Commission of Inquiry into the Decline of Sockeye Salmon in the Fraser River

Commission d'enquête sur le déclin des populations de saumon rouge du fleuve Fraser

Public Hearings

Audience publique

Commissioner

L'Honorable juge /
The Honourable Justice
Bruce Cohen

Commissaire

Held at:

Room 801
Federal Courthouse
701 West Georgia Street
Vancouver, B.C.

Friday, August 26, 2011

Tenue à :

Salle 801
Cour fédérale
701, rue West Georgia
Vancouver (C.-B.)

le vendredi 26 août 2011

Errata for the Transcript of Hearings on August 26, 2011

Page	Line	Error	Correction
32	14	thickness of the fish	fitness of the fish
36	14	no population, in fact	no population impact
50	31 and 40	known hypothesis	null hypothesis
57	4	Rick Beamish	Dick Beamish

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APPEARANCES / COMPARUTIONS

Brock Martland Kathy L. Grant	Associate Commission Counsel Junior Commission Counsel
Mitchell Taylor, Q.C. Jonah Spiegelman	Government of Canada ("CAN")
Clifton Prowse, Q.C. Tara Callan	Province of British Columbia ("BCPROV")
No appearance	Pacific Salmon Commission ("PSC")
No appearance	B.C. Public Service Alliance of Canada Union of Environment Workers B.C. ("BCPSAC")
No appearance	Rio Tinto Alcan Inc. ("RTAI")
Alan Blair Shane Hopkins-Utter	B.C. Salmon Farmers Association ("BCSFA")
No appearance	Seafood Producers Association of B.C. ("SPABC")
Gregory McDade, Q.C.	Aquaculture Coalition: Alexandra Morton; Raincoast Research Society; Pacific Coast Wild Salmon Society ("AQUA")
Tim Leadem, Q.C.	Conservation Coalition: Coastal Alliance for Aquaculture Reform Fraser Riverkeeper Society; Georgia Strait Alliance; Raincoast Conservation Foundation; Watershed Watch Salmon Society; Mr. Otto Langer; David Suzuki Foundation ("CONSERV")
Katrina Pacey	Area D Salmon Gillnet Association; Area B Harvest Committee (Seine) ("GILLFSC")

APPEARANCES / COMPARUTIONS, cont'd.

No appearance	Southern Area E Gillnetters Assn. B.C. Fisheries Survival Coalition ("SGAHC")
No appearance	West Coast Trollers Area G Association; United Fishermen and Allied Workers' Union ("TWCTUFA")
No appearance	B.C. Wildlife Federation; B.C. Federation of Drift Fishers ("WFFDF")
No appearance	Maa-nulth Treaty Society; Tsawwassen First Nation; Musqueam First Nation ("MTM")
No appearance	Western Central Coast Salish First Nations: Cowichan Tribes and Chemainus First Nation Hwlitsum First Nation and Penelakut Tribe Te'mexw Treaty Association ("WCCSFN")
Brenda Gaertner Crystal Reeves	First Nations Coalition: First Nations Fisheries Council; Aboriginal Caucus of the Fraser River; Aboriginal Fisheries Secretariat; Fraser Valley Aboriginal Fisheries Society; Northern Shuswap Tribal Council; Chehalis Indian Band; Secwepemc Fisheries Commission of the Shuswap Nation Tribal Council; Upper Fraser Fisheries Conservation Alliance; Other Douglas Treaty First Nations who applied together (the Snuneymuxw, Tsartlip and Tsawout); Adams Lake Indian Band; Carrier Sekani Tribal Council; Council of Haida Nation ("FNC")
No appearance	Métis Nation British Columbia ("MNBC")

APPEARANCES / COMPARUTIONS, cont'd.

No appearance	Sto:lo Tribal Council Cheam Indian Band ("STCCIB")
Steven Kelliher	Laich-kwil-tach Treaty Society Chief Harold Sewid, Aboriginal Aquaculture Association ("LJHAH")
Krista Robertson	Musgamagw Tsawataineuk Tribal Council ("MTTC")
No appearance	Heiltsuk Tribal Council ("HTC")

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Vancouver, B.C./Vancouver
(C.-B.)
August 26, 2011/le 26 août
2011

THE REGISTRAR: The hearing is now resumed.
MR. MARTLAND: Mr. Commissioner, as we resume today, one easy, and one more complicated item of housekeeping, I hope. The easy one should be the PPR, the Policy and Practice Report we've referred to. Yesterday it wasn't available. You'll see before you the Policy and Practice Report on Aquaculture Regulation B.C. I'm going to ask that that please be marked as the PPR in these proceedings.
THE REGISTRAR: That'll be PPR number 20.

PPR 20: Aquaculture Regulation B.C.

MR. MARTLAND: The second item of housekeeping comes out of the question of the databases, the data, really, that were provided in relation, Mr. Commissioner, to your ruling in December of last year. We've had a series of discussions with counsel with the view of trying to see where -- ultimately what people's positions are in terms of Mr. McDade's request that what I'll call very broadly "the data", but what "the data" refers to is really the data that Dr. Korman's report describes and what he used in his report and his analysis is what we're referring to with that for this purpose.

Through those discussions - and I welcome counsel to suggest otherwise if they take a different view - but through those discussions, I don't understand document-holders to object, vis-à-vis B.C. Salmon Farmers or Canada, in terms of the data being made an ordinary exhibit, which is to say a public exhibit before you. So I'll be looking to do that in a moment.

Indeed, because Mr. McDade has made the request, he has the footing for making the request and asking to have this material put in evidence. Given the support that a number of participants - not all but a number of participants support his position - our view as Commission counsel wishes to facilitate that and have those materials put

in.

The objection that we did hear through our discussions was made on the part of the Province, and is made in relation to the data that the Province provided to the Commission and that was used in Dr. Korman's report. So with respect only to the Province's materials, as Commission counsel, we're trying to balance the need to keep these hearings on track. We're on the fifth day of disease and aquaculture; we are on track. I don't want to see that fall off the rails. So I do not want to lose an hour or more of time today with objections and skirmishing over this question of the documents.

In light of that, given the objection raised, it's important to participants, some participants, that the data from the Province be properly in evidence before you as an exhibit so that they can use it for questions and ultimately can use it in submissions to say what they like about it.

Our proposal as Commission counsel is to try and strike a balance between recognizing that there are real objections that the Province makes and, on the other hand, that there's a number of reasons why this ought to be in evidence before you.

What we're proposing, Mr. Commissioner, is that the data from the Province be made an exhibit proper, but not made a public exhibit as has been the ordinary course for exhibits through this hearing, but made a non-public exhibit, not permanently so but simply in the short term. What we're proposing is really analogous to what Rule 17.2 governs with respect to application materials which is that, in the short term while this question over the objection is under consideration by you, what would happen is that the Province's data would be put into evidence as an exhibit but marked as a non-public exhibit.

We would propose a timeline for the exchange of submissions. We'd suggest the Province objection to the data being public would make written submissions by two o'clock on Tuesday, August 29th; that all participants, including Commission counsel, would provide any written response by two o'clock on Thursday, which is September 1st; the Province would be able to reply

to those submissions by Friday, two o'clock on the 2nd.

At this point I haven't seen the -- I haven't read or understood all of the Province's objections. I do expect Commission counsel may well be supporting Mr. McDade and other participants in asking that these materials be made public, but I'll obviously look to read what submissions they have. That's based simply on our discussions to this point.

What that would do, Mr. Commissioner, is simply take this question over whether the province's data should be public or private -- it would be an exhibit, it would be in evidence. It would take the question of public or private offstage in terms of the hearing process today. That would allow for that question to be resolved by way of an exchange of written materials. So that will be my proposal.

Now, vis-à-vis that proposal, I expect one or more participants may disagree with what I've outlined. I don't want to lose too much time arguing about the process here either, but on the other hand, before you agree to that, I think it's fair that you hear what those concerns are.

MR. TAYLOR: If we're going to go in the order that the participants are numbered, I'll go next. Mr. Martland said that we, amongst others, don't object. I just want to make it clear. While I don't have complete clarity what documents we're talking about, although Mr. Martland very kindly last evening sent a note to counsel which has attached to it some lists which is tremendously helpful, so I appreciate that. At the same time, it's a long list and I've only glanced at it.

Not objecting is on the basis that, as I understand it, none of the documents in question are sourced from Canada, so it's on that basis that we're not getting involved, if I could put it that way, in a big way. If I'm wrong on that, I'll be sure to want Mr. McDade or Mr. Martland to correct me.

Having said that, the normal process is as I outlined it yesterday, and I'm not going to go over that again. I'll just leave it at that.

It's our further position, though, that only what went to Dr. Korman should be what's being

talked about here, and not everything on Mr. McDade's list went to Dr. Korman. More specifically - and we'll come to this sometime, I guess - numbers 3, 4 and 20 didn't go, I'm told. On the other side of the coin, Mr. McDade hasn't sought to put in everything that went to Dr. Korman. There's other things that went to Dr. Korman too that Mr. McDade is not pursuing, it seems.

You'll hear from the Province. They're going to raise a particular concern and I only note that the concern that they raise, which has to do with voluntary disclosure and chilling effect on voluntary disclosure - I'll leave it to them to describe - is a valid concern and certainly the Government of Canada would be concerned about that too. It doesn't apply, as I say, to us in this particular context, but if you are known as a government to give out documents, there can be some trouble that arises when you're trying to get people that you're regulating to give you information. So I only say it that it's a concern even though it doesn't arise with our documents right here, 'cause we're not involved document-wise. Thank you.

MR. MARTLAND: I think it may be the first time I've corrected Mr. Taylor, but I think there indeed are two Canada databases that are part of what were described in our letter of last night and propose today. They deal with Atlantic escape records.

MR. TAYLOR: All right. Well, that's interesting. I'll have to revisit what I think on those documents then. I don't know what...

(OFF-THE-RECORD COMMENT BY MR. SPIEGELMAN)

MR. TAYLOR: We're fine. Not to take away from the general principle, however.

MR. MARTLAND: Next time I'm going to call on Mr. Spiegelman to answer.

MS. CALLAN: (Microphone not on) Sorry about that, Mr. Commissioner. Callan, C-a-l-l-a-n, initials T.E., appearing on behalf of Her Majesty the Queen. The Province is in agreement with the proposal put forward by the Commission counsel.

Our concerns are twofold: There's an interest of public -- there's a public interest

issue that arises with respect to voluntary disclosure, and the Province, in its role as a regulator, regulates a number of other farmed animals including cows and chickens and pigs. In addition, it receives all -- does all of the diagnostic testing still for fish in the Province for DFO.

Our concern, and it's raised by page 94 of Dr. Stephen's transcript, is that if the Province is known to give out confidential information and not try to protect it, then voluntary disclosure could stop occurring. This is a concern because there've been outbreaks in the Province before, as avian influenza, and there are a number of diseases that we monitor and track. The Province wants to know and get voluntary disclosure in a timely manner by the farmers that they have a problem, and if their farm source data weren't protected, there's a risk that they'll stop reporting and timely disclosure won't occur.

Our public interest grounds is that we really want to make sure that this data is confidential so we can facilitate a good environment to keep the farmers reporting every time there's a problem and this goes to the veterinarians as well.

The second issue that arises is Dr. Marty is in the middle of publishing a process paper, so he has created a novel way to coordinate all of the histo-pathology reports, and this is unique to Dr. Marty. One of -- some of the documents that are referred to that went to Dr. Korman employ this method, and it's in the middle of being published. It won't be publishable if it becomes a public document. So the Province is concerned on those grounds.

But if it does remain private, it's something that the Province is willing to share with the Commission because they are good-news documents. It's not that these are bad documents that are at all going to be found to show criticism on the Province's behalf. It's that we're really concerned about these two issues, and on those grounds, we're going to be objecting.

But we do support the Commission's position and think it's a fair middle ground.

MR. BLAIR: Good morning, Mr. Commissioner, Alan Blair appearing for the B.C. Salmon Farmers' Association

and with me today is Shane Hopkins-Utter, my associate.

On perhaps a lighter note, I will say that on the issue of public and private, the greatest concern I've heard expressed today is that Mr. Martland is now going public, rather than private, with his scorecard by the number of times Mr. Taylor's made an error. I think he's just at one. I would ask Mr. Martland to be private if he has a scorecard on the rest of us.

More seriously, B.C. Salmon Farmers' position, Mr. Commissioner, is that the salmon farming documents can be made public and we don't oppose them being marked as a full exhibit. They've been used by the authors of the various reports here today. Dr. Korman and others have looked at them in some detail and we think it's in the public interest that the public has access not just to the reports, but to the underlying documents. We haven't been able to come forward with that position in part because of the issue around public and private, and the various concerns of some of the levels of government has been something that my client has been respectful of, and it seems as though today we may have worked out a procedure to deal with, in particular, the province's concerns which certainly make sense on a chill effect argument, as Mr. Taylor outlined it.

So we have no opposition to B.C. Salmon Farmers fish health database becoming a full exhibit. Thank you.

THE COMMISSIONER: Thank you.

MR. McDADE: Mr. Commissioner, for the Aquaculture Coalition. Our concern here is the public nature of the inquiry. This is a public inquiry. It's been closely followed by a large number of members of the public. They are not all in the gallery here. They look to the Commission website to be able to look at these exhibits and form their own conclusions.

The idea of an exhibit that's somehow private, but not for identification, is a novel one to me, and it's a novel one for this hearing. It's simply unacceptable. In my respectful submission, that's not appropriate for a public inquiry, and the idea that we're going to stretch

that out in secret or private written submissions, and not know anything for a week or two about where this is going until this panel is gone is, in my submission, unacceptable to us. We need a decision on this now.

What I really wanted to say, though, is I don't understand the Province's objections. The documents that we're trying to submit fall into two categories, as I understand it. Category 1, and the primary category that we're seeking, is the audit results. Those are not voluntarily disclosed from the fish farms. They're the provincial audit results. So that whole question about somehow people won't cooperate with us is simply off the table there.

The second category of documents that it might apply to are the documents that come from the B.C. Salmon Farmers Association. The B.C. Salmon Farmers Association are standing up staying, "We don't object to them going in."

So my friend's objections clearly have some other route to them, or -- simply unacceptable. We're looking at a very significant problem here. These disease records hold the key to it. The derivative - as you've heard already in chief - the derivative reports are not able to tease out on a global level the actual implications. We have to look at these in detail. I cannot, in 75 minutes that the Commission has given me to cross-examine, go through those records in an adequate way through these witnesses. The records have to be a primary exhibit, and they have to be one that the public can look at for it to make up their own minds. Thank you.

THE COMMISSIONER: Thank you very much, Mr. McDade. Mr. Leadem, do you have something to add?

MR. LEADEM: The only part that I wanted to add, other than to support Mr. McDade with respect to his submission, is that I want some clarification from the Province exactly who is asserting his public interest, and I want that on the record, because there are some legalities associated with that. So I want them to stand up and say -- if it's the Province that -- the Province, Her Majesty The Queen in Right of the Province of British Columbia, that is making this assertion, then so be it. But there are some ramifications to that.

THE COMMISSIONER: Yes. Thank you. What we're going to do this morning is to follow Commission counsel's proposal, and I want to go back to the commencement of this panel yesterday. There were some exhibits, Mr. Martland, that were marked for identification purposes that I think should now be marked as exhibits. Let's start with the knowns. There was Dr. Korman's report which I think was SS for identification. That will be marked as an exhibit.
THE REGISTRAR: That will be marked as Exhibit 1543.

EXHIBIT 1543: Project 5A June 2011 Salmon Farms Korman - Final, formerly marked SS for identification

THE COMMISSIONER: TT was the errata sheet, I believe.
MR. MARTLAND: TT, I have a note, Mr. Commissioner, that was the Excel spreadsheet that Dr. Korman prepared and relied on for his database.
THE COMMISSIONER: All right. Thank you.
MR. MARTLAND: For his report.
THE COMMISSIONER: That will be marked as the next exhibit, then.
THE REGISTRAR: That will be marked as Exhibit 1544.

EXHIBIT 1544: Spreadsheet prepared by Josh Korman, formerly marked TT for identification

THE COMMISSIONER: Then - correct me if I'm in error - there were other documents marked. I believe Dr. Connors' report was marked VV for identification.
MR. MARTLAND: My note is UU.
THE COMMISSIONER: I'm sorry, UU.
MR. MARTLAND: Yes.
THE REGISTRAR: UU will be marked as 1545.

EXHIBIT 1545: Technical Report 5B - Examination of relationships between salmon aquaculture and sockeye salmon population dynamics, formerly marked UU for identification

THE COMMISSIONER: Again, there was a supplemental document marked VV of Dr. Connors.
THE REGISTRAR: That will be marked as 1546.

EXHIBIT 1546: Errata of technical report 5B, formerly marked as VV for identification

THE COMMISSIONER: And there was an SS, I believe, for identification.

MR. MARTLAND: I think that is now Exhibit 1543, the Korman report.

THE REGISTRAR: That's correct.

THE COMMISSIONER: I'm sorry, 1...?

MR. MARTLAND: I'm sorry, 1543.

THE COMMISSIONER: Okay. Does that cover all of the exhibits for identification?

MR. MARTLAND: That, I think, addresses all of the ones that we addressed through evidence yesterday, yes.

THE COMMISSIONER: All right. Now, to assist the Commission, Mr. Martland, if you could identify the exhibits that are going to be marked this morning, and there are exhibits over which I understand currently Canada and the British Columbia Salmon Farmers Association do not object to be marked as a public exhibit; that is to say, that they will ultimately be posted on the Commission's website. Can you identify which documents will be marked as that exhibit?

MR. MARTLAND: Yes. I'll look to do this -- and just by way of our plan - and I have spoken at least only briefly with the Registrar and Mr. Lunn about this procedure - would be that rather than taking you to Exhibit 2000 today, what we would look to do is simply have marked as the exhibit, whatever our next exhibit number would be, assigned to a document which would be the list of all of the series of subdocuments.

We would then, in shorthand, be -- and counsel would need to do this for the purpose of their hearing, for example, saying Exhibit 1550, subdocument 7, subdocument 20.

THE COMMISSIONER: Oh, I see. All right.

MR. MARTLAND: So that I hope that will become clear as we do it. Why don't I start, if I can try and do it this way. This won't be very elegantly done, but I'll do my best. Let's deal with the Canada documents first. There are two different Atlantic salmon escape datasets, and I will be asking Dr. Korman -- but I don't see any concern with us putting these forward and addressing them. And I'll ask a question or two of Dr. Korman to

confirm our understanding of this as we do so.

So the first, Mr. Lunn, if you can bring up the Atlantic salmon escape data. There's one that's a CAN number which I'll deal with first, 285273.

MR. LUNN: Was there previous notice given on this document before?

MR. MARTLAND: These were only things (sic) last night, I'm afraid.

MR. LUNN: I'm afraid I don't have it. I can get it in a few minutes, sorry.

MR. MARTLAND: All right. So then I'll leave that down the list. Now, I don't know, Mr. Lunn, if we may fare a little better with respect to what we emailed around. There was a -- secondly a Canada list, I believe, which was an Excel spreadsheet in fact.

MR. LUNN: Yes, the lists I have.

MR. MARTLAND: So if I could ask if the Canada -- so Canada's the exception to the list. In fact it's -- Ms. Grant just told me it was the stomach contents which sounds ominous. I hope it's not the stomach contents per se, but the data that describes them. But I hope you'll see on screen -- Dr. Korman, it may be hard for you to see, but we had a discussion about this.

At a broad level, I'm going to be showing onscreen, sir, some documents and asking if you're in a position to confirm your understanding that what we are presenting here is indeed the data that was provided to you and that went into your analyses performed in the course of your technical report.

DR. KORMAN: This file has been sent to me and I've seen it.

MR. MARTLAND: All right. And, yes, it does describe what you received from the Commission and relied on in your report?

DR. KORMAN: Yes.

MR. MARTLAND: All right. Not all of what you relied on but --

DR. KORMAN: Right.

MR. MARTLAND: -- in part.

DR. KORMAN: This is a very tiny subset, of course.

MR. MARTLAND: Okay. So in the absence of objection, I'll ask that this please be marked as the next exhibit.

MR. TAYLOR: I don't pretend to understand what these documents are, quite frankly, but I'm told this is the document that is the evidence of stomach content of escapees?
MR. MARTLAND: That's my understanding.
MR. TAYLOR: Okay.
MR. MARTLAND: You'll see fish number eight had grey muck for breakfast. So --
THE REGISTRAR: That'll be marked as 1547.

EXHIBIT 1547: Atlantic Salmon Stomach Contents (Excel)

MR. MARTLAND: With respect to next, the Salmon Farmers Association, I hope this one is a Word document, Mr. Lunn. It would have been part of our emails from last night.

Dr. Korman, we provided this to you as well and indeed I understand that you took the step of checking as against the data you received from the Commission to confirm that what we were listing here accurately described the data that were supplied and that you used in your report; is that correct?
DR. KORMAN: Correct. What I did was take the original CD that was sent to me from the Commission and then went through and confirmed that the files listed here were actually sent to me, at least in blocks. Given the hundreds or the many files on the list, I wasn't able to like go one by one, but I've looked at sort of chunks and then found them on here which are on my hard disk.
MR. MARTLAND: I understand then you compared it with a view to confirming that these were the same documents and indeed confirmed they are the same; is that right?
DR. KORMAN: Yes.
MR. MARTLAND: I'll ask, then, that this list of -- it's a list of databases, Mr. Commissioner, so in this case I'll be asking that the list be given the exhibit number --
THE COMMISSIONER: Right.
MR. MARTLAND: -- and then in terms of subdocuments, we would then use the list on the left column of that document.
THE REGISTRAR: The list will be marked as 1548.

EXHIBIT 1548: List of BC Salmon Farmers databases provided to Dr. Korman (Excel)

MR. MARTLAND: I'll advise counsel that because of questions around document preparation and notice, I hope this doesn't create a problem for us but Mr. Lunn simply does not have all of these datasets available for quick production in the course of hearings today, so counsel who look to go to a particular document using that identification system, we'll need to give Mr. Lunn some notice. They could do so at the morning break. It may be it doesn't arise till Monday, but they'll need to coordinate and give notice of a particular document that they want to move to within that dataset if they want to do that.

MR. BLAIR: Alan Blair for the B.C. Salmon Farmers. Just for clarification, so that we're all clear, especially since we're coming up upon a weekend, I want to be clear what is public and what is private. I think I know for sure the list is public. I want to be clear that we're all clear on whether the underlying documents are also public over the weekend before we refer to them or not. Our view is we have no objection to them being public, but I want to make sure that everybody's aware of whether they are public or not based on their underlying status under Exhibit 1548.

MR. MARTLAND: Thank you for that. Mr. Commissioner, our proposal would be -- there's two components. At the principle level, yes, they're public, they're public exhibits. On the practical level, I see it as highly unlikely that our staff are going to be able to have all of this onto a website by end of day on a Friday. So it's not -- that's a question of logistics as opposed to a lack of will or intent to do that. We'll look to do that. It's simply not feasible, I expect, to have that done before the end of today.

MR. BLAIR: As a very brief follow-up for those of us who might be able to access those databases and wish to make them public for whatever reason, based on the participants' view of this, are the participants free to talk about them or not? Just they won't be --

MR. MARTLAND: Mr. Commissioner, unless you have a

different view, I don't see any difficulty with that.

MR. BLAIR: I just wanted to be certain.

THE COMMISSIONER: Thank you.

MR. MARTLAND: Thank you. Now, sorry, so just confirm, Mr. Registrar. We've given an exhibit number now to the BCSFA database, or dataset.

THE REGISTRAR: Yes, 1548.

MR. MARTLAND: We now move to the Province of British Columbia, and if I might just take a moment.

So this time, Mr. Lunn, I hope that from the email attachments last night, you'll see an Excel document which itself is a list of different documents, similar to the last one. What we're proposing to do is have this made the exhibit, and then relying on subdocument references to deal with particular doc IDs. We see those in the left column using the ringtail numbering system.

Now, this one, Mr. Commissioner, would be given an exhibit number but with a notation that, in the short term, it is a non-public exhibit, and that'll be the subject of written submissions before you.

THE COMMISSIONER: So the exhibit number, then, Mr. Giles is...?

THE REGISTRAR: For this next document, it'll be 1549.

THE COMMISSIONER: Yes.

EXHIBIT 1549: List of Province of BC databases provided to Dr. Korman (Word)(Private)

MR. McDADE: Can I just make a short logistic suggestion? As I see it, those numbers are all consecutive. Rather than have to renumber everything to 1549-1, could we have it 1549-2646 and beyond? That'll avoid a lot of confusion in the future.

MR. MARTLAND: They may not all be consecutive I understand. They go from -- I haven't looked at this myself, but I'm being -- Ms. Callan's indicated that.

MR. TAYLOR: Well, to the extent that there's a number with them already, whether they're consecutive or not, Mr. McDade's proposal seems to make sense so we don't --

MR. MARTLAND: All right.

MR. TAYLOR: -- even get more confused.
MR. MARTLAND: That's fine. There's some wisdom to that, so let's do that. In terms of what we're referring to, then, will be subdocuments based on the doc ID, ringtail number, on the B.C. production. I'm sure Mr. Lunn appreciates all this.
MR. LUNN: I may have to, just in terms of our own internal database, get back to you on whether that's feasible for exhibit numbers. They may not be.
MR. MARTLAND: Maybe, Mr. Commissioner, we can defer the question, and counsel can do their best to refer to either or both of the column number and the doc ID number and we'll try to do that. We can sort through how to identify them as exhibits.
I should ask Dr. Korman, with respect to -- doing this after the fact, I appreciate -- but with respect to what you see on screen, sir, do you recognize this as being the production received from the Province?
DR. KORMAN: Yes, I do, and it's worth noting that there's some duplication in here. Some of these, for example, histo-pathological reports would also be summarized in a larger provincial database which is further down the list in this larger list. So just so you're aware you're looking at various hierarchies of information here.
MR. MARTLAND: Thank you. So, Mr. Commissioner, I'll maybe just confer with my colleague to see if I've addressed all these matters.
So as an outstanding, I've made a note that we need to return to the escapee Canada ringtail document, and perhaps if counsel don't mind me intervening for that logistical reason after the break, I'll look to just do that briefly at that juncture.
I think that concludes our logistical work today. It's used up some time, but we've made it at least that distance down.
THE COMMISSIONER: And I just want to make it clear to all counsel and participants in connection with Mr. McDade's remarks earlier, that these documents that have just been marked, in one case using terminology they're not for public but for private until I've received submissions, does not preclude any of the counsel or participants from cross-

examining on these documents. They are now in as exhibits with that one exception relating to the Exhibit 1549.

MR. MARTLAND: Thank you. That having been done, and mindful of the timing that we're on here, I've effectively covered the ground I wanted to cover through my questions of the panel. Just to narrate the issue as opposed to going through it, Dr. Connors and Dr. Noakes --

THE REGISTRAR: Excuse me. Before you get started, Mr. Martland, did you wish to mark the document you were referring to earlier as CAN number 285273? We can do that now if you wish.

THE COMMISSIONER: It's 1547, is it not?

MR. MARTLAND: I think we're talking about the second Canada database. Mr. Lunn wasn't in a position yet to put that on screen.

MR. LUNN: I'm still not, but if you wanted to mark it consecutively --

MR. MARTLAND: If there's no objection to doing that, I think it makes sense that we simply go ahead and do that, and if --

MR. TAYLOR: Mr. Spiegelman says it's fine.

MR. MARTLAND: Oh, good. Well, I'll see what Ms. Grant says.

THE REGISTRAR: So that would be marked as Exhibit 1550.

EXHIBIT 1550: Atlantic Salmon Escape Data

MR. MARTLAND: Thank you, Mr. Registrar, for that point.

EXAMINATION IN CHIEF BY MR. MARTLAND, continuing:

Q Dr. Connors and Dr. Noakes -- first of all, Dr. Noakes, in your technical report, sir, would you agree that you, in a sense, went out of your way to analyze and critique Dr. Connors' technical report? Is that a fair way to put it?

DR. NOAKES: I wouldn't characterize it like that. I think it's important to note that I raised these issues and concerns at our data meeting on February the 10th, and at least at two other occasions did I raise them. So from what I understand in terms of the statement of work for Dr. Connors, he was to provide that information to

both Dr. Dill and I to use. So in that sense, I was responding to it because I was either finding it useful or not useful.

So my criticism was basically addressing the issues and concerns that I raised in my evidence.

Q All right. And certainly I don't have it in front of us, I don't know that I -- I don't plan to ask a lot of questions, but I think it's page 5 of your report, and for some part of your report you engage in a written critique.

In addition to that, one of the documents now in evidence is your response -- or, rather, your -- what happened here is I think I used a tennis analogy. You fronted the first volley over the net which was in your report proper. Dr. Connors responded through a written document, and his response is now Exhibit 1542. In turn, you responded August the 10th with what's Exhibit 1538, the document we showed you at the outset. Is that fair to say?

DR. NOAKES: Yeah, the timeline is fine.

Q Okay.

DR. NOAKES: As I say, though, it's not an issue that came up immediately when we saw Dr. Connors' initial draft report.

Q Okay.

DR. NOAKES: These are issues and problems that I identified very early in the process.

Q And, Dr. Connors, for your part, likewise, you did your report, it was circulated. You then, I take it, in terms of the sequence of this, you then read Dr. Noakes' report and saw that he addressed a number of criticisms about your work, and you did a written response to those criticisms.

DR. CONNORS: That's correct. That was the first time I'd heard any criticisms of the report from Dr. Noakes when I read his final version of his technical report.

Q And your response is Exhibit 1542. That's the document I showed you at the outset of our proceedings.

DR. CONNORS: That's correct.

Q So it really narrated the debate without engaging in it quite deliberately, and it's simply because I fully expect that other counsel may engage in it. I don't plan to ask you further questions about it. I will give this, if you will, a one or

two-minute opportunity if you wanted to make an additional point to what's made in the written exchange of views on this.

Dr. Noakes, did you want to do that, sir?

DR. NOAKES: Just very briefly highlighting -- I mean the two areas of concern that I have are: one, in terms of using the fish production as a proxy of pathogen exposure, I go through, in terms of my critique, basically showing that it's not a reasonable proxy of pathogen. It's not consistent with the available evidence, fish health evidence or disease evidence that we have from the farm, and it's not consistent in a number of other areas.

So on that basis, really, in terms of looking at interactions between farm salmon and sockeye production, the analysis that was presented in Dr. Connors' report is not useful.

That said, if you ignore that, then basically if you go onto the modelling, I do have some fairly technical -- I don't want to get into the technical issues here, but there are some fairly technical details in terms of problems that I identified with the modelling process and the modelling outcome itself. I'll just leave it at that. The documents, as I say, are --

Q They capture your concern.

DR. NOAKES: They capture it and it's -- yeah.

Q And with the same constraint, I'm afraid but, Dr. Connors, did you wish to outline in basic, the response that you have to Dr. Noakes' criticisms?

DR. CONNORS: I do. And I do want to point out that I was instructed not to submit anything further after seeing Dr. Noakes' --

Q Yes.

DR. CONNORS: -- response to --

Q When you say that, that's the Commission directing that we don't want any more tennis.

DR. CONNORS: Correct. And I think we can characterize that we disagree on the usefulness of the number or weight of farm salmon along juvenile salmon migration routes as being useful or not in terms of asking questions about whether or not there are associations between salmon aquaculture and the patterns that we seek to explain in Fraser River sockeye salmon.

My position on this is that the number or

abundance of farm salmon hosts or hosts in general, and their spatial and temporal distribution, is an important part, a fundamental part of pathogen transmission. As such, I think it is still informative to consider it, and I think we'll have an opportunity to get further into this down the road. I disagree strongly that any inference that's then drawn further on down the line should be categorically dismissed.

MR. MARTLAND: I will, on that note, Mr. Commissioner, conclude my questions of this panel. I have next counsel for Canada with an 80, 8-0, minute allocation.

CROSS-EXAMINATION BY MR. TAYLOR:

Q I'll start, Dr. Connors, if I may, with where Mr. Martland was. Do you agree that at the heart of the debate that's just been discussed between you and Dr. Noakes is the question of whether production levels on a farm is a key point and driver in determining the pathogen levels?

DR. CONNORS: So the question is whether or not I agree that the heart of our disagreement is whether or not the farm salmon production is a key driver of pathogen levels on farms; is that correct?

Q And their distribution, if any, or shedding, if you like, of pathogens outwards from the farm?

DR. CONNORS: Well, my response to that would be that I'm not making the argument that farm salmon production is a key driver of the abundance of pathogens on farms. The argument that I'm making is that the abundance of farm salmon hosts is likely to play an important role in the overall degree of exposure to pathogens for other salmon that migrate past them.

Q And, in that regard, and taking it as you characterize it, do you agree that that's a matter of biology and fish health science?

DR. CONNORS: I agree.

Q And that's not your expertise.

DR. CONNORS: Fair enough.

Q Now, I'll begin, if I may, with Dr. Korman and your report, which is 5A, and that is Exhibit 1543. If you turn, please, Dr. Korman, to page (i), that is, little (i), in the second paragraph you make reference to -- I'll see if I've got the

right paragraph here. There's a paragraph where you refer to B.C. Now conducting 120 -- or, sorry, 100 audits a year. I may have noted the wrong paragraph here, but do you recall that in your report?

DR. KORMAN: Yes, I say approximately 100, about 100 audits in the second paragraph.

Q Okay. I understand that in point of fact, that there's about 120 health audits a year or annually, and a further 50 sea lice audits. Do you have knowledge of that?

DR. KORMAN: The number per year varies depending on a number of factors, so that's why I used the word "approximately" and used a sort of conservative value rather than -- I wasn't trying to say that was the average number across here, so I agree with what you said, but...

Q All right. On page (ii), or two little "i's", the next page there, in the first sentence, you refer to fresh silvers. We've heard of that before. You refer to them as having potentially died of disease. I think I may, like Mr. Martland, have looked at a slightly different printed version when I was making questions than I've got in front of me now, so I regret this, but I may be a little off in my page numbers.

But there is a reference in and around that area to fresh silvers and potentially dying of disease, and you spoke of that yesterday and you're familiar with that in your report.

DR. KORMAN: Yes.

Q Now, in the next paragraph, though, you clarify what I think otherwise might be left as a wrong impression, from what I just said, and you say later - and speak to this - you say that, in effect, in fact -- or let me put this to you. In fact, is it correct that most fresh silver show no sign of disease or infection?

DR. KORMAN: Yes.

Q And, Dr. Noakes, do you agree with that?

DR. NOAKES: Yes.

Q And Dr. Dill?

DR. DILL: My understanding is that a large percentage of them are not found diagnostically to have an infection.

Q Thank you. And, in fact, there's many causes of death other than disease or pathogens, aren't

there, Dr. Korman?
DR. KORMAN: I'm no veterinarian.
Q All right. Well --
DR. KORMAN: But, yes, it makes sense to me intuitively.
Q Okay. Dr. Noakes and Dill, you agree with that, do you?
DR. NOAKES: Again, given the limitation of my expertise, yes, I would agree that there's many causes of death other than disease.
Q Dr. Dill?
DR. DILL: Yes. Predators, for example, poor water conditions.
Q But in terms of fresh silvers, they could have been trapped, they could have suffocated, they could have had metabolic problems and so on?
DR. DILL: I'm sure some of them fit that category.
Q Now, at page little (v) -- and again, I regret this because -- well, at page little (v) and then page 14, which should have Table 2 on it, this includes -- your lice numbers include herring lice or *Caligus*, doesn't it?
DR. KORMAN: That's -- just reading the caption here, sorry. Yes, these are both associated with herring lice, and this is what we discussed yesterday are the numbers which are basically the numbers per -- across the total number of fish examined per pen. In fact, what's in the caption is "per salmon". So these are the numbers that have now been corrected and are 20-fold more than what's presented in this table. Although the patterns across years and across areas and seasons would be the same, the numbers are 20-fold lower than what's presented here.
Q Okay. But with that, also, is it the case, to your knowledge, that most fish biologists don't consider *Caligus* or herring lice as particularly significant to farmed or wild salmon in terms of negative impacts?
DR. KORMAN: That's my understanding from the literature as far as their sort of parasitic effect. You know, Dr. Dill would be in a better position to speak to the potential as infectors for disease. I think there's also a concern there.
Q All right. Well, we'll come to Dr. Dill in time. At page 7 in the first paragraph, and this is

under the heading "Trends and Mortality". In Figure 4 on page 18, you refer to three million dead fish a year from farms, and that's about a 12 percent mortality rate as I understand it. I think I've got that right so far, don't I?

DR. KORMAN: Yes.

Q And then you point out that of that three million, about 20 to 25 percent is fresh silver.

DR. KORMAN: Correct.

Q Now, you seem to take fresh silvers and equate them with death due to disease. Have I got that right as to the premise that you proceed on?

DR. KORMAN: No. I think it's fair to say - and I've said this in the text previously - that this is the maximum that potentially died of disease.

Q All right.

DR. KORMAN: And that's why in that paragraph I say "suspected to have died due to disease or unknown causes." So that's not exactly how you characterized it.

Q All right. So then the 600,000 number that's in there, which would be the -- roughly all of the fresh silvers, I guess. That's the absolute highest, then, is it?

DR. KORMAN: That's correct.

Q Still with that same figure, Figure 4, do you attribute the spike in 2003 to any particular cause?

DR. KORMAN: There are higher levels of IHN in the database in 2003 and 2004, actually, than in other years. So that's a strong possibility.

However, what I haven't done is gone through and looked at -- to actually confirm what you're saying, one would have to look at the mortalities on the individual farms that had IHN and determine what fraction of that bar was represented by IHN mortality. So I suspect that's the case, but I haven't done that background work to determine that.

Q Okay. I understand that this is the first year - 2003 that is - is the first year that there was a comprehensive set of data being fed into the province from salmon farms; is that right?

DR. KORMAN: Yes.

Q Do you have knowledge whether, given that it was the first year, there would have been mistakes in the data coming in and therefore the results that

you get out are reflective of mistakes going in?
DR. KORMAN: I don't have any knowledge of that. I would say that there were a limited number of farms contributing. They had been getting industry reports during the pilot years of the study from smaller numbers of farms, so if there were any bugs in terms of the database structure, I would have thought that they would have been worked out in the earlier years.
Q Okay. If there was no known significant event in 2003, and yet you see the spike that you see, does that make you suspicious of the numbers that you're seeing there? That is, the number for 2003, the spike.
DR. KORMAN: Well, as I said, IHN could be a possibility. I just don't want to swear to it in a court of law that that was in fact the cause. So there is a very likely -- there is a likely cause for that spike and that's why I'm not particularly concerned why it's anomalously higher than in other years.
Q Okay. And whatever that cause is, then, if it's a health-related cause, that would be for people with expertise other than yourself to speak to, would it?
DR. KORMAN: Yeah, probably the best person to ask would be the veterinarians working for BCMAL in charge of this database at the time, or perhaps some folks from -- some of the veterinarians from industry would be the ones most qualified to answer you.
Q All right. And then I want to turn to Figure 1 on page 15, if I may. You point out in that figure that there was a moratorium on tenures from 1995 to 2002. So that's a moratorium on new sites, is it, that the province put in place?
DR. KORMAN: That's my understanding.
Q And yet during that period of a moratorium, the production levels went up.
DR. KORMAN: That's what the figure shows.
Q Do you know why?
DR. KORMAN: Yeah, I would suspect it was the number of fish produced for tenure increased is the only explanation for that. So farms would have probably expanded their production, individual farms.
Q And do you know, Doctors (sic) Noake and Dr. Dill,

the reason why production would go up with the same footprint in place?
DR. DILL: Well, Dr. Korman's explanation is the only one. It's just more fish per farm.
Q All right. And did it result, then, from such things as improved husbandry?
DR. DILL: I'm not able to answer that question.
Q All right. Okay. The only think you know, then, is it went up. All right. Is that the same with you, Dr. Noakes?
DR. NOAKES: Yeah, that's true. This is about the time they started to actively vaccinate fish as well, so there were some improvements in husbandry. I mean I was with the Department at that particular time, so I had some personal knowledge of what was happening in the industry. So they did increase volumes and there was improvements in husbandry to accommodate that.
Q Which would mean better survival, right?
DR. NOAKES: That's correct.
Q And do you know whether better feed and better feed conversion was being instituted about that time?
DR. NOAKES: I'm aware there were several projects that we did in the Department, and I know that industry was also working on producing better feed in order to improve the feed conversion ratio, but also in particular, at the departmental research what we were trying to do is reduce the waste components, nitrogen and phosphorous that was being produced by fish waste.
Q All right. Now, if I may, Dr. Korman, go to page 19 and Figure 5. I want to ask about the sea lice. Now, am I right that the death that's being attributed to sea lice in this graph is not death caused by sea lice, but rather death where the fish are killed so they can take the count of sea lice?
DR. KORMAN: The fish aren't necessarily killed. So a fish health event, which is what this graph is plotting, is defined as any action that involves veterinary involvement or the use of medication. That would include anaesthetics which are used - I'm not sure in all cases, but in many cases - to handle the fish to do the sea lice monitoring. So some of these sea lice events are strictly -- the events are associated with monitoring the fish --

Q So it's --
DR. KORMAN: -- and not even to do with the fish that -- in fact probably the majority, if not all of these cases, they're not deaths associated with sea lice, simply sea lice monitoring events that involve the use of an anaesthetic.
Q All right. Thank you. Now, Dr. Connors, I'd like to ask you some questions. Your report is now Exhibit 1545, if I have that right. I'd like to go to page 7. There you say at the top of the page:

> I estimated the total number of sea lice (in the millions) on farmed fish in a given month by multiplying the average abundance of [sea] lice on fish examined for lice...

So you're -- and I think you spoke to this -- you might have spoken to this before. You're including all lice in this calculation, are you?
DR. CONNORS: In this calculation for -- if you read further down the paragraph, for four different measures of louse abundance. These include motile salmon lice, gravid female salmon lice, motile herring lice or *Caligus clemensi* and the total abundance of motile lice from both species.
Q All right. Dr. Noakes and Dr. Dill, in the case of - and I'm going to say *L. salmonis*, the L-lice, as I would call it 'cause I can't say the other word - is it that the only stages of sea lice that we really want to pay attention to for salmon lice are the adult females and the pre-adult females? They're the only ones that are infectious?
DR. DILL: Well, it's the females that are producing the next larval generation, and so that would be a measure of the potential impact of that number of lice on a wild population. The stages which seem to have the most pathogenic effect on individual fish are those sub-adult and adult lice that we call the motiles.
Q All right.
DR. DILL: By the way, we all have problems pronouncing and so we usually call them *Leps*.
Q *Leps*, all right, thank you. I'll try to remember to use that. I appreciate that.
DR. NOAKES: I would agree with Dr. Dill and his interpretation.

Q All right. So as I understand it, and you're agreeing with me, that it's some of the lice that you really want to pay attention to as a potentially infectious one, and you can put to one side, if you like, other lice in these numbers are being lice, the L-lice or *Leps* that really have any negative impact.
DR. DILL: Yes, that's correct.
Q Okay.
DR. NOAKES: Yes.
Q Now, Dr. Connors, the same question that I asked Dr. Korman, are you aware that *Caligus* or herring lice is primarily a non-salmon sea lice?
DR. CONNORS: Correct. And, by that, I mean it's commonly found on other hosts besides just salmonids.
Q Right. And it's not really a concern for salmon, is it?
DR. CONNORS: That's my understanding.
Q And I'll ask this of any of you, whoever thinks they're best able to answer, would the number of *Caligus* on a farm be directly related to the number of *Caligus* that's on the non-salmon population in the area where the farm is?
DR. DILL: I think that's a very reasonable assumption. Those lice have to get onto the farm from somewhere and the non-salmonid hosts, herring, but other species as well, are probably the major natural source of them.
If I could just comment on Dr. Connors' answer to the other question, though, I think while it's true that most of us don't believe that *Caligus* are likely to have a major impact on salmon, no studies have been done, so we're kind of making an assumption. There's just been no experimental work on that.
Q All right. Thank you. And in your answer just now, you hit upon something that's important, I think. Can we agree that the fish come into the farms free of lice?
DR. DILL: We can.
Q And therefore, if they get lice, it's coming from the outside into the farm, right?
DR. DILL: That's true, and that's true regardless whether it's *Leps* or *Caligus* that we're talking about.
Q Right. Now, Dr. Connors, in your report, you

discuss pathogens, but in doing that, you're not offering any opinion about the impact of pathogens, are you?
DR. CONNORS: That's correct.
Q And you're not offering any opinion on how they function or interact or whether it's negative or positive with the host?
DR. CONNORS: That's correct.
Q All right. Now, I want to put some things to you, Dr. Connors, and ask if you can agree with me. It has to do with pathogens. I'm informed of these things, and I'm asking you if you can tell me if you had information on the following points and incorporated them in your work. The first is in order to become infected by pathogens, the host must be exposed to a minimum infectious dose. Were you aware of that in doing your work?
DR. CONNORS: Related specifically to this report, I make no assumptions about minimum infectious doses.
Q Okay.
DR. CONNORS: But I do agree with the statement that in order for a pathogen to infect a host, it has to be exposed to that pathogen.
Q Are you aware, and did you take account of where you have a single-point source releasing an infectious agent, the further you are away from the point source, the more dispersed the pathogen is going to be?
DR. CONNORS: That's a good point. And unfortunately in this analysis, because of the very coarse aggregate level of information that I had, and lack of information on the exact migration routes of the different salmonids, I had to make assumptions about the regions that they pass through. So that's a key uncertainty and I certainly do agree with the statement that the infectiousness of various pathogens is likely to decrease the further away from the point source of infection a susceptible host may be.
Q All right. Well, what you just said injects some uncertainty into your conclusions then, doesn't it?
DR. CONNORS: You'd have to point that out. You can maybe clearly lay that out. I don't believe it does.
Q Well, what I mean by that is you haven't taken

account of the distance from the point source to were pathogens might be and their impact because you didn't have data giving you information on that.

DR. CONNORS: I don't know if that injects uncertainty into the conclusions that I draw. It's an assumption that has to be made that because we don't have any more finer-scale information that salmonids pass through a given region are coarsely exposed to salmonids there, and I would argue there's a reasonable assumption given the limitations to the data.

Q Okay. Is what I said, Doctors Noakes and Dr. Dill, correct that if you have a single point-source releasing an infectious agent, then the number of infectious agents per unit of water will decline as you move out?

DR. NOAKES: Yes, that's correct.

Q Do you agree, Dr. Dill?

DR. DILL: Yeah, that's basic physics, I guess. But, in addition --

Q Common sense too, probably.

DR. DILL: Yes. But the further they get, the longer it takes them to get there, and there'll be fewer of them that are viable as well. So there's two reasons why that would be.

Q All right. And, Dr. Noakes and Dr. Dill, is it correct that different pathogens have different shedding rates and differences in how far they can be spread?

DR. NOAKES: Yes, I think that's correct. As Dr. Dill pointed out, even the distance -- some pathogens are particularly sensitive to UV, for instance, so that can affect their viability the further away they get from the farm simply because they're exposed to more UV.

Q All right. Now, this is a question of Dr. Dill and Dr. Noakes. If farms were producing large numbers of infectious agents, then would you expect that the fish within the farm would get the highest dose, have the greatest risk, and you would see that manifested on the farms?

DR. NOAKES: I would certainly agree with that. It makes perfect sense that the highest concentration would be at the -- if there was a point source of disease, that would be the highest effect.

Q And so, in short, if there is a problem posed by

pathogens and/or disease emanating from a farm, it'll show up in bright lights, if you like, at the farm itself.

DR. NOAKES: Assuming the farm fish are susceptible to that disease, and I think the diseases that were identified as high risk by Dr. Kent, then, yes, you would expect to see that unless...

Q Dr. Dill, do you have anything to add?

DR. DILL: Yeah, I mean, that is a reasonable assumption or reasonable expectation, but there are two assumptions underlying it. One is that the susceptibility of a farm fish and the wild fish that you're talking about are the same, and we're talking about Atlantic salmon, or in some cases chinook salmon as opposed to sockeye salmon, so we're not always sure of that.

The second is that there aren't asymptomatic fish in the pens which are resistant for one reason or another to disease, but are still shedding pathogens that might be affecting wild fish.

So subject to those assumptions, what you said is correct.

Q All right. Thank you. And, really, what that takes us to, then, doesn't it, in part at least, is good record-keeping and good fish health management on the farms. This is for any panel member. Do you agree with that?

DR. NOAKES: Certainly it's a conclusion that I drew in terms of looking at the very low level of mortality that you have on the annual basis. Two percent fresh silvers which could possibly have died of disease, so yeah, so, I mean, they're generally very, very healthy in terms of the fish on the farms.

Q What I'm really getting at here, though, is if farms were identified - and I'm not suggesting they are - but if they were identified as a point source, and in many instances at least, or most or all, depending on your view, the problem would then manifest itself on the farm and you would see it in bright lights. The key then becomes record-keeping and fish health management to guard against that, and if it happens, to know it. Do you all -- does anyone disagree with me on that?

DR. KORMAN: No, I agree. I mean, they're mandated as a part of the licence requirement to report all

fish health events. So if there's a large die off associated with a disease, they're mandated to report it and I would think it would be tricky to hide something like that because they're very likely going to be audited in that quarter or the second quarter.

Q All right.

DR. KORMAN: And so there's, you know, extra motivation for the industry there, I would think.

Q Do the other panel members agree with what Dr. Korman just said?

DR. CONNORS: I agree.

DR. DILL: I agree with that as well. I mean, it's clearly essential that the farms manage the disease on the farms. I think it's also essential that the sort of audit procedure that the BCMAL was undertaking is continued under the federal government regulations, and even extended. I'd like to see more audits and I'd like to see fish checked for a wider possible panel of disease agents.

For example, I don't -- I believe I'm correct in saying that, at the moment, there's no screening for retroviruses, notwithstanding that that's probably quite an extensive undertaking. I would like to see that added to the panel of disease.

Q All right. Dr. Noakes, do you have anything to add?

DR. NOAKES: No.

Q That's a "no". I think your mike was off, but that's a "no" for the record.

Just on the move to the federal regime, are the panel members aware of the terms of the licence that are in place now for fin fish aquaculture under the federal regime? I don't mean that you have to cite it chapter and verse, but are you aware generally of the licence conditions and the type of conditions in them?

DR. CONNORS: No.

Q Any panel member aware?

DR. NOAKES: I have some knowledge of it having been involved with this previously and having conversations with Andy Thomson about what conditions would be attached to licences and whatnot.

Q All right. And you know Andy Thomson to be the

Director of Aquaculture for this region, British Columbia or Pacific Region for Fisheries, do you?
DR. NOAKES: I do. Andy used to work for me.
Q And are you aware, then, that the terms of licence in the federal licence are very extensive?
DR. NOAKES: I am.
Q And it includes an awful lot of fish health management and fish health reporting requirements.
DR. NOAKES: Yes, I understand that from my conversation with Andrew Thomson.
Q All right. Are you aware of that too, Dr. Dill?
DR. DILL: No, I'm not. I'm not aware of the regulations.
MR. TAYLOR: Thank you. And you'll be hearing more on that on Tuesday, I think it is, Mr. Commissioner. Do you want me to stop now or keep going?
THE COMMISSIONER: If it's convenient for you, Mr. Taylor, to stop now, that would be fine.
MR. TAYLOR: That's fine. Temporarily stop, that is.
THE REGISTRAR: The hearing will now recess for ten minutes.

(PROCEEDINGS ADJOURNED FOR MORNING RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: The hearing is now resumed.
MR. MARTLAND: Mr. Commissioner, an outstanding housekeeping item which I'm going to simply read into the record rather than anything more formal. Earlier this week, Mr. Leadem asked questions of Dr. Kent in relation to an unpublished manuscript. We'll see that on-screen. That document is now Exhibit 1494 and I'll just simply read into the record. That was published. The reference is to Michael Kent, 1994. The title is "The Impact of Diseases of Pen-Reared Salmonids on Coastal Marine Environments - Proceedings of the Canada/Norway Workshop on Environmental Impacts of Aquaculture, Fisken, F-i-s-k-e-n Og, O-g, Havet, H-a-v-e-t, 13:85-96". So that's the formal citation. I think that's an adequate way for us to simply record that indeed that document was ultimately published with that citation. Thank you.
Mr. Taylor?

CROSS-EXAMINATION BY MR. TAYLOR, continuing:

Q Dr. Connors, do you agree that the pathogens that you go over are not exclusively pathogens of sockeye salmon?
DR. CONNORS: Correct.
Q And so any shedding of pathogens from farms should impact or get to other species, such as pink and chum, shouldn't they?
DR. CONNORS: All other things being equal, if all those same species were passing, same point source at the same time then, yes, I agree they would be exposed to the same pathogens.
Q And of course there's always a temporal element to it, yes. You're aware that pinks are doing quite well these days or these years, aren't you?
DR. CONNORS: I am aware of, yes, strong returns to the Fraser in recent years.
Q Now, as I understand it, the pink abundances that you used in your report are based on adult pink abundances in the North Pacific; is that right?
DR. CONNORS: That's correct. This is the abundance of pink salmon that come back to key watersheds throughout the North Pacific. And I believe it represents about 85 percent approximately of total pink salmon abundance in the North Pacific.
Q So you're looking at the adults incoming, of course, and going in the vicinity of the salmon farms in terms of the pinks?
DR. CONNORS: No. This is a proxy so --
Q Okay.
DR. CONNORS: -- the abundance of pink salmon in North Pacific is used as a proxy for potential competitive interactions the year actually preceding in the open ocean between pinks and sockeye.
Q Is there a reason why you didn't take a count or include or incorporate juvenile pinks and chums and their abundance in Georgia Strait?
DR. CONNORS: Yes, so the reason that I considered the abundance of pink salmon in the North Pacific, as opposed to juvenile abundance or abundances in a suite of other regions is because specifically I was addressing the hypothesis that's been already examined and identified by an independent expert panel that looked at and suggested there's considerable evidence for competition in the open

ocean with pink salmon across North Pacific.
Q All right. Dr. Noakes and Dr. Dill, is there competition in Georgia Strait between juveniles, pink and chum and salmon as well?
DR. NOAKES: I would expect so given the high abundance of all of those species, as well as other species that eat things at the same trophic level as juvenile salmon, such as herring and hake and other species down there.
Q Dr. Dill, do you have anything to add?
DR. DILL: I think we need to be a little bit careful in how we're defining competition. To an ecologist, competition means that there's an impact on the thickness of the fish caused by another fish or another species of fish. And just because there's dietary overlap doesn't mean there's competition. To give you an example, if you and I both decide to have a pizza for lunch today, we could walk outside and probably end up in one of any of 50 pizza parlours within a few-block radius of here. We'd have a very high dietary overlap but we wouldn't be competing for anything because there's an excess of pizza.
Q Yes, but you've hit on the point. It's dependent upon the food abundance in a given year, isn't it?
DR. DILL: That's correct.
Q So if there were low food abundance in Georgia Strait, that would present a problem and one should take into account the pinks and the chums and what their competing for food might do to sockeye, shouldn't you?
DR. DILL: It would certainly increase the likelihood of competition if there were low food abundance. But again, we'd have to know exactly what the dietary overlap was between them. They don't eat exactly the same things. There's some differentiation in their diets. But again, you know, this is somewhat irrelevant to Dr. Connors' analysis because he was just testing these hypotheses that have been put forward by others.
Q Yes, we moved off his report a little bit here for a moment. But just on this, all of the salmon feed on plankton-like food, don't they?
DR. DILL: Yes, but different species, different size classes.
Q All right. Back to you, Dr. Connors. And I just want to sum up on this, if I may, and see if I've

got it right. We've established, I think, that you used all developmental stages of lice in your study, correct?
DR. CONNORS: That's not correct.
Q Okay.
DR. CONNORS: If we go back to the paragraph on sea lice that I considered, I believe it's on page 7, at least in the report that I'm looking at.
Q Yes?
DR. CONNORS: The four different measures that I considered starting with number 1 are motile *Leps*, right? So this includes pre-adult one, pre-adult two and adult developmental stages. That does not include any of the developmental stages preceding that. I also considered just gravid adult females and that alludes to the conversation earlier about them likely being the best proxy for the number of infectious or copepods and not that are being released from the farm. The third is motile *Caligus clemensi* so this doesn't include the earlier developmental stages of *Caligus*. And the fourth is total abundance of motile so it's just the sum of those three previous different variables.
Q All right. Thank you for that clarification. You used both species of lice, though, didn't you?
DR. CONNORS: Yeah, I considered both of them in combination and independently, yes.
Q And as well as I understand it, and I think you've said this already, you used all cases of fresh silver mortality in your estimations?
DR. CONNORS: Correct. If we go to farm salmon mortalities, yes, I took the estimates of fresh silver mortalities, as defined in the database.
Q So when you were then using what we've just gone over to then deal with pathogens and disease and numbers and impacts and so forth, you were presenting high numbers, or you used high numbers to then come up with your conclusions, did you?
DR. CONNORS: Depends what you mean by "high numbers". If you're asking if the fresh silver mortalities is the upper bound of the estimate of mortalities due to disease or unknown causes, as the definition that I used here, then, yes, that would be the upper bound for that metric.
Q Right. And so you may not have used the worst-case scenario but you've certainly used a high-

impact scenario?
DR. CONNORS: I've certainly used, yes, an upper bound for that metric, yes.
Q And even with that you didn't find a problem? You found no evidence of a problem?
DR. CONNORS: No. So to be clear, I didn't find a statistically significant relationship between the proportion of fresh silver mortalities in the months that juvenile salmon migrate past those farms and their productivity in that analysis which covered about, I believe, in that analysis four years of information in terms recruits-per-spawner.
Q Okay. And just one final question, at least for now, Dr. Connors of you, and this comes from something that one of the reviewers of your report was saying, and it's at page 62, if you want to go to it. But am I correct that you did not include the 2010 returns in your study?
DR. CONNORS: That is correct. I included all the information that was available at the time of writing the report. That did not include the 2006 brood year for which the majority of fish that returned would have returned in 2010.
Q And are you in a position to say what change there would have been to the results if you had had that data and had included them?
DR. CONNORS: I am not in a position to say how these conclusions would be influenced by updating that information, which is why I stress the importance of revisiting this analysis once that data becomes available.
Q Okay. Dr. Dill, I wanted to ask you some questions about you report. And I'll start, if I may, at page 9 of your report. And this is Exhibit 1540. There's a sentence in the middle of the second paragraph where you say:

> There is no evidence to suggest that the sockeye enter the Broughton archipelago and pass by the many farms there...

And therefore, do I take it that you're saying that there would be no direct impact on sockeye from farms?
DR. DILL: That statement is based on my understanding from people who have done sampling in the

Broughton. They very, very rarely find sockeye salmon. When they do, they're probably from local area streams. And as a result, the Fraser fish are probably migrating past the mouth of the Broughton, not turning in there and being directly impacted by the farms there. But they may be indirectly impacted, as I've said, by interacting with pink and chum coming out of there.

Q All right. At page 12 at the top, there is a sentence in the third line, if the print you have is the same as mine. It is, thank you. Where you say:

> On balance, I believe the science strongly supports the conclusion that pink salmon in the Broughton Archipelago, and perhaps other salmon species there as well, have been negatively impacted by lice from fish farms.

Now, that's speculative, isn't it?

DR. DILL: No, I wouldn't characterize it as speculative. I'd characterize it as my opinion based on the science that I have read and also the science that my students and I have done and participated in and published.

Q Isn't the word "strongly" in that sentence itself too strong?

DR. DILL: I don't believe so based on my understanding.

Q Dr. Noakes?

DR. NOAKES: Yeah, I wouldn't have made that statement. As I say in my report, I think there is some serious flaws in many of those investigations. And certainly the only one that has been published, there's one come out recently and again there's some problems with that, but the only one that was available to us at that time was the one by Marty *et al* that used farm salmon data. And it had an opposite conclusion.

Q So you disagree with Dr. Dill's statement?

DR. NOAKES: I disagree with that statement.

Q Then at page 17, Dr. Dill, you're dealing with the benthic impacts here. And at the bottom of that first large paragraph, there's the sentence that begins:

> However, it seems highly unlikely that such

local effects could impact Fraser sockeye survival...

Do you agree with me that the period should be right where I've put it, after "survival" and not carry on the way you have?

DR. DILL: No.

Q What do you mean by "to any great extent"?

DR. DILL: Well, I mean that individual fish that might, for one reason or another, hang out locally or on the farms might be impacted but that it's unlikely it's going to have a major impact on the population as a whole.

Q So there's no population, in fact?

DR. DILL: I think it's highly unlikely.

Q Page 19, you're dealing with SLICE there. And I believe this is what you're saying but I want to be clear. Do you agree that SLICE is not a factor in the decline of sockeye?

DR. DILL: My understanding of the sort of dynamics of SLICE in the marine environment is that it, again, is very unlikely to be a factor in the decline of Fraser sockeye.

MR. TAYLOR: At page 25, in the first paragraph, 25, it starts just at the bottom of the previous page, if you want to maybe, Mr. Lunn, just go to the previous page so they can see the study. Thanks.

Q So you're speaking of ISA here. And you then speak about what Dr. Sheppard and Dr. Marty are having to say. Was that in conversations with them that you're getting the information that you're postulating there, or in documents themselves?

DR. DILL: With respect to Mr. Sheppard, that was a phone conversation that all of us had with him in the Commission offices. The information about the classic symptoms of ISA is from the document quoted in there.

Q All right. And what about with Dr. Marty?

DR. DILL: That's what I meant, with Dr. Marty. The classic symptoms of ISA quote is from BCP002864.

Q Did you have a conversation with Dr. Marty then?

DR. DILL: Not directly.

Q Do you have notes of the conversation with Dr. Sheppard?

DR. DILL: Yes.

Q Can you produce those?

DR. DILL: I think I have already have.
MR. TAYLOR: Mr. Martland?
MR. MARTLAND: I think he already has and I think that in the course of production by the Commission of the working files of these experts that ought to have been included. I can't say firsthand whether it is but I expect that would have been part of that production.
MR. TAYLOR: All right. Well, I'll check that some more but I'll leave it there at this point then.
Q Now, in the section following that where we just were, Dr. Dill, you deal with what you characterize as "The so-called 'Miller virus'". And you're referring to Dr. Miller there, are you?
DR. DILL: Yes, Dr. Kristi Miller, who testified yesterday and the day before.
Q All right. And were you here for her evidence?
DR. DILL: I was not here for the first day; I was here yesterday morning.
Q You say in your first sentence:

> A recent paper by Miller et al. (2011)...

Just pausing there. That's the paper that was produced in January of 2011, is it?
DR. DILL: In the journal, *Science*, that's correct.
MR. TAYLOR: Yes. And that's, if I've got my numbers right, Exhibit 558, I think. The Commission seems to magically come up with numbers all the time so they may confirm or correct me but I think that's the exhibit number.
Q You say there that:

> A recent paper by Miller et al. (2011) provided evidence for a virus-like particle associated with early freshwater entry (by returning adults)...

I want to suggest to you that she did not provide evidence; she has a hypothesis that is being worked on and they're trying to work this through, as scientists. Is that a fair characterization?
DR. DILL: Yeah, what she provided evidence for was a genomic signature that was associated with early freshwater entry and high PSM. And she suggested that this genomic signature looked like something that might be produced by a virus and so she

hypothesized that it was a virus. So that would have been a better way for me to write that sentence.

Q All right. And you, of course, defer to Dr. Miller in terms of getting an accurate characterization of her work, do you?

DR. DILL: Absolutely. And I met with her at DFO and heard her speak yesterday and it's my understanding that the hypothesis, some of its predictions are being met and it appears that there is, in fact, a virus involved which yesterday I heard her call a parvovirus.

Q Now, at pages 26 and 27 you go on to speak about transmission or possible transmission. I think we've clarified this but just to be sure. You're not an expert in disease, are you?

DR. DILL: No, I'm not.

Q And you're not an expert in transmission of pathogens either, are you?

DR. DILL: No, I'm not.

Q And you're not an expert on the impacts of pathogens?

DR. DILL: I am an expert on the impact of sea lice but not of other pathogens.

Q And your expertise in that regard is as an ecologist?

DR. DILL: It's as a behavioural ecologist and author of several studies.

Q And your opinion on sea lice is they are not causing a problem or they're not part of the reason for the decline.

DR. DILL: My opinion is that sea lice directly are not a cause of the problem but I don't believe the numbers of sea lice that we're talking about, given the size of the host that we're talking about, are likely to be directly causative of any declines in Fraser sockeye but I do caution that they could be acting as vectors for disease and they could also, if you read the last section of my report -- I've forgotten what it's called -- the futility of looking for the cause they may be interacting with other factors. So directly, no, they're not the smoking gun that we're looking for.

Q All right. At page 27, there is, in the second paragraph in the second sentence, these words, and this is dealing with IHN and whether it may spread

from farm-to-farm in the water and you've referred to Dr. Saksida's paper of 2006. You then say:

> If so, there is no reason it could not be spread to passing sockeye.

That statement is speculative?

DR. DILL: Yes.

Q Just going back for a moment to what you were saying two minutes ago, you're not an expert in vectoring, are you?

DR. DILL: No, I'm not.

Q We then turn, if we may, to page 34 where you have a section entitled "Summary". And you start there by saying:

> The relationship between farm production and Fraser sockeye survival in the long-term data set suggests that the farms are having some sort of negative impact on wild salmon productivity, most likely in concert with other factors in the marine environment.

Now, first, given what you say there, I take it you'll agree with me that whatever be the cause or causes of any decline, it's multi-factorial, is it?

DR. DILL: That's what is suggested by the Connors analysis that surface temperature, which is an index of food availability for the fish when they first enter the ocean and pink salmon among them. Those are the factors that I'm talking about there. I also suspect that in the more general use of that term there are probably a lot of interacting factors that have affected Fraser sockeye. So yes, I agree with you.

Q Now, it seems to me, and I put it to you as a suggestion, Dr. Dill, that what you say in that sentence, namely, that farms are having some sort of negative impact on wild salmon productivity is unsubstantiated.

DR. DILL: I disagree with you. I believe it's substantiated by the results of the Connors analysis which others may disagree with. But that is the information I take as given in writing my report.

Q All right. Dr. Noakes, do you have something to

say on that?
DR. NOAKES: No, I had spent a fair amount of time looking at the Connors analysis and, in my opinion, it's flawed on several levels. So in terms of that, I would agree that it's highly speculative that there is an impact from farms. But I'd also like to go back to the point that you've just made in terms of multi-factorial. I mean we've had 11 other reports in terms of particular causes that might have contributed to the decline of sockeye and there's lots and lots of those.

So to try and isolate this farm salmon or any particular other variable in this kind of analysis, again, it was done very quickly, this analysis, but it's a much more complex problem than that. And I know even in our first data meeting when we were talking about one of the things we went through was trying to generate hypotheses about what we would check. And some of the things that came up were actually looking at things like abundance of herring in the Strait of Georgia and hake and other kinds of variables that we might look at. So I think, as I say, the time restrictions limited the kind of analysis that could be done but this is far too restrictive and I think it's not really a supportable statement.

Q All right. Thank you. Just by the way, I'm asking questions on Dr. Dill's report so I've been turning to him but of course, Dr. Noakes, if you have at any time anything you want to say, just jump in or shout and we'd be pleased to hear from you even if I haven't specifically gone to you. So thank you for interjecting on that and adding your point.

Dr. Dill, is it the case that going into the work that you did for the Commission, that you presumed there was a problem or impact of farms on the wild stocks?

DR. DILL: I would have to say that based on my experience in the Broughton and the pink salmon situation there, I probably went in with a slight expectation in that direction but essentially I went in with an open mind and looked at what the data had to say.

Q And on page 1 of your report, you say, and I take this to be your working hypothesis:

> The hypothesis that there is an effect of farms on sockeye survival was tested by examining the support for its predictions that there would be negative relationships between fish farm production levels...and Fraser sockeye survival.

That's the hypothesis and the line of thinking that you went into this report with, is it?

DR. DILL: That's correct. And what I'm reporting there, by the way, is the first part of the Connors analysis.

Q All right. And then, having done the study, relying on the information you did rely on, it seems that you found nothing to support that hypothesis or conclusion and you refer that, as I read it, at page 2 in the second paragraph there, the paragraph beginning "Despite". Do you agree with me that you found nothing to support the hypothesis that you set out on page 1 in terms of causation?

DR. DILL: No, I disagree. I believe that the Connors analysis provides some support. It's weak support because, as Dr. Connors himself has indicated, there is quite a bit of uncertainty around his results and you can't draw very strong inferences from it but nevertheless, I believe there is some support for, in effect.

Q Well, page 2 in that paragraph, beginning "Despite", you say "there's only correlation".

DR. DILL: That's often all we have. We haven't been able to experimentally manipulate the farms to see whether or not it has any effect on Fraser sockeye so we're always going to be stuck with correlations.

Q Is it fair to say then that the furthest, or the most you can take it is correlation?

DR. DILL: That, in addition with what I try to do in my report, which is to look at what we know about the various mechanisms and see what each ones, if any, might be involved.

Q Yesterday, Mr. Martland used a criminal law analogy in the difference between not proven and shown to be innocent or something to that effect. If we back out of criminal law and come back to fish and science. Thank you, Ms. Gaertner. I

understand the fundamental difference between the two of you, Dr. Noakes and Dr. Dill, is that Dr. Noakes has looked at material and I'll ask you one at a time. Dr. Noakes, you've looked at material and found that there is no evidence to support any causal or connection between farms and the survival of Fraser sockeye; is that right? Or put it in your own words.

DR. NOAKES: Yeah, that's basically correct. What I did was I tried to focus on the evidence and minimize any speculation and basically using sound scientific principles and analyses come to the conclusion, and looking at, as I say, the level of impact whether it be in escape farm fish, sea lice or disease and looking at all of the information as a whole, I didn't see any evidence. I would be leaning towards acquittal rather than just a finding of not guilty.

Q Dr. Dill, for you part, I understand the situation to be that you, too, didn't find any evidence to show a connection but nonetheless you still think or say there could be a problem?

DR. DILL: No, I wouldn't characterize it that way. The statement I made is that in the short-term analyses, we cannot find statistically significant relationships between sockeye productivity and the various metrics that Dr. Connors looked at. In the longer-term analysis, I believe there is a signal there that tells us that there is a relationship between farm salmon production and health of wild sockeye. And despite the fact that there is some uncertainty about that, that's what the analysis tells us. Now, Dr. Noakes may disagree with that analysis. I know, in fact, that he does and he's said that very clearly. But that's an issue for statisticians to decide. But I take it as a given that there is this relationship that Dr. Connors has shown and I reported as such.

And I really want to make sure that there's no misunderstanding here. I hope that Dr. Noakes was not implying that he uses proper scientific methodology and I rely on speculation. I think I also used scientific methodology. But the facts that I choose to base my inferences on may be different from his. I'm not simply looking at the numbers of things reported in the database but

also looking and using my knowledge of general ecology and fisheries biology in trying to put this together into perhaps a more sort of higher level examination of the situation rather than just looking at what diseases were on what farms in what year.

Q All right. I should give Dr. Noakes a chance to speak.

DR. NOAKES: I wasn't implying that you didn't use proper, but what I was implying was that, in terms of my scientific expertise, in terms of statistics in time series analysis that we did different kinds of analysis and came to different conclusions. And again, the evidence sort of speaks for my report and my rebuttal so to speak to the deficiencies I see in the Connors one. But getting back to the short-term one, one of the problems with short-term data is you often end up with the data points being around the mean. What we have in this particular case is we also have them at the extreme.

We have in 2009 the lowest returns on record or very near the lowest returns on record. And in 2010, although we may not know the exact number, they're certainly one of the highest on record. So we have a unique situation here. Even though it's a short one, you have a bit more power in terms of an ability to look at the relationship simply because we're looking at the extremes, at both the high and the low. So if we're going to see some sort of a signal associated with aquaculture or something else, then you should be able to see it when you're looking at those extremes because, as I say, most of the time when you're limited with a short-term time series, you're dealing with things right around the mean and you don't have a lot of ability to try and see those signals. But if there was something that caused the huge decline in 2009, it should jump out at you.

And the same thing would happen in 2010 in terms of a large return. Even though we don't know exactly what the number is, we know it was very large. So again, what changed to give us that kind of high contrast that we see in the two returns from those two years? It's a unique situation. As I say, most of the time when you

only have three or four years of data, the data points are typically closer to the mean and you don't have that kind of high contrast to actually look and see what the signal might be. So it's very powerful and it's very unique and it gives us a lot more information simply because we've got that huge range.

Q And do you also agree that the juxtapositioning is important as to 2007 and 2008, the outgoing years, and in terms of whether anything drastically different was happening in each of those two years, one to the other?

DR. NOAKES: Yeah, the unique part of it is not only it's the highest and lowest but they're back-to-back so it's not as if they were 20 years apart and you can argue that, well, things changed in 20 years. We're dealing with something that happened over a two-year period. So if you're looking for something, the smoking gun, then you should be able to see something in terms of what caused the dramatic decline in 2009 returns and what changed in order for the 2010 returns to go so high. And as I say, it's multi-factorial. I'm sure there's lots of issues that govern the survival. But if you're looking at particular ones, you should be able to see something in that signal. What changed in, say, aquaculture or salmon farms that would cause that kind of dramatic change?

Q Dr. Dill, I think you want to get in on this.

DR. DILL: I just wondered if I could refer you to the next sentence in that paragraph.

Q Sorry. You're the next sentence beyond the --

DR. DILL:

> Despite the a priori predictions...However, the fact that the 2006 brood year interacted with half as many pink salmon as the 2005 brood year, and that the corresponding 2010 returns were much greater than those in 2009 --

And that would be predicted by the Connors model.

> -- suggests that the Connors statistical model may be capturing some underlying causal relationships, and thus motivates the search for what these might be.

Q I see. And pinks are high numbers in odd years, are they?
DR. DILL: There's so much variation up and down the coast in what's and even years that that's not a particularly accurate statement. But perhaps Dr. Connors would be better placed to respond to that.
DR. NOAKES: Yeah, just to add onto that, though, as I say on one of my responses, if you were seeing a high impact from pink salmon, and of course my suggestion is you have to look at all of those species because it's like feeding teenagers. If you've got two teenaged boys, they're going to eat a lot, versus two teenaged girls. So if you're looking at an impact on your grocery bill, you'd like to know sort of a biomass or what kind of people you're feeding. But if you are seeing, in terms of pink salmon and if they are having this high and low in terms of the numbers, you should be seeing some sort of a two-year cycle, or a two-year signal in the survival for Fraser sockeye and you just don't see that. So what it implies to me, and again I've looked at a lot of salmon data, is that if there is an interaction out there it's with more than pink; it's with the whole sort of ecosystem in the North Pacific in terms of the interactions in terms of all of those salmon species because you have sockeye, pink and chum, that are eating at least at the same trophic level, and there may be other things out there as well. So as I say, if it is only pink, you should be seeing a stronger two-year signal in the survival and you just don't see that in the data.
Q Right. Dr. Connors?
DR. CONNORS: If I might add, we do find support for an influence of pink salmon and not just in my analysis and other analyses, which is why it was considered as one of the hypothesized contributors to the decline. And in regards to whether or not pink salmon are always stronger odd year/even year, it does vary up and down the coast depending on the region. But as across the entire North Pacific, particularly in recent years, there was very strong year/even year patterns. I could refer you to a figure in my report that plots out the abundance of pink salmon across the North Pacific across the entire time series that consider.

Q Dr. Dill referred to 50 percent pink in '06, I guess it is, versus '05, sorry '08 versus '07. There was still, even at the 50 percent level, an awful lot of pink, wasn't there?

DR. CONNORS: Yeah, if we could maybe pull up that figure?

Q Which one are you thinking of?

DR. CONNORS: Just give me one second and I'll find it here. Figure 5, page 12, I think is the one I was thinking. Now, with the *caveat* here. We're looking at the top right panel. With the *caveat* this has been standardized across the means so zero is the long-term average for the variable. The final data point would be the pink salmon that the 2010 returning salmon the majority of them interacted with whereas the data point preceding that which is the highest in the time series would be the data point that the 2009 returns interacted with. And so yes, there are still pink salmon in the North Pacific and a number of them, in the millions. I'd have to back out to get exactly where that turns out to.

But I think the point that Larry was making, and an interesting point about the analyses, is that you get a very strong contrast between these years, particularly towards the end of the time series. And that appears to be driving the interaction and the statistical support that we see for this interaction in the analyses. So it would qualitatively fit with an idea of good returns and bad returns. But I really caution (a) extrapolating this beyond the data that's been considered here and I again strongly advocate for re-examining this once we have that 2010 data. The argument that we don't qualitatively see a two-year pattern and time series of sockeye salmon productivity in the Fraser, in my opinion, is a weak argument. One needs to quantitatively examine the relationship between the two before one makes a statement like that.

Q All right.

DR. NOAKES: Actually, if I could respond to that. In fact, if you actually look in my report, when I look at the -- it would be in the appendix, one of the figures. When I pre-whiten, when I'm looking at whether there's a causal link correlation between, say, farm salmon production and the

returns-per-spawner, when I go into Appendix 2, I believe it is, where I actually look at pre-whitening that, one of the things I do is actually calculate the autocorrelation for that time series. And if there was a two-year cycle, you would see that in that autocorrelation plot. And so you don't see that.

What you will see is, if you look at the autocorrelation plot for that, you will see that it's decaying and then if you look at the partial autocorrelation function you will look at -- and again, I apologize for the technical aspects of this but if you look at that then you will see a spike at Lag 1, which means that there is a correlation one year but not at two years. So as I say, if there was a strong signal with relation to the number of pink salmon in there, you would see a two-year cycle coming through and you just don't see it. There's no significant correlation at Lag 2 and I apologize for that.

Q All right. That's fine. Dr. Connors?

DR. CONNORS: I'll just make one last comment.

Q Mr. Martland spoke of tennis earlier.

DR. CONNORS: Yeah, I just want to make one last comment on this. I'm of the opinion that the most rigorous analysis to consider the hypothesis for pink salmon abundance would be considering not just 20 years of information on the Fraser, but considering all the available information we have across both the individual stocks in the Fraser, as well as other sockeye salmon populations in B.C. and elsewhere. And so I maintain that that's a more rigorous examination of the relationship or the support for that hypothesis. And I also want to reiterate that this is not just a hypothesis that I have put forward. I'm simply considering this because it's been independently identified by experts in that field as being a likely to possible contributor to declines in Fraser sockeye.

Q All right. Well, we'll leave that point there with that, I think. At page 34 of your report, Dr. Dill, there is a paragraph there where you call for a consolidated database. Now, I know you said earlier that you're not familiar with the conditions of licence and reporting and so forth. Dr. Noakes said he is. Under the federal regime

that's now in place for the last nine or ten months now and going forward, that's exactly what DFO is doing, isn't it, Dr. Noakes, creating a means to have a comprehensive long-term database of information?

DR. NOAKES: That was my understanding with my conversation with Andy but I don't know the details of it.

Q All right.

DR. NOAKES: Andy Thompson, that is.

Q Now, I want to ask Dr. Noakes and Dill about the material that Dr. Dill used and referred to and relied upon in terms of reference to articles and so forth. I suggest to you, Dr. Dill, that you use literature in your report and to support the propositions in your report, that is literature that tends to be against aquaculture. Is that a fair assessment?

DR. DILL: No.

Q All right. Do you consider the literature you used to be a balanced set of literature?

DR. DILL: I believe I mentioned the controversy in the Broughton, if that's the area you're speaking of specifically. I think that's the only place where there might be any concern.

Q Well, certainly there's mention of controversy. But in terms of reliance, is it literature that tends to be against aquaculture that you relied on?

DR. DILL: I relied on what I considered to be the best science.

Q All right. Dr. Noakes, do you have a comment on this, and that is, whether the literature that is relied on in Dr. Dill's report, represents a balanced approach to literature or whether it's not?

DR. NOAKES: I don't have a comment with that respect. I mean I respect Dr. Dill. We approached the same problem from different aspects and our backgrounds dictated sort of what we looked at. So I'm not in a position to say whether it's balanced or not because obviously he has different expertise and a different insight into the issue. And it was certainly different than my literature review which again reflects the approach that I took. So I don't really have a comment on whether it's balanced or unbalanced. I'm assuming that it

reflects his understanding and his background in expertise in the way he approached the problem.
Q All right.
DR. NOAKES: I don't want to put words in his mouth.
Q Okay. Thank you. I'm almost at my time now. I've probably got about four minutes so I'll just have a couple of final questions. I just want to be clear on one point. It may be there in the evidence but I'm not sure. Dr. Dill, Dr. Connors is a recently-completed PhD student of yours, isn't he?
DR. DILL: That's correct.
Q And you agree with that, I take it, Dr. Connors?
DR. CONNORS: I do indeed.
Q So you have a long-term association, professionally, in your work, the two of you?
DR. DILL: We do.
DR. CONNORS: That's correct.
Q How long have you worked together?
DR. CONNORS: I started my PhD with Larry in the fall of 2005. And I believe we probably met six months to three-quarters of a year before that.
MR. TAYLOR: All right. Thank you. Those are my questions.
MR. MARTLAND: Mr. Commissioner, counsel for the Province with 60 minutes.
MS. CALLAN: Mr. Commissioner, Callan, C-a-l-l-a-n, initials T.E., appearing on behalf of Her Majesty the Queen in Right of the Province of British Columbia.

CROSS-EXAMINATION BY MS. CALLAN:

Q Dr. Noakes, you analyzed where disease has been reported in relation to sockeye salmon migrations route. Since 2003, there have been 38 cases reported of furunculosis and 29 of the 38 occurred on the west coast of British Columbia?
DR. NOAKES: I believe that's correct.
Q And specifically, Vancouver Island's west coast?
DR. NOAKES: Yes.
Q There are no reported instances of IHN since 2003?
DR. NOAKES: That's correct, based on the data.
Q Since 2003, there's also been a significant decline in BKD, or bacterial kidney disease, within the main migration route of sockeye salmon on the Fraser River?

DR. NOAKES: That's correct.
Q And only one farm around the Fraser River sockeye salmon's migration route reported BKD in 2007 and, specifically, that was at Bennett Point?
DR. NOAKES: I believe so. There's a figure in my report and I can't recall the figure number that I identify for each year where the BKD outbreaks were. Most of them were in salmon in Sechelt and Jervis Inlet in the last few years. And for each of the last three years, there was one farm. One I think actually was in the Broughton.
Q Have you reviewed the paper entitled "The Abuse of Power: The Pervasive Fallacy of Power Calculations for Data Analysis"? And it's at Provincial Tab 3.
DR. NOAKES: Yes, I'm aware of that. I think John Hoenig sent it to me right after it came out.
Q Could you summarize this report in a few words that a non-statistician could understand?
DR. NOAKES: Okay. Basically, what it's saying is that there's people who are doing statistical analysis, and this is a point that's maybe missed in terms of the title, but the emphasis should be on generating an appropriate hypothesis rather than looking at doing post hoc power analysis. So what you want to do is when you're looking at how you want to analyze data and test hypothesis is pay a lot more attention to what you want to test, make sure that it's worded properly and also you want to set the level in terms of how much evidence you need to, say, prove or at least reject the known hypothesis. Usually, the known hypothesis is that there's no effect. So you normally wouldn't say I'm assuming that there's an effect and then you want to test for that.

So normally you say there's no effect and then what you're looking at is, do the data provide evidence of an effect? And if they don't at a particular level, so it's, say, the burden of proof, if they don't then you simply can't reject the known hypothesis. So you can't prove that there's no effect. All you can say is the data do not provide sufficient evidence for an effect. What often happens, and I teach my students not to do this, is what will happen is once you've done your analysis and found that there is no effect, or at least a particular significant level, then you go back and do what's called a post hoc, in

other words, after-the-fact power analysis to say, well, I had too few data and if I'd had more I could have detected that.

It really doesn't give you any more information than what you're already done if you've properly identified what your hypothesis is and set the bar in terms of evidence. So for instance, to use a judicial example, there's a certain level of proof that's required to convict somebody. And it would be as if after the trial you suddenly decide, well, you know, maybe we'll just lower that a bit because I think he's guilty or we'll raise it a bit because he seems like a nice guy. What you want to do is, the statistics, set this all ahead of time. And the reason you do that is you want to avoid either inadvertently or purposely putting in your own bias into how you're going to analyze that.

So you identify those ahead of time so that you don't have to go back and then make an adjustment which can introduce bias into your conclusions and your inference. So what he's doing here basically he's doing in a very mathematical and technical point, is showing that in terms of doing these analysis, what will happen is you can often find after the fact low power. But that really doesn't give you any more information than you already had if you properly actually identify what your hypothesis and set your bar. As I say, it voids the problem of after the fact saying if you didn't get the result you want, saying, well, if only I had more data or if I'd lowered the bar instead of, for instance, the normal burden of proof is, say, significant levels of 5 percent.

So in other words, what you're looking at is these data wouldn't have been -- there's only a one-in-20 chance that you would have observed this extreme a result or more extreme in terms of looking at your data. So what it avoids is saying, well, I'll accept a one-in-ten chance rather than the one-in-20. So it's a way of just making sure that everything's done up front rather than, as I say, inadvertently introducing bias into the conclusions at the end.

Q And this is a paper you agree with?

DR. NOAKES: Oh, yes. I mean, John's a first-rate

statistician. I have known him for many years.
MS. CALLAN: Can we mark this paper as the next exhibit?
THE REGISTRAR: Exhibit 1551.

EXHIBIT 1551: The Abuse of Power: The Pervasive Fallacy of Power Calculations for Data Analysis

THE COMMISSIONER: Ms. Callan, I wonder if I could just ask the witness a question?
DR. NOAKES: Sure.
THE COMMISSIONER: On this page on the screen, and I don't know how to -- I'm looking at the right column on the screen and the paragraph starts, "It is important to understand." I hope everybody is with me on which column I'm looking at. About five sentences down, it says:

> However, there is increasing recognition that a reversal of the usual scientific burden of proof...

I want to try and move away from analogies to the legal burden of proof because it's well-established in both civil and criminal law so it doesn't change from case-to-case. It's always the same. Are you telling the court that there is not the usual burden of proof or that there could be a different burden of proof for each different hypothesis?
DR. NOAKES: There can be. And it really depends on, as I say, if you get back to looking at what your hypothesis is and the level of proof or the significant levels you're willing to accept, basically can be done on a case-by-case basis but it should be done before you do your analysis. So for instance, the normal level of proof is a one-in-20 or a P level of .05. In scientific papers, that's in the normal level of proof. If it was a particularly important problem that you were looking at, say, for instance, for human health or whatnot, you might want to put a higher level burden of proof there so you might be .01.
So for instance, if I'm testing a vaccine and I want to make sure that that the effect I'm seeing is positive, then you might put a higher

burden of proof on that. But it always should be done ahead of doing the analysis because one of the voids is then going back if I didn't reach that burden of proof then I'll simply reduce the significant levels because you just want to avoid even accidentally including some bias into the results and interpretation.

THE COMMISSIONER: So may I just ask then, if you and Dr. Dill were given the identical terms of reference around a research project, is it possible that in engaging your research you would each have in your premise a different burden of proof that you wished to establish?

DR. NOAKES: That's certainly possible. And as I say, when you do statistical analysis, the usual or the normal and accepted burden of proof is an alpha or a P level of .05. But there are possibilities where you could adopt a different burden of proof. The key is that you set that level before you do your analysis and you don't adjust it afterwards.

THE COMMISSIONER: Dr. Dill?

DR. DILL: In truth, you should set that level before you even collect the data but that wasn't an option that was given to us. The data was collected for other reasons and provided to us to do the best that we could with it. I just do want to clarify one thing, though, and that is that the type of analysis, the long-term analysis, is not based on what's commonly called "frequentist statistics", it's based on a different kind of statistical analysis in which we look at the degree of uncertainty around alternative hypotheses rather than looking at a simple null versus a research hypothesis.

DR. NOAKES: That's actually not quite correct. I mean the models are based on frequences. And I apologize for the technical term. They're basically statistical models, which are dealt with in the normal fashion that you would deal with the model. The difference is that they're using a criterion actually to select the best model and that's a slightly different mechanism that they're using. But the models themselves are frequent in the sense that they're using the data. They're assuming a probability distribution and an error distribution and then estimating the parameters from those particular models. And it's only after

the fact that they're using some criterion to say this model is better than that model. So it is a frequentist in terms of looking at estimating the models. And again, I'm sorry for the technical.

DR. DILL: But it's not in terms of setting a P value or a conference interval in the same way.

DR. NOAKES: Oh, absolutely. When you're looking at these models the P values in terms of looking at the parameter estimates to see whether they're significant, looking at the significance of the model, you are using that P value exactly as I described in terms of here's the level of proof that I'm looking for in those particular models. It's only after you've estimated the models that you then go back and use the results of that analysis to actually determine, is there a model or a set of models which are better than the others?

THE COMMISSIONER: Ms. Callan, I think I've taken us to the lunch break. I apologize for using up a little bit of your time.

MS. CALLAN: That's all right. Your questions are always the most important ones.

DR. KORMAN: Can I clarify one issue with relation to this paper? I'm not exactly sure why you're bringing this here but if we're talking about the issue of whether there's enough salmon, from the salmon farming database, if there's enough information to establish a correlation with Fraser sockeye survival. If that's the context you're bringing this up in, then this paper does -- basically when you have a sample size of three or four years, you are going to have very low power to detect an effect of salmon farms on Fraser survival. And this paper does not in any way counter what I just said.

MS. CALLAN: Oh, I'm not going there.

DR. KORMAN: Okay.

MS. CALLAN: I have a specific reason that I'll bring up later in my cross-examination.

DR. KORMAN: Okay. I just wanted to make that point.

MS. CALLAN: So tune in at two o'clock to find out what that is.

THE REGISTRAR: The hearing is now adjourned till 2:00 p.m.

(PROCEEDINGS ADJOURNED FOR NOON RECESS)

(PROCEEDINGS RECONVENED)

THE REGISTRAR: Hearing is resumed.

CROSS-EXAMINATION BY MS. CALLAN, continuing:

Q Dr. Korman, you tracked a number of diseases that Dr. Kent identified as high risk.
DR. KORMAN: Yes.
Q And specifically furunculosis, IHN, BKD, and vibrio or VHSV?
DR. KORMAN: Yes, those four.
Q Now, you identified a statistically significant declining trend towards the number of high-risk diseases reported by salmon farmers between 2003 and 2010?
DR. KORMAN: Yes.
Q This comprised of 496 groups of between five to eight fish?
DR. KORMAN: Let's -- can we go to the page there, because I'm... I think the correlation you're referring to is from the B.C. Salmon Farmers' Fish Health Event data file, but let's just go check that if we can. If you can tell me --
Q It should be page 7.
DR. KORMAN: Page 7. Yeah, that's right. So it's not -- so it's not associated with that 496 fish, so that that is is, you know, salmon farmers have to report on all fish health events, right, and that goes into a database. I then classify those high-risk ones and correlated those over time so the 496 fish, I believe, would be an example of the number of fish sampled during the audits in any one year randomly. So I don't think that applies to that, if I'm understanding correctly.
Q Okay. And the 496 is identified on page 8 paragraph 1.
DR. KORMAN: Right. So that refers to the audit data where they randomly pick fresh silver fish and sample them for a series of viruses and bacteria but that is not the same data that's supplied by the salmon farmers. Just to clarify.
Q Would you agree with the statement that the audit and surveillancing data was quite encouraging?
DR. KORMAN: It shows, you know, fairly low frequency of disease in these -- of the diseases that are monitored in these fresh silvers, yeah, very low

frequency.
Q And specifically, a low number of positive findings for VHSV?
DR. KORMAN: Is -- are you referring to --
Q Vibrio.
DR. KORMAN: -- vibrio? Yeah, and that's summarized in -- we have -- there's a table that will document that. Just find that very quickly, so I make sure it's accurate. Oh, no, it's in a Figure actually. Yeah, Figure 5 on page 19 is -- yeah, that purple is indetectible in that figure or very hard to see, so that indicates very low frequency of that.
Q There were no reports of ISAV?
DR. KORMAN: No, that's one of the -- that's one of the viruses that's tested for in this random screening and there's been no positive occurrences of the virus from that testing.
Q And you would agree with your statement at paragraph -- at page 10 that negative effects of salmon farms on returns of Fraser River sockeye between 2002 and 2010 were not apparent based on a qualitative comparison with salmon farming data?
DR. KORMAN: Yes, I wrote that and I agree with it.
Q My next set of questions are for Dr. Connors. Your statistical analysis based on B.C. MAL's record showed no statistical support for a relationship between Fraser River sockeye returns and sea louse abundance on farmed salmon in the Spring or summer of marine entry, the occurrence of high-risk pathogens on farmed salmon in the year sockeye migrate to sea and the proportion of farmed fish fresh silvers -- it's a bit of a tongue-twister.
DR. CONNORS: I agree with that statement except for the very beginning where you say returns. So I just want to be, you know, for the record completely clear that what I related there are what I termed survival anomalies which we can go into if we want, but not specifically relating the number of fish that returned and spawned in the river.
Q Now, for the second part of your analysis you analyzed fish farm production numbers, winter sea surface temperatures and pink salmon return numbers. However, your results had a high uncertainty that was associated with the calculation such that taking anything from it

would be speculative.
DR. CONNORS: I wouldn't say that taking any -- I mean, that's a very broad statement, taking anything from it would be speculative. I think that given the uncertainty around some of these estimated effects, the inference that we draw from it should be done with caution and should be balanced given the weight of other evidence that you guys are considering.
Q Now, including the three variables as you did, it makes it impossible to figure out if the ocean conditions alone or pink salmon alone were positive?
DR. CONNORS: Not a hundred percent sure what you mean by that, but what I can say is the approach that I took considered a suite of different working hypothesized -- different hypotheses for the decline. Those hypotheses included just the influence of sea surface temperatures, just the influence of pink salmon abundance, just the influence of farmed salmon production, as well as combinations of those. And so in that sense, I did consider the hypothesis that just sea surface temperatures alone had the greatest support given the data and the analysis certainly suggests otherwise.
Q Now, I understand that you did an analysis excluding the pink salmon abundance and the surface temperature. So specifically, you were doing farmed salmon numbers versus sockeye returns?
DR. CONNORS: Yes. One of the analyses looked -- the hypothesis just considered farmed salmon production along early marine migration routes.
Q And I understand that no correlation was found between sockeye returns and farmed salmon production numbers?
DR. CONNORS: Okay. So I should just clarify it. I think I was interpreting your initial question as referring to the longer-term analysis that I did, but I believe you're referring to what I call the first component to my analysis that looked at the number of salmon during the shorter time series that we had; is that correct?
DR. CONNORS: No. I'm talking about the second series.
Q Okay. So then can you restate what the conclusions were from it? I apologize. I just

want to be clear which analysis we're talking about.

Q Okay. No specific statistically significant correlation was found between sockeye returns and farmed salmon production.

DR. CONNORS: Okay. So I do not report the results of a correlation test between just farmed salmon production and sockeye salmon productivity. If we go to Table 5 in my report on page 19, this table illustrates the different hypotheses that I considered and on them, the two that would most -- would refer to the question you asked would be number 19, which is no, which is not the consideration of any of these variables, and number 17 would be farm which is the consideration of just farmed salmon production with the exclusion of or not considered in sea surface temperature or pink salmon abundance. So the question is whether or not based on that, I found a statistically significant correlation between farmed salmon production and sockeye productivity, although I don't report as such in the report, nor do I couch it in a statistically significant framework, if we look at the support for those different models, particularly the column that says "Delta AICC", we see that the difference roughly is about seven or a little less than seven between number 19 and number 17, and so that's fairly strong support for the inclusion of a term for farmed salmon production. But as this -- I believe this analysis illustrates, it would be unwise to just consider -- I mean, given the suite of different hypothesized drivers and candidate models I considered, that's a very, very unlikely model relative to the ones at the top of the list.

Q Dr. Noakes, do you have anything to add?

DR. NOAKES: In my -- in the first part of my analysis in my report, I actually looked at the correlation between farmed salmon production and log recruit per spawner which is an index of sockeye productivity, and in my analysis I didn't find any significant correlation between the two.

DR. CONNORS: So if I could just add a comment there then. Comparing the two analyses is really difficult. Dr. Noakes' analysis considered a portion of the sockeye Fraser time series in aggregate related to farmed salmon production on

the East Coast of Vancouver Island. This analysis is based on sockeye salmon populations both from the Fraser, as well as elsewhere, so it includes spatial contrasts and the exposure to farmed salmon production, as well as these other variables, across the entire time series for which we have available information.

Q If the commission could turn to provincial Tab 1, I'll put this to the group generally. Would you agree that if a correlation had been found, it doesn't mean causation? For example, in this figure, it's fair to say that fish farm production has been going up in a similar fashion to the population of British Columbia?

DR. CONNORS: I guess -- can I start? So of course, as is clearly stated in my report, correlation does not equal causation. And we can get into a discussion about correlation versus causation. This is an interesting graph. It shows that over a time period from 1985 to almost 2010 the human population in B.C. has been increasing and during that same period the human population in -- sorry, the production of farmed salmon has also been increasing along the inside of Vancouver Island.

I want to be clear that we want to be cautious comparing this to what I've done. What I've done would be more analogous to considering the human population across all these populations across the entire time series. And so I just wanted to be clear that we're not directly comparing the two kinds of analysis.

Q Now, would it be reasonable to assume that increases in B.C. human population might be associated with decreased sockeye salmon returns?

DR. CONNORS: Yeah. That's an absolutely legitimate hypothesis but again, I'll direct you to the Pacific Salmon Commission expert workshop that considered a whole suite of different hypothesized drivers to the decline of sockeye salmon and of those, they considered four that were possible-to-likely and those are the ones that are included in my report.

MS. CALLAN: If we could mark this as the next exhibit and to give a bit of credit for the document, this is a graph that was created by Dr. Marty.

THE REGISTRAR: Exhibit number 1552.

EXHIBIT 1552: Table showing correlation between B.C. population and farm fish production

MR. MARTLAND: This one at least, if I might, Mr. Commissioner, pause to observe because I think whatever the approach is with respect to documents created for the purpose of questions on other occasions there have been objections to those, in other cases counsel who made objections are trying to lead the documents. I don't know where that takes us. I don't hear any other objections. We don't raise an objection if that's what it is. I suppose Dr. Marty is here and could be asked about it. I do simply want to make that observation though.

MS. CALLAN:

Q And this is another question for the panel generally, so whoever feels most qualified to answer it can jump in. What is psuedoreplication? Dr. Connors, feel free to answer if you want.

DR. CONNORS: Okay. So pseudoreplication would be the inclusion of a suite of observations that you treat as being independent observations when they aren't. By treating them as independent observations, you increase the likelihood that you might find a statistically significant relationship when in fact a given observation isn't truly independent of another one.

Q Now, pseudoreplication is -- in mathematical modelling should be avoided?

DR. CONNORS: Absolutely.

Q Dr. Noakes?

DR. NOAKES: Yes, that's correct.

Q Now, Dr. Connors, Dr. McAllister reviewed your report and stated at page 72, seven lines down:

> It appears that all of the statistical tests reported are invalid due to pseudo-replication within each year.

Can you explain the basis of this comment and what you did in response?

DR. CONNORS: Yes, absolutely. That's a very fair point to bring up. So in the analysis that I did, I considered the suite of different sockeye salmon populations. And in a given year, for example,

for Fraser stocks that migrate up the inside of Vancouver Island we only have a single estimate of farmed salmon production along that migration route. As a result, if we considered each individual population as an independent observation of the relationship between productivity and farmed salmon production, we would be committing the error of pseudoreplication.

Now, in the draft report that I wrote for this technical report I didn't discuss that element of the analysis and though I attempted to account for it, he very correctly pointed out that the way that I structured my models did not. As a result, I changed the way that I structured my models such that correlations in observations within a given year at the unit with which things are measured, so for example, farmed salmon production and the stocks that are all exposed to a single value, are appropriately accounted for in terms of the fact that they're not independent observations but are, instead, correlated.

Q So this analysis for your report after the changes were made made it more defensible?

DR. CONNORS: This analysis after the changes were made accounted for concerns with regards to pseudoreplication.

Q Now, pseudoreplication could be used to increase the apparent power of a statistical test, but it would not increase the true power of the statistical test? And anyone can jump in if they want on this question.

DR. NOAKES: Yeah, that's true, because when you have pseudoreplication you assume that you have more degrees of freedom than you actually do and that affects the -- that negatively affects your ability to detect a significant -- you over-estimate the response or statistical significance of the test.

DR. KORMAN: If I could jump in here, the modification that Brendan Connors made with respect to the issue of pseudoreplication is in his equation 4 of his report page 14 and I've looked at those comments and the work you've done and I feel that the inclusion -- the modification of his model to account for the correlation in survival rates among stocks within a region basically deals with

the pseudoreplication issue, thus, you know, the extent -- thus I think is estimate of what is significant or not then, based on a reasonable model and therefore, probably reasonable estimates of significance.

Q Now, in the last ten years, Fraser River pink sockeye salmon have been having some extraordinary returns; would you agree?

DR. CONNORS: Is this directed at me?

Q Yes.

DR. CONNORS: Yeah. I would agree qualitatively that my understanding is that they've had very strong productivity over the past while, but I wouldn't say that I'm, you know, expertly qualified to attest to those patterns.

Q Since pink salmon have had a number of extremely good runs since 2000, would you assume that farm production numbers improved the chances of pink salmon survival?

DR. CONNORS: Well, that's a good question. I haven't done the analysis that looks at all the available information to ask whether or not there's a positive, negative or no relationship between the productivity of pink salmon populations across a given area and farmed salmon production. But I will point out, I mean, this is an important point. How do we rectify these apparently very contradictory observations? Pink salmon are doing great, sockeye aren't doing well. There's this interesting observation that Harrison River sockeye salmon that have a life history that's more closely to pink salmon, are also doing well, and I don't have any, you know, magic answer for that but I think one interesting piece of information that comes from my analysis is that it does suggest that the species identity does matter. And we know that there are -- there is strong evidence for competition between pink salmon and the open ocean and sockeye salmon and it may be, and my analysis certainly suggests that that's a very important determinate of any influence or association with salmon aquaculture.

Q Okay. I'm going to move topics a little bit now, so my next series of questions are for Dr. Dill, but if anyone has anything they want to add with respect to it, feel free to jump in.

In what years do you think sea lice from fish

farms started to infest sockeye salmon?
DR. DILL: I actually haven't any idea.
Q Would you agree that the first time sea lice were reported in a journal to infect juvenile sockeye salmon was in 2005 in Morton's journal entitled "Sea Louse Infestation in Wild Juvenile Salmon and Pacific Herring" which is Provincial Tab 4?
DR. DILL: I actually can't remember if sockeye salmon were in there or if that was pink salmon. Could you put it up? It is? Yeah. Yeah, I'm not aware of any earlier work than that.
Q Now, are sockeye infested with sea lice difficult to diagnose?
DR. DILL: It's sometimes difficult to diagnose the species of sea louse when they're in the juvenile stage and often they have to be brought into the laboratory to distinguish between the two.
Q Would you agree though that juvenile sockeye salmon were probably infested with sea lice before Morton observed it in her 2005 paper?
DR. DILL: If they were on the farms, I think it's probably a good bet.
Q Would you agree that sea louse infestation is probably -- oh, you actually just answered the question, that it's been going on as long as farms have reared Atlantic salmon.
DR. DILL: Sea lice infestation of Pacific salmon has been going on a lot longer than that. This is a natural host parasite system. The issue is at what stage they get onto wild salmon.
MS. CALLAN: I think this document is already marked, Morton's paper. Can you just confirm for me?
MR. MARTLAND: I'm going to suggest, Mr. Commissioner, that we mark this as an exhibit. If during the break it turns out that -- perhaps we should defer that till after the break and we can see if we're able to learn whether this has already been put in.
MS. CALLAN: If we could now turn to Provincial Tab 6.
Q Now, I understand Dr. Dill and Dr. Connors were both authors in this paper?
DR. DILL: That's correct.
DR. CONNORS: Correct.
Q If you look at this paper, it describes the years preceding salmon louse infestations as 1975 to 2000 and then 2001 to 2002, 2004 and 2006 as during recurrent salmon louse infestations?

DR. CONNORS: During recurrent documented salmon louse infestations on juvenile pink and chum salmon, yes.
Q Would you agree that this paper says that in 2000 that year pre-dates louse infestations?
DR. CONNORS: At the time, we did not have any evidence of infestations on those juvenile salmonids prior to that.
Q So for the purposes of your paper you assumed then that there were zero lice on the farms?
DR. CONNORS: We made the assumption in this analysis that infestations on juvenile pink and chum salmon, yes, were zero during those time periods.
Q When writing this paper, did you have access to farmed sea lice counts?
DR. DILL: No.
DR. CONNORS: No.
Q Did you examine any wild coho salmon in 2000 to confirm they had no louse infestations?
DR. CONNORS: No. An important point here is that there's only two years of data. It's a proceeding paper that this cites for which we had comparable observations of sea louse infestations on coho versus pinks and chums. So we were making the assumption in here that during those years where there are infestations on pinks, there was coincident infestations on coho salmon.
Q And Dr. Noakes, do you have anything to add to this point?
DR. NOAKES: I don't have anything specific to this study, but I think it's pretty reasonable to assume that there were sea lice on juvenile salmon for a very long time before salmon farms were here. They're naturally occurring, as Dr. Dill points out. They're naturally occurring parasite and you would expect to find them on juvenile salmon of all species from -- for however long salmon has been around.
DR. CONNORS: Nor do I disagree.
DR. DILL: Yeah, I disagree completely because studies that have been done in parts of the coast where there aren't salmon farms have found either no lice on juvenile salmon or very low levels compared to those that were found in the Broughton Archipelago in the years of this study. So they are natural parasites, but on much larger fish.
DR. NOAKES: Well, I think common sense would suggest

that if they're on adult salmon that they're also on juvenile salmon. The first documentation, I mean, and there's back to 1964 I think there was a paper by Parker documenting sea lice and those sorts of things, and I think there's -- I mean, it's -- I think it's common sense and logical to -- and reasonable to assume that there were sea lice on juvenile salmon well before salmon farms were here.

DR. DILL: Well, you may say it's common sense and logical, but I would contrast that with a number of studies done on the north coast and in Alaska in which it's simply not the case. They were not found on juvenile fish. And when I'm talking juvenile fish, I'm talking the size that they're parasitizing in the Broughton Archipelago. They simply were not found or found in very low levels until they got further out into the marine environment where they could interact with larger salmonids, either feeding in that area or coming back to spawn. But in the shallow near-shore environment there's almost no evidence with the exception of that Parker paper, which I believe was *Caligus*.

Q Now, Dr. Marty published a paper in the proceedings of the *National Academy of Science* in 2010 --

MS. CALLAN: Oh, sorry. We'll mark this as the next exhibit, Provincial Tab 6, before I move on.

THE REGISTRAR: Ms. Callan, we can mark Tab 4 if you wish. We've found that it has not been marked.

MS. CALLAN: Okay.

THE REGISTRAR: So that will be 1553, that will be for Tab 4. And Tab 6 will be marked as 1554.

EXHIBIT 1553: Sea Louse Infestation in Wild Juvenile Salmon and Pacific Herring Associated with Fish Farms off the East-Central Coast of Vancouver Island, British Columbia - Morton et al

EXHIBIT 1554: Coho salmon productivity in relation to salmon lice from infected prey and salmon farms - Connors et al

MS. CALLAN: And while we're in the marking mood, if you could turn to Provincial Tab 7. If we could

mark this as the next exhibit.
THE REGISTRAR: 1555.

EXHIBIT 1555: Relationship of farm salmon, sea lice, and wild salmon populations - Marty et al

MS. CALLAN: So Provincial Tab 7 is Dr. Marty's 2010 PNAS paper.
Q Now, Dr. Marty had access to the farm lice counts and estimated the number of adult female sea lice on farms in March 2000 as 9.1 million, which was slightly greater than the amount in March 2001 of 7.5 million; would you agree with that?
DR. CONNORS: So we're referring to Figure 1?
Q That looks like it.
DR. CONNORS: Okay. I agree. I can't remember the exact numbers you said, but I certainly agree that there were lice documented on farmed salmon right up to the 2000s, absolutely.
Q Okay. Now, this seems to be one of the big areas of disagreement between yourself and this -- between Provincial Tab 6 or Exhibit 1554 and Exhibit 1555 and your new paper; is that correct?
DR. CONNORS: Not quite. I would agree that the data that was then made available upon the publication of Tab 7 here, the Gary Marty paper, certainly provided more information that needs to be evaluated relative to the dynamics of adjacent wild salmon populations. It also points out that the assumption that juvenile salmon were -- or, sorry, the assumption that farmed salmon had no lice on them prior to 2001 is, you know, an incorrect assumption. And so I think we're going in this direction, but as a result we've taken this information that's been -- that was released as part of this, it's the first time that we had access to the number of sea lice on farmed salmon and related -- and re-examined those relationships in those two data sets and that's, I believe, probably one of the next tabs that you're going to come to.
Q Does anyone else want to add any comments to that question?
DR. KORMAN: Well, I mean, just in a larger context, the debate about these papers, the controversy in terms of Noakes' interpretation versus others and

all the rebuttal papers has largely been around the fact that we don't have a long-term reliable index of sea lice abundance and so various authors have had to, you know, make guesses as to when -- the extent of infection and sea lice abundance prior to dates when that information was routinely collected. And that's led to a lot of the debate among the validity of the conclusions from these papers. So I just thought I'd provide maybe that helpful overview.

DR. NOAKES: There's also another point too is that even though we don't know the number of lice before 2000, but you need to consider that in the ten or 20 years leading up to that, particularly in the Broughton, there was a large increase in the number of pink salmon returning, so there's an inconsistency in terms of sort of ignoring the fact that you had this large increase while farmed salmon was actually increasing, as well. And the other thing that -- one of the problems here and also in the coming -- in the paper that has just come out, is that everything is being referenced with respect to the highest returns of pink salmon in that area that we've basically gone on record. So essentially, you're reviewing things from the top. So when you look at it in terms of what direction they're going, you're exaggerating the decline because your reference point is not the mean, it's not the lowest one. It's certainly the highest one. So I think that that's a real problem in addition to just assuming that the lice were zero before then. You have to take into account the salmon production that was happening in that area before and you also have to take into account where you're viewing all of this from, so you're viewing it from the top and as I say, that tends to exaggerate any relationship or any perceived relationship that you might have. It's the same problem in the paper that just came out.

Q Go ahead, Dr. Connors.

DR. CONNORS: So just to be clear, I mean, I think we can certainly probably all agree that an understanding of what was going on during this black box period of about I believe it's ten years prior to the early 2000 during which there was an escalation of the number of farmed salmon in the Broughton but for which we don't have information

on the number of lice on farmed fish or information on the number of lice on juvenile fish is an area of considerable uncertainty.

The approach that we took in our most recent publication is we treated that as missing information in our analysis. We otherwise included information during this time period that you're looking on the figure, as well as adjacent reference populations and periods preceding aquaculture's presence at all during which it's not possible for there to be lice from salmon farms being transmitted.

Q Okay. So for a bit of housekeeping, is Provincial Tab 9 your paper that you just published a couple of days ago in the proceedings of the National Academy of Science?

DR. CONNORS: Yes.

MS. CALLAN: If we could --

DR. DILL: Could I just make a comment before we move on?

MS. CALLAN: Oh, I'm not moving on, but if I could mark this first and then --

DR. DILL: Oh, sure. Yeah.

MS. CALLAN: Okay. If I could mark this as the next exhibit?

THE REGISTRAR: 1556.

EXHIBIT 1556: Effects of parasites from salmon farms on productivity of wild salmon - Krkosek et al

MS. CALLAN:

Q Now you can go on, Dr. Dill.

DR. DILL: Thank you. I just wanted to say that I think this controversy or this contrast between these two papers illustrates something really good about science, and that is that science builds incrementally on other knowledge. And there are two really good things about the Marty, Gary Marty et al paper. Quite apart from the fact that I disagree with the analysis and I think our analysis is an improvement, but one of them is that the data is available to us now. When someone publishes a paper and they have access to data that no one else does, then it behoves them to make that data available to everyone and it was the fact that they published their paper at all

that made us -- made it possible for us to do this kind of analysis and so I think that's great.

The other thing is that there was one finding in this paper that - I'm talking about the Marty et al paper - the finding that the number of lice on juvenile pink salmon was closely related to the number of lice on the farm. That pretty much nails now that relationship to the lice on the wild fish are coming from the farm. This is something that the fish farm has denied for many years but I now think that it's incontroversial.

Q Now, I'm going to -- oh, go on, Dr. Noakes.

DR. NOAKES: It's okay. It's a criticism I have of the Marty paper, as well, is I think they could have -- there are other papers that came out after the Marty paper or at least about the same time in terms of other hosts for *Lep s.* in terms of three-spine sticklebacks and those sorts of things. So as Marty correctly points out and as Terry Quinn, who's a very competent stock assessment person, there is a good predictor in terms of the number of lice on returning salmon in the Fall is a good predictor of the number of lice that you're going to see on farms in the previous year.

But it ignores the fact that there are other -- there are other syncs or at least hosts of lice that are from those fish that are returning in the Fall, and again, I don't think there's any question that some of those lice are coming from the farms and onto pink salmon, but there are other hosts there, as well, that probably could have been mentioned in the Marty et al paper.

Q And what do you think those hosts are?

DR. NOAKES: Well, as I say, in Jones and Prosperi-Porta, and I don't know if that paper's been entered into evidence or not --

Q I don't think it has.

DR. NOAKES: Okay. There is another paper out there that talks about high level -- it was -- I referenced it in mine and I don't know if you did in yours, but there's -- but there are other -- there's another paper out there that references high levels and high prevalence of sea lice, salmon lice, on three-spine stickleback, which is another tongue-twister.

DR. DILL: But, correct me if I'm wrong. There have been only a very few adult lice and absolutely no

gravid female lice ever found on a three-spine stickleback, so they cannot be the source of lice to farm fish.

DR. NOAKES: Yeah. I mean, it's a limited study and it's true that I don't think there has been adult lice found there, but that doesn't mean that they aren't competent for infecting other fish, in terms of having motile lice.

Q So my next set of questions will be to try to parse out some of the differences between your paper and Dr. Marty's paper. So you've reviewed Dr. Marty's PNAS paper on sea lice where they reported no relationship between lice levels on farmed fish and -- in the Broughton and pink salmon survival levels. Now, I understand that in at least Dr. Dill's opinion in his report at page 11 at paragraph 2, that the analysis had a very small probability of being able to detect such effect. It had what statisticians call low power; is that correct?

DR. DILL: So where (indiscernible - microphone off)?

Q It's page 11, para 2.

MR. LUNN: (Indiscernible - microphone off).

Q I'm referring to Dr. Dill's technical report. So 5D.

DR. DILL: Yes, I believe that's correct.

Q Okay. Now, I put it to you that this is the type of analysis of low statistical power that was criticized in Hoenig's paper?

DR. DILL: It is that sort of post hoc analysis.

Q Dr. Noakes, did you want to add anything?

DR. NOAKES: That's correct, it is the same analysis that was criticized by Dr. Hoenig.

Q And I won't get into the debate we had before lunch, but I take it that amongst the panel there's differences of opinion between whether or not that type of analysis is correct?

DR. CONNORS: I certainly don't disagree with the paper that Hoenig wrote. I mean, I haven't gone through it in detail, but I think the general criticisms of post hoc power analysis where you use an observed effect and ask whether or not it matters, I believe, you know, believe in general with that statement, yeah.

DR. NOAKES: Yeah, I would -- I think -- there are many -- certainly there are many people that don't believe in doing post hoc analysis, power

analysis. The power analysis should be done when you're planning an experiment, rather than after the fact. You deal with that in terms of significance and testing and creating an appropriate hypothesis to test.

Q Dr. Dill, did you have anything to add?

DR. DILL: (No audible response).

Q Okay. Now, at page 2 of Dr. Hoenig's paper he says [as read]:

> Because of the one-to-one relationship between P values and observed power, non-significant P values always correspond to low observed powers.

Do you agree with this statement and anyone can jump in and feel free to comment on that.

DR. NOAKES: I certainly agree with that statement.

Q And I take it from your nodding, Dr. Korman, that you do?

DR. KORMAN: Yes, it's common result.

Q And Dr. Connors also nodding?

DR. CONNORS: Yes, correct.

Q Okay. And Dr. Dill is also nodding.

Now, in your paper which is Provincial Tab 9 and Exhibit 1556, the missing data was just ignored for the purposes of it and wasn't counted in the conclusion. I put it to you that that's a key assumption and without this assumption, you wouldn't have reached the same conclusion. And specifically to help you, I've just got a quote from page 3 of your study which says:

> If lice were present but at a regionally negligible abundance before 2000, then there would likely be little change to the results. However, if lice were in fact abundant and infestations of wild juvenile salmon occurred in the 1990s, the estimated effect of lice on wild salmon survival would likely be diminished due to high salmon returns in those years.

Do you agree with that statement?

DR. CONNORS: Yes, I agree with that statement. I think the important thing to note here is that that doesn't say that if we had information that

went back then that these results wouldn't hold, or these patterns wouldn't hold. We have to make an assumption or in this case we have to, you know, leave that data out because we don't have that data for this analysis. Now, we go on to detail a possibility or a plausible scenario which has been demonstrated elsewhere whereby a regional host threshold might be passed, but again, that's simply some speculation given some of the available information and so we felt the most rigorous way to do this was to treat that as missing data and use those reference populations, as well as information back through time, to better estimate all the other aspects of our model and better isolate any potential influence or correlation with sea lice on farmed salmon.

DR. NOAKES: I think you're correct though in the sense that by setting those to zero, you're not significantly different than the analysis that you had in the first Krkosek paper and where they explicitly assumed that it was. I mean, essentially setting them to zero is implicitly having that assumption that lice were a problem before that time.

DR. CONNORS: That's a really important point. We did not set it to zero. That would be assuming that there were no lice on farmed salmon during that time. We simply omitted that data from the analysis, which is different than setting farmed -- the number of lice on farmed salmon - our covariate - to zero during that time period.

Q And that was a change in assumptions from Exhibit 1554 to 1556, so that's your first paper on coho salmon productivity and your latest paper.

DR. CONNORS: That's correct, a change in assumptions as to whether or not regional infestations of lice on juvenile salmon occurred during that period, correct.

DR. NOAKES: I don't see a big difference between omitting it and setting it to zero. I mean, you know, if you omit it, by default it's zero. So...

DR. CONNORS: But by setting it to zero, you are including information on the dynamics of those populations during that time period. Right. And so -- and that's certainly not what we did in this case. And so I just want to be clear on that distinction. It's not that we treated louse

abundance on farmed salmon as being non-existent and then interpreted our relationships including information on pink and coho populations during that time period. We removed that entire time period from the analysis.

DR. NOAKES: I just don't see the difference. Sorry.

Q That's fair enough. And that's the beauty of the panel, I think, is that you get everyone's opinion in real time and don't have to go through the same questions four times.

DR. KORMAN: But if you just -- in that particular example it's actually -- it's non-debatable. I mean, Don. If you're -- think of a simple regression of one variable on another and there's a certain number of observations that have, let's say, zero for the "X" value, right? So in one case you're going to estimate the regression and include those zeros as part of the estimation. That's going to give you a different answer than if you drop those zero values. Then they won't even be shown on the graph and you'll come up with a different estimate, which is what Dr. Connors is saying, so that you will get different results dropping numbers versus including them in the analysis as zeros. That's -- just like it's not debatable.

DR. NOAKES: No, no, no, I'm not debating that. What I'm saying is with respect to the inference you're drawing with respect to the impact of lice from the farms on the pink salmon there's no difference in that particular -- your case, whether you admit them or assume that they're not there. I mean, you're basically just using the data from that 2000 point on and, as I say, whether you admit them or whether you set them to zero, that will cause a difference, but, I mean, realistically in the grand scheme of things you're doing exactly the same thing as you did in the first paper.

Q Now, would you agree that in 1999 the brood year that returned returned in record high numbers?

DR. NOAKES: This is for Fraser River sockeye, you're...?

Q That's right. No, actually, I think it would be --

DR. NOAKES: No.

Q -- for pinks, sorry.

DR. NOAKES: I haven't seen any data presented and I

don't have it in front of me on pink salmon returning to the Fraser in 2009. But I do believe that they were -- there was a considerable return.

Q Now, another question that I have with respect to your models is that I understand that in your paper and specifically the Exhibit 1556 that your model had all stocks of fish in a given year going by the same number of lice. Would you agree that this is a form of pseudoreplication?

DR. CONNORS: I would agree that if it's not appropriately accounted for then it is a form of pseudoreplication. It gets right back to the heart of the discussion we had at the beginning of these discussions after lunch about how to appropriately account for the non-independence of observations across populations being related to a single regional variable in a given year. And so this exact same approach that I took in my technical report for the Cohen Commission is the same formulation that we took in this analysis.

MS. CALLAN: And I'm coming quick to the end of my time so I was just wondering if the salmon farmers would give me a couple minutes? Fifteen? Thanks.

Q Now, I also understand in contrast to Dr. Marty's paper, your paper addressed pink salmon numbers, as well as coho salmon?

DR. CONNORS: Correct.

Q Okay. And I understand that Ms. Morton also published a paper in October which is Provincial Tab 13 and if we could mark this as the next exhibit.

THE REGISTRAR: Exhibit number 1557.

EXHIBIT 1557: Sea lice dispersion and salmon survival in relation to salmon farm activity in the Broughton Archipelago - Morton et al

MS. CALLAN:

Q And on this paper at page 155 she stated:

> Based on the escapement data, there were no significant differences in survival that corresponded to sea-louse abundance and juvenile salmon mortality on the migration route containing active farms relative to unexposed populations north of the Broughton Archipelago.

Do you agree with the conclusions of the study?
DR. CONNORS: Is that directed at any one person?
Q If you'd like to answer it?
DR. CONNORS: Yeah, I have not read in detail through the entire paper, but my -- but I do in my coarse reading of it last night, after I did see that it was going to be entered into evidence, I agree with the results of that analysis.
Q Okay. And it looks like Dr. Dill wants to jump in, as well.
DR. DILL: Only to say that I received this paper at about seven o'clock yesterday morning and haven't had a chance to look at it.
Q Okay. So I take it that's the reason why it wasn't cited in your PNAS paper, because you haven't seen it prior to the Cohen Commission?
DR. DILL: I had not seen it until yesterday morning.
Q Now, on -- or on Tuesday Dr. Johnson testified regarding coho susceptibility to sea lice. He said at page 13 lines 5 to 21 - and if we can just put that up --
MR. LUNN: Sorry, Doctor...?
MS. CALLAN: Dr. Johnson on Tuesday, page 13 and lines 5 to 21.
MR. LUNN: This is from the transcript?
MS. CALLAN: That's correct.
Q And it says:

> Q And my last question is are you aware of any controlled laboratory studies with sea lice and coho salmon?
>
> Dr. Johnson answered:
>
> As part of my Ph.D. thesis, I did conduct some studies with sea lice and coho salmon, looking at susceptibility of coho salmon to infection in comparison to Atlantic and chinook salmon as well as looking at the role of processes such as inflammation and the ability of coho salmon to remove sea lice.
> Q And what were your findings?
>
> And then Dr. Johnson answered:
>
> It was found that coho salmon, of all the salmon species that we've examined, are very

> resistant to infection, and this is a single pulse infection within the laboratory when compared to Atlantic or chinook salmon.

Do you agree with that statement?

DR. CONNORS: Yeah. I mean I agree with the paper that he published on that work that showed that when you exposed the three species, it's my recollection of the paper when you expose the three species to a single pulse infection of "X" number of sea lice, coho end up with the least number of sea lice on them at different given points in time afterwards.

Q And, as well, if we could turn to Provincial Tab 12.

DR. DILL: May I just comment on that, as well?

Q Absolutely.

DR. DILL: As I understand and remember Dr. Johnson's Ph.D. thesis, this was done like a lot of the similar kinds of studies, with sea lice copepodids, so juvenile stage lice in a single pulse. If you remember or if you've read our other paper on coho salmon, the one that was co-authored by Drs. Jones and Hargreaves, we believe a lot of the lice that are getting onto the coho salmon are actually getting onto them as motile pre-adult and adult lice. And so the situation may not be directly comparable because these different life stage may be having a very different effect.

Q Okay. So if we could turn over then to Provincial Tab 12. This is another paper by Dr. Johnson and he came to similar conclusions as what he wrote -- or what he testified. And specifically at page 188 on the last line he says:

> And specifically said coho salmon appear to be the most resistant species having significantly fewer copepods than both chinook or Atlantic salmon at 15 and 20 post days infection.

Now, I understand, Dr. Dill, that you did have some concerns. Is this something -- this statement that you agree with or do your earlier concerns raised that you just said apply to this paper?

DR. DILL: I don't have any concerns with the paper and I agree with the statement. It's copepodids, by the way, not copepods. They're all copepods. But I was just simply pointing out that it's the same caveat would apply here that we're not for the most part probably talking about lice that get on at that early stage of their existence.

Q And you didn't cite this in your PNAS paper last week?

DR. CONNORS: No, this paper was cited in the preceding papers that looked specifically at the interactions between pink salmon and coho salmon and drew some inference on whether or not there were detectable -- likely impacts from louse infection, et cetera. I don't believe this is said in the PNAS paper.

MS. CALLAN: Okay. If we could just turn subjects quickly. Oh, could we mark this document as the next exhibit?

THE REGISTRAR: Exhibit 1558.

EXHIBIT 1558: Comparative susceptibility and histopathology of the response of native Atlantic, chinook and coho salmon to experimental infection with *Lepeopphtheirus salmonis* (Copepoda: Caligidae) - Johnson et al

MS. CALLAN:

Q Dr. Dill, at page 3 of your report you stated that infectious salmon anaemia has not been confirmed on B.C. fish farms but several of the veterinarian records refer to symptoms that are highly suggestive. Can you identify for me the symptoms that are highly suggestive?

DR. DILL: As you were quick to point out yesterday, I am not a veterinarian or a fish diagnostician, so I am going from the report cited in my report, my technical report and that's Dr. Marty's statements that the fish had ISA-like symptoms.

Q But you'd have to defer to Dr. Marty or to Dr. Kent with respect to their conclusions that they reached with respect to whether the symptoms were causative of ISA or another disorder?

DR. DILL: Well, one of the things I've heard is that the same symptoms may be characteristic of other diseases and so we can't take too much from the

statement that they are ISA-like. But it strikes me as rather curious that the pathologist would write ISA-like instead of BKD-like or IHN-like or whatever else these diseases are. It's also my understanding that when those ISA-like symptoms appear, there is some process that's supposed to kick in to do more detailed analyses, diagnostic work, and it's supposed to be reported to the world food agencies and so forth.

Q But, again, you're not an expert so...

DR. DILL: I'm not claiming to be an expert. I'm not claiming to know that this was ISA, I'm not claiming to know it isn't ISA.

Q Okay. Now, as well, another question for you, Dr. Dill. You've cited Johnson's 1996 paper for the proposition that adult sockeye can be killed by lice in sufficient numbers and under adversarial environmental conditions. However, you would agree that the measure number of *L. salmonis* in the paper was 300 lice per fish and the range observed was 49 to 1,372 lice per fish?

DR. DILL: Well, I don't have those numbers in front of me, but yeah, it was a very unusual event. It was an event with very high temperatures in Alberni Inlet and the fish weren't able to get up the river and they were milling around in there and they basically had their skin almost eaten off by high levels of these lice. It was a very unusual circumstance.

Q Okay. And is Provincial Tab 2 the paper that I'm -- the Johnson paper?

DR. DILL: I -- could you pop it up? Yes, that's the paper.

MS. CALLAN: Okay. If we can mark that as the next exhibit?

THE REGISTRAR: 1559.

EXHIBIT 1559: Disease induced by the sea louse *(Lepeophtheirus salmonis)*(Copepoda: Caligidae) in wild sockeye salmon *(Oncorhynchus nerkai)* stocks of Alberni Inlet, British Columbia - Johnson et al

MS. CALLAN:

Q Dr. Dill, now I understand that you are of the opinion that you need more data. How many more years of data would you require to come to the

conclusion that there is no correlation between salmon and sockeye returns, and specifically, farmed Atlantic salmon?

DR. DILL: Well, that's where we could do, I think, the kind of power analysis that probably would agree is valuable. It's the sort of situation where you can look at your estimated effect size and predict how many data points you would need to reject a null hypothesis. I can only guess at that. I would, you know, say perhaps eight to ten years might be sufficient. Four or five, which is what we have now, is certainly not.

Q Now, in your view, what was the most limiting factor in your ability to perform a rigorous analysis capable of answering these questions with certainty? Would it be a lack of sockeye salmon disease data or a lack of farm disease salmon data?

DR. DILL: It's a lack of -- I won't say disease farm -- pathogen data. It's the very short nature of the time series. That is the most limiting factor I think we faced.

Q So you'd specifically disagree that it's a lack of wild sockeye salmon disease data?

DR. DILL: I think that's important but in terms of testing the hypothesis that we're examining, the wild sockeye data is not useful for testing a hypothesis. It would test some predictions of the hypothesis that would follow if you were rejecting the null hypothesis.

Q Dr. Noakes, do you have anything to add?

DR. NOAKES: I don't think I'd agree with that. I mean, if you're trying to look at causation and you're trying to evaluate whether, for instance, in this case farmed salmon has an effect, you really do need the data from the wild salmon to actually make some sense of what's going on. Because if you don't even know if they have a disease, how can you suggest that a disease on a farm is causing (a) a disease and then (b) mortality in sockeye salmon? You really do need to know what's going on in the wild salmon. I mean, if they have -- if there's no IHN on farms, but they have IHN on wild salmon, then that tells you something in terms of what's going on. So there's no way you can look at causality without having those kind of data there.

Q And I think --
DR. DILL: Just to be clear, I don't disagree with that. I was answering in the context of the way that I understood you to phrase the question.
DR. NOAKES: Right. And, I mean, to be clear, I thought the question was how much more data do you need to establish a correlation between disease on farmed salmon and the dynamics of Fraser sockeye. And I also agree that information on the diseases present, their distribution and abundance on wild fish is absolutely important to establish any causality or establishing the legitimacy behind any correlation you find. I just wanted to clarify the question.
Q And my last question is for Dr. Noakes. Can you tell me what you meant by your comment at page 7 paragraph 3 and specifically [as read]:

> Not surprisingly, Connors' 2011 found that the data from the 2005 brood year, the 2009 returns, exerted a high degree of leverage that observation significantly influenced the results and would by itself tend to exaggerate any negative association. Also, Connors' 2011 elected to not include data from the record 2010 returns of Fraser River sockeye salmon in his analysis for a variety of reasons. Like 2009, I would fully expect the 2010 return data would exert significant positive leverage that would tend to reduce the association between and among the various factors?

DR. NOAKES: Yes, first of all, in terms of not including the 2010, there are reasons why that wasn't done. We don't have the five-year-olds and we don't have an exact number. We know it was very large. So not including that particular data point is understandable, as I say, for a variety of reasons.
But the reason I say the 2005 and talk about the leverage on the 2010 is it goes back to a point where I made before where we may have a short time series, but we have a time series which includes the highest and the lowest values that we've ever seen in terms of returns to the Fraser in terms of sockeye. So what happens is when you

have a data point which is far away from the mean, it's like a lever on a wheelbarrow. The further away you get from the wheel, the easier it is to lift it up. So when you have data points that are far away from the mean, they exert high -- it's called high leverage on the relationship. So a data point that's particularly low will tend to pull the relationship towards that point, the way the statistical estimation procedure works. So a data -- for instance, the 2009 would tend to exaggerate a negative effect whereas the 2010, because you've got an exceptionally high, it would counter-balance that. Essentially what it would do is it would tend to pull the relationship in the other direction because it's exerting high positive leverage in terms of that. So as I say, it's quite powerful to have those two points there, even though they only have a few years of data. It gives us extremely high contrast.

Q And Dr. Connors?

DR. CONNORS: Just to follow up on that, and I don't disagree with Dr. Noakes' characterization of that. I do want it just clear for the record that there was no election to not include any data in this analysis. I used all the available information that was there for me. And when it comes to the statement that I would fully expect the 2010 return data to significantly -- exert significant positive leverage that would tend to reduce the association between and among the various factors, I think Don would maybe agree with me that that's speculation. And it may be, given this very, you know, the support for this odd year/even year pink salmon influence on farmed salmon production, that including 2010 data may strengthen that relationship. We don't know until we include that data in the analysis. But I just want to make that point clear.

DR. NOAKES: I don't think there's a lot of question of whether including something that's the maximum. You just need to look at what the residual would be from that. We have what the value of the mean -- we can calculate the mean and it's pretty easy to see the 30 million is well above the mean. So you're going go have, whether it's a 25 million in terms of the residual or it's a 20 million, it's a very high leverage point. I don't think there's

any question that it would exert high positive leverage.

MS. CALLAN: And those are my questions. I want to thank you for making a lot of difficult math concepts easy to understand. Thank you.

MR. MARTLAND: Mr. Commissioner, I have counsel for the B.C. Salmon Farmers' Association remaining with just under 60 minutes on his time.

THE COMMISSIONER: Would it be convenient to take a ten-minute break now?

MR. BLAIR: As you wish. I'm happy to get started. I'm also happy to take a break now. Your choice always.

THE COMMISSIONER: We'll take the break -- well, not always.

MR. BLAIR: Thank you.

THE COMMISSIONER: We'll take a ten-minute break. Thank you for your cooperation.

MR. BLAIR: Thank you.

THE REGISTRAR: Hearing will now recess for ten minutes.

(PROCEEDINGS ADJOURNED FOR AFTERNOON RECESS)
(PROCEEDINGS RECONVENED)

MR. BLAIR: It's Alan Blair, appearing for the B.C. Salmon Farmers Association, and with me is my associate, Shane Hopkins-Utter.

CROSS-EXAMINATION BY MR. BLAIR:

Q Gentlemen of the panel, I'd like to start where the Province left off, but I'm not sure that I totally understand where that is. So I'll ask a more general question, rather than the somewhat specific questions around statistics that we've been listening to for most of the day. It does touch on the statistics, and also a little bit on biology. And I'm thinking of the comments we've heard from Dr. Noakes earlier, when he was referring, as you all have, to the relative short time series of data from the B.C. Salmon Farmers fish health database, and also the audit numbers that we have in the provincial database.

And Dr. Noakes made an important point, or it seemed important to me, with respect to the significance of the very low returns in 2009 and

the very high returns in 2010. And I think -- I think, subject to the information that's in the databases and the length of the time series, I think those of you can give an opinion on this will be of one mind, that there's no, in quotes, "strong signal". Dr. Noakes is looking for a strong signal in the data if one existed. And I think I -- I think you're all in agreement that in the data that you looked at, there was no strong signal which would have predicted, statistically at least, to a low '09 or a high 2010 return.

So we go left to right.

DR. KORMAN: Yes. I said something very much along those lines in one of the final paragraphs of my report, and it's not really -- one doesn't have to use a statistical comparison to make that inference. So, I mean, it's just a standard observation that if you have, you know, very low and strong survival, very contrasting values, and very similar values in an independent variable, like the number of fish farms, or the amount of disease, then your qualitative assessment of that is that there's not that much of a linkage based on that very limited sample. So you can make that inference without it invoking statistics at all. In fact, the sample size is so low there's really not much point in invoking statistics.

Q So I just want to summarize. You're in agreement that the B.C. Salmon Farmers fish health database and the provincial audit database on fish health audits doesn't show any signal which is predictive of the low returns in '09, but the high returns in 2010.

DR. KORMAN: Absolutely.

Q Yes. Dr. Connors.

DR. CONNORS: And I agree, as well. But I just want to be clear, I did not do any analysis that included the 2010 returns; just so that's clear for the record.

Q I'll put another note in the column next to your name on that point, then. I have three. Dr. Noakes.

DR. NOAKES: Yeah. No, I would agree with that.

DR. DILL: As would I.

Q Thank you. Now, picking up on the strong signal but moving from the database entirely, but looking for a strong signal in some other evidence than

those two databases that I've just referred to. And specifically I want to direct the four of you to ocean conditions, and specifically ocean conditions relative to the out-migrating Fraser River sockeye notably in 2007 and 2008 for the returns in 2009 and 2010.

We've heard evidence of a one-two punch, or a one-two-three punch, and in the -- in the order that the out-migrating salmon might have seen them, we heard evidence in 2007 of a one-two-three punch, and I'll summarize those for you. And my question so you can be thinking about it is, is there anything similar that would have affected the out-migrating salmon in 2008, positive or negative, that you're aware of.

But I'll give you the 2007, what I'm calling the one-two-three punch.

One, there was a *Heterosigma* bloom in the Strait of Georgia. We heard that, Mr. Commissioner heard that from Dr. Rensel when he gave his evidence. He also demonstrated the very strong correlation between *Heterosigma* bloom and sockeye returns.

Number two was the low feed availability, that you probably all know was Dr. Beamish's evidence before this Commission. His evidence, in summary form, was that the fish weren't eating. There wasn't much in their stomachs as they were moving through the Strait of Georgia.

And three was the evidence we heard from Dr. McKinnell. And his evidence in part was dealing with the ocean conditions that the fish would have encountered as they were migrating up towards the Gulf of Alaska. And you may or may not know, but the Commission heard that there were three particular data points clustered together at the very northern tip of Vancouver Island to the north end of Johnstone Strait. He described for all of us that there's a grid and devices in the ocean which measure temperatures. And so we had up on the screen three bright red spots, which was basically - you may not know this evidence, but I'm summarizing it for you, and if you do know it, you can tell me when you answer the question - that the migrating salmon heading north at the north end of Johnstone Strait near the northern tip of Vancouver Island would have encountered

abnormally hot water, warm water, warmer than average, and certainly warmer than any other point of the Gulf of Alaska at that time period, and at the time period it was right for the out-migrating salmon.

So one-two-three punch is what we heard from various witnesses; the Commissioner has that evidence.

Firstly I'd ask you individually, perhaps left to right again, if you're familiar with that evidence. And by that I don't mean personally, but that the Commission has heard that evidence. Dr. Korman?

DR. KORMAN: Yes, I've heard this story for 2007.

Q Dr. Connors?

DR. CONNORS: As have I.

Q Dr. Noakes?

DR. NOAKES: Yes, and I've read those papers.

Q And, Dr. Dill.

DR. DILL: I was not aware of the *Heterosigma* work until just recently. But my understanding was that much of the testimony of Dr. Beamish was called into question, that his data didn't actually support that. Nevertheless, I think it was very good evidence that 2007 was a particularly bad year for ocean conditions.

But I also want to take this opportunity to point out something that's been sort of nagging at me throughout all of the proceedings here today, and that's that people seem to insist on taking things one factor at a time. And I'm really glad that where you seem to be going is looking at interactions of factors. So there are a variety of different oceanic and other conditions that might affect the fish simultaneously, synergistically, antagonistically, with farms. And it would be a mistake to look at factors one at a time, like how much IHN was there in a particular year, or how much BKD was there in a particular year, what was the sea surface temperature in a particular year. It's these factors interacting with one another which are determining the dynamics of fish populations, whether they're sockeye salmon or any other species.

Q Thanks for that clarification for all of us, Dr. Dill. So left to right again, and again I'll

phrase the question in summary form. I'm suggesting that 2007 from an out-migrating sockeye salmon, Fraser River sockeye salmon, dealing with ocean conditions only and not many of the other factors or variables which may have been in place, amounted really to a very, very tough year for those out-migrating salmon in the one-two-three punch I've described?

DR. KORMAN: Yes.

Q And, Dr. Korman, as it relates to out-migrating ocean conditions, you're unaware of any similar one-two punch, one-two-three punch in the out-migrating 2008 as it relates to ocean conditions?

DR. KORMAN: Right. It's not really something I track as part of my regular job.

Q So your answer to that part would be "I just don't know"?

DR. KORMAN: I just don't know.

Q Fine. And, Dr. Connors?

DR. CONNORS: I'd have to defer to the same statement in the sense that I don't have a really intimate understanding of all the different processes across those two years.

Q And so you're not able to answer either '07 or '08, out-migrating, you just don't know?

DR. CONNORS: Correct.

Q Correct. Thank you. Dr. Noakes?

DR. NOAKES: Yeah. I'm certainly familiar with the '07. The '08 I can't recall a McKinnell issue in terms of the temperature up there, but --

Q And the water was also '07 in terms of the high temperatures. Those were all in '07.

DR. NOAKES: Yes. No, as I say, I don't know -- I'm aware that -- I've read the Rensel paper in terms of the *Heterosigma*, and I'm aware of Beamish's work with respect to food availability in the Strait for juvenile salmon, but I don't know if -- I can't recall whether McKinnell had any data for 2008 for --

Q I'll take you to that. I'll take you to that in a moment. But do you agree that it was a one-two-three, meaning a strong signal as it relates to ocean conditions in 2007?

DR. NOAKES: Yes, I agree.

Q And, Dr. Dill?

DR. DILL: 2007 was definitely a worse year than usual. Anecdotally I've heard that 2008 was very much

better.
Q Thank you. Mr. Lunn, could you go to Salmon Farmers Tab 3, please. On the screen, gentlemen, is a document prepared by Dr. Rick Beamish, Richard Beamish, on July 2011, and the title is somewhat self-evident, "Assessing the Impact of Salmon Farming on Pacific Salmon at Population Level in British Columbia." My questions are for Dr. Noakes, but as earlier questioners have encouraged, I'll also encourage anybody who has a viewpoint on these questions to jump in when appropriate.
Firstly, Dr. Noakes, are you familiar with this work?
DR. NOAKES: Yes, I read this paper.
Q And can you indicate what conclusions Dr. Beamish comes to in this paper? Would it assist you to go to page 7?
DR. NOAKES: Page 7.
Q And in particular where he's making references to the distinctions between --
DR. NOAKES: Yeah.
Q -- two different works.
DR. NOAKES: Yeah. As I say, it's been a week or two since I read -- or whenever you sent out this document. I guess it's been a week since I read it. And he's contrasting the work of Marty et al and Krkosek and Ford and Myers. And essentially there's the issue with Krkosek, 2007, and Krkosek and Hilborn in 2011. And it's an issue that I had pointed out before in the sense of they're assuming that there wasn't an issue with sea lice, for instance before 2001. And it's the same issue in each of those papers.
In the Ford and Myers, what the Ford and Myers paper did was they -- and I don't know if it's exactly on this page, but there's a difference in -- if I recall this, there's a difference in what's said in the abstract and a difference which is said in the paper. In the abstract, I think he makes references to three species of salmon being impacted by aquaculture, and in the body of the paper, he only refers to, I believe, pink salmon. And I stand to be corrected, but I think that's what I recall.
And I think Beamish's concern was that most people will just read the abstract and they won't

go into the report. So they'll be left with the impression that three species of salmon have been shown to be impacted, or at least a particular study suggested that three species of salmon have been impacted by sea lice. Where in fact it was only one, and that there was this inconsistency in the reports.

Q And, Dr. Noakes, does it further refresh your memory of this paper if I suggest to you that what Dr. Beamish notes was that there were different conclusions reached by the Krkosek and Hilborn 2011 study, and the Marty et al 2010 study, and this was apparently due to the fact that the former inferred sea lice abundance on farms and assumed the infection began in 2001, whereas Marty et al in 2010 used actual data from the salmon farms.

DR. NOAKES: Yeah, and that was a big difference, and I think that's been alluded to in the discussion here by everyone in terms of it really demonstrates the importance of needing the data from the farm to actually make some assessment in terms of whether there is any -- any significant relationship or causal relationship between it. And Marty, as I say, it was a good paper in the sense that it combined experts in fish health and also Terry Quinn, who is a very well respected analyst and stock assessment person. It combined those skills to look at the data in a comprehensive way and came up with a different conclusion than the other two authors.

Q Thank you, Dr. Noakes. I see Dr. Dill signalling.

DR. DILL: I just wanted to comment that Ran Myers and Ray Hilborn, co-authors on those papers, are also well-known experts in analysis of fish populations. But when I read this, one thing that I was a little bit confused about is where was this published? I didn't see that on the information.

Q To my knowledge it's not published. It's a new document just completed by Dr. Beamish.

DR. DILL: Ah, right.

Q The purpose of my question is to have witnesses before the Commissioner comment on the works, and you both have. Do you have any further comments, Dr. Dill?

DR. DILL: No, not at this time.

MR. BLAIR: Thank you. I wonder if this could be marked as the next exhibit.
THE REGISTRAR: Exhibit 1560.
MR. BLAIR: Thank you.
MR. LEADEM: Before it gets marked -- sorry, for the record, Leadem, initial T. Before it gets marked as an exhibit, I think that this fits the category of documents that are prepared expressly for evidence tendering into the Commission. It's not a peer-reviewed journal. It's essentially written by Dr. Beamish, who has come and testified already at these proceedings, albeit on another point, and tendering it in this fashion, I would submit, is akin to someone just being able to take any dataset, any evidence that we've heard so far and comment on it, and then proffer their testimony in that fashion. As such, I would suggest, with all due respect, that this not be tendered and accepted at this stage.
MR. BLAIR: Mr. Commissioner, I'm happy to respond to that. We just moments ago had Dr. Marty's paper which was prepared in much the same light and tendered by the Province entered as an exhibit. The scope of Project 5 mandated more broadly than any other project reviewers that, as it turns out, the Drs. Dill and Noakes were to look at all literature available. They were to talk to anybody they wanted to. They could look at grey literature, peer-reviewed journals, anything. This certainly is something. This is from a leading expert who this Commissioner has heard from. I don't think it matters whether it was prepared in July 2011 or July 2010. In fact, it ought to matter more perhaps that it was prepared this year, because we have a Commission that is stressed to get all the work done in a limited time period.

And we covered this when we raised issues last week, or perhaps it was earlier this week. We can't call every witness. This is a summary that we hope to have marked in evidence and, Mr. Commissioner, you can look at this like all of the other reports. We have entered 1,500, closing in on 1,600 reports, and many of them are from far less well-known scientists than Dr. Beamish, and to think that this would not be relevant and useful, would not fall within the scope of the

literature which they were mandated to look at by the project, would be a great stretch to exclude it, in my respectful submission.

MS. GAERTNER: Sorry, Mr. Commissioner, but having been through this many of these days of this inquiry, this has obviously been produced after Dr. Beamish was a witness, after these reports have been tendered. I'd like to know if was produced for the sole purpose of being put to these witnesses during this evidence. I mean, I don't know why he produced this document at this time, or one that hasn't been peer reviewed, which is generally the basic requirement that we've used in this inquiry to get documents in.

THE COMMISSIONER: I don't know if you know the answer to that query, Mr. Blair.

MR. BLAIR: I do. Dr. Beamish -- this goes back to the limited number of spots there are in those coveted four seats over there. Commission counsel has to decide, really, which witnesses you'll hear. We're invited to make recommendations of who might be on the various panels, and we have, as I'm sure other parties have, as well. We obviously can't get everybody on a panel. We can't get Dr. Beamish in here to opine on this. He's well-known to this Commission now, and this is another way of getting his opinion in, we can ask these four. But why wouldn't we ask these four. We're going to hear from Dr. Marty later and his report was just entered as an exhibit. Why don't we enter this. Why is this in some special class because it was prepared for this Commission. And indeed, it was prepared so that the Commissioner could have the benefit of that knowledge, and the evidence which would be the *viva voce* evidence of the two doctors you've just heard from.

THE COMMISSIONER: Well, I don't know what the answer of the Province is with respect to your comments re Dr. Marty, but I think just in the interests of time, Mr. Blair, we'll mark this for identification purposes. It certainly is on the record, and that's not to say it won't be marked as an exhibit. I just wish to at least have an opportunity to know more about this assertion by Mr. Leadem and your response, and from any other of the participants' counsel who may want to weigh in on this at some point, if it becomes relevant.

So I'll mark it at this for identification purposes.

THE REGISTRAR: The document called as Exhibit 1560 will be withdrawn and that will be marked for identification as WW.

WW FOR IDENTIFICATION: Beamish, Assessing the Impact of Salmon Farming on Pacific Salmon at Population Level in British Columbia, July 2011

THE COMMISSIONER: Thank you.

MR. BLAIR: Thank you, Mr. Registrar. And, Mr. Commissioner, and for the benefit of participants' counsel, I know none of us wish to take up valuable time. I'm sure we'll be revisiting this very issue again. And as for a date and place, I can indicate that the first B.C. Salmon Farmers' witness, Dr. Peter McKenzie, will be on next week with a panel. I intend, and I'll just give notice to my colleagues, I intend as the client, as the person who commissioned these reports, or at least one of the member companies and a fish veterinarian, to put through him the *c.v.*s of all of these people who have prepared reports. And I will again be seeking to tender them as expert reports with requisite *c.v.*s. Dr. Beamish's *c.v.* won't be necessary, but others will be provided and have been provided recently to my friends.

Thank you for that. I just wanted to give that explanation in terms of the timeline. Mr. Martland and I can work on that if we need to, to keep precious court time, hearing time free.

Could we please, Mr. Lunn, go to Exhibit 1540, which is Dr. Dill's report.

Q Dr. Dill, I'd like to direct you just to the bottom of page 1, to the bottom of the Executive Summary, and starting with "Unfortunately, it turned out" -- yes, that's the paragraph. Just at the top of the screen there to make it a little larger. You note here, Dr. Dill, that -- I'll just read it into the record:

> Unfortunately, it turned out that the data provided by Provincial government (BCMAL) and the BC Salmon Farmers Association (BCSFA) were insufficient in both quantity and

quality to allow a rigorous analyses capable of answering these questions with certainty.

Do you still stand by that, that statement, sir?

DR. DILL: Yeah, I don't mean to imply that that was any fault of the B.C. Government, or the Salmon Farmers Association. That was just the cards we were allowed to play with.

Q And I want to turn to Dr. Korman, if I may, because Dr. Korman opined that he was -- it was an impressive array of data, and we've heard all of you describe the limited time series. But in terms of the quality of data, I gather, Dr. Korman, you might disagree and you might say that there was excellent quality.

DR. KORMAN: Right. So I think we're both in agreement here that the real issue here is the short time series that's available, because the program only began in full swing in 2003. So there's no conflict there.

I'm not exactly sure what Larry Dill means in terms of the quality. I'm guessing perhaps testing for more pathogens than is currently done. You know, from my view, as a non-veterinarian, the amount of testing that's done, and I guess my perspective is also I'm thinking about other monitoring programs that looked impressive. Whether or not it -- it's certainly not exhaustive, and so I think you'd have to ask Dr. Dill what he means by the quality. To me it seems generally pretty high quality for the data, monitoring datasets that I've seen, but...

Q Thank you. I just wanted to confirm that with the ability to look at that executive summary, you could --

DR. KORMAN: I agree with his statement on quantity. I'm a little less -- I'm unsure of what he means by the problem with the quality.

DR. DILL: You characterized it correctly. It's the fact that there are a large number of these events and audits that identify mortality, but without a diagnosis present.

Q In fact, in terms of the number of audits, there were about 800 audits that you looked at from the B.C. Province, is that correct?

DR. KORMAN: Let's see, that would be pulling up a hundred and -- just roughly 100 to 120 a year

times -- for seven or eight years, so, yeah that would, you know, it may be more. So lots of audits. I guess the question that's nagging Larry Dill, and that seems that maybe would be a veterinarian would have to answer this, is why are there so many fresh silver mortalities and with no sign of disease. And therefore he's using that to say, well, I'm not convinced that this dataset is -- that this program is fully rigorous. I mean, am I capturing -- and I'm not qualified to respond to that.

Q We're going to go to fresh silvers soon.

DR. KORMAN: Okay.

Q Before we do, Mr. Lunn, could we go to pages 15 and 16 of the same report. And if you can split it, or just the bottom of 15 and the top of 16. Just while he's finding it. This is, Dr. Dill, your report, a summary of Connors' analysis. It's starting "It is important to bear in mind" -- yes, and then the next paragraph as well. Can you possibly put both that paragraph and the paragraph below it. Okay, harder to read, but, thank you.

So I want to direct everyone's attention to the last several lines at the bottom, starting "In addition, the dataset". So right down four or five lines from the bottom, Mr. Lunn. In addition at the right-hand margin at the bottom of the page. Thank you. So these are Dr. Dill's words describing the dataset generally, and I'll just read it into the record:

> In addition the dataset did not allow for a closer look at the effect of individual farms (data were aggregated across fish health zones), or for a breakdown according to proximity of the farms to the presumed migration route of the majority of juvenile Fraser sockeye.

So looking at that statement just by itself, it's my understanding that that statement is not correct. Is that -- do you agree with that -- with my summary, Dr. Korman?

DR. KORMAN: Yeah, the data was provided -- and maybe probably just a clarification here. The data was provided on a farm-by-farm basis, all of it. And if an analyst wanted to use that at a farm level,

it was possible to do so. In my report, for brevity, I summarized it as fish health subzones. So, yeah, but the other uncertainty, of course, is without knowing the details of the migratory pathways, it might be difficult to use all the site-by-site, the farm-specific information. So Dr. Dill may be referring to that. But we do have farm-specific location information for all the data.

Q And, Dr. Connors, you agree.

DR. CONNORS: I do agree, and I was the one that then aggregated these at levels for exactly the reason that Josh pointed -- Dr. Korman pointed out, and the fact that I felt like I would be criticized if I assumed migration routes, or if I didn't assume migration routes. And so given those assumptions, I had to aggregate them at the...

Q And, Dr. Noakes, you agree that the statement is wrong, and in fact there were individual farm data available?

DR. NOAKES: That's correct. And, in fact, I actually looked down to the farm level because I wanted to see what particular disease was occurring on what farm in exactly what geographic area so that I could determine whether in fact there was a risk of pathogen transfer from that farm to Fraser River sockeye.

So, for instance, in the -- with respect to BKD in recent years, many of the farms are in Salmon and Sechelt and Jervis Inlet, which are not on the migration route. So say five out of the six -- five or six of the farms that are experiencing BKD outbreaks are not -- the Fraser River sockeye are not being exposed to the pathogen, BKD pathogens from those particular farms.

So, yes, I did go down to the -- so this doesn't -- this isn't consistent with the dataset, that information.

Q Yes, I really hate to rush any of you, but we're under such time constraints, and Dr. Dill's itching to go.

DR. DILL: Yeah, I just want to put this in context. This is the last paragraph in a section that's describing the Connors' analyses, and so I'm referring there to the aggregation that was done on the Connors' analyses, not the fact that the

raw data were not available on a farm-by-farm basis.

Q Yes. So to be clear, then, Dr. Noakes was in the position, because of his statistical background, to take the individual farm point data and work with it yourself professionally. Dr. Dill, your skill sets are different and you didn't do that. You relied on Drs. Korman and Connors to do that and aggregating the data, and that's what you're referring to here?

DR. DILL: That's right. But it's not because of my skill set, it's because I chose to do it that way.

Q Oh, I'm sorry. I'm sorry, I didn't mean to offend you. I thought that statistical magic we've heard from the other three rested with them entirely, but perhaps you can give me a lesson in statistics later, after Monday.

DR. CONNORS: To be clear, there was no analysis that related data from the individual farms to productivity of sockeye in any of the reports.

Q Well, this is a perfect lead-in to what I'm afraid I'm going to get back into the ping-pong match, or the tennis match once again, and I'm going to frame my question this way. These are questions, Mr. Commissioner, on the Connors and Noakes disagreement, or disagreement on conclusions. And I'm going to centre my question on this as a matter of biology as opposed to pure statistics, because we've been hearing all of the statistical models, and I'm lost. And I think Mr. McDade threatened to walk out if I raised any questions on statistics, and I said I'd be out the door before he would be. And so you're losing us in the statistics.

So I want to take it to the biology. I understand that the major distinction between the Connors' assessment of the data and the Noakes' assessment of the data, and I'll let you both answer, is that I understand, Dr. Noakes, you took the individual farm-by-farm and said, "We don't have just one common aggregation of farms and applying it across the board and saying the disease happens uniformly," you looked, Dr. Noakes, at the individual farms and said, "So we found some diseases in these subsets, and I looked at where they were, and predominantly they're not on the migration pathway, the presumed migration

pathway for out-migrating Fraser River sockeye salmon, and therefore you can't assume that they were."

And I believe Dr. Connors, because you didn't want to presume the out-migrating pathway, assumed it would be better to look at all the data as one set, and therefore didn't take the step of excluding those farm sites in years where there were disease recorded and excluding them from the impact on the out-migrating Fraser River sockeye.

So that's my general question. Who would like to go first?

DR. NOAKES: Commenting on what I did, yes, that's correct. I mean, I looked at the disease and specific farm in relationship to the presumed migration route for Fraser River sockeye. And what I was looking for was what is the potential for pathogen exposure to those fish swimming by. And as I say, as I said in my report, it's quite important to figure out where those farms are, because if they're -- if they're tangential, such as being in Salmon and Sechelt and Jervis Inlet, then even though there's a disease outbreak there, they really don't contribute, or at least they -- there is very unlikely to contribute any exposure to pathogens in migrating --

Q And I don't mean to cut you off, but while --

DR. NOAKES: Yes.

Q -- you're speaking to this, I do want to have Mr. Lunn put up Exhibit 1536, the Noakes report.

DR. NOAKES: Yes.

Q We'll give you to agree at the end of the day. And small "ii", it's the "Key Findings", paragraph 5, next page. And there having interrupted you, Dr. Noakes, this paragraph summarizes what you were describing in terms of the work you did to break out where the diseases were?

DR. NOAKES: That's correct. And there's one figure with respect to BKD in there, showing which farms in three years that they were on. But that's what I did. I basically looked at the -- separated the ones on the West Coast from those within the main migration path, and then within those, identified how many farms and specifically for BKD exactly where they were.

Q And so --

DR. NOAKES: Because BKD represented 74 percent of the

high risk diseases, so...
Q So sticking with you just for a second, and then we'll get to Dr. Connors, you're saying that using farm production numbers, increasing farm production numbers can't be looked at without -- in terms of a proxy. or some -- some determinant for disease, for disease potential, transfer. You can't look at that generally when you can specifically take the actual diseases out of the migration pathway. So, Dr. Noakes, you took them out of the migration pathway, which is why you say farm production can't just be looked at as a -- as a straight line.
DR. NOAKES: That's certainly one reason. There are a lot of other reasons why you can't use it as a proxy. And I don't know how much time you want me to spend on this.
Q Go ahead.
DR. NOAKES: Okay. So, for instance, let's use an example of a consumer price index. When you're using a proxy, there are certain things that you need to look at in terms of the properties of that proxy and what you're using. It has to basically be representative of the time series, or the -- or what you're trying to represent. So there's been a lot of work go into identifying what products they'll put in to calculate a consumer product index.

Well, the same thing in terms of the Connors' analysis, in terms of using farmed salmon production as a proxy in disease. First of all, it has to match up with the disease evidence, because we do have some disease evidence. There's a certain number of years that we have, and at least if you're going to use it as a proxy, it has to match up with the evidence that you do have. The other thing that it should do, is it should be in the way that this model is being formulated, it should be proportional to. So, for instance, whatever proxy you're using should be proportional to the disease or the pathogen exposure, as Dr. Connors puts it. So that proxy should be proportional to that value. And the last thing is, it needs to be consistent over time, because there's no sense using a proxy that's only good for five years and then it changes.

So I go through in my comments to Dr.

Connors, basically I go through the four high-risk diseases that Mike Kent provided, and the sea lice, and what I do is I demonstrate how they're simply not proportional.

So, for instance, we have the IHN, and if you go to the B.C. Government site you'll see on there they document outbreaks of IHN. And they're sporadic over time and they have occurred over -- since the 1990s. But one of the things we see is that there's been no IHN detected on farms since 2003. So using farm salmon production for IHN isn't consistent in terms of over time, because you have the sporadic nature and, of course, there's no IHN since 2003.

For BKD, BKD is primarily a disease which impacts Pacific salmon, chinook and coho. And if you look at the production numbers, that there's a graph in Connors' -- or, sorry, Korman's, Dr. Korman's report, breaking down the percentage of Atlantic and Pacific salmon that are farmed. It varies widely over time, and since about the last several years, it's gone from about 30 percent down to about 10 percent. So there's not a consistency, and it's certainly not proportional to overall farmed salmon production.

The other thing, of course, is because it only affects a small portion of the farmed salmon production, it's not reasonable to use it as a proxy for the total salmon production in terms of Atlantic salmon.

For the other two diseases, the high-risk diseases, vibriosis and furunculosis, there's been vaccines for those two diseases since about the mid-1990s. That's the information I received from the vets on the farm.

So what you have in those cases, and also with IHN, because there's a vaccine there, is you have a discontinuity in the time series. So you don't have a consistency in the pattern of diseases which is proportional to the farmed salmon production.

For sea lice, since 2003 -- presumably there were sea lice on the farms before, but certainly since 2003 there's been mandatory treating of sea lice. And there's a trigger of three lice per fish, and once that trigger is reached, or once that level is reached, then there's automatic

treatment. So again you don't have consistency over time in terms of the lice production, because it's being artificially held low, because there's treatment at three lice per fish.

So overall, for the four high-risk diseases and the sea lice, farmed salmon -- using farmed salmon production is not consistent with the observed disease evidence we have from the farm. It's not proportional, and there's inconsistencies over time, simply because the use of vaccines has reduced substantially the amount of disease that we found on there.

Now, the effectiveness of the vaccine varies by disease. But it's certainly not zero, and for IHN, it's in the high 90 percent.

So, for instance if you used a human example in terms of measles, before 1964, when a measles vaccine - I Googled this when I was doing the report - if you looked before 1964, there was an average of around 500,000 measles cases per year in the United States. And once they started using vaccine, you can see that it went down -- went down to essentially zero, very few cases of measles. There's certainly some, but they're certainly not in the hundreds of thousands.

So that's the kind of structural change you see in the time series and the disease time series, and you're going to see exactly the same thing in -- in the farmed salmon pathogen output, as well.

So there's a real problem. Using farmed salmon production as a proxy is not a good approximation. So essentially what -- in terms of the analysis, if it doesn't approximate disease, or it doesn't approximate pathogen exposure, then for the purposes of basically looking at a relationship between farmed salmon production and sockeye productivity, it is not a useful analysis. And that's (indiscernible - overlapping speakers).

Q Thanks very much, Dr. Noakes. Dr. Connors.

DR. CONNORS: So my interpretation of this is that the comparisons that Don just made are not the most rigorous, you know, examinations that can be made between farmed salmon production, and in this case the number of farms that test positive for a disease or a vaccine. So fish health events are at the level of a farm. Audits are at the level

of a farm. Sea lice are -- you can scale up and ask how many sea lice are on a farm.

At the most basic level, the abundance of hosts is a fundamental component of pathogen transmission. And all other things being equal, the abundance of infected hosts is, you know, a fundamental component to the exposure that other hosts may have to a pathogen.

Now, a perfect example is that if you have no farmed salmon on a migration route, you cannot have pathogens from farmed salmon being transmitted. If you have many, many farmed salmon on a migration route and they have pathogens in them, or there are pathogens present, then you can have more pathogens transmitted.

I agree that the shape of that relationship may take various different forms. It may be influenced by vaccines. It may be influenced by the application of SLICE, environmental conditions, et cetera, but it doesn't negate that fundamental relationship. And so as a result, I would argue that it can still be considered as a proxy for these processes, albeit it has to be considered, you know, within the light of the fact that it's a coarse approximation

We use proxies all the time for a number of different things, including sea surface temperature and its relationship with the dynamics of wild salmon population. The fact that sea surface temperature is a very coarse and poorly understood proxy for the biological conditions that salmon experience when they enter the marine environment, or when they're in the marine environment, hasn't precluded much progress being made in the salmon world, much progress from being made. And so that's my interpretation, you know, of the argument.

Q Yes, and, Dr. Connors, I don't want to cut you off before I cut Dr. Noakes off, but the clock's going to cut us both off. So I'm going to come back to you, Dr. Connors, with another follow-up question, and that is that I'm right to characterize it that when Dr. Noakes was doing his assessment of your assessment, he took the high-risk disease fish that he found in the individual farms in individual months, and placed them geographically, based on the coordinates, and found that an

overwhelming number of the high risk diseases were off what would be considered -- or what the Commissioner may consider to be the evidence of a traditional migration pathway. He did that step, you didn't, correct? Is that yes or no, he did that, you didn't?

DR. CONNORS: I have to qualify the answer.

Q Sure.

DR. CONNORS: He did a qualitative examination of the distribution of pathogens on farms.

Q I thought when I read his report and I heard him say "I found five BKD fish in the Sechelt Inlet, and that's not the migration path." That's not qualitative, isn't that quantitative?

DR. CONNORS: Well, in regards to the productivity of Fraser River sockeye salmon. All right? And so I absolutely agree that if -- if one wanted to make assumptions about migration routes that they follow, one could have a more refined measure of diseases along migration routes. And I think that that is, you know, a good idea to do.

But the important -- the important point to make is that in the case of contrasting the two, there was no analysis between productivity and diseases on farms at a finer spatial scale.

Q Let me put it to you this way, Dr. Connors. I think what we're doing here, all of us, is we're in a mystery novel. We're trying to figure out what happened to the declining sockeye, and in particular, 2009 became the, you know, the exclamation point on that. And so we are making assumptions about migration paths. I hear you saying it's okay to use the whole model because it's -- just let me finish the question and then you'll understand where I'm going. I hear you saying statistically it's okay to use the whole model. But if this is a mystery novel, and most of the fish are going one way, and we're looking specifically at risks and high-risk diseases, and one of the statisticians takes all of the high-risk diseases and puts them because the data shows them to be off the migration path, and another statistician doesn't do that, isn't it fair to acknowledge that you're going to get different results for that reason alone?

DR. CONNORS: But we didn't get different results.

DR. NOAKES: I think we did.

Q In terms of farm production and the relationship to the declining sockeye.

DR. NOAKES: Yeah. No, I think we did. And you can't, I mean, you can assume, if you want to speculate and assume that farm production is proportional to pathogen outlay, the only way you can do that is if you dismiss the disease evidence on the farm. I mean, that's -- that's fundamental when you're using an index, it has to be represented by the data that you have. I mean, any proxy that you're using has to at least be consistent with whatever data you have, and there's all sorts of other assumptions.

Now, you can't say that, I mean, that the whole issue about identifying a particular farm and a disease on there is you can't say that there's a disease on the farm if there's no disease evidence, or there's no evidence of that disease. And that's why I went to that point.

Q And your -- and your point was you looked where the disease was and you found overwhelmingly the high-risk diseases, as described before this Commission, overwhelmingly by number, numerically they were off of the presumed migration path. Is that the quick summary, Dr. Noakes?

DR. NOAKES: That's the quick summary basically.

Q And now, Dr. Dill is going to get the last word before Mr. Commissioner sends us all away for the weekend, correct?

DR. DILL: Yeah, I didn't want to wait till Monday, so if I could just quickly add two points to this.

Q Carry on.

DR. DILL: You said all high-risk diseases. All high-risk diseases that we know about, which are those four.

Q Yes.

DR. DILL: We don't know that there might not be some other pathogen on the farms. The Kristi Miller testimony makes us worry that there might be.

Secondly, there may be other mechanisms, other than disease, which might relate production to salmon productivity. Now, I know in his report the implication was that it was a good proxy of disease transference. But it's also possible that it could be a proxy of anything else. It could be chemicals, you know, chemical therapeutants, whatever. So we needn't get too tied up in the

disease issue, I don't think.

MR. BLAIR: Thank you. Mr. Commissioner, we're at the stroke of 4:00. I'm worried that I'll be taking your time on the weekend. I worry about that more than Mr. Martland's time through the day.

MR. MARTLAND: I won't comment. Mr. Commissioner, I think we can now adjourn. I will canvass, though, we're on schedule, but we have some time pressures on this panel. This is an important panel, I want to make sure I'm not in the position of unilaterally cutting time allocations. So I would ask that we convene at 9:30 on Monday, rather than 10:00, if that's agreeable. Because that will allow us to conclude this panel's evidence on Monday within the usual timeframe otherwise. That would be my request.

The other points, if I might just canvass a few points briefly. First, I wanted to express my gratitude to all counsel in the room for their significant cooperation. It's been exceedingly helpful. It has allowed us to stay on schedule in a very dense week of hearings to this point.

The second point is just to narrate again and make it clear that in relation to the process vis-à-vis Exhibit 1549, which is the Province's dataset, which is now a non-public exhibit in these proceedings, the timelines that we'd outlined were to have the Province's submissions by two o'clock on August the 3rd, participants' submissions - I'm sorry, let me try that again - August 30, I think I've missed a decimal there, two o'clock on Tuesday, August 30, other participants by two o'clock on September the 1st, which is the Thursday, the Province responding by 2:00 on September 2nd. I'll ask those materials be provided by email to Natasha Tam, as well as myself and Ms. Grant, please, so that we're clear where they're going.

Those would be my final points, Mr. Commissioner. Thank you.

MR. BLAIR: And just to be clear, do I still have 15 minutes?

MR. MARTLAND: Yes.

MR. BLAIR: Thank you.

THE REGISTRAR: The hearing is now adjourned to 9:30 Monday morning.

(PROCEEDINGS ADJOURNED TO AUGUST 29, 2011 AT 9:30 A.M.)

I HEREBY CERTIFY the foregoing to be a true and accurate transcript of the evidence recorded on a sound recording apparatus, transcribed to the best of my skill and ability, and in accordance with applicable standards.

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Diane Rochfort

I HEREBY CERTIFY the foregoing to be a true and accurate transcript of the evidence recorded on a sound recording apparatus, transcribed to the best of my skill and ability, and in accordance with applicable standards.

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Karen Acaster

I HEREBY CERTIFY the foregoing to be a true and accurate transcript of the evidence recorded on a sound recording apparatus, transcribed to the best of my skill and ability, and in accordance with applicable standards.

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Susan Osborne

I HEREBY CERTIFY the foregoing to be a true and accurate transcript of the evidence recorded on a sound recording apparatus, transcribed to the best of my skill and ability, and in accordance with applicable standards.

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Pat Neumann