. I say I am going to test that. I see if there are differences. If there are differences then it allows me to throw out the theory. But I find that there are no differences, so at the present time I accept the theory."

My Lord I would submit at this time that there is no way the scientists can test their theory if they only run the profile of one individual through their data base. There is no way you can possibly check for substructure by that method. The only possible way to check for substructure is to go out into the community and at least gain a number of individuals, check the commonness of their band frequencies together, and then compare that with your data base. Doctor Carmody states that if there are differences then it allows me to throw out the theory.

Volume 7, page 118, Doctor Carmody on cross-examination.

"Q. Okay. I would look at it ignorantly and I would probably say, well, it is the numbers that are influencing your decision rather than the principles upon which you use to justify the numbers.

A. Well, that is partly true but that is I think always the case. One has to make some decision based on empirical evidence, and I am using the empirical evidence to inform my decision rather than coming upon it in some abstract way from prior principles.

Q. As I noted in your direct examination when you were talking about the representative samples, you stated that if the sample represents accurately the actual population you are trying to make references about, so again you found at that time that you should have references about the actual population of which the person fits into.

A. Yes, but I am saying that after we look at the data it wouldn't make any difference now. After we have looked at the data."

Page 138, volume 7, I asked Doctor Carmody a question.

"Any way you can tell from the data base that -- how many bands I might share in the five probes that the R.C.M.P. use, how many bands I might share with any individual out there?

A. If I could look and if that were your --

Q. In unrelated individuals.

A. Yes. I could look through the data base and say how frequently each of those bands occur in the database. I could say that and I could give you that figure."

And if the Court recollects, that's when I was wanting to ask Doctor Carmody to compare the commonness one might expect of somebody sharing distinct bands of

any individual or any fragments that they had in their data base. And as I picked certain bands out - certain fragments of Mr. Legere's profile I got Doctor Carmody to calculate the probabilities of anybody fitting those particular fragments, sharing those particular fragments, and at page 69 of volume 8 it says:

> "Q. Now, so that's one chance in 5,475 - we'll leave off the .62 - that somebody out there has that same profile as Mr. Legere?
>
> A. That has that exact same profile, that has those exact bins that would fall into those - bands that would fall into those bins, yes."

And that was Doctor Carmody's answer. And I got Doctor Carmody to go through the calculations of those frequencies again as comparing it with, I believe it was at least with one of the Daughney sisters - I just forget offhand which one it was, and at page 71 he states:

> "We'll just square that, and it becomes one in 1.8 million.
>
> Q. So it's almost one in two million?
>
> A. Right.
>
> Q. Now, the point I was asking you yesterday was where you see on the probes that were run in this case - where you see kind of a matching pattern over five probes, again I suspected yesterday and I believe you said I was wrong, that there is a chance, or a good chance, that if you use another five probes you're going to get more of a matching pattern between these two individuals.
>
> A. Possibly."

So as I went through the exercise with Doctor Carmody to see how common it would be for somebody

to match - for two people to match on those particular band sharings between Mr. Legere and I believe it was with Donna, right offhand, but I may be wrong - it may have been Linda, and it came out to 1.8 million, and that was over the five probes that the FBI -- or the R.C.M.P. do use. But if we took another five probes that aren't in use yet and run them through these two individuals again it's possible that they are going to share more bands which would drive that frequency up that much higher.

The Court will recall I also went through that procedure with Mr. Murphy and the other Daughney sister.

The evidence of Doctor Carmody was that there was a lot of band sharing between the individuals - unrelated individuals such as Mr. Legere and one of the Daughney sisters, Mr. Murphy and one of the Daughney sisters, which appeared to be band sharing on a level higher than what was actually exposed by the comparison between the two sisters. And if you recall the evidence by Doctor Kidd was that there's only 25% chance of the band sharing between siblings and that usually for individuals not related it's 1 in 50 or 1 in 70, and I would submit, My Lord, that under the evidence provided to this court that the band sharing in the community, at least, of the Miramichi area or New Brunswick is much higher than what one would normally expect, or at least it's not as uncommon as what was expected by Doctor Kidd.

Also at page 151, volume 7, cross-examination of Doctor Carmody, I was questioning here about the hair samples.

"Q. And it is proven that they do come from somebody else so that puts both individuals there at the same time. What were the probabilities that both people were there at the same time?

A. And both left a separate hair sample.

Q. And both left --

A. And there were two hair samples found.

Q. Two distinct -- distinctly two different people. No question about it.

A. Well, in that case the random match for one is one in 4,500. The random match for the other is one in 4,500.

Q. Could you multiply that?

A. For the joint concurrence of those two, yes, I would think so."

On cross-examination of Doctor Bowen - I have to apologize, I don't have the volume number --

MR. WALSH: I think it's the same volume, Mr. Furlotte, volume 8, or 9. Doctor Bowen's direct is in volume - part of his direct is in volume 8; I believe his cross-examination is in volume 9.

MR. FURLOTTE: At page 71, and I will state volume 9, if you recall I had got Doctor Bowen to go through a similar exercise as I did with Doctor Carmody and I had picked out the most common band sharing with a person who was not from the Newcastle area, and that either the human control cell line or the female control which was rated as NM, I calculated as to which one of those most frequently shared the bands with any of the other individuals from the Newcastle

area, that the two suspects and three victims, and at page 71 he answered:

"A. That would be one in five times one in four times one in nineteen times one in 1.9. Is one in 720.

Q. That would fall in the range of basically your data base -- the number in your data base. Be within -- you might be able to expect to be able to pick that frequency out within your data base.

A. It might be possible, yes."

Down further in the question I asked:

"Is that into your database also? That human control line?

A. I do not believe so, no.

Q. You don't believe so.

A. No.

Q. But that person is not from the Newcastle area?

A. Not that I am aware of, no.

Q. The person I compared that with who might share the same amount of bands is Linda Daughney, but I don't imagine you checked for any kind of comparisons of this rate have you?

A. No, I have not."

My Lord the purpose of that exercise was to show that picking somebody other than from the Newcastle area and comparing somebody else from the Newcastle area you are not going to get the same degree of band sharing that you did get from the people within the Newcastle area.

At page 106 - I assume still volume 9, question - and this again is to Doctor Bowen:

"Q. But that is not the issue. You must have had a number in mind when you said you have to have at least a minimum of -- or at least three probes.

A. I had no number in mind at that time. I am sorry. I beg to differ.

Q. Dr. Carmody testified that in his opinion -- and it is all subjective. You are entitled to yours too as maybe each member of the jury would be. He figured his would be one in 10,000 would be enough to convince him. What number would convince you?

A. I have never really thought of it in those terms. In the order of one in 10,000 is a reasonable number to me.

Q. So you too agree with Dr. Carmody that one in 10,000 would be enough to go to court and say that this is a rare or a very rare occurrence.

A. I would say --

Q. Proof beyond a reasonable doubt is basically what we are looking at.

A. I wouldn't say that with one in 10,000 personally. If I am going to say beyond a reasonable doubt I would say one in 100,000 or one in a million. In that range. I prefer to be a little more conservative in that respect."

Again, My Lord, when we are dealing with numbers on this level, and subjective opinions, this is what the expert witnesses have been testifying all too long as to what they consider to be no meaningful difference. If they can come up with a number that would convince them that it might be good enough to bring to court to put before a jury, and that is what they're doing, and again my comment at this time is that this no meaningful difference that these experts are referring to is totally subjective, it's not a scientific opinion, and therefore that opinion in fact should not be admissible in court.

On direct examination of Doctor Fourney, Volume 10, page 96, Doctor Fourney was explaining - he states:

> "Essentially, we took out five individuals."

And he's talking about the data base now when they rebinned it.

> "After doing the bin frequency and analysis of this data base, we became aware in January that there was a possibility of duplication within the data base, and this could be for several reasons, one of which could be that individuals contributing to the data base that would be donating blood at the time may have donated twice. It could also be possible, and it certainly became evident from talking to members of the Red Cross, that there are also identical twins that often give blood, and we could essentially in there have two individuals giving blood but essentially give the same DNA typing pattern. So in order to be very conservative, we removed these individuals from this data base."

I would, again, question the validity of their ability to do that and in their statement of saying being conservative as when they're testing their data base for empirical evidence that they do have a valid data base. They are supposed to be testing for occurrences as this to invalidate their data base rather than to find something that would be contrary to their hypothesis and working model and then just throw it out without justification.

As Doctor Fourney stated at page 100 of volume 10, he says about the collection of these samples:

> "It should be completely understood that these samples are all anonymous and we have no way of really retracing the identity of these samples."

So there is no possible way that they could retrace to find out whether these samples actually come from twins, actually come from two different people. As a matter of fact we don't even know if they even come from the same collection, whether they come from the Ottawa collection, the Kingston collection or the Vancouver collection.

Also at page 101 Doctor Fourney states, under a question by Mr. Walsh: "What would you ask the Red Cross for? What are you specifically requesting of them?" Answer: "We would specifically ask for no duplication.".

So if their control procedure was to get no duplications in their data base and they ended up with duplications, then it's just as wrong to assume that they got duplications as it is to assume that they have legitimate samples from two different people.

On cross-examination of Doctor Fourney, volume 11, page 31:

> "Q. And how did you know they were duplicates?"

And I'm talking now about duplicate matches, not duplicates from the same person.

> "A. We have a program that the FBI has been working on called 'Dysmatch" and it's designed to look at large data base rays and compare literally the bin frequencies ..."

I believe, as the court will remember, I questioned Doctor Fourney at that time about the possibility of going into their computer and finding out -- into their data base and find out exactly how many people shared two probes, how many shared three

probes and so on, and what would those frequencies be and then check that with their data base to see if the frequencies were relative to it. He said yes, those tests could be done but it would take time. He couldn't do it right away but it would take a little bit of time. I would submit, My Lord, without any evidence of those tests having been done - without the results of what those tests would possibly be, then this Court is not in a position to accept the mere opinion that the empirical evidence does support -- or in their case -- would support the validity of their data base, and validity of the use of the Hardy-Weinberg formula and product rule.

Still in volume 11, page 41, cross-examination of Doctor Fourney:

"Q. Doctor, if the chances of something occurring was 1 in a million out of a certain number of events, it was 1 in a million, but maybe daily when there's only a thousand events occurring of the possible million, and out of that thousand events that are occurring one is coming up, you know, almost every day, would that cause you concern?

A. It would be like suggesting that you had a coin and you are going to flip it ten times. You would expect to get heads and tails; five times for heads, five times for tails, but you may in fact get seven heads and three tails.

Q. Right. But if every day you ended up getting nine heads and one tail --

A. I would suggest you would have a loaded coin.

Q. And there would be something wrong with using that model?

A. There is nothing wrong with the model; there is something wrong with that coin."

Again, My Lord, when we're comparing a small geographical area against a data base by the R.C.M.P., it wouldn't matter whose data base it is, and I would in a sense have to agree with Doctor Fourney that there is nothing wrong with the model. The model is an abstract that has been put forward, to which the admission of Doctor Waye is that the Hardy-Weinberg - you just can't apply it to populations but they are applying it to populations anyway, the model is great if the community that they are testing is homogeneous and there is no substructure. The mere fact that the coin may not be coming up as predicted does not destroy the model because it's a theory. You don't prove theories and I guess in a sense you're not disproving the theory, but here where you're comparing the model to the actual empirical data that is being tested against the model, we are finding out that there is something in Doctor Fourney's analogy here that there is something wrong with the coin and that the problem with that coin is that that coin is substructured. It's simply that the model and the empirical data don't fit. There is no reliable match between the two.

I am into Doctor Shields' testimony here now. In Doctor Shields testimony we introduced into evidence VD-121 where he had made comparisons of a match profile in this case between the R.C.M.P. data base, the F.B.I. data base, and a frequency calculation or a paper put forward by Nichols and Balding, to show how the differences might vary. When Doctor Shields put this in he was not putting this paper in - and I

believe the Crown may have misconstrued the purpose - the purpose was not for the defence to argue that we should be using Nichols and Balding. The purpose was to show that different scientists in the field of population genetics and who know something about this are suggesting that this may be a different way to correct for substructure. Nichols and Balding has not been validated or peer-reviewed to any great degree. It has not received the scrutiny in the scientific community that it deserves any more than the procedure by the F.B.I. or the R.C.M.P. We are not relying on Nichols and Balding as being reasonably reliable or reliable to any degree. We are just showing that there are people out there who are trying and working to overcome the problems, as noted by Doctor Kidd, with using continuous allele systems.

Again, if you were to use as the probability figures that were used in here, even if you were to use Nichols and Balding, which I believe Mr. Walsh was saying this is the best case scenario for the defence, which I submit My Lord this is far from the best case scenario for the defence; the best case scenario for the defence, as Doctor Shields stated in his testimony, was the actual figurations of the band sharing that he was able to do with the people in the Miramichi area; these figures that are here also need a correction factor, an upper limit interval correction factor, whether we are using a 95 or a 99 or a 99.7 or a 99.9, whatever. There is no way of figuring out what these figures actually ought to be. You just can't calculate that. The upper

confidence levels are a matter of feeling what you should use based on the fact that all you can come up with is estimates.

In answer to the criticisms of Doctor Carmody and Doctor Kidd of Doctor Shields' affidavit about a person taking out of his own sub-population that he would always be biased by his being figured into a data base which he did not actually belong, at page 40, volume 13, Doctor Shields states:

> "It also illustrates one of the statements that I've made that was criticized in earlier testimony at this particular matter, that you can expect that if you take an individual out of his sub-population and test him in another sub-population that that individual will be biased against by doing that form of analysis. There might have been a misunderstanding of what I mean by sub-populations but if we're talking about true sub-populations there is going to be a loss of alleles, O.K., there are a number of different things that happen with sub-populations. You don't expect each sub-population to carry as many alleles as the entire what we call meta-population does. When that happens, when there are fewer alleles within a sub-population, the frequencies of the common alleles automatically go up. Because they automatically go up as they do here in this population the probability of matching at that particular locus with that particular allele is very high for an individual from this population, much higher for an individual in this population than if we were to test it in either of the other two populations where there are more alleles, and that's the kind of bias that I was talking about in an affidavit that I provided in a different trial."

Here, My Lord, if, as the R.C.M.P. have done, they have taken what they assume to be the population data base from the general population of Canada getting samples from people all over the country, if there were not two sub-populations - but if there were 10 or 12 sub-populations out there statistically

different, by putting everybody into one you are taking everybody out of their own where they are all more common and putting them into one, and that is why no matter which one they come from you are going to compare them with a common one which is not a true representative of everybody, then everybody is going to be prejudiced by being compared with that common data base and bin frequencies.

It is not simple fact that Doctor Carmody was not saying that there are only -- just because he compared the F.B.I. to the R.C.M.P. he's not saying there are only two substructures which make up one North America. That was just a test to show that there is substructure within the Caucasian population. It doesn't say how many substructures there are and there is no way you can tell as to what degree the substructure actually is, but by taking an individual out of the structure to which he belongs and putting him into such a calculation then he is going to be biased against because you are comparing him with people who don't share the same band frequencies that his local community and geographic community does share, or ethnic group or whatever.

He continues on page 41.

"Usually, if you take an individual and test them in a data base other than their own sub-population the probability that results will be biased against that individual and the reason why is because if you're talking about true sub-populations that are sufficiently isolated from one another there are going to be higher frequencies of all of the common alleles in a sub-population than there would be in the general population using a mixed population or in a different sub-population. They'll have high

frequencies at different alleles
in the two sub-populations."

Page 44 Doctor Shields states:

"I also made the statement, for example, in the Bourguignon case, that I thought that there might be significant differences between the French-speaking Canadians and the English-speaking Canadians. I was aware of no data at the time that would indicate whether that was or was not true, but you did show me the piece of paper that Dr. Carmody produced in evidence.

Q. I believe VD-65, and I'll show it to you again, Doctor.

A. Thank you. Yes, it is VD-65, and I would just note that there's a line for France in which the frequencies of alleles at two of the loci that are currently being used in forensic cases are presented and that the frequencies there are larger for the two alleles that are involved in the Legere case and they do not fall within the Canadian 99% confidence interval, and under those circumstances the French-speaking Canadians who are descendants of the French may indeed have significantly different allele frequencies no matter how you decide to talk about significant differences in allele frequencies..."

At page 45 he continues.

"... and in fact if there's sub-structure what might happen is depending upon which sub-population you sample you'll end up with the same size confidence limits but you'll have a different point estimate in the middle so it does not take into account substructure, confidence intervals do not." (Take into account substructure.)

At page 47:

"Q. And would you tell the Court what you did and what the significance is?"

And this is about the case specific evidence.

"A. What I did is I looked at the autorads and I looked and said how many bands match between individuals who are not related, recognizing that, for example, Donna and Linda as they're listed in the evidence are sisters. Not using that as a comparison but simply comparing unrelated individuals, Murphy with Linda, Mr. Legere with Murphy, Mr. Legere with Donna, and Mr. Legere with Nina, who I presume are all unrelated pairs. One can use all of the assumptions of the R.C.M.P.'s analysis and production of probabilities to determine the probability that the individuals in question could match at as many bands as they do, and if the R.C.M.P. data base and the frequencies it produces are the right frequencies, then we would have some estimate of how likely it is that all of these could match, so for example, Murphy with Linda --

Q. Now, when you say match, Doctor, are you talking bands match or just that bands fall in the same frequency bin?

A. No, I looked at these autorads last night and these are matches. These are what I would call explicit matches. Visually they are at the same place, looking at all the sizings after the visual match --

Q. So it's not just that they fall in the same bin?

A. No, these to me are the same alleles. They migrate - I cannot distinguish them from -- I cannot distinguish that they are different alleles, either visually or through the computer analysis, and under those circumstances, for example, Murphy shared four bands with Linda, Legere shared four bands with Murphy - not the same bands, by the way, they're different bands for the different pairs - Legere shared four bands with Donna and Legere shared two bands with Nina, and you can develop the probabilities of those sharing patterns if the R.C.M.P.'s frequencies as produced in their data base are truly representative of the frequencies of those bands in that population of interest, the New Brunswick individuals who are involved in this particular case as either victims or suspects, and under those circumstances, for

> example, the probability of Linda's matching Murphy at four bands is one in 10,807 ..."

And maybe I would just point at that time, My Lord, before I continue, when I had Doctor Carmody do the tests I had him do them as the ones who shared the bin frequencies and there were in some of them sharing five bands because there was one bin under one thousand and some everybody was put into the same bin, and Doctor Shields did not take that into consideration when he did his because there, although they shared the same bin, they weren't visual matches. And, also, as the R.C.M.P. is claiming that their binning frequencies and methods is very conservative. Also, that applies for this case. The ultimate figures reached by Doctor Shields again are as conservative as the R.C.M.P.'s.

I'll continue.

> "... and the probability of Legere matching Murphy at four bands, the four bands explicitly that they match at, is one in 2,749, and the probability of Legere matching Donna at the four bands at which they match is one in 5,616, and the probability of Legere matching Nina at two bands, in this case it was a genotype match as well, is one in nine. Now, what that means is that those probabilities are supposedly independent samples from the general population data base. What's the probability of an individual sharing four bands if this is the distribution of alleles, explicitly these four bands, and in the next comparison if we draw two individuals at random from a data base, from a general population, what's the probability that they will share, and it's those probabilities, and if those are statistically independent events the probability of all four of those events happening,

> Murphy sharing four with Linda, Legere four with Murphy, Legere four with Donna, and Legere two with Nina, would be one chance in 1,496,600,000,000," --

so you may as well say for argument sake 1.5 trillion, and that is being conservative according to the R.C.M.P. --

> "which implies to me that there's probably structure in the New Brunswick population, and that the frequencies that are used are not perfectly correct."

He continues on to state:

> "Now, one can play, O.K., one can say that Murphy matches Linda, and then I have Legere match Murphy, and that's not independent, unless all of those bands are different, O.K.? One can do all of that and you still - you can get it down to two bands matching, three bands matching independent, two bands matching independent, and you still have a probability of all four of those events happening that's well above one in a hundred million. In other words, it's supposedly impossible if that's the true data base. What that says to me is that there's background band sharing, there is background relatedness among Murphy and Linda and Legere and Donna and Nina and the other suspects who I finally saw the sizings last night and there's lots of band sharing there as well,..."

And when Doctor Shields mentions about the sizings he finally saw last night that was the sizings of the other five suspects that the gel was run on which was the gel number 89-0-L-11-91-7, and I had got Doctor Bowen to do the sizings for me because I wanted Doctor Shields to prepare the same type of comparisons amongst the ten rather than just the five people known from the Miramichi area, but the sizings only came back May 23rd, 1991 and I had just

got them on the Friday before Doctor Shields testified and he didn't have time to go through the binning data of the R.C.M.P. to make those comparisons but he did a visual comparison and he did find that there was a lot of band sharing.

I will continue.

> "... I finally saw the sizings last night and there's a lot of band sharing there as well, and I did the analysis on this set of individuals and it came up to be that there was approximately 3.3 bands shared out of a dozen, so I used all six probes, which is good, it's making that band sharing as small as you can given that population, and if you do that, if you make the background relatedness represent what's done in terms of the band sharing that you observe in this sample, then the probability of a two-locus match becomes one in 44; the probability of a 4-locus match becomes one in 1,910; the probability of a 5-locus match becomes one in 12,633. Those numbers are very, very much larger than any of the numbers using the standard techniques. This is a standard technique, however, just as that is, when you know that there's background relatedness. It is the standard technique used, for example, by Cellmark and the British when they're using multilocus probes. They always have to calculate in the background level of band sharing in order to determine the weight of how many bands are actually shared in a particular case.

Q. Now, would this be an indication that there would be inbreeding, or just an indication that you couldn't use the general data base that the R.C.M.P. are using?

A. I personally would take it as evidence that there's structure and that the New Brunswick population that these people are sampled from is genetically different from the general Caucasian population represented as the R.C.M.P. data base. They're genetically different probably because of the generic sense of inbreeding, that they have a different pattern of co-

> ancestry, that they have a higher probability of mating among themselves than they do of having individuals come from outside their group and mating with them. They're not a random mating group, they're a sub-population - part of a sub-population."

At page 52 Doctor Shields states:

> "A larger probability means more likely, it would get closer to one in - well, the probability even if there's no background band sharing of two siblings having identical bands at four loci is one in 256, O.K., so if you were to take 256 pairs of brothers, one of those pairs would likely be identical at four loci. Now, if you had background band sharing as well as sibling relatedness, instead of one in 256 it would get larger, it would become 1 in 128. That's - please put in the record that I'm doing that out of my head and it's not a number that I would like to say is exact."

It's close and it shows the comparison that even if you have that common band sharing among unrelated individuals that it's even much greater to have band sharing between siblings than which would normally be expected.

> "Q. So when you quoted the figures of one in 12,000 of sharing five loci that that again would drop down relatively to maybe one in 6,000 or -
>
> A. No, no, no. A brother would be one in 256 even if there were no band sharing, O.K.? It's two different questions."

And Doctor Shields discussing the fixed bin method states at page 56:

> "... fixed bin is conservative with respect to defendants, the fixed bin paper and others in testimony have claimed that it's sufficiently conservative to make up for the problems of substructure, and he

> explicitly noted in this paper that accompanies that that's still what we would call a scientist's handwaving, ..."

That the conservativeness is sufficient to make up for substructure.

> "... that you can't know whether it's sufficiently conservative until you know how much substructure there is and we don't know that yet, and now that the data are coming in that there is substructure, you need more data before you can do it."

When questioning Doctor Shields about the peer-reviwed and published papers, what that means, again at page 56, prior to being published, it says:

> "Prior to that it's a very small sub-set of individuals that get a chance to judge it, so being peer reviewed and published is the first step to acceptance. It's not anywhere near the end step."

I understand the peer review is there's a peer review committee where you submit your papers to as the fixed bin paper and the Promega paper that were put into evidence, and they review it to see if everything in it sounds - I suppose I could use the word 'reasonable', if it sounds reasonable then it ought to be submitted in that form to the general scientific community. They often will have the criticisms that say well look, this doesn't make sense, you better do something about that, and I suspect that's why the Promega paper was so long being reviewed and the first draft copy was in November of 1990 and it was a year later before it was actually okayed for publishing and parts had been removed from it. But, again, that's just a guess on my part.

Page 57 Doctor Shields says:

> "Well, I think as I pointed out here, if I do calculations I come up with differences from one in 12,633 for a 5-band match to one in 666 --"

and it's marked billion here but whether Doctor Shields said the word 'billion' or the court reporter, but it should be million instead of billion. In VD-21 it's 666 million. So, again:

> "Well, I think as I pointed out here, if I do calculations I come up with differences from one in 12,633 for a 5-band match to one in 666 million. I do not find that reassuring in making a decision about what probabilities to use in forensic cases. There are still empirical probability estimates that can be derived from these that almost nobody would argue about, they're just bigger than the ones that are currently presented, and those empirical probability estimates have to do with the fact that if you have a data base you can state unequivocally how many times you've seen a complete genotype that matches one that you're interested in, a suspect or a victim, and you can divide that by the total number of individuals in the data base if you've never seen it before, and that gives you a probability that most of the problems - the only problem that would still be a problem for that probability is if you have extreme substructure, true inbred isolates that are so different from the rest of the population that they couldn't be handled even by that kind of empirical estimate, they'd need their own data base."

Now, when Mr. Walsh was saying that the best case scenario for the defence would be using Nichols and Balding, the one in 226,000 which, again, upper interval limits would have to be put on, the best case scenario that Doctor Shields would have and could have come up with - or could have I should say, for the defence in this particular case, would be 1 in 12,633 for a five band match. Again, you

would also have to put upper confidence intervals around that figure which it's anybody's guess what it would be. We are not, for the record, we are not assuming that the Court ought to take that figure and present that to a jury. Our arguments is that until we know the degree of substructure that is out there it may - those numbers may actually be favorable to Mr. Legere but, then again, they could be biased against him because we do not know the degree of substructure that is out there in the Miramichi or in New Brunswick, and until you know what it is it's not reliable to use any figures whatsoever.

Continuing at page 58 Doctor Shields says:

> "One thing that I would do in Canada is certainly have a comparison between French-speaking Canadians and English-speaking Canadians, the two biggest groups, the two most likely on the basis of what I've seen so far that I would predict would be different, and if they are, then you'd need a data base for each rather than a general Caucasian data base. That would go some way towards it."

At the bottom of the page:

> "Q. Now, Doctor, I'm going to recall the evidence given by Dr. Fourney as to the rebinning of the R.C.M.P. population data base, and what I understood his testimony that when they rebinned it they found that there was five people in the data base who matched across the five probes, so therefore they removed those five on the assumption that they tested the same person twice. Would that be a proper scientific call to make without knowing where the samples come from?
>
> A. No, but it would explain why people have, "never found five-band matches between unrelated individuals", which has been testified to in a number of cases. In my own opinion, I think

> that unless you know that it's the same individual one could assume that they were actually true random matches and therefore it's not probably appropriate to throw them out."

That goes back to Doctor Shields explanation as to what would be an appropriate way to gain probabilities would be for the R.C.M.P. to actually go to their data base, see what the frequency would be of this particular profile, and compare it with other people to see what the probability matches would be, and to make such tests of your model by doing that with everybody in your data base. The R.C.M.P. have not done that.

Again, when I asked Doctor Shields about different hair samples, at page 63 I asked:

> "Are you able to use the product rule in this to come to a figure or would it be one in 4,500?
>
> A. If they're statistically independent I can't tell from the data because I don't know about hair data bases, but if they were statistically independent you would multiply the two probabilities together, which would be an exceedingly low number."

Or low probability.

Again, Doctor Shields is pointing out that the frequencies for which you are multiplying have to be independent which is what the R.C.M.P. and the F.B.I. are doing with their data bases. They are multiplying these frequencies without any proof of their independence whatsoever. But it would appear from the statistical evidence and data that is coming into the courts, finally, because the experiments and the testing are being done more often that there is empirical data to show that there is not independence.

That there is statistical significant differences in bin frequencies between different populations within the same races and between races. So if they're not independent then you simply cannot multiply them. You can't use the product rule.

Page 64:

"Q. You said maybe you shouldn't use either. For what reason would you state that maybe it's not right to use either data base?"

Here we're talking about basically the F.B.I. and the R.C.M.P. or when you just have two data bases, and when you have a statistical significant difference in them.

"A. The data that are there, the actual differences that you see, are indicative that there may be greater differences in the particular sample that you're looking at. The fact that there are differences at the level of the whole Canadian population versus the whole U.S. population implies that it's at least possible there may be even greater differences between New Brunswick and the rest of Canada. Until you know that that's not the case, then the difference between one and I believe it's five million and one in 9.9 million may actually turn out to be a difference of one in 400,000 versus one in 5.2 million, and you don't know."

Again, these figures are again all indicated as without any upper confidence interval being applied to them.

On cross-examination of Doctor Shields at page 68 Mr. Walsh has:

"Nichols and Balding - correct me if I'm wrong, you referred to a correction factor?

A. That's correct.

Q. And the correction factor is to allow for what? What is the purpose of the correction factor?

A. To correct for substructure.

Q. And that would be related to inbreeding, for example, isolated sub-populations?

A. It would be related to either selection or non-random mating generating allelic differences between sub-populations within what we call the meta-population."

Again, the example by Nichols and Balding used by Doctor Shields was to show that there are people out there who are attempting to work at overcoming the problems of substructure within populations and they are looking for methods to assist the forensic fields in coming to courts with reliable methods and applications that hopefully we can all gain benefit from someday.

At page 99 on cross-examination, a question by Mr. Walsh.

"Q. O.K., and, Doctor, your opinions this morning with respect to going out and sampling the French-Canadian population because there may be some differences between the French-Canadian and the English-Canadian population in Canada, does that data have any bearing on - affect your opinion now in any way?

A. Absolutely not, it is totally irrelevant. What happens is that you may have sitting in this data base people from Quebec who are the equivalent of - the analogy that I was using - pinochle decks, and people from Ontario who are the equivalent of regular decks of cards, and when you do the whole population you average between the two.

Q. And the same with New Brunswick, Doctor, you -

A. Yes.

Q. So what you're suggesting is - what you're saying, then, Doctor, is that there's not adequate representation, is that right?

A. Adequate representation. What you want is an adequate data base for any potential sub-population.

Q. So the representation, the division of representation on the CFB Kingston base is not an adequate representation of Canada, is that right?

A. Representation -

Q. Is that particular -

A. It's not an adequate sample.

Q. Why? I thought you -

A. For why I just told you.

Q. Well, I thought you talked about this all along, Doctor, that it would be and that it was?

A. It is a representative sample, it is not an adequate sample. I have never said it was an adequate sample.

Q. Why wouldn't it be adequate, Doctor?

A. I just explained it to you, I will try once more. It is at least possible that the Quebecois are genetically different from the English-speaking Canadians. If they are, putting them into a single data base will merge whatever differences there are, and having this representative sample of all of the Caucasians is like taking pinochle decks and regular decks of cards, shuffling them together and saying that the frequency of aces is 12 out of 100. When the frequency of aces in one population is 4 out of 52 and the frequency in the other population is 8 out of 48."

And I think that's what Doctor Shields, again, is trying to explain as the same analogy here is the same analogy that you have to use when you compare the F.B.I. to the R.C.M.P. You know there is sub-

structure but you don't know how much, and to use either data base where they are mixed substructures it would be incorrect because it would be to the bias of any person who fit into his own substructure, and if there is a mixed substructure it's logical to conclude that everybody is being biased by that central data base because everybody would have to fit into their own.

Page 105 on cross-examination, questioned by Mr. Walsh:

"Q. But, Doctor, you're completely discounting the fact that CFB Kingston would have representatives from the Province of Quebec, representatives from the Province of New Brunswick, representatives from Manitoba -

A. I'll try and show you why it doesn't matter. If we take a big population, we take a population that is, as you're suggesting, homogeneous because it's primarily British, and it has 80% of the decks of cards or 90% of the decks of cards or just 80% are actually regular decks of cards, and you throw in 20% pinochle decks and then compare that mixture to all regular decks they're not going to be very different, but if you were to take all pinochle decks, French Canadians, and compare those to all regular decks, English-speaking Canadians, there may be a difference.

So it's apples and oranges. Yes, I said maybe. I do not know, I have not seen any explicit data that say one way or the other other than the data I just saw the past two days that say that the French -

Q. In France?

A. In France.

Q. At two loci.

A. At two out of five loci are different."

My Lord the Crown appears to be making the assumption that just because we only have data on two loci that it's not reasonable to assume that there's going to be a difference on the other three. I would submit that because you do have a difference in two loci that it's very reasonable to assume that you're going to have as big a difference on the other three, or at least two of the three. However, to show that a population is a subpopulation you only have to show statistical differences on two loci. The other three could be exactly the same and it would still constitute sufficient substructure to throw it outside of linkage equilibrium - to show linkage disequilibrium. You only have to show statistically significant difference on two loci.

At page 110, cross-examination, questions about comparing the Kingston, Ottawa and Vancouver.

"Q. And that's an indication as well, Doctor, that there's no one indication, one example, that perhaps there is no significant substructuring going on in the Caucasian population, is that correct, in the Canadian Caucasian population?

A. That's an indication that there may not be, but now I would point out that I have a suspicion that were you to ask Dr. Carmody, and certainly if you asked me, if you ran the same test using D17S79 and another locus between the U.S. and Canada you'd discover that there is linkage disequilibrium, or as we say, gametic phase disequilibrium.

Q. I see.

A. Because there are significant allele frequency differences between loci."

Page 111, a question from Mr. Walsh:

"Q. ... if there is inbreeding going on in the Province of New Brunswick, particularly in the area which Mr. Legere comes from, that using Nichols and Balding's correction factor those are the frequencies that you would correct for?

A. So you'd end up with one in 5.9 million or one in 226,000, depending upon whether you were talking about a 5-locus or a 4-locus match."

Page 136 there's a question:

"Q. And isn't it in fact true, Doctor, that the samples that you're referring to for this background band sharing is, to use the words of Dr. Carmody, pathetically small?

A. I don't know what he meant by pathetically. It's small.

Q. Can you make any statistically valid conclusions from such a small sample?

A. I really don't want to do this to you but you better hope you can because that's what you do when you develop the probabilities for forensic analysis."

I would like to point out at this time that when I was questioning Doctor Carmody about possibly using the five figures from the Newcastle area as representative of a data base for the Miramichi area that he said that was pathetically small but that was in reference for trying to come to some conclusion as to what a representative data base might be for the Newcastle area. It had nothing to do with the commonness of band sharing which is what Mr. Walsh is referring to here, it's in the factors that he used to show the probability of band sharing and there's not a doubt, statistically, that that is a proper method to do it because basically that's what

the R.C.M.P. are doing when they compare one individual to their general data base. So if it's reasonably possible to compare one individual to the general data base then it's reasonably possible to compare five individuals to the general data base.

"Q. And do you think a sample population of five is equivalent to the sample population of the R.C.M.P. Caucasian data base?"

Page 137:

"A. You're missing what I did. All I did was take the R.C.M.P.'s frequencies, use their logic, and generate probabilities that you'd get four bands to match.

Q. But you have actually taken, though, Doctor, five individuals from that particular area, and based on those five individuals you've extrapolated a theory, and the theory is this high coefficient of inbreeding, am I right?

A. The probability that results when you ask what's the likelihood that you'd get this by chance in a small population, in a sample size of the size that we're looking at, is one in trillions. That says that it's probably an incorrect assumption to assume that the R.C.M.P. frequencies are truly representative of the population from which the five individuals are drawn. Let me finish because I'll explain to you what the problem is. If you have a very, very large sample you can get statistical differences quite easily. When you have a very, very small sample, and that shows the kind of pattern we're talking about, all of the individuals share bands, that's less likely. Only if you were to have that small sample thousands and thousands and thousands of times, and it only showed up once with a lot of band sharing, would you be correct in assuming that because it was a small sample you got a statistical glitch. The fact that it happens with a small sample, and it's true of all five individuals, is to me good evidence for band sharing, not bad."

Page 140 Doctor Shields states:

> "... the question isn't here whether you have one, and that's the key to it, here you have five different individuals that share bands. It's not that they are either rare or common with respect to the data base, it's that they are common with respect to each other, and it's not all of the same loci. If it was only common loci, then maybe, sure."

At page 147 he states - I'll have to go to the question, a question by Mr. Walsh:

> "Q. Assuming, Doctor, that there is no substructure affecting the VNTR frequencies and assuming Hardy-Weinberg equilibrium and physical and statistical linkage equilibrium, O.K., those are assumptions I wish you to make and I realize you probably say they're big assumptions, but just assuming, would you agree that obtaining allele frequencies, bin frequencies, by the fixed bin method using the Hardy-Weinberg, then using the Hardy-Weinberg equation to determine probe frequencies, and the product rule to determine overall genotype frequencies would be a generally accepted method of calculation in the scientific community and/or reasonably reliable method of calculation?
>
> A. With one small caveat, yes, the small caveat being that even if you have linkage equilibrium or you can't demonstrate that it doesn't exist and even if you have Hardy-Weinberg equilibrium and you can't statistically demonstrate that disequilibrium exists, there is still the potential that there is sufficient substructure that will not show up in that form so that the final thing that I would like to see is moderate size samples of appropriate ethnic groups to indicate the degree of substructure directly rather than through the intermediates of the statistics, but all of those things - if you did all of those things, yes, I would agree that that methodology would be scientifically acceptable and reliable.

Q. In essence, satisfied ourselves with respect to the existence of sub-structure and its extent and its effect on frequencies?

A. Yes."

At page 149 Doctor Shields says:

> "If you can demonstrate to the people like myself, and there are others who feel that substructure is a potential problem in this, that substructure does not have a major impact, O.K., I think we would agree. The question is going to be as a scientist I choose personally, and I did read these pieces of the testimony, to disagree with Doctors Carmody and Kidd that differences of one in a million to one in ten million are not important. I don't find that difference to be what I would call scientifically acceptable, even in forensic practice."

On cross-examination of Doctor Shields at page 151 Mr. Walsh was asking Doctor Shields about the significant difference between 5.2 million and 9.6 million.

"A. Depends on the sample size.

Q. The sample size, you know the sample size.

A. I would have to do the calculations. I think -

Q. They differ by a factor of two, don't they, Doctor, 5.2 and 9.6?

A. Little less than two.

Q. Well considering the sample sizes of the R.C.M.P. and F.B.I. Caucasian data, you run the data through it, do you consider that to be a statistically significant difference?

A. I suspect if you run the statistics it's not statistically significant."

And I, too, would agree with Doctor Shields that if you run the statistics it's not statistically significant because once you put the 99% upper

confidence interval there the end product would not be statistically significant. But when Doctor Shields was comparing the bin frequencies he is comparing the bin frequencies as to whether or not they were statistically significantly different, and a set of criteria is used for matching the bin frequencies, as the R.C.M.P. have been doing and the F.B.I. have been doing, as was explained by Doctor Waye when I questioned him of how you would distinguish between bin frequencies to show that there was sub-populations - substructure. A factor by two with Doctor Waye was considerably high. As a matter of fact, as I recollect, they're not even in the same ballpark when he said the difference between 1 in 50 and 1 in 26 is, if I remember correctly, not even in the same ballpark.

Doctor Shields is saying that, yes, if you run the statistics it's not statistically significant using the 99% upper confidence interval. The R.C.M.P. and F.B.I. do not use the 99% upper confidence interval, had not in the past, because they would have then had to admit that all they were coming to court with are estimations and not facts, and you can't use the product rule when you're only using estimations.

At page 152:

"Q. So in this particular case with a 99% confidence interval, that is a pretty good scale to judge how much weight to place on the particular number?

A. That depends strictly on sample size and how well the sample represents the population of interest. You can also put a confidence limit around the one in 226,000, O.K.,"--

-- and here he's referring to VD-121, the figure used from Nichols and Balding --

"that's right next to the 5.2 and the 9.6 million."

Again, in discussing at page 153 of Doctor Shields' testimony, a question by Mr. Walsh:

"But you will say here today that it's not a statistically significant difference?"

-- and that's the difference between, again, 5.2 million and 9.6 --

"A. No, I think it's a big difference and it's not statistically significant."

So he's saying yes, it is a big difference in the number but, no, the end product, if you use the 9 -- the 99% upper confidence interval, then it would not be statistically significant, because you are using statistics to arrive at those figures and to calculate the frequencies.

Again at page 153:

"Q. And if you applied the 99% confidence intervals to the calculations you did when you run Legere's through the F.B.I. and you compared it with what was run through the R.C.M.P., it shows that there really isn't any difference, is there, Doctor?

A. No, it does show there's a difference. In fact, if you want to give me Dr. Carmody's little thing I'll show you the difference."

And, again, they were talking about the 99% upper confidence intervals.

At the bottom of page 154 Doctor Shields states:

> "If you did the same thing with the FBI's it would be in the neighbourhood of one in - oh, probably, looking at this, I would say one in four million, maybe one in five million, to one in 25 million, and the point I would make there is that even though they overlap there are values that the FBI's could take that do not exist for the R.C.M.P., and vice-versa."

Page 163, cross-examination, Doctor Shields states:

> "If all of the assumptions used to generate that frequency were correct, I would agree, but I believe there's evidence that suggests it's not correct."

And we're talking here about the assumptions of Hardy-Weinberg and assumptions of linkage equilibrium.

> "Q. No, but what you've indicated, Doctor, is that you believe there's a need for more data before you can actually conclude those?
>
> A. But there are data in this case that tell me that it's not correct, and that's the band sharing.
>
> Q. The band sharing, and you --
>
> A. And the fact that French are different."

Page 164, still on cross-examination.

> "Q. And then I asked you the question with respect to the probability of this coincidental match of 5.2 million and you said that's correct.
>
> A. No, I"m not changing my testimony. What I'm saying is that I don't think you can use the simple binomial expansion and the product rule, O.K., because of the problems of substructure in particular. Given the problems of substructure, if one has independent evidence for how much substructure there is one can use band sharing to generate a new probability of coincidental match that will be sufficiently conservative that it is not likely to be

biased in the wrong direction, because as you suggested, just as I admitted, when more data come in maybe Dr. Kidd and Dr. Carmody are going to be right and it really doesn't make any difference, but when more data come in it just might be the case that I'm right and Lander is right, and Lewontin is right and it does."

In fact that it does make a difference.

As Doctor Shields explained on page 165:

"Q. But you don't feel it's necessary to actually go and look at his actual opinions, do you?

A. I have his data.

Q. Now, you have his data, Doctor, but you haven't taken into consideration, have you, his opinions?

A. I'm sorry, but I think you misunderstand what science does. Science doesn't care about opinions. Science cares about what the data tell you."

On page 166 on cross-examination Doctor Shields states:

"Again, the data in this case tell me that it's not correct.

Q. And that's based on that background band sharing that you've done, Doctor?

A. Background band sharing and the fact that the French VNTR's at two loci are different from the Canadian, the general Canadian data base."

Again, on page 167:

"Q. And do you simply discount the fact, Doctor, that the same kind of tests were done on the Canadian Caucasian populations and there is no differences?

A. No, I say that that's reasonable and that's prima facie evidence that maybe Canada has less substructure in their Caucasian population, but I also have evidence from this case of background band sharing and from --"

-- and he says affidavit but he's referring to VD-65 which was put in by Doctor Carmody --

> "... that says that there are significant VNTR differences between the French and the Canadians. They're both Caucasians."

When Doctor Shields testifies that he's not concerned about the opinions of Doctor Kidd or Doctor Carmody on what makes forensic differences, he is relying, again, on the data about the band sharing in this particular case to show that there is substructure, not just for the Miramichi area but at least to this degree in the Miramichi area, and that he says he relies on the data to show that the R.C.M.P. cannot use their data base in this particular case.

The evidence that was produced by Doctor Shields, I would have to state to the court, that he considers to be not bad evidence but good evidence that there is sufficient amount of band sharing in the area where Mr. Legere had come from, that it would be totally inappropriate to use the general data base to calculate the frequencies in this particular case.

I would like to point out to the Court that the Crown made ample criticisms about Doctor Shields' testimony of just comparing the FBI and the R.C.M.P. in the Vanderbogart case and in another case and that one of the cases - well, they let it in because that evidence was unrebutted, and the crown did everything they could to rebut the evidence in this case about just the differences between the R.C.M.P. and the FBI, but the evidence here of background

band sharing in the community where the accused comes from, that evidence is unrebutted. That evidence is unrebutted that it is a significant difference that it invalidates the use of the general R.C.M.P. data base. The Crown had ample time to decide whether they were going to rebut this evidence and the probability factors that Mr. Shields come up with that it was a valid procedure and a statistical procedure that is commonly used, as he testified. Doctor Carmody was in court during the evidence of Doctor Shields. They did not only have the time over break, they had the time over lunch hour after most of Doctor Shields' testimony was in, before the Crown had to cross-examine, and even after the cross-examination of Doctor Shields I believe Mr. Walsh checked with Doctor Carmody as to whether or not they wanted to call any rebuttal evidence and they decided not to call any. I would submit, My Lord, that the crown at that point has basically accepted the data presented to this court by Doctor Shields and accepted the reasoning and the opinions behind it.

THE COURT: Well, you are not suggesting, surely, that the crown would have to take advantage of the opportunity to call rebuttal evidence. Surely the failure to call rebuttal evidence doesn't mean they accept everything the defence evidence has said. I'm sure here that Mr. Walsh probably, whether rightly or wrongly, took the view that every point that had been made through Doctor Shields' testimony had been adequately answered already through anticipation when his own witnesses were called. You're not suggesting that failure to rebut --

MR. FURLOTTE: I am suggesting that the failure to rebut is an admission on the crown's part that Doctor Shields did come up with legitimate data which legitimately supported the position that there is significant substructure out there to show that it is improper to use the R.C.M.P. general data base to calculate the frequencies.

THE COURT: Yes, but in a few minutes - or at least when you finish, I'm going to be asking Mr. Walsh if he wants to argue in rebuttal as he has the right to do in this thing, and if he doesn't want to exercise that right does that amount to an admission that he recognizes the authority of everything you said in your own argument.

MR. FURLOTTE: Oh, it's not what I -- What I say in argument has no authority, and it has -- my opinion carries very little weight. What this court has to rely on making its decision is the evidence put before it. My position is the evidence put before this court by Doctor Shields clearly shows that there is strong evidence out there, and in this case particular, that there is substructure which would invalidate the use of the R.C.M.P. data base to calculate frequencies on the general population --

THE COURT: Yes, well --

MR. FURLOTTE: -- not only in this particular case but for any subsequent cases that might follow it because substructure does exist. We've proven it. And with that evidence, what I am saying is that the crown had the opportunity to have Doctor Carmody come in and testify that Doctor Shields' calculations were

improper - it was not a proper thing to do, that his calculations were wrong, and for him to attempt to explain that phenomena of substructure away if it was possible. And basically they chose not to do it because it could not be rebutted. And I'm saying, yes, the opportunity was there for them to rebut; the witnesses were here. I assume that's the purpose that Mr. Walsh had Doctor Carmody here during the testimony of Doctor Shields in case there was something new that Doctor Shields come out with that was necessary to rebut. And further to that, I always find it - or at least I would consider it carrying much more weight if I can get these admissions out of the crown's witnesses on cross-examination which I was leading up to doing in my cross-examination of Doctor Carmody to get at all this but, unfortunately, I wasn't able to continue, but having not been able to get that out of Doctor Carmody it did come out of Doctor Shields and Doctor Carmody then would have had the opportunity to address that issue because it was a new issue - it wasn't something that was totally brought up in the blue, and my position is that, yes, they had the opportunity to rebut it if Doctor Carmody had disagreed with Doctor Shields and I would submit, My Lord, that Doctor Carmody would have been and is in complete agreement with Doctor Shields.

My Lord the Crown, I suppose, in the sense if you want to stay on this common band sharing for a minute, either the Crown would have to accept the fact that it's improper to use the R.C.M.P. data base to calculate the frequencies in this case - and no doubt any case thereafter, that it's improper to

use that or they would either have to accept the evidence of Doctor Shields that the events of randomly picking out two suspects, three victims, and comparing those profiles that the events of that happening, which is one in 1½ trillion, they have to choose between the two. They can't pick one and ignore the other. So it's either - if they want to continue and say that the data base of the R.C.M.P. is valid and it's reliable, then they would also have to say, well, there's only 1 chance in 1½ trillion that there is no substructure out there, and we're willing to take that chance, and that they were then willing to continue on with other cases and proceed to court with this. The basic line is that there is only 1 in 1½ trillion chances that there is no substructure that would significantly affect and invalidate the use of the R.C.M.P. general population data base.

My Lord in looking at the weight to put on this type of evidence I see this type of evidence as replacing eye witness identification, and I think the same principles should probably be applied to this type of evidence which the courts have been applying to eye witness identification, and the law and the weight to be placed on single eye witness identification is usually to be very small when it's only a single eye witness I.D. In this case we only have identification from a single witness being this DNA profile which was run on a single test. Not only has it been run on a single test, the data base that we're relying on to calculate the frequencies has only been run once. There has only been one test to verify

that the bin frequencies would be consistent and reliable.

I would submit, My Lord, that that type of identification under these circumstances is basically no better than a blind man going to court and giving evidence as an eye witness.

Just as the elements of each offence of a crime must be proven, and the elements of an offence must be proven beyond a reasonable doubt, in this type of evidence where there is no doubt that if this type of evidence goes in that juries are likely to accept it as proof beyond a reasonable doubt, so I would submit, My Lord, that the elements do need a high degree of certainty before they should be admitted. Just as the elements of an offence - each element has to be proven beyond a reasonable doubt I would submit, My Lord, that the elements of DNA identification also need to be proven beyond a reasonable doubt, and that's each one. And basically, at law, assumptions are simply insufficient as they are determined at law to be totally unreliable. If you look at the DNA evidence and all of the assumptions that have to be drawn to rely on the use of the Hardy-Weinberg formula and the product rule, then that is what the R.C.M.P. is asking the courts to use to convict people - mere assumptions. No proof whatsoever. They're coming to court and saying well, look, it's too onerous on us to prove that there is Hardy-Weinberg equilibrium; it's too onerous on us to prove that there is no linkage disequilibrium; it's too onerous on us to prove that there is substructure.

That's going to take a few bucks and three or four months work. Well, maybe Quebec is doing it but that's their business. We're not concerned about that - or at least they didn't have the foresight or the insight to do it. It's just asking much too much of a Court to bear, asking much too much of an accused to bear.

On the pretrial brief on the law which was submitted by the Crown Prosecutor at page 21, paragraph 2, he states:

> "Because an expert can give an opinion based on hearsay provided that the basis for such opinion is proven to exist by more direct evidence. It is sole a question of weight. This represents Madame Justice Wilson's majority judgment in R. v. Lavallee"

--.and he quotes:

> ... The ratio in Abbey can be distilled into the following propositions:
>
> (1) An expert opinion is admissible if relevant, even if based on second hand evidence.
>
> (2) This second hand evidence (hearsay) is admissible to show the information on which the expert opinion is based, not as evidence going to the existence of the facts on which the opinion is based.
>
> (3) Where the psychiatric evidence is comprised of hearsay evidence, the problem is the weight to be attributed to the opinion."

And (4) is the crucial factor.

> "(4) Before any weight can be given to an expert's opinion, the facts upon which the opinion is based must be found to exist."

And in this case, for the admission of DNA evidence for the reliability of the probability factors, the Crown, the R.C.M.P., are relying on assumptions that

there is Hardy-Weinberg equilibrium; they're relying on assumptions that there is no linkage disequilibrium; they're relying on assumptions that there is no substructure out there which would affect the computation and probability factors.

That, My Lord, I would contend, that does not fit with the number (4) as stipulated by Madame Justice Wilson that "Before any weight can be given to an expert's opinion, the facts upon which the opinion is based must be found to exist." I would submit, My Lord, that that does apply to all the assumptions that the Crown is asking this court to accept; that it's not proving the elements whether upon a preponderance of evidence or beyond a reasonable doubt or anywhere's near it.

At the bottom of page 21 of Mr. Walsh's paper he says:

> "Regardless of upon which basis of justification the evidence of pattern frequency is allowed it is important to note that at this early stage of the development of DNA forensic evidence for court use, it will be necessary to provide direct evidence, through the expertise as previously discussed, of the statistical validity of the data bases used in calculating such pattern frequency."

I would submit, My Lord, that there is no evidence submitted by the Crown which supports the statistical validity of the data bases used in calculating pattern frequencies because there is no evidence that Hardy-Weinberg equilibrium exists; there's no evidence of linkage equilibrium and there is no evidence that substructure does not exist.

Continuing on page 22 Mr. Walsh says:

> "In this vein it is important to appreciate the distinction to be made between proof based on the pure theory of mathematical probabilities which is not permissible as evidence, and statistical evidence based on supportable facts, which is a firmly established method of assessing the probative value associated with particular evidence."

Again, My Lord, there are no supportable facts which the Crown has produced into evidence which can support their assumption that Hardy-Weinberg equilibrium exists; there are no supportable facts in evidence which can support their assumption that linkage equilibrium exists; and there are no supportable facts which show that there is no substructure which could have a devastating effect on the results of the probabilities.

As cited in People v. Collins, it says:

> "The former category is illustrated by People v. Collins where, in an attempt to prove identity: A mathematical expert was asked to assume the individual probabilities shown below ..."

-- and, again, the word assume --

> "that they were mutually independent..."

It's clear that the People v. Collins would not allow in statistical probabilities based on assumptions.

At the bottom of the page it says: "The conviction was quashed on the basis, inter alia, that there was no proof of the individual probabilities or that the identifying characteristics were independent of each other.".

The evidence is clear that there is no evidence before this court to prove linkage equilibrium and that each of the characteristics were independent of each other. As a matter of fact, the evidence before this court is that to bring this proof to court it would be too trying. It would be too onerous on them.

Again, I will repeat at page 23 what I repeated earlier, where it says:

> "The evidentiary principles applicable to this type of statistical evidence were fully addressed in Kansas v. Washington, in relation to blood type characteristics, as follows:
>
> > Expert testimony of mathematical probabilities that a certain combination of events will occur simultaneusly is generally inadmissible when based on estimations..."

And all we are dealing here is with estimations and assumptions.

It states:

> "By contrast, population percentages on the possession of certain combinations of blood characteristics, based upon established facts, are admissible as relevant to identification."

My Lord, again, I would submit that there are absolutely no established facts. Everything is based on estimations and assumptions.

When Mr. Walsh - which I couldn't understand in his closing address as to whether he was asking this court to use the reasonable reliability test, and then it seemed on the other hand he was saying that, well, everything should go towards weight to the jury. I could understand that when in the middle of this quotation from Kansas V. Washington it says:

> "Attacks on the validity of the underlying statistics go to the weight of such evidence, not its admissibility."

I would agree with that because once the Crown would be able to prove that there was reasonable reliability, that if they were able to prove all their assumptions of Hardy-Weinberg equilibrium and linkage equilibrium, and to prove the assumptions that there is no substantial substructure out there which would affect the probability factors, if they could have proven all that and, again, if they weren't relying on explanations, then and only then would the attacks on the validity of the underlying statistics go to the weight. But when they can't prove their first principles upon which their hypothesis is set, their theory is set, their working model is set, when they can't prove the first principles and they openly admit that they can't -- well, they say they can but they openly admit that they don't want to be bothered, then I would submit, My Lord, that they shouldn't come to court with hat in hand begging leniency on their part to the prejudice of the accused.

On page 24, again, Mr. Walsh states:

> "These principles are routinely applied in the DNA context with the recognition that any dispute as to the statistical basis for the evidence is one of weight."

Again, no. The arguments before this court is not one of weight; the arguments before this court is one of admissibility as to whether or not the principles - the first principles have been found for the legitimate use of Hardy-Weinberg. The Crown in the Promega Paper, Doctor Waye, it is well admitted that they recognize the fact that it's not

valid to use the Hardy-Weinberg but yet they contend that their fixed bin approach is conservative enough to make up for all those errors, and yet their own admissions they have no way of calculating how conservative it is.

I just want to go through Mr. Walsh's - the notes I took on Mr. Walsh's address to the court. Mr. Walsh contends that DNA evidence is routinely admitted in Great Britain and through hundreds of cases in the United States. I am not going to get in and argue all the case law that's in the case book or even the - which is either in support or not supporting the defence's position. I think this court is simply -- it's a matter of common sense and the other courts' decisions are not going to be much help. But the DNA evidence, agreed, it's routinely admitted in Great Britain but they used the multi-locus probe in identifying DNA particles, and it's totally different than ours in that they don't even need the data base. It's more on the identification of what -- They don't need a data base; they don't have to be concerned about substructure because it's based on the frequency of people sharing bands, whether they are brothers or not, and whether they are related. The only possible exclusion that it wouldn't take in there would be identical twins. So they couldn't care what the degree of substructure is out there; they don't need data bases and they're taking a very simple approach. While it might be true that they don't - it might not be as sensitive as they would like nevertheless it's indisputable.

Hundreds of case law in the United States I think the court has to realize that many of the cases had no defence representations, no expert witnesses to support them. Many of those cases were decided long before the defence even realized that what the R.C.M.P. and those labs were doing was improper. It's only since it has come to their attention, and I suppose we have to thank Doctor Eric Lander to a great extent because it's only since the Castro case, where Doctor Eric Lander spent I believe some 350 hours of his own time voluntarily to show that what these forensic laboratories were doing was improper - at least was not reliable, and was extremely questionable. It's only since that time that not only defence counsel but even scientists in the general community became aware that there was a problem out there. Agreeably, with Doctor Kidd, that most scientists in the general fields are not concerned because it's not their problem. They have their own business and their own interests that they want to get along with and they don't want to get involved. But it's clear from the evidence of Doctor Kidd that those people who do want to get involved most of them, and a lot of them, are doing it out of principle. It sure would not be for money because if it was just for money then there would be as many proponents as there were opponents. According to Doctor Kidd's own testimony the opponents outnumber the proponents four to one. Even if Doctor Kidd does consider half of them unqualified to base their opinions they still outnumber two to one. And, again, numbers is not what counts here; it's the dispute that counts.

Mr. Walsh in his argument says the confidence interval is the best estimate. Now, while the confidence interval is the best estimate it is also debatable and it is only a feeling as to what upper confidence interval should be used. Again, no clear evidence or established fact as to what should be used. So we don't know what the best estimate is but, nevertheless, it's an estimate. It's not a proven fact as to what these calculations ought to be so that you could use the product rule.

Now, Mr. Walsh criticized Doctor Shields for the evidence he gave in his affidavit in the Vanderbogart case and the other case where he says in his affidavit he left it hanging because he didn't address the question that there was no statistical differences between the numbers of 5.2 million and 9.6 million, and I was very surprised at Mr. Walsh because he says this was in fact misleading. You know, whether it was deliberate or not he was misleading the court and, you know, questioned why wasn't it mentioned. Well, the fact is that in the Vanderbogart case there was no question of using an upper confidence interval, and the reason it's not mentioned, because when you don't use an upper confidence interval there is a great statistical significant difference, but if you are going to use an upper confidence then there is none. And in the Vanderbogart case, the FBI cases, they don't use it.

I might submit, My Lord, that this is the first case that even the R.C.M.P. is proposing -- At least the R.C.M.P. is -- I guess the R.C.M.P. is not proposing to use it, but at least their witnesses are. One has to ask themselves why all of a sudden have they had a change of heart. The change of heart came because of the evidence given by Doctor Shields and then they criticize him for it. But we saw the opinions of the crown's expert witnesses as to what their meaning is as when they say no meaningful difference and no forensic differences.

Clearly, the Hardy-Weinberg and the product rule the first principles that that formulation - mathematical formulation is based on have been shot down. So now they are going to argue from another level, but it's not a level of scientific expertise.

The crown commenting on Doctor Shields background sharing, I notice when Doctor Shields was testifying about the high degree of background sharing and what degree of inbreeding this would show, I believe Doctor Shields' comment was that it was probably the highest that he had ever seen in the world, and it appeared at that time that Mr. Walsh got extremely upset at his suggesting that New Brunswick or at least the Miramichi area showed the highest degree of inbreeding in the world. I don't know whether he took that personally because he was from the Miramichi area but in our terms we tend to think of inbreeding as some form of incest which in the scientific world of population genetics it doesn't necessarily mean that. But, yes, with --

THE COURT: He will be relieved that you put that interpretation on it.

MR. FURLOTTE: But even the irony of it, My Lord, is that even at the degree of inbreeding on scientific terms rather than ours, for it to be so great in Newcastle I guess there would also -- or New Brunswick, there would also have to be a high rate of incest going on. My Lord, that only holds true so long as the R.C.M.P. data base is a valid one to base those conclusions on because in order to compare as to whether or not there's a high degree of inbreeding one has to compare it against something else which is the R.C.M.P. data base. So it would be interesting I suppose to go to a jury and tell them according to the R.C.M.P. data base the people are all a bunch of perverts. I think, My Lord, it's just another example of how the R.C.M.P. data base can be used to show that it is totally unreasonable.

Again, Mr. Walsh heavily criticized Doctor Shields for using the .05 ratio of inbreeding, the highest that he ever seen in the world, by Nichols and Balding, but I believe as Doctor Shields has pointed out, those were not his figures. Those were the figures used by Nichols and Balding and all he was using Nichols and Balding for was to show a different method and approach that other scientists in the field were suggesting may be appropriate. Those were not his figures. He wasn't using the most extreme examples. That's not a worse case scenario used by Doctor Shields; it was Nichols and Balding that used it.

My Lord in closing, I believe, I would like to point out that the Wesley case in the States has been reopened because of the evidence of unreliable evidence and there's applications for the reopening of other cases in the States because of the unreliability of the procedures used, and I believe it's mostly because of the ones used by Life Codes.

When this court makes a decision as to whether or not that either under the Frye test or under the reasonable reliability test, as to whether or not this case should go before the court, I think as Doctor Shields explains to me, that he feels sorry for crown prosecutors because they're relying on their expert witnesses to come to court to testify and convince judges that this is reliable, not only reliable in the scientific community but reliable totally and without question - they're not making any mistakes. The problem that if they were wrong, which most of the scientists do believe, that if they are wrong and in the end, as the defence keeps hacking away at the reliableness of these tests, if in the end we do prove that there is substructure, that it was improper to use these data bases, heaven forbide, all these cases are going to have to be tried over again, and when you look at the cost to the taxpayers, not only this trial which I suspect will be about two million dollars to prosecute, but all the trials that have been in the United States and which are going on in Canada, once the defence proves - or if the Supreme Court of Canada finds that these are not reliable tests, all these trials will have been for naught.

I suppose one could categorize this as some kind of a war between the prosecution and defence; that while the prosecution may be winning the battles they lose the war. All these prisoners of war, accused people so to speak, will have to be released and we'll have to do it all over again. It's a poor analogy but it's fitting as to how serious your decision is going to be.

In closing, My Lord, I would submit that the Frye test, I think it's clear from the evidence that that procedure and calculation of probabilities, especially the calculation of probabilities, is not accepted by the general scientific community as being reliable. Under the reasonable reliability test, again, if the general scientific community had not got involved, had those scientists out there who have nothing to do with forensic evidence were totally selfish and just went around their own business and said heck, it's none of our business, then I suspect the defence would have had one heck of a hard time to come up with witnesses to say that it's not reliable, and the Crown would not have had much difficulty in coming to court with their expert witnesses, the few that they could get in the forensic field, to come and say look, we have a model here, a workable model, we believe it's reliable, and the court would basically have only been able to assess their opinion as to whether or not it was reliable. That, under the reasonable reliability test, I could see judges being influenced by it, but when there is so much opposition to whether or not

this is reliable in the general scientific community, that is clear evidence that the court ought not to accept the few witnesses the crown can bring in to say that they consider it reliable when there is so much opposition against it.

One can use that as a question of weight but, more, it's a question of common sense. The crown's expert witnesses, as Doctor Carmody said, a jury - it's totally inappropriate to ask a jury to resolve these issues. Maybe a judge. But if you allow this into evidence it's not the judge that's going to be resolving this issue - it's going to then be up to the jury, and that's not fair to any accused person. It's not even fair to a jury.

I would submit, My Lord, under any test that you feel comfortable with that you feel should be appropriate for the Canadian legal system, that it's strictly a matter of common sense. I think this case specific evidence clearly shows that there is substructure. There's only one chance in 1½ trillion that there is not which would substantially affect the numbers produced by the R.C.M.P., and with those kinds of specific evidence I would submit to this court that we have proven that substructure does exist. The degree of band sharing that Doctor Shields has shown to exist is almost as common as between siblings. Between siblings it would be 1 in 4. Between the substructure which exists it is 1 in 3.3, when according to the evidence of Doctor Kidd it should only be 1 in 50 or 1 in 70.

I believe, My Lord, the defence has clearly shown and rebutted all the assumptions that the crown is relying on in its ability to come to court and ask this court to allow mathematical probabilities by using the Hardy-Weinberg formula and the product rule.

Again, in closing, the crown - if the crown is allowed to use those probability figures and methods that they are merely multiplying and multiplying and multiplying their mistakes much, much to the prejudice of any accused person.

THE COURT: Thank you very much Mr. Furlotte. Mr. Walsh do you wish to reply?

MR. WALSH: Yes, My Lord, I do.

THE COURT: It's 7 minutes to 1. Were you going to be very long?

MR. FURLOTTE: My Lord before I close maybe, I have one more point to make.

THE COURT: All right.

MR. FURLOTTE: Which I forgot.

THE COURT: Well, let's do it this way. I have in mind we should probably have a recess for lunch now and continue right after lunch. It's just depending on how long you are likely to be Mr. Walsh.

MR. WALSH: I expect at the outset a half hour My Lord.

THE COURT: Well, given that, and also I have some housekeeping matters I want to bring up which will perhaps take 20 minutes or so, so why don't we adjourn now. You can either conclude now before lunch --

MR. FURLOTTE: I can conclude. There's only one paragraph I wish to read.

THE COURT: All right. Okay.

MR. FURLOTTE: My Lord in a report on DNA, the report of the New York State Forensic DNA Analysis Panel dated September 6, 1989, which was a government issue, and many members on the panel, and actually they have a 67 page report, and at page 7 - I don't think I have to issue the whole report, the Crown mightn't like it, but at least for my purposes when we're talking about forensic differences that the crown's expert witnesses are coming to court and stating, at page 7 it says "Most of the enzymes --

THE COURT: Well, are you putting in new evidence now or what?

MR. FURLOTTE: No, it would be a -- No, no, it's not putting in as evidence. Because I don't -- Well, it's argument on --

THE COURT: Well, you want to adopt the argument in that item by reading it.

MR. FURLOTTE: Going to adopt the argument in here as --

THE COURT: Go ahead.

MR. FURLOTTE: Because I am saying that -- as I have been saying, and I submit that the crown's expert witnesses in their opinions said, there's no forensic differences; that those are too subjective and they're not based on scientific facts so therefore they shouldn't even be allowed those. So I want to support that with this.

> "Most of the enzymes used in characterizing blood are not present in sufficient amounts for forensic analysis in semen or other bodily fluids. In sexual assault cases obtaining useful enzyme data from semen stains is the exception rather than the rule. Legal controversy about the reliability of widely used methods for enzyme analysis has reduced the utility of the technique in some jurisdictions.

> While its ability to discriminate between individuals is vastly superior to the ABO blood typing system, enzyme analysis cannot pin-point with specificity the source of a blood stain. Rather where a match is found the technique can generally demonstrate that the probability of a match occurring by chance is one out of a hundred. In the rare case it may be possible to demonstrate a 1 out of 50,000 probability of a random match. Such limited degrees of certainty should be insufficient in the criminal justice context."

And to point out maybe once again, the fact that the R.C.M.P. test that hair samples, one in forty-five hundred, they are not allowed to court with those figures and say that it even probably came. All they could go to court and say was that it's consistent or we can't rule it out.

THE COURT: Thank you very much. Now, I think we will recess until 2 o'clock and then go on. I have no great objection to going on now except we have had a long --

MR. WALSH: No, and with the housekeeping matters it would probably be a wise decision to break.

(NOON RECESS - 12:55 - 2:00 P.M.)

COURT RESUMES: (Accused present in prisoner's dock.)

THE COURT: Now, Mr. Walsh, you were going to reply.

MR. WALSH: My Lord, thank you. Just as an initial item since it was the last matter addressed by Mr. Furlotte, although that report that he referred to is not in evidence I would like to point out to the court that the enzyme analysis that he's referring to there has nothing to do with DNA evidence. It's apparently a test that was developed in the seventies

in addition to the ABO blood grouping system. I just make that as an initial point.

With respect to some of the aspects - and, again, I recognize Mr. Furlotte puts much interpretation in the theory of when you should rebut and when you shouldn't, if I don't cover some of the points that he has raised I can make a statement that the crown would hope that the court would not take that as an indication that we accept what he said. I will comment on some of the points that I feel are, at least at this point, necessary to shed some light on.

He indicated at the outset with respect to the reasonable reliability, the Frye standard, and my reference in argument to matters of weight for the jury and perhaps I have abandoned the original claim I have made in the prehearing brief, again, I think Mr. Furlotte has misinterpreted, greatly, the position of the crown and I can only recommend that a rereading of the prehearing brief and the pre-argument brief that I filed would make it, I expect, very clear as to what the crown's position is as to the nature of the evidence and what are matters of admissibility and what questions are of weight.

He has pointed out at one point that the forensic community are furthering their own aims, obviously, by promoting this particular type of technique. Again, I would ask the court to remember one of Mr. Furlotte's comments as to why he wanted Doctor Carmody to testify about the molecular biology and that is he had a high opinion of Doctor Carmody's views, and those views would, separate from the

R.C.M.P., support the very claims that the crown are making here.

He made one statement that I would suggest is completely incorrect, one of several. One is that he read early yesterday that the R.C.M.P. are relying on assumptions. Assumptions of Hardy-Weinberg, assumptions of linkage equilibrium, etc., without any statistical testing. Now, I don't know whether he had that prepared from some material that may have applied a couple of years ago, however, it's a complete putting blinders on to the statistical tests that were done in this particular regard, and I refer the court to the non-parametric median testing that Doctor Carmody did with respect to the issues of Hardy-Weinberg equilibrium and linkage equilibrium and in fact Seymour Geiser was a favorite topic of Mr. Furlotte's and, if you remember, Doctor Carmody applied one of Seymour Geiser's recommended tests for that, and I would ask the court to remember Doctor Carmody's opinions that those tests indicated no high correlations and that any low correlations would not have any effect.

Another statement of Mr. Furlotte's that I thought should require some comment involved that there is no evidence of individuals from the province of Quebec or from the province of New Brunswick included in the data base. I simply at this point in time wish to refer the court to volume 13, page 99 of the transcript of evidence which is the testimony of his own witness, Doctor Shields, and it would be very clear from there that that is not a position

that Doctor Shields would support of Mr. -- Mr. Furlotte's position in that regard is not supported by the opinions of Doctor Shields, and I would refer the court to that volume at page 99.

Again, I would go on, there is something that requires some clarification in the crown's respectful view and is that Mr. Furlotte has done two things that we take exception to. One is that he has misinterpreted and blurred the differentiation between statistically different numbers and forensically different numbers. The difference is this, My Lord, to harken back to the evidence. The difference of the famous numbers of the 5.2 million versus the 9.6 million, if you run Legere's data through the R.C.M.P. or the FBI data base, those numbers in themselves have no statistically significant difference at those high powers, and that is the evidence of the experts, particularly Doctor Carmody. They only differ by a factor of two. You don't need the confidence -- The confidence interval will certainly demonstrate that. But when you only move a couple of decimals that shows that there is really no difference. Mr. Furlotte's comparison at low powers is not appropriate In addition to that then there are bin frequencies that are statistically different but have no forensic difference because when you multiply the various locus or the various loci, the various probes, you come up with a number that is in fact no forensic difference to it when it's multiplied over the appropriate loci. It ends up with no statistical difference. Those are definite -- What is happening is that these

concepts are demonstrated by the confidence interval. They demonstrate statistical differences and they demonstrate forensic differences.

I can't understand - I've tried to - but I can't understand how Mr. Furlotte could characterize the argument that although you may see statistically significant bin differences but when you multiply them across the loci there are no forensic differences, and his argument is, in quotations, it's a question of feeling. He keeps using the phrase that it's just a matter of how one feels -- a feeling of some kind of subjective nature. And at this point in time I would simply refer the court to Doctor Carmody's testimony, volume 7, pages 42 to 43, and he clearly sets out what he means by that and what he's setting out is a mathematical theory. It's nothing to do with subjectivity. It's a simple fact. If Mr. Clerk wishes - or the Court wishes later to simply refer to exhibit VD-65, that is not a matter of feeling. What's exhibited in VD-65 which are all the various frequencies that Doctor Carmody calculated using the confidence interval are expressions of fact. They have nothing to do with how one feels and I thought that that was important that that be brought out.

Mr. Furlotte mentions the excess homozygosity being perhaps an indicator of Hardy-Weinberg equilibrium and I would ask the court to remember that initially that was thought it may be, although it was questioned, it has been challenged by the Yale scientists in that article, VD-53, but more importantly, if you look at Jakobetz. In Jakobetz both Doctor Lewontin, Doctor

Nadeau, Doctor Kidd, they all agreed that that is a poor test for Hardy-Weinberg equilibrium and really nothing substantial can be taken from that Wahlund's test because of the fact that we are dealing with quasicontinuous allelic systems, and that there are coalescence problems. There are problems in trying to determine if it's two or one bands and so when you are trying to determine if there is an excess of one band it's an artifact, or according to the exhibit VD-53, they have shown that at least from the data base they're looking at it's proven to be an artifact of the system. It has nothing to do with proof of Hardy-Weinberg disequilibrium.

He says that Doctor Kidd appears to -- Mr. Furlotte appears to indicate that Doctor Kidd is raising these whole issues of confidence intervals for the first time, and I would ask the court to, in reviewing Doctor Kidd's evidence, I would suggest to the court that you will see that that question was actually put to him by Mr. Furlotte and the evidence would not support Mr. Furlotte's conclusions in that regard. In fact it appears that the strength of Doctor Kidd's evidence has driven Mr. Furlotte, in attempting to lessen the impact of it, to make a statement, not that you can accept or reject Doctor Kidd's opinions but that he has gone so far as to make a statement in this courtroom that most or a lot of his arguments or opinions are those of a person, not of a scientist and, again, the logic applied to that - the only conclusion I can take is that he was driven to that extent by the strength of Doctor

Kidd's evidence and his inability to distinguish it in any other fashion.

He mentions - and, again, it relates to the confidence interval, whether you apply 95% or 99% confidence intervals it's all a matter of feeling. Well, in fact the application - the evidence will show that the application of 95% or 99% confidence interval depends on what you want to demonstrate. And when you apply 99% confidence interval you are demonstrating that there is a wide variation because you can say with over 99% confidence the figure is going to be in this particular area. If you wish to show less variation you apply less confidence. And for forensic purposes it would seem from the crown's point of view that you would want to apply 99% confidence intervals, give the wide range, and gives you a better scale, as I have indicated yesterday, from which to make your own assessment. So it's got nothing to do - and again I wrote the words down that he used - with feeling. It's got to do - it has everything to do with scientific fact.

I was in one respect flattered that Mr. Furlotte has made much of the prehearing brief that I had written, initially, until some of his interpretations of what I had said came out. I can only refer the court and ask the court to look at pages 22 to 24 which is the area that I covered, and in fact I am very confident and I am pleased that Mr. Furlotte has referred to that particular section because I am very confident that the complete reading of that section, not taken out of context, will demonstrate the very points that we're making here, and I simply

make one brief reference. At the very beginning of page 22 I have said this:

> "In this vein it is important to appreciate the distinction to be made between proof based on the pure theory of mathematical probabilities which is not permissible as evidence, and statistical evidence based on supportable facts, which is a firmly established method of assessing the probative value associated with particular evidence."

The footnote for that particular statement - it was my statement but I used a reference and I referred to McWilliams In Evidence and this particular quotation at page 37 of the annotated footnotes, and I have said this: "One text writer subdivides probability theory" -- and perhaps this is where Mr. Furlotte is blurring the lines -- "into subjective, and that is based on one's feelings; objective, for example, a game of chance; and empirical probability, that is calculations based on information acquired from data collection, the latter of which the author points out would apply to DNA pattern frequency." And the author I was referring to was Doctor Lorne Kirby, a text writer, DNA Fingerprinting and Introduction, and if you remember in Doctor Shields cross-examination he considered Doctor Kirby's text to be an authority in the field. So, again, I thank Mr. Furlotte for referring to that particular section, however, his interpretation of what's being said there is mistaken in our respectful view. In fact when he refers to Kansas v. Washington as an example of where we're using estimations, in fact the opposite is that particular case has been referred to and used in DNA cases as justification

for what they have actually done, and I would refer you in that regard to Wesley, Castro, Spencer, Andrews. He's misunderstanding the difference between an estimation and basing it on something other than data collection. In fact what the Washington case does is brings out - and he's quoted from it and perhaps I'll just refer to it My Lord: "Expert testimony of mathematical probabilities --

THE COURT: What page are you on?

MR. WALSH: Page 23 of the prehearing brief. In referring to Washington he made this -- Mr. Furlotte has referred to the statement:

> "Expert testimony of mathematical probabilities that a certain combination of events will occur simultaneously is generally inadmissible when based on estimations ..."

And what he is actually referring to is his pet theory that please tell me what is the probability that two people - that two hair samples will be found in the same room at the same time. That's the kind of conjecture that the courts and the law will not allow, and the particular reference to the Collins case which is the most famous case in the United States as to the misuse of statistics is a substantiation for that.

The other aspect that I felt was deserving at least of some comment is that he has not addressed the question of the fact that his own expert has apparently endorsed DNA typing and interpretation. That his own expert has endorsed the findings with respect to - or at least the implication would be, and the direct evidence, that he endorses the findings

in this particular matter with respect to the matches, and when it came to the fact of trying to handle or deal with the fact, and I say fact, that his own expert considered that the calculations that he made to mean that the matches demonstrate rarity, in his words exceedingly rare, these particular matches that are shown in VD-88, and when he came to try and deal with that particular issue he makes the statement oh, but we were only using Nichols and Balding for this purpose, to show that there are people correcting, and you cannot -- Really, what he is saying is you can't rely on that. The reliability has not been shown. I don't remember Doctor Shields saying that the Nichols and Balding test was unreliable. In fact my understanding of Doctor Shields' testimony, and I would suggest it's supported by the evidence, is that the particular test that was applied, Nichols and Balding, is in fact a correction factor for substructure, and this comes in conjunction with another statement of Mr. Furlotte that we don't know the degree of substructure and therefore they can't correct for it if that was his meaning. What in fact Nichols and Balding has done is used the highest degree of inbreeding or substructure ever seen in the world, and if that's not overcorrecting I don't know what is. So assuming for a moment that Doctor Shields is not using unreliable formulas and bringing them before the court, keeping in mind that he has used the same test - he applied it in the Passino case, United States, that if in fact the Nichols and Balding test is reliable even applying

Nichols and Balding, and if you look at VD-121 you will see that the matches show rare events - they still show very rare events if you overcorrect to the extreme for any substructure assuming, and I won't get into it, assuming for a moment that you accept that New Brunswick or at least the area where the crime is committed has somewhere between the highest in Europe and the highest ever seen in the world of substructure, but even allowing for that, and even applying that to what he has found by band sharing, the evidence will show that they still come up with exceedingly rare matches. And it would be something that certainly he could give to the jury and demonstrate to the jury but the crown, again, will be content to point out the weaknesses in that particular logic. But even as I say, and I said yesterday, putting Mr. Furlotte's case in its best possible light, that's the bottom line, and you can only draw from it. You cannot, I would suggest, reasonably and rationally suggest that but that isn't the purpose we applied the test when in fact all we want to know about the evidence is what is the effect, not so much what was the purpose of the person calling it. If the effect is to show, which it does, that if we put the defence case in its best possible light, they still have very rare patterns.

With respect to the actual findings of Doctor Shields on the question of substructure and the extent as high as in Europe, Mr. Furlotte didn't seem to have the same reluctance as he had with Nichols and Balding in commenting that perhaps that's not a reliable test to demonstrate band sharing. I under-

stand from the evidence that band sharing is something that's used in multilocus probing, and I understand from the evidence that - and I know that the evidence will confirm that - and I mentioned this yesterday, that Doctor Carmody would not agree that band sharing was an indicator of inbreeding but true excess of homozygosity, if you could truly prove it would be, and that some of the tests that Mr. Furlotte had Doctor Carmody do with respect to comparing bands and bands in that particular type of regard Doctor Carmody's answer was that you're dealing with pathetically small samples to derive any kind of statistically valid conclusions from that. And, again, I am not going to rehash what I said yesterday but it flies in the face of all logic in terms of the scientific opinions that had preceded him, and I ask you to consider his particular experience in this regard and his understandings of human demography.

The other comment that I couldn't help but write down is that Mr. Furlotte has come to the conclusion that after all these days and all this evidence that DNA typing could be equated with a blind man providing eye witness identification. I don't see how he could even possibly get by with that particular statement because it's all rhetoric and certainly not backed up by any logic in terms of -- I suppose it would be if we ignored the five crown witnesses and Doctor Shields and ignored the 150 or 130 exhibits that we have filed it probably it may be a proper statement.

The final comment I wish to make with respect to Mr. Furlotte's summation yesterday afternoon and all this morning and is that the politesse characterization that the crown would give to his summation that he provided to the court in defence of his client is that he has taken the most outrageous liberties with the interpretation of the facts and the law that I have ever seen or ever read about or ever heard in relation to what has occurred here in the last three weeks or so, and because of that I expect, and perhaps unfortunately, it makes it difficult to respond to each and every discrepancy that the crown sees with respect to what he's saying is the evidence that has occurred or has been called in this particular courtroom.

Thank you My Lord.

THE COURT: Thank you very much Mr. Walsh. There were two little points, one in your post-trial brief - or at least the pre-argument brief. At the foot of page 10 - I haven't got it right here - but I think at the foot of page 10 there was a reference there to some of Doctor Shields' testimony and you left the volume, which would be 13, and the page number blank. Could you - presumably you will provide that, will you?

MR. WALSH: Yes. When I wrote that I didn't have the volume but I will include that. I believe it will be in the brief that I filed.

THE COURT: Well, now that concludes the -- You have nothing to add Mr. Furlotte. That concludes it. There were a few other - there were a few points I wanted to bring up just generally here. One is the

-- I am going to take the matter of the voir dire under consideration. We do have the arrangement, as I prescribed yesterday, that I think I suggested that within one week - let's make it 10 days which brings it to a week from the coming Monday which from typing points of view and so on perhaps it's more realistic, you will file your brief but you will be confining it, Mr. Walsh, to the argument that you have made.

MR. WALSH: Argument I made yesterday.

THE COURT: And with the quotations which you referred to but which you didn't read off in your argument. And you will have a copy of that to Mr. Furlotte by say a week from Monday and then Mr. Furlotte will have ten days from that time, if he wishes, to respond to anything in that brief, to file a brief with the crown and with the court. Then Mr. Furlotte had raised yesterday the possibility that he might wish to make oral representations if there are new matters brought up. I said yesterday and I reiterate today that I think it's probably unlikely that that will be necessary but if you do feel, Mr. Furlotte, that you want to bring up - or if the Crown, as far as that goes, feels they want to bring up something further in argument, perhaps you could get together and request of me through the clerk or otherwise and we will arrange at some --

MR. FURLOTTE: My Lord the only reason that I could foresee my wanting to bring it up orally is if the crown submits something in his written brief totally new from what he presented in his oral brief which you already said that you would reject and send it back to him.

So in case you don't reject it and send it back to him then I feel I would have to for the benefit of Mr. Legere make oral arguments so he can hear them also and not just yourself.

THE COURT: Yes. Well you can get together with Mr. Walsh if you feel there are further oral representations required.

There is one other thing that I had mentioned with regard to further matters or further argument and that is that I'm not going to immediately give an answer on this, and I think it's very probable, actually, that I will give my reply at a continuation of this voir dire which will convene immediately after the jury has been selected. I don't think I am going to do it before that. I explained earlier in this voir dire that I am going to leave my options open until the late date because of the possibility that the Supreme Court of Canada or some other appeal court in Canada may bring down judgments or decisions, not only on the -- I think it's unlikely on the DNA matter because there aren't that many matters before the Canadian courts but certainly on the body substance aspect of it. My delay in giving a decision until August 26th or thereabouts hopefully won't prejudice the crown. Certainly as far as your DNA evidence I think you had indicated, Mr. Walsh, that that would come late in the trial; that you would be disposing of the factual evidence before you got into DNA.

MR. WALSH: That's correct My Lord.

THE COURT: Or perhaps Mr. Allman it was but one or the other brought that up.

What I envisage is -- Just on this point of further representations, I would appreciate it, Mr. Walsh, if you would continue to feed to Mr. Furlotte and myself copies of any further - and there will be coming along from time to time - further judgments from the United States courts.

MR. WALSH: That was my intention My Lord.

THE COURT: And Mr. Furlotte get his copy, and if any come to your attention, Mr. Furlotte, that Mr. Walsh hasn't provided if you wouldn't mind doing the same, and you send a copy to Mr. Walsh and a copy to me if you wouldn't mind. If there are, and mind you the Supreme Court of Canada perhaps before August could come down with decisions on body substance questions, I don't think there are any matters before the court now - they delivered 7 judgments yesterday on a variety of things, but if they do I would hope that one counsel or the other would send - or both counsel - would send me copies of the decisions that are relevant. And if counsel feel that they want to make further representations on any new law that developed, again, if you would bring that to my attention we could perhaps reconvene the voir dire hearing at any time, or at some suitable time say in mid August or something like that, and I would hear argument on it. Again, that's probably not going to be necessary but I just wanted to look after the eventuality.

I said earlier in the voir dire that I would enlarge -- Mr. Furlotte had asked that the expert witnesses for the crown on the DNA aspect be excluded from the courtroom while the other witnesses testified or while the -- until they had testified themselves and I refused that application and I indicated at the time that I would give further reasons at sometime in the voir dire. Actually I had intended to do it the same day - later the same day or the next day or something but I haven't done it up until now and I merely say at this stage that I don't think really it's necessary for me to enlarge on the reasons I gave at that time. Whether witnesses are excluded is a matter of discretion, judicially exercised of course by the Court, by the presiding judge, and I felt that an insufficient case had been made out for the exclusion of witnesses at that time. Certainly I have in the past, and will do it in the future, will exclude witnesses in proper situations but I do it somewhat reluctantly, I think, I might say, because the need for it is not always apparent. I quite recognize the fact -- I'm mentioning this because it might - it's likely, I suppose, to come up perhaps at the oncoming trial, again, and I would indicate that I have always made a practice of rather reluctantly granting an application of that nature unless it can be shown pretty clearly that a defendant will be prejudiced by having another witness hear what some earlier witness has said. I take that attitude for a number of reasons. Firstly, in most cases, of course, before a jury there has been a

preliminary hearing and witnesses do know in advance, either through counsel - crown counsel say, or through the witnesses themselves, or through reading the transcript or through being present at the preliminary hearing, they know what other witnesses are going to say anyway. There hasn't been any preliminary hearing here but I understand that statements have been prepared by all the witnesses and I think that those have been given to the other side, or given to the defence, and I would imagine that crown counsel if there are discrepancies between what witnesses are saying these have probably been discussed, quite legitimately, by crown counsel with those witnesses, or certainly will be before they testify anyway.

As far as expert witnesses go I don't think there are as compelling reasons for excluding scientists, people who are going to give expert testimony, as perhaps there are in the case of people who are just going to testify as to factual situations, what time something happened or what the color of the tree was, or the hour, or how long the gun was or something of that nature. That type of thing. If there are individual witnesses which either side wishes to be excluded while some other particular witness testified I would ask you to limit any requests for exclusion to that type of situation.

You know in all my years of experience on the bench I haven't known too many witnesses to lie. Witnesses tell the truth, and I think this is true more perhaps in a jury trial setting perhaps than it is in the provincial courts where perhaps the setting

isn't quite as formal and there is no jury present and so on. I think most witnesses try to tell the truth. It doesn't mean there aren't some who do lie because I've certainly seen it myself, but the number it would be is extremely small or the proportion is extremely small.

There's another factor about exclusion of witnesses, of course, and that is where a witness is excluded while another witness testifies, and if their two stories do match and there's no discrepancy it puts a trial judge in a position when he charges a jury that he's almost got to point out that that evidence of the excluding witness may have greater weight because it matched up than if he had been present and heard it. It's a thing that can back-fire to some extent.

There is another factor in the matter of exclusion of witnesses too and that is that witnesses in a trial share with all members of the public the right to be present at a trial which is held in public and of course when you exclude them you're transgressing on that right and therefore it's not a thing that should be -- The right of removal or exclusion shouldn't be exercised lightly.

Well that's all I have to say about that. I'm just trying to give a little guidance as to what might occur at the trial.

I have got here about the decision on the voir dire. In a normal trial the voir dire would be held during the course of the trial and the crown I suppose in most cases anticipates that the evidence sought to

be entered will be entered, whether rightly or wrongly, and has its witnesses available and I suppose the same would apply here. If the evidence isn't admitted the witnesses can be sent home.

May I ask you Mr. Walsh, or Mr. Allman, it would be my intention as soon as the jury is selected to proceed immediately with the crown's case. If you don't have the answer on the voir dire questions does that pose an embarrassment as far as the first or second day's evidence is concerned? Assuming you will have -- you will have the answer immediately the jury is selected. You're prepared to go right ahead?

MR. ALLMAN: As far as the voir dire aspect of it is concerned I don't think that will present a problem because as Mr. Walsh indicated we will be calling, if we do call DNA evidence and so on, that will be well into the body of the case. I did want to ask Your Lordship what you propose to do about starting the evidence because -- and I have a number of other comments regarding jury selection. You are proposing to start August the 26th. We really don't know how long that process is going to take.

THE COURT: It's going to take one day or two.

MR. ALLMAN: Well, if we have that guarantee, and we really don't. I mean the fact is Mr. Furlotte can take a certain length of time - and I'm not criticizing him - it's we don't know it's going to be August the 26th. We just don't. Or 27th.

THE COURT: Let me say before you go on, unless you have a particular question, but I was going to deal with this matter of what's going to happen on the 26th.

Would you care to have me say that - review that first.

MR. ALLMAN: Fine, because I do have some comments on jury selection.

THE COURT: All right, then you can come along later.

I made a few notes yesterday. I had one note here the agenda for the 26th. The hearing on the 26th, of course, will mainly be concerned with jury selection. I say it may take one or two days. That's an estimate on my part. I have in mind asking the sheriff to call a list of 350 jurors. Now, based on the best statistics, on the election list and the voting list which are now five years old I believe, from which the voters are -- or the jurors' lists are prepared, we would perhaps get 225 or perhaps 250 jurors present at that time. There's no courtroom in this judicial district or in the province where you can get that many jurors at the one time as is necessary into a courtroom and, therefore, I have arranged with the sheriff for the use of the theater in the Oromocto High School which has a large auditorium. I think it's capable of holding 3 or 400 or something like that, and if 250 people from that list turn up that day they can be accommodated there. I don't like sitting in a building like that. This type of thing has been done before. I had a trial in Newcastle once with 8 accused and there were 250 jurors summoned or 200 some, and we sat in the Beaverbrook Hall, or whatever it's called, in Newcastle at that time. I was up on the stage. I forget how we distributed people. Here, what we would do, there's

quite a large stage there, it's only slightly elevated above the floor of the theater and we'll have to arrange for a judge's bench, tables for the clerk and reporter, and a prisoner box, and the jury box can just be 12 chairs with a sort of segregated wall in front of it or something, and two tables for counsel, very much as here, and we'll be conducting the court on the stage of the theatre and the jury panel will be seated down below. The same facilities as we have here will be duplicated there in the high school.

The trial, again, estimates might vary as to how long the trial itself might take. My best guess would be that it will take about 8 to 10 weeks. That is perhaps something less than figures you may have heard earlier. I think the trial should be completed say in ten weeks. But even with a ten week trial, that's over two months, there are an awful lot of people who can't serve as jurors and have to be eliminated because it simply works too much of a hardship on them to serve that length of time, particularly if they have one person businesses or they -- What are they paid now? $25.00 a day is it, or something, jurors are paid. Well, they can't live on that sort of thing, supporting families and so on, and there are a lot of people who will plead that they can't afford the time and will have to be relieved. There will be quite a few others, of course, who will have failed when they were given their original notices that they are on the jury list - would have failed to advise that they are looking after elderly people, or

children who are in school, or who are over-age, and so on, and their names are still on the list, so quite a portion of that 350 would be people who fall into that category and I would like to get them eliminated even before August the 26th. Actually, what I have in mind is preparing with the sheriff some sort of a letter to go out with the jury notices. I wouldn't do this without providing counsel on both sides with a copy of that letter and giving you a chance to object if you feel there is anything in it that there shouldn't be, but it would ask people who would not be qualified to serve, who are over-age, or who are removed from the judicial district, or who are looking after children, or old people, or who have illnesses and can produce medical certificates, I would ask those people to report the fact to the sheriff and provide whatever evidence they can to the sheriff and a ruling can be made or they can be discharged. This is normally done in jury trials anyway. I would like to get rid of those people beforehand. Now, if there are other people who say well I can't serve because I can't afford that amount of time that type of person, in my view, should be required to attend on the 26th and they can present their reasons at that time. I am not going to take it upon myself as a judge to excuse those people. I don't think that's fair either to the crown or to the accused. I don't think a court should be releasing people on that type of ground. There will be people who will raise that objection and I will invite them. As I indicated earlier at the pre-trial hearings, it's my practice to address the jury

panel and to tell them - outline to them how the jury is selected. Talk to them or tell them about the different types of challenges, stand asides, and so on. I will also invite them that as their names are drawn and they are called up to be sworn that they indicate if there is some reason why they can't serve. Presumably at that time we would be down to people who can't serve because they can't afford to take, say, 2½ months to do it, and then we will have to decide. Perhaps counsel can agree. I would hope that counsel could agree if some man says look, I've got a contracting business, there's no way that I can be away from that business for 2½ months in the fall. I would hope that counsel could say well, look, let's let him go. I am sure the crown don't want to use their stand asides up on something like that. It surely could be agreed.

I will be explaining the matter of challenges, and I don't want to get into the matter of challenges now, what type of challenges. I gather that very possibly the defence might wish to exercise challenges for cause which they have the right to do. The trial - the earlier trial in Moncton indicated that reliance was put on the challenge of cause on the ground of lack of - what's it called?

MR. ALLMAN: Not indifferent.

THE COURT: Not indifferent. Lack of unindifference, or whatever it's called, between the crown and the accused. Mind you, it's embarrassing to a defendant in a case like this if the defendant has to challenge every juror as they come forward on the ground of

being not indifferent because what you're doing, you're accusing that juror of being biased, and if there's anything that's going to make a jury biased I suppose it's being accused of being biased.

An accused, here, has what - 12 challenges I think - 12 challenges in this case, and, mind you, if a trial within a trial is held to determine whether a juror is biased or not, or not indifferent, and he's found by the mini jury to be indifferent, is that the word, unbiased anyway, then the accused has to decide well then do we use up one of our peremptory challenges to get rid of this witness. Well, having challenged him and called him biased the accused has almost got to use one of those 12 peremptory challenges up and, of course, there are only 12 of those peremptory challenges.

I would ask counsel to do this. Perhaps you people can get together beforehand. Are there - is there any question that you would like me to ask jurors. They will be called up four at a time. Are there any questions or question you would like me to ask them to narrow down the cases that you might want to challenge on. For instance, there is this question of a publication or something here, the booklet that has been sold - the book that's been sold in the stores. If the Moncton experience is any indication it would suggest that there may be a feeling that that has prejudiced potential jurors.

I think the challenge for cause on the ground of lack of indifference was I think made against a good many jurors there, if I recall correctly, and

I don't -- it's my recollection also from looking quickly at the transcript that actually very few people had read the book or had even seen it. I think somebody had a friend who might have read the book or some darn thing like that.

What I am saying is do you want me to put a question to the jurors: have you read a certain book? Are you familiar with the book? Have you seen it or are you aware of it? Are there questions of that nature you would like put so that that would narrow down the number of cases where you might want to exercise your challenge. You get the point I'm making here anyway, so you people can talk about this. I'm not going to talk to you before that day about it. On that morning if you want to propose that.

I will be instructing - and I'll say this flatly - I will be instructing the jury before the selection and in my general remarks to them that the criterion for jury service - I'll be instructing them along these lines, not using these particular words perhaps, that the criterion for jury service is whether they can act objectively or not. It's not whether they have made up their mind beforehand; whether they have read books and books and books on it or read newspapers one after another; or whether all their friends have told them that an accused is guilty or is innocent; or what opinion they may have formed beforehand; the prime question and the criterion is can they put everything out of their mind that they have heard beforehand and can they decide the case on:

the basis of what they hear in court. And I will say that I have went through quite a few jury trials where it has been demonstrated that jurors are able to do that and I think jurors do take their duties conscientiously and pay attention. I have never -- I think I may have commented earlier in this trial - or if I haven't I certainly have in other trials - that I have never known in over a hundred jury trials that I have presided over, a juror to miss a single word of evidence through any day of the trial. I qualify that by saying that once in Carleton County a fellow sat in the front row and went to sleep but I think he had been to the tavern at noonhour, but that is the only - the only time out of what - twelve hundred jurors that would be represented.

Well, I am not going to say anything more. I am just trying to give you a little general guidance on this matter now. We'll come back -- Do you have any particular questions at this time that you --

MR. ALLMAN: Well, I have some observations to make at whatever time is appropriate.

THE COURT: Well, let me run through the rest of the thing and then we'll come back to anything you have.

On the length of the trial there, bearing in mind the procedures that I have suggested and realizing that there will be perhaps some trials within a trial to deal with we don't know how many jurors, perhaps a few, if the result - if a few are challenged for cause on the ground of indifference the accused runs out of peremptory challenges, standing those people who have been accused of being biased, it's not very likely that the process is going to

continue through very many more people after that. If we can get down through questions asked by the court of witnesses who might have read a book or might have had some reason to be bias or prejudice, a particular reason, then perhaps the trials within a trial can be confined to those people, and if that were the case I would suggest probably there aren't going to be - based on the Moncton experience certainly, and what I have witnessed in Fredericton and my own experience as far as sales of books go and that type of thing, I would say that it wouldn't take very long. So I can't really see -- Possibly the jury will be selected the first day and if not the first day certainly the second day I would say. It could go on longer, I don't know. But, anyway, what I would have in mind is that as soon as the jury has been selected then we would move over here to this courtroom and we would have our -- we would proceed. We would probably resolve into the voir dire which I would give a decision on the voir dire questions at that time, and if it's late in the day well then we'll send the jury home and tell them to come back the next day or if it's early in the day then we'll put the jury in the jury room there while we deal with these other matters and continue on that day. But I do see going on and I think it's likely that there will be Monday, Tuesday -- there will likely be Wednesday, Thursday and Friday of that first week for the crown to call witnesses, so it should have witnesses available from say Tuesday on.

Does that answer your question or do you have something else just on the jury question.

MR. ALLMAN: I have a number of things on the jury question and I don't know when Your Lordship has finished your observations on this so I--

THE COURT: Well, I think I have --

MR. ALLMAN: And I imagine Mr. Furlotte may have some observations to give too.

THE COURT: Yes. Well, I think I have covered everything, I think, that I want to say about jurors. Do you want to speak to the question of jurors?

MR. ALLMAN: Yes, I would appreciate doing so My Lord. First of all with regard to the location arrangement, going over to the high school, I certainly have no observations at all to make on that. With regard to the size of the jury panel, in light of the number of people who may well have valid excuses not to show up, and in light of the fact that there may be some challenges for cause that are successful, I would think that 350 was an appropriate number. I would like to know if Mr. Furlotte has any input that he feels he should make on the number. I wouldn't want a number that he felt was inappropriate. At least I would like Your Lordship to hear if he has any input on that.

With regard to the challenge for cause, and I can be fairly brief about this, there have been a number of recent decisions, Supreme Court of Canada decisions among them, and it's getting to be a fairly complicated branch of the law. Your Lordship's observations are very helpful and I would hope would be correct. A moderate, restrained challenge for

cause I would think could be completed in one or two days. I want to know what Mr. Furlotte's position is on that. I may be being gloomy but I suspect you are going to be faced with a challenge for cause of every single witness - of every single juror. Now --

THE COURT: Well I can tell you there we're not but I mean I'm not ruling on that at the present time.

MR. ALLMAN: I am prepared --

THE COURT: I don't feel that we can put Mr. Furlotte on the spot here today to say what his policy is going to be. He hasn't seen the jury list. I don't think we can --

MR. ALLMAN: I think Mr. Furlotte has indicated before at some of the previous court proceedings that that will likely be his position. I think - and I can be corrected if I'm wrong about any of this - but I think his position is there has been so much pre-trial publicity, including but not limited to the book, that he feels that every juror may be potentially biased and therefore he will wish to challenge every juror. If that is the case - and, as I say, there's a good deal more on this and I'll come back to that in a moment.

THE COURT: Well, if he were to do that wouldn't I have to protect his client against him. I'm not saying this will arise but surely I can't go along letting the defence counsel challenge every juror of being biased and then hoping to get a fair juror.

MR. ALLMAN: I don't want to get into the merits of whether Mr. Furlotte could or could not successfully challenge every juror for cause. Not at this stage at any rate

THE COURT: I think you're being unduly pessimistic here Mr. Allman.

MR. ALLMAN: Everything I have seen so far in these proceedings leads me to believe that we should err on the side of pessimism rather than optimism when it comes to --

THE COURT: I think we have made extremely good progress with the thing. It's been slow and we have taken more time than I had hoped it would require but things have moved along.

MR. ALLMAN: We will have our -- Let me say two things. The first is I would think it might be appropriate for counsel, crown and defence, if they wish, to give you our submissions on the appropriate procedures under the current state of the law for jury challenges. We could do that either orally or if Mr. Furlotte will waive his objection we could do it in written briefs. Whether you want us to do that or not, or whether Mr. Furlotte wants to do that I don't know. It might be. It's just a suggestion.

THE COURT: Well, I have no -- I want all the guidance I can get on the thing. I think I have got a pretty good idea of what the law is on the thing now. As you pointed out there are a number of recent cases on the thing.

MR. ALLMAN: I certainly wouldn't want to try and teach Your Lordship - well I'm sure you know better than we do - the law that --

THE COURT: Oh, I need -- no, I don't really.

MR. ALLMAN: But it never hurts to exchange information.

THE COURT: I'm very humble. Always open to instruction on these things.

MR. ALLMAN: I thought that might be an idea if Mr. Furlotte -- I'd like to know what Mr. Furlotte feels about that.

THE COURT: Well, we'll hear what Mr. Furlotte -- Firstly, Mr. Furlotte, what about the numbers and then, secondly, only if you want to, express an opinion on these other matters. I'm not saying that you have to, but if you can give any guidance or if you can put Mr. Allman's mind at rest on any of these points.

MR. FURLOTTE: As far as on the numbers, My Lord, I suppose it's very difficult at this time for me to tell the Court what the appropriate number would be since I don't know the restrictions that His Lordship might put on my challenging for cause. As you have already stated, you have no intention of allowing me to challenge everyone for cause or a lot of people for cause because you might feel it's in my client's best interests for you to take control of the case. So --

THE COURT: Let me just interrupt there for a minute Mr. Furlotte. What I am saying is that -- Well, let's suppose we don't have these questions. I have suggested perhaps you or counsel can - other counsel can propose questions that I might ask that would serve an advantage for yourself or serve an advantage for counsel on the other side to narrow down the number of people that you might want to challenge, or that you might want to challenge for cause. I can see a definite benefit in that as far as an accused person is concerned.

MR. FURLOTTE: I expect there's going to be a number of questions that once co-counsel, Mr. Ryan, and myself get together that we are going to ask the court probably to put to a jury panel before we even start the selection and I --

THE COURT: I want to make it clear at this time, I'm not offering to ask every question that's proposed by counsel. I am going to reserve the right to myself. I mean the responsiblity of supervising the jury process, the jury selection process, falls on the court and I will reserve to myself the right to decide what are proper questions to be asked.

MR. FURLOTTE: My Lord, with all due respect I've been too busy to even read the Keegstra case yet on jury selection so I don't even know what the up to date law is from the Supreme Court of Canada on my ability to challenge for cause so it's very difficult for me to comment on that at this time. But I can only --

THE COURT: Well, let me say this. Supposing there are no questions asked and supposing on the first juror called, there are 250 jurors to be -- the order is determined of course by drawing the ballots out of the box, the first four are called up and suppose the first one you challenge for cause on the ground of bias. Until recently I would have been inclined to say you have got to establish for me that you have some good ground for bias. I doubt if I'm going to do that on the first one. We'll have a -- I'm just speaking theoretically here now, or academically, we have a - and suppose though you indicate look, I don't know whether this person has read a book that's been published or available for sale, we have a trial.

within a trial, and it's determined and it emerges in the answers from the juror that he has never heard of the book, or he has never read it, or he has never seen it, or he may have heard of it but he has never done anything else, and he has had no particular pressures on him to determine the thing one way or the other, and the mini jury says the challenge is not well taken. You then have to decide whether you are going to challenge him peremptorily. If you don't challenge him peremptorily then he is sworn as a juror. And then the second one comes along and you go through the same thing, and if we get down to six jurors, all of whom have said I have never heard of the book before, well I'm not going to let you go through two hundred and fifty people on the ground that those two hundred and fifty might have read the book. I'm going to say at some stage look I'm going to ask these people, whether their answer is on oath or not, I'm going to ask them have you read the book, and if they say they haven't read the book or they haven't seen the book or whatever and you say well I still want to challenge them I'm not going to permit that challenge to be put to them.

MR. FURLOTTE: No, that's fair comment.

THE COURT: I'm merely giving you an indication now about --

MR. FURLOTTE: That's fair comment. If a person says they haven't read the book then it's pointless for me to even cross-examine or ask him questions about what influence it may have had on him. It's irrelevant.

THE COURT: But I can't see, really, from a practical point of view, there seems to be some fear perhaps on Mr.

Allman's part and perhaps you might feel that there's a possibility yourself even of going through 250 people all with mini trials and --

MR. FURLOTTE: I can tell the court right now as a matter of record, I have had numerous people approach me and whether jokingly or seriously ask if they could get on the jury so that they could convict Mr. Legere. Now I have to protect Mr. Legere against people like that who may be coming to court who would hide their true feelings about the presumption of guilt of Mr. Legere so that they could have the pleasure of convicting him, and I have to protect Mr. Legere against that and I am going to take whatever means is necessary, which --

THE COURT: Yes, but what is your reaction to my suggestion that if you challenge peremptorily on the grounds of lack of indifference all 250 people on the jury panel - in other words you accuse them all of being biased, you are going to come up with 12 -- Well, you don't get through the whole panel because you run out of peremptory challenges and the crown no longer wants to exercise stand asides so the jury gets selected somewhere along the line. You know. The mini jury are not going to find people who say that they can decide the matter objectively and can convince those two jurors that have to determine the thing, they're not going to reject those jurors.

MR. FURLOTTE: Okay. If I am given the opportunity to question potential jurors, which I hope the court will allow me, there is a good possibility that it will never have to go to minijuries and it's only

when I feel very strongly that they are hiding their true feelings about whether or not they can be objective that I will want to put it to a mini jury, and if once it's put to the mini jury, and I felt that strong in putting it to the mini jury, and they say no, it's okay, then I'll probably use my peremptory challenge. But I have to be able to screen those jurors by whatever legal means I have.

THE COURT: Well, I think we have all -- Well, as far as the number is concerned you have no objection to 350.

MR. FURLOTTE: My Lord with your optimism definitely 350 would be sufficient. With my pessimism it may not be sufficient. And you might be quite surprised that maybe under the 250 that show up we may only end up with about 40 because maybe they will all exclude themselves. That they themselves say that they can't be unbiased.

THE COURT: Well, the only reason - you know - I'm not directing or I don't have in mind a panel of 350 because I anticipate any great difficulty in getting a jury. The large number is dictated by the fact there are going to be such a large percentage of people who aren't going to be able to devote that time to it.

MR. FURLOTTE: I agree with that.

THE COURT: If it weren't for the time factor, if this were a one week trial or a ten day trial, I would say look, let's bring in a 100 people and have a crack at it. Mind you, we will have the sheriff with a bus - a couple of buses I suppose, standing aside so that if we run out of jurors he is going to have to go -- or

perhaps the best way to do it would be high-jack a bus coming down from Fredericton and drive them right to the school where we will unload the passengers and decide on that. There is a provision under the law is what I'm alluding to to go out and - what is the expression?

MR. ALLMAN: Talesman.

THE COURT: Talesman. That's it. I did it once in Dalhousie at a trial up there in a criminal negligence causing death trial about 20 years ago and it was at 12 o'clock noon when we ran out of jurors and the sheriff said yes I'll bring in 15 more and he brought 15 more in and the jury was completed. The jury selection was completed shortly after that and the accused, who was probably guiltier than the devil, was acquitted, and it later turned out he had gone to the pub and brought them all in and of course the consumption of alcohol -- He had gone to the tavern and brought them all in from the tavern.

Well, I don't think we can --

MR. FURLOTTE: So there's no way I can agree that that number is sufficient at this time.

THE COURT: Well, I have got to -- I've given you the opportunity to comment on it.

MR. ALLMAN: I will have my witnesses ready for - subpoenaed. We will have witnesses available on August the 28th. I just want to know one thing. Mr. Furlotte resolutely refused to commit himself to what number he expects to challenge so insofar as I am concerned he may be going to challenge 250. He never said he wasn't.

THE COURT: Well, the Court has a little control over what -- I mean the challenges may be exercised but the court has the control over how many challenges are allowed to be tried. And I'm not saying that by way of threat because I'm going to take - I'll certainly bend over backwards in favour to an accused, but if I see that we are going through dozens, as I have said before, of jurors and nothing is being accomplished well we're going to put an end to whatever practice is going on up to that point.

MR. FURLOTTE: My Lord I understood our last day in court that we were going to be selecting the jury during the week of August 26th and start the trial proper on September 3rd.

THE COURT: That got into the newspaper --

MR. FURLOTTE: Have you since changed --

THE COURT: No, that hasn't -- The newspapers have been reporting that but it is nothing I have said.

MR. FURLOTTE: Oh, I'm sorry, I misunderstood that too then. Not just -- I didn't get that --

THE COURT: It was always my intention and I think I -- I hope I've -- We did say -- I may have -- well, I don't know. No. Originally we planned on starting the trial proper on September the 3rd, the day after Labour Day, and then I think at one of the pretrial hearings or in my notes to the pretrial hearing or in something or other, I indicated to counsel that I thought we should start on August 26th because the reason being that a schoolhouse would be available at that time and we would have access to a larger facility for a courtroom at that time. But it was

always my understanding that the whole trial would start on that August 26th date. I do want to get underway that week because --

MR. ALLMAN: Would August 28th be all right to issue my first subpoena for or do you think I should make it the 27th? I mean can we be that optimistic?

THE COURT: I would suggest that you have a token witness available on the 27th.

MR. ALLMAN: Fine.

THE COURT: Mr. Furlotte may fool you completely and pull the rug right out from under you.

MR. ALLMAN: Oh yes. I don't need any for August 26th. I don't need any witnesses for August 26th. I can be safe on that one.

THE COURT: No, I will guarantee you you don't need witnesses for the 26th.

Enough on that point. One other point was the number of expert witnesses. The crown I think - or someone raised - the crown I think raised the question of whether special permission were needed for more than five expert witnesses. I think the Criminal Code confines the number to five, doesn't it. You have put in a brief on this, I think Mr. Allman, earlier. The suggestion - the Court of Appeal said well that doesn't really mean what it says or something.

MR. ALLMAN: Five on a topic.

THE COURT: I can't follow the Appeal judgment. I think if you propose more than whatever number is limited under the Code I think you should ask for permission for that before we start.

MR. ALLMAN: Then I hereby do ask for permission. I don't know what the process is of going through this. I

can't very well tell Your Lordship what my expert witnesses propose to say, obviously. All I can tell you is that we have a number of expert witnesses. Mr. Furlotte knows what all of them are going to say. And we do need them all.

THE COURT: Well, certainly that limit is put on there to ensure that the business of calling expert witnesses isn't abused. If the crown in some particular case were to call witness after witness to prove - all to testify to the same thing the court can point to that section in the Code now and say sorry but you're cut off. I can in a trial of this magnitude insofar as the number of counts in the indictment is concerned and the number of issues before the court, as you have explained it so far in the pretrial addresses and so on, I can't see a court refusing permission to call more than five. How many, Mr. Walsh, if the DNA evidence were allowed, how many witnesses would you be calling on your DNA?

MR. WALSH: Five My Lord.

THE COURT: Five would be the number of your witnesses. How many others have you got? I think it was indicated that you only had 2 or 3 or something more. I think it was indicated earlier that you would only have 2 or 3 --

MR. ALLMAN: Oh no, a good deal more than that. 26.

MR. FURLOTTE: 36.

MR. ALLMAN: There is a problem here. Again, I don't want to get into the merits of it. A number of these witnesses are on matters that I don't know yet whether Mr. Furlotte is going to seriously challenge or not. I could get rid of a great many expert

witnesses, perhaps several expert witnesses at any rate, by agreement. Medical experts as to the cause of death. There's four deceased so that's an aspect. Forensic evidence as to fires. I don't know how much of this Mr. Furlotte is going to challenge. Of my witnesses there are some that are vital to the crown's case and there are others that are establishing things which I don't know whether Mr. Furlotte is going to want us to establish or not. But I can certainly say this to Your Lordship. Let's set aside the DNA for the moment. The crown's respectful submission is that we need all the witnesses, and the proof that we need all the witnesses will be if Mr. Furlotte refuses to concede their evidence. I mean the very fact that he says no, I'm not prepared to concede that indicates it's certainly a live issue. So I was going to mention this. One of the things I am going to do between now and the trial is try and get together with Mr. Furlotte orally or correspondence-wise, and see if we can get some measure of agreement on some aspects. But if we don't then I take that as an indication that all the issues that our experts will be addressing are live issues and we do need those experts. I would point out one other thing. It's not limited to the crown actually. It says neither the crown nor the defence. What it is, as Your Lordship indicates, is to ensure that counsel don't abuse this right. If Your Lordship feels that as we are going through the trial that a certain witness - that that aspect has been so thoroughly covered that you don't feel we should go into it

again, and if that's what we are doing, then fine. But prima facie and at this stage we need, unless it is indicated otherwise, all our expert witnesses.

THE COURT: Well, I don't know - I don't think I do now at this point - you haven't gone over this before I mean at the pretrial hearings or during the voir dire the nature of the testimony. You say there will be four doctors who will testify as to death presumably.

MR. ALLMAN: This is the problem I have with this matter. How do I explain to Your Lordship why I need this witness or that witness without in essence telling Your Lordship what this witness or that witness is supposed to say. Now, if you want -- I mean we can do that but I don't know whether that's appropriate or not. If Mr. Furlotte has no objection we will do that.

THE COURT: Well, why don't you do this. Why don't you and Mr. Furlotte get together on this and see what agreement you can come to. I will simply say I am not going to rule on your application right now. I'm not sure I am even going to treat it as an application but you have made it so it's before the Court. I'm certainly not going to rule on it now and I can't rule on it without really having some indication of just what testimony they would be giving.

MR. ALLMAN: I understand that.

THE COURT: But I think you can agree on most of these. And the general principle that I would apply in ruling on a thing like that would be if there is n¢ undue duplication or unnecessary duplication I would

grant permission.

MR. ALLMAN: Then I don't think we'll have any problem.

THE COURT: And certainly with four counts in the indictment and four different circumstances - at least three, maybe four different circumstances as far as medical doctors and coroners and so on are concerned, certainly you have got to be allowed more than five. That will be -- You haven't anything very much to worry about as far as having the number increased.

MR. ALLMAN: Well, I guess the only thing, I agree with you in respect except that I'm a congenital pessimist. I find that from the prosecution's point of view the best thing to do is always to assume the worst.

THE COURT: I think it's a good policy to adopt. If Mr. Furlotte can convince me after he talks to you or, you know, at the appropriate stage of the trial that the -- and you will want to know early on in the trial, you will want to know right at the start probably, or very close to the start anyway. Perhaps it's a matter that we could discuss at that voir dire that's held immediately after the jury is selected. Perhaps we could deal with it there. I don't think that's a problem you are going to have a great deal of trouble with.

The description of the offence in the indictment was another matter that I had raised myself. I raised the question of whether in charging first degree murder the indictment itself or the wording of the counts should indicate what section of the Criminal Code is relied upon to constitute first degree murder. The Crown put in - was it you Mr. Allman -

put in a brief on that matter at that time. We haven't discussed it further and I haven't had any representation. I think Mr. Furlotte you indicated earlier and perhaps at the hearing on February the 5th here I think you indicated that during the voir dire you might want to make some application. I must say that I don't quite see the problem about it that I suggested earlier might exist. There are two grounds on which the crown I suppose might rely. One would be that the victim in respect of any count was a police officer, sheriff, police constable, warden, deputy warden, and so on. Certainly that isn't what you are relying on in any of these cases here. The second thing would be that it was planned and deliberate and I suppose that is one that you want to keep available and open.

MR. ALLMAN: That's an option we wish to keep open.

THE COURT: The other one would be - and I base this on the preliminary addresses that counsel made on the voir dire here, the other would be on the question of sexual assault.

MR. ALLMAN: And unlawful confinement.

THE COURT: Sexual assault; sexual assault with a weapon; threats to a third party; or causing bodily harm; aggravated sexual assault; or forcible confinement. Those are the --

MR. ALLMAN: Yes.

THE COURT: And you are telling the defence now that those are the --

MR. ALLMAN: I can give you an oral indication now which in my opinion should be sufficient. We are relying in all four murders on planned and deliberate. We are

relying on the female murders on accompanying sexual assault and accompanying unlawful confinement, and in the case of Father Smith we are relying upon accompanying unlawful confinement. Forcible confinement I suppose I should say.

THE COURT: Forcible confinement, yes. Well, Mr. Furlotte, that provides you with the particulars I suppose that you might expect.

MR. FURLOTTE: Yes, My Lord.

THE COURT: The other - another matter was -- Well, the amendment of the indictment. On the amendment of the indictment the crown had indicated - as a matter of fact did move I think at an earlier stage of something or other, the voir dire --

MR. ALLMAN: The situation about that was that we couldn't file an amended -- We had always said that the comparatively minor amendments that we had agreed to would be incorporated in a new indictment if that is what Your Lordship directed, but we couldn't do it until we knew if we had to make major amendments to the indictment. Now, what we will do in light of what Your Lordship just said is we will file a new indictment reflecting the minor changes, at or near, and removing Father.

THE COURT: Well, the normal practice I have always followed in a jury trial is even though an accused has been arraigned at the initial hearing or at an earlier hearing when the date has been fixed for trial, after the jury panel is summoned and appears in the courtroom and the jury selection process is about to begin, I have always followed, and I think most of the judges have, the practice that the accused be rearraigned

even if the indictment is the same. If there's a new indictment or a changed or revised or amended indictment then he is rearraigned on the amended indictment, and what I propose to do here is as soon as we assemble on the 26th the first item would be that the crown would announce that it has an amended indictment to substitute for the original indictment and you would move - the crown would move that the accused be arraigned on that indictment and the arraignment would then take place.

MR. ALLMAN: We will do that.

THE COURT: As I understood, the changes that you proposed were 'in or about' - on or about a certain date or in or about a certain --

MR. ALLMAN: I think 'on or about' is already in. I might be wrong but I stand to be corrected. So that the indictment as it ultimately finishes up, the amended indictment will say 'on or about', 'at or near' and omitting Father.

THE COURT: Right. The exhibits I don't think should stay here all summer or until the trial so Mr. Pugh would you as clerk please take charge of the exhibits and take them to the exhibit room in Fredericton where they will be safer there than they are perhaps here. And if counsel do require to see any of the exhibits that can be arranged through Mr. Pugh. That probably isn't necessary. I think you have probably got copies of most of the stuff.

MR. WALSH: My Lord the exhibits, if I may, the one that has been marked - the summary chart, VD-88, obviously is an exhibit. The other demonstrative items are marked but they were not formally entered into

evidence because we had paper reproduction of them entered into evidence. Do you wish the clerk to take those or do you wish me to take custody of those?

THE COURT: I think perhaps we will ask the clerk to take them. Are these actual exhibits? These aren't exhibits. These are just copies.

MR. WALSH: They are copies. The exhibits were filed as paper reproductions and marked and we used the charts just for easier reference during the trial.

THE COURT: Well, I wonder would the clerk mind taking these and perhaps you could put them in the chambers room in back here and they may be of some help to me in the summer when I do some work on this thing.

Another point I was going to raise is we are not going to have quite as much room here once the jury gets selected and when they sit here, and if you have easels and so on whether for the same exhibits or others, you're probably going to have to use a space over here for the easels. Do counsel have any suggestions on that point? The screen can still go up here if it's to be used, either for the same or other purposes. The easels can go there I suppose at the end. Do counsel see any problems with that?

I don't think I have very much -- There is one point I wanted to mention and that is the matter of television cameras which are the bane of my existence. Television camera people take advantage of every situation. I was watching TV earlier this week on a trial in Newcastle and television camera people shoving their cameras in the face of witnesses going in and last night following the jurors out of the courtroom. Well, I'm not going to tolerate that sort

of thing here. And another thing, of course, is molesting an accused person. I don't think an accused person who has no way of getting out of the way of television cameras has to be subjected to that against his will at least, and I earlier felt that perhaps television cameras should be banned from coming on the courthouse property here. In other words they should stay out at the road or beyond the parking lot there. I'm not going to prohibit them from taking pictures on the public highway or something like that but I am not going to have them sticking cameras in -- Certainly in the courtroom they're absolutely - or the courthouse they're absolutely banned here, but I'm not going to have them sticking their cameras in the face of witnesses or in the face of jurors or in the face of the accused if he doesn't wish it. And so if counsel do have any representations they want to make to me in that regard I'm open to it completely.

MR. FURLOTTE: My Lord I would consent if you would keep them on the other side of the bridge.

THE COURT: Or throw them off the bridge.

MR. SLEETH: With heavy weights and chains.

(Laughter.)

MR. ALLMAN: I have absolutely no comment to make at all on the question of the media.

THE COURT: You're afraid they're not going to give you coverage.

MR. ALLMAN: I have absolutely no desire for media coverage. I have a couple of minor things when Your Lordship is finished. One of --

THE COURT: Just on --

MR. ALLMAN: I have a minor thing relating to this. I believe there is a TV camera in the courtroom for a monitor in a room outside for Mr. Legere. Is that going to be done or something of that kind? I thought at one time that was suggested.

THE COURT: There are monitors outside under the end of the building.

MR. ALLMAN: Okay.

THE COURT: There are no other --

MR. ALLMAN: I thought that suggestion had been made at one time but if it hadn't then I have no views on it.

THE COURT: I think as part of the security arrangements they have a - I think - I don't know where they are. I think there's one camera out under the eaves out here and there's another camera I believe under the eaves somewhere else in the building --

MR. ALLMAN: I wasn't talking about cameras outside the courtroom. I have no views on cameras in the courtroom.

THE COURT: There are no cameras?

VOICE: No.

THE COURT: On the question of cameras, last night I saw on television, the late news, the New York Times is even pressing to have a camera cover the electrocution of a convicted person in the United States, California. I don't know - that seems to be -- All they want to do is provide entertainment, really. I respect the right of people to know what's going on and to follow and so on but it seems to me it's terrible abused.

In our sister province of Nova Scotia they seem to have absolutely no control over what the cameras do down there. They poke their cameras through the doors of courtrooms - into courtrooms. I think there was a report from Newfoundland a week or so ago where they had the cameras right in the courtroom.

Well, look, those are the only points I think I have to bring up. Counsel have anything else?

MR. RYAN: Yes, My Lord, if I can jump ahead of Mr. Allman there. My Lord given the length of the voir dire and now that we're well into June it had been the intention of defence counsel to put forth at least two motions with respect to very serious aspects of the case preferred by the crown, and those motions effectively are going to be, (1) with regard to a stay of proceedings, and that had been indicated by Mr. Furlotte well early on, and possibly another motion, and more likely probably another motion, with respect to severance of counts. I think that Mr. Furlotte and I have briefly discussed this over the last week or two and I think that it would probably be best to put both of those motions this summer prior to the jury selection. So I was going to ask the court for some sort of guidance with respect to timing of those two motions. Now whether we're able to do them at the same time or individually I'm really not sure yet.

THE COURT: Well, let's just talk about severance of counts first. That can be done in chambers actually. It doesn't have to be done in court.

MR. RYAN: No, and that could be fairly fast - I would think. It wouldn't take a very long period of time to prepare for that.

THE COURT: No. Although, mind you, if we're meeting for some other purpose - I mean if there's a court session for some other purpose it can be done at that as well. The other thing would be the other one of stay of proceedings that would be on the ground of what?

MR. RYAN: Well, unfortunately My Lord, I'm looking at now at about six different issues with respect to the stay, including the indictment itself being a preferred indictment without any preliminary hearing ever having occurred. In any event Mr. Furlotte and I have not had the time to strategize that particular motion but there will be definitely a motion with respect to the stay of proceedings, the effects of the media - and when I say media I mean the multi-media during Mr. Legere's escapades and supposed escapades while he was on the loose and what occurred afterwards. There are other items which are not as broad and as serious but I think that a number of them will take me some time to prepare and we're just at the discussion stage right now.

THE COURT: Insofar as a stay of proceedings goes, there is the Vermette decision --

MR. RYAN: Yes, My Lord, and then I've briefly --

THE COURT: If it's a matter of - you know. The Vermette decision says you can't question whether you can get an unbiased jury or not until the actual time comes for selecting the jury.

MR. RYAN: Until the actual time comes, that's correct, My Lord, but there are some very strong indications beyond the Vermette decision as to a very unique situation that we have in this case and I think I

want to be able to explore that and see if that is the case or not before I can even say that look, we're limited.

THE COURT: Well, if you were going to make --

MR. RYAN: Basically, I guess what I was going to ask you My Lord is I know everybody here has probably got some time scheduled off in the summer. I don't want to be an inconvenience to anybody. If there are matters where Mr. Allman and myself or Mr. Sleeth and myself could in courtroom number seven in Fredericton or someplace, instead of having all five counsel present, to argue these motions, perhaps one from one side and one from the other side, and Mr. Legere it's available for him to attend to argue those motions at a location where we wouldn't have to have full regalia. Because I don't think either one would be taking an extremely long period of time to do because with the pieces -- most of the motions will be done in affidavit form and paper form prior to argument.

THE COURT: Well probably they would probably be heard here actually. It's not inconvenient to come here.

MR. RYAN: That's no problem.

THE COURT: Well, assuming that those applications were going to be made you would have made up your mind by what - July 1st or --

MR. RYAN: Oh yes, I think so. Within the next two weeks we are going to know exactly where we are and how we are going to proceed.

THE COURT: Well, from your point of view I understand you are quite content to have them done in chambers or are

you suggesting --

MR. RYAN: The severance count I think you are quite right, My Lord, that could be done in chambers as it was set up but I would prefer to do both of them at the same time, and in open court because the --

THE COURT: Well even the other one is -- I'm assuming here you're within time in doing it.

MR. RYAN: It may be broader than what you -- Yes. And the second - or the stay of proceedings motion, My Lord, may take considerable scope. It may -- I may be expanding it between now and the next two weeks as to what I know exactly what I am going to do and how I am going to phrase my material.

THE COURT: But if you have made up your mind by the end of June as to whether you were going ahead with it or what you were going to do in that regard then you would in any event be ready what - within two weeks of that or -

MR. RYAN: 2 or 3, yes My Lord.

THE COURT: Two or three weeks.

MR. RYAN: Yes.

THE COURT: Say by the end of July.

MR. RYAN: By the end of July, yes My Lord.

THE COURT: Do the crown have any observations to make on these points?

MR. ALLMAN: Yes, My Lord, certainly. First of all, I'm grateful to my learned friend for giving these indications as to what he plans to do. Given that they're not that precise I would hope that when he does have them down precisely we will get them in writing and accompanying the written notice of motion will be adequate data, affidavits or whatever

that are intended to be used, and at a time fixed. So these are purely hypothetical but let's say that he's decided by July 1st I would hope to get the notice and the affidavits and a hearing date two weeks later - something. Ten days later or something of that kind. But we do want adequate notice and the nature of the motions and the information relied upon for them. I would hope that the same would apply to any other motions that may come up at any time from either side. If we have motions to make in advance of trial we will give adequate notice and I am sure the defence will do likewise.

THE COURT: Well, it's rather difficult to know at the present time as to whether this must be done in court or not.

MR. RYAN: Yes, I understand that. Actually, I think My Lord maybe I was just going to try to get an indication from the court if there was some time period that was definitely not available to the court, to the crown, to the defence, so I wasn't going to be targeting in an area where --

MR. ALLMAN: If there's a week that's bad for us it's the week when the Canadian Bar Criminal Law Conference is here and I'm booked to go to that. That's the week of the 15th to the 20th of July.

MR. RYAN: It wouldn't be any time before that, I'm sure of that.

THE COURT: So it would be after the 20th.

MR. RYAN: I'm sure of that My Lord.

THE COURT: Well, I'm available the whole time.

MR. ALLMAN: Somewhere around the 23rd, 24th, 25th, 26th, 29th and 30th July would be fine provided we had

adequate notice that it's coming down.

THE COURT: My wife gets awfully angry if I book anything for her birthday but I forget when it is.

Well, did you have another point you wanted to raise Mr. --

MR. ALLMAN: The point I was going to make was to see if there were any additional motions. I now know there are. And to make sure that we got adequate notice of what they were and when they were coming and we did get that so I'm happy.

THE COURT: Well, on this matter, just to sort of pin it down a little better, you will have made up your mind by the end of June.

MR. RYAN: I'm certain about that My Lord, yes.

THE COURT: And then so that you can give the other side and perhaps the court some notice by letter, even, perhaps some preliminary notice anyway of what is required. And then you would after that be --

MR. RYAN: Filing a Notice of Motion.

THE COURT: Be preparing your affidavits and so. But that's going to take a little while after July 1st.

MR. RYAN: I would think so My Lord.

THE COURT: But you would be preparing your formal notices then and I suppose the only thing - you want to incorporate a date in that don't you.

MR. RYAN: Well what I was thinking, My Lord, is that if I - but, again, I may be rushing ahead of myself or slowing myself, I'm not sure, but my thought was that perhaps that an informal notice three weeks before I was looking for a hearing date and then perhaps a week from that I would be able to give a full 14 days notice of exactly everything that I was going to be

entering as documentation for the motion itself. But what I was trying to make sure of is that I didn't target something where everybody was going to be gone away, and basically yourself. The crown I was hoping that with the three of them somebody would be available. But I didn't want the court to be absent and I wanted to ask that now.

THE COURT: No. No, I'm available any time. Well, is there anything else to be -- I think I have covered all my points that I -- Yes, I have covered everything I had in mind.

Well, look, I'm going to adjourn until August 26th at 9:30 at the improvised courtroom in the Oromocto High School subject to the right to reconvene the voir dire, or the trial, before that.

MR. ALLMAN: There was one thing we discussed and I don't know if we ever came to a conclusion on it. Does Your Lordship have any views on crown counsel and defence counsel, if they both wish to do so, providing you with a brief on jury challenge procedures as they now stand?

THE COURT: Well, I have no objection if you want to provide me with a brief provided you give one to Mr. Furlotte or Mr. Ryan -- whoever is going to be concerned with it there. Do that, and if the defence wishes to do the same thing either before or after having received the crown's brief defence counsel may do so. I would like to have it sort of early August or, you know, in time to look at it. I do have a couple of weekend appointments or things I'm tied up in but I think they fall on Fridays and Saturdays and that sort of thing in August. Regimental reunions and that type of thing.

Okay. Nothing else?

One other thing just before we adjourn and that is I want to, for the record, compliment the court reporters on the excellent job that they have done up to this point in the trial. They have turned out the material quickly. I think it has been a big help to counsel. I haven't had a great need for referring to the transcripts although I have read some of it and I have certainly read enough to know that a really professional, accurate job I think has been done on all the work so far, and I'm sure counsel agree with me on that.

MR. RYAN: Oh yes, certainly My Lord.

THE COURT: Mrs. Brewer if you would convey that to the Chief Reporter and to your colleagues. When the trial itself gets underway I'm not sure that -- Well, we'll be hoping to have the transcript available with the large number of witnesses sort of within three, four or five days or within a week anyway of when it's done. We may not be able to expect it quite as quickly as we have done here because there are going to be other trials in process and the number of reporters has been cut down and it's going to be quite a test to see if they can keep up with the work.

Okay.

(COURT ADJOURNS - 3:55 A.M.)

IN THE COURT OF QUEEN'S BENCH OF NEW BRUNSWICK

TRIAL DIVISION

JUDICIAL DISTRICT OF FREDERICTON

BETWEEN:

HER MAJESTY THE QUEEN

- and -

ALLAN JOSEPH LEGERE

AFFIDAVIT

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.

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 19th day of June, 1991.,

BEFORE ME:

Elizabeth Nicholas

A COMMISSIONER OF OATHS
Being a Solicitor

[signature]

SCHEDULE "A"

RECORDING OF EVIDENCE BY SOUND RECORDING MACHINE ACT

CERTIFICATE

I, Verna Peterson, of Fredericton, New Brunswick, certify that the sound recording tapes labelled #1, #2, #3, #4, J.D., R. v. Allan Legere, June 6, 1991, Voir Dire, initialled by me and enclosed in this envelope are the record of the evidence (or a portion thereof) recorded on a sound recording machine pursuant to Section 2 of the Recording of Evidence by Sound Recording Machine Act at the voir dire held in the above proceeding on the 6th day of June, 1991, at Fredericton, 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, N. B., the 6th day of June, 1991.

Verna Peterson

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

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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.

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 24th day of June, A.D., 1991.

BEFORE ME:

A COMMISSIONER OF OATHS

Commissioner of Oaths
My Commission Expires
December 31, 1995

Dolores M. Brewer

SCHEDULE "A"

RECORDING OF EVIDENCE BY SOUND RECORDING MACHINE ACT

FILE:

CERTIFICATE

I, Dolores Brewer of Fredericton, New Brunswick certify that the sound recording tapes labelled:

HER MAJESTY THE QUEEN

- and -

ALLAN JOSEPH LEGERE

(Tapes #1- #5, Judge Dickson, Voir Dire)

initialled by me and enclosed in this envelope are the record of the evidence (or a portion thereof) recorded on a sound recording machine pursuant to Section 2 of the Recording of Evidence by Sound Recording Machine Act at the Trial held in the above proceeding on the 7th day(s) of June, A.D., 19 91 at Fredericton, 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 7th day of June, A.D., 19 91.

Dolores M. Brewer

Dolores M. Brewer