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.

Monday, September 19, 2011

Tenue à :

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

le lundi 19 septembre 2011

Errata for the Transcript of Hearings on September 19, 2011

Page	Line	Error	Correction
ii	--	Brock Martland	Wendy Baker, Q.C.
75	4	mimed	mined
85	28	man	many

Suite 2800, PO Box 11530, 650 West Georgia Street, Vancouver, BC V6B 4N7
Tel: 604 658 3600 Toll-free Tel: 1 877 658 2808
Fax: 604 658 3644 Toll-free Fax: 1 877 658 2809
www.cohencommission.ca

APPEARANCES / COMPARUTIONS

Brock Martland Maia Tsurumi	Associate Commission Counsel Junior Commission Counsel
Tim Timberg Charles Fugere	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.

Phil Eidsvik	Southern Area E Gillnetters Assn. B.C. Fisheries Survival Coalition ("SGAHC")
Chris Harvey, Q.C.	West Coast Trollers Area G Association; United Fishermen and Allied Workers' Union ("TWCTUFA")
Keith Lowes	B.C. Wildlife Federation; B.C. Federation of Drift Fishers ("WFFDF")
No appearance	Maa-nulth Treaty Society; Tsawwassen First Nation; Musqueam First Nation ("MTM")
John Gailus	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.

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

TABLE OF CONTENTS / TABLE DES MATIÈRES

EXHIBITS / PIECES

Vancouver, B.C./Vancouver
(C.-B.)
September 19, 2011/le 19
septembre 2011

THE REGISTRAR: The hearing is now resumed.
MS. BAKER: Thank you. Good morning, Mr. Commissioner. Today we'll be dealing with Technical Report number 6 that came out of impact assessment with David Marmorek.

Before we get started, there's a couple of housekeeping matters that are outstanding. In the July marine hearings, Dr. Tim Parson was a witness and a number of documents were referred to by him. Ms. Gaertner asked him to produce those articles to us over the course of the break, which was done, and that was circulated to all parties just after September 1 when it was sent to us. So I'd like those three articles now marked as exhibits.

The first one would be something that I can't even pronounce. "Scientific Values as Indicators of Trophic Position and Competitive Overlap for Pacific Salmon", which is an article by Welch and Parsons in 1993. That would be the first exhibit.

THE REGISTRAR: It will be marked as Exhibit 1892.

EXHIBIT 1892: Welch and Parsons, d(13)C-d(15)N Values as Indicators of Trophic Position and Competitive Overlap for Pacific Salmon, 1993

MS. BAKER: Thank you. And the next article is "Sea Surface Temperature and the Pre-Season Prediction of Return Timing in Fraser River Sockeye Salmon," an article by Blackburn. That's it.

THE REGISTRAR: Exhibit 1893:

EXHIBIT 1893: Blackbourn, Sea Surface Temperature and the Pre-Season Prediction of Return Timing in FRSS, 1987

MS. BAKER: And the last one is "Locations of Marine Animals Revealed by Carbon Isotopes," an article MacKenzie Palmer et al." Sorry, I don't see the year.

THE REGISTRAR: Exhibit 1894.

EXHIBIT 1894: MacKenzie, et al, Locations of Marine Animals Revealed by Carbon Isotopes, 2011 [Scientific Reports]

MS. BAKER: Thank you. And then coming out of the marine hearings in August, a request was made of Sergio di Franco by Mr. Rosenbloom which was followed up in writing and circulated to all parties as well. The response from Canada is in an email to me and Ms. Tsurumi, and it has some text provided by Mr. Di Franco as well as a table of figures which is attached to the email, and that would be, once it's pulled up, could be marked as the next exhibit.

THE REGISTRAR: Exhibit 1895.

MS. BAKER: I believe we're still waiting to get it on the screen.

EXHIBIT 1895: Email from DOJ (Grande-McNeill) to Commission re Information Request of S DiFranco, with Attached Chart "Cohen Enquiries Recoveries 2006 to 2011," Sep 9 2011

MS. BAKER: There, and then there should be a table behind as well, yes. Thank you.

So we have today --

THE COMMISSIONER: Just before you get underway, Ms. Baker, I just wanted to welcome to the hearing room a group of students, I think perhaps 30 in number, who are here from Simon Fraser University, and they are students in the resource and environmental management faculty, so we welcome them here today. Thank you very much.

MS. BAKER: Thank you. Our witness today is David Marmorek with ESSA Technologies and I'd like to have him sworn, please.

THE REGISTRAR: Good morning, sir. If you could just turn on your microphone, please. Thank you.

DAVID MARMOREK, Affirmed.

THE REGISTRAR: Would you state your name, please?

A David Marmorek.

THE REGISTRAR: Thank you. Counsel?

MS. BAKER: Thank you.

EXAMINATION IN CHIEF ON QUALIFICATIONS BY MS. BAKER:

Q Your company and you are the lead author of Technical Report 6, Fraser River Sockeye Salmon, Data Synthesis and Cumulative Impacts?
A Yes.
MS. BAKER: Thank you. Could I have that marked as the next exhibit?
THE REGISTRAR: Exhibit 1896.

EXHIBIT 1896: Marmorek et al, Cohen Commission Technical Report 6 - FRSS: Data Synthesis and Cumulative Impacts, Apr 2011

MS. BAKER: Thank you.
Q And there was an addendum done to this report which has actually already been marked as an exhibit in these hearings. It's been marked as Exhibit 1575 if that could just be brought up. Thank you.
That's the addendum that was prepared?
A Yes.
Q Thank you. And lastly, you prepared an errata sheet on September 13 to correct a few typographical errors and clarify a number of things in the report. It's a one-page document on the screen now. Do you see that?
A Yes.
Q Thank you. And that's the errata sheet you prepared?
A Yes.
MS. BAKER: Thank you. I'll have that marked, please.
THE REGISTRAR: Exhibit 1897.

EXHIBIT 1897: Marmorek et al, Errata Sheet for Exh 1896, Cohen Commission Technical Report 6, Sep 13 2011

MS. BAKER: Mr. Commissioner, I would like to go over a little bit of the background of the witness, but I have also previously circulated the areas of expertise I'd like to have the witness qualified in. I circulated this to all parties in advance, and I think I've given a copy of this to you as well so you can follow along. The areas of expertise I'd like to have him qualified in are aquatic ecology, including the effects of human

activities on aquatic ecosystems, fish habitats and fish populations, environmental impact and ecological risk assessment, adaptive management, experimental design, decision analysis and modelling, and technical facilitation of interdisciplinary scientific workshops.

Q Now, just to review some of your background, you are an aquatic ecologist and you're the president of ESSA Technologies?

A Yes.

Q And you're also an adjunct professor at the School of Resource and Environmental Management at Simon Fraser University?

A Yes.

MS. BAKER: Sorry, Mr. Marmorek's c.v. has already been marked in these proceedings, and I'll just have it pulled up. It's Exhibit 566, just to follow along. Mr. Marmorek's c.v. was marked in the course of the Technical Report number 3 hearings.

Q Now, you have spent a number of decades working in areas like simulation modelling, ecological risk assessment; is that right?

A Yes.

Q Aquatic ecology?

A Yes.

Q Experimental design, statistical analysis?

A Yup.

Q Integration of large-scale research and monitoring programs?

A Yes.

Q Adaptive management and decision analysis?

A Yes.

Q All right. And you've also applied some of those skills working with humans and facilitation and team leadership; is that right?

A That's right.

Q Okay. I'd like to ask you a little bit about some of your past experience. I'm not going to spend too much time on it, it's written in the c.v., but just in terms of the work that you've done can you talk to us a little bit about the work you've done in the Columbia River and the PATH process there?

A Sure. Starting about 1993, I was asked to facilitate comparisons amongst different simulation models that were being used to forecast the survival of endangered chinook salmon, both in the river and over their entire lifecycle. And

that work sort of morphed into something called PATH, Plan for Analyzing and Testing Hypotheses which involved about 12 agencies over about six years looking at the question of whether it was better to barge salmon down the Columbia River past the Snake dams and Columbia River dams, eight of them, or whether it was better to breach the four Snake River dams. So that was a very controversial topic involving fairly adversarial circumstances.

Our team from ESSA basically led both the technical facilitation of that as well as integration of models and publication of various results which were extensively peer-reviewed by that inter-agency group as well as reviewers before they were published in journals.

Q Right. And have you done any work in the Fraser River basin?

A Yes. Going back to the times of the green plan in the early 1990s, we looked at the fate and effects of pulp mill effluents, did various work on the Fraser as part of the State of Environment Report that -- B.C.'s first State of the Environment Report in 1993.

Going back even earlier, in the 1980s we looked at various harvest management questions, return of Fraser River stocks for in-season management, and more recently, of course, worked with the Pacific Salmon Commission on the Fraser sockeye decline last year in June 2010.

Q Thank you. And you've of course authored numerous peer-reviewed publications; is that right?

A Yes, that's right. They're listed in the c.v.

Q That's right, pages of them, and a number, over 100, I think, technical reports as well.

A Yes.

MS. BAKER: Mr. Commissioner, I asked my friends to advise me if they had any difficulties with the expertise that I proposed to have him qualified in and I have heard nothing from them, so I propose that he be qualified in those areas.

THE COMMISSIONER: Yes. Thank you, Ms. Baker.

MS. BAKER: Thank you. And the report that's now marked as Exhibit 1896, the Technical Report 6, had a number of different authors involved. Some of them have already had their c.v.'s marked in these proceedings. Those would be, just for the

record, Marc Nelitz --
MS. TSURUMI: I don't see him as an author on that.
A Katie.
MS. BAKER: Anyway, sorry, Katherine Bryan. Bryan's c.v. is Exhibit 564. And Katherine Wieckowski, her c.v. is Exhibit 570.
We have a number of other authors on this report. Our practice has been to mark the c.v.'s of the authors in these proceedings, so I'll do that now. The first one would be Darcy Pickard, if that could be pulled up. Thank you.
Q Now, this is the c.v. of Darcy Pickard who worked on this report with you?
A Yes.
MS. BAKER: I'll have that marked, please.
THE REGISTRAR: Exhibit 1989:

EXHIBIT 1898: *Curriculum vitae* of Darcy Pickard

MS. BAKER: Thank you.
Q The next one would be Liz Martell, and this is similarly the c.v.?
A Yes.
MS. BAKER: Thank you. Have that marked, please.
THE REGISTRAR: Exhibit 1899.

EXHIBIT 1899: *Curriculum vitae* of Liz Martell

MS. BAKER:
Q The next one would be Clint Alexander. Again...?
A Yes.
MS. BAKER: Thank you. Have that marked, please.
THE REGISTRAR: Exhibit 1900.

EXHIBIT 1900: *Curriculum vitae* of Clint Alexander

MS. BAKER: Thank you.
Q Lorne Greig?
A Yeah, it's Gregg (phonetic), yes.
Q Greig, sorry. This is his c.v.?
A Yes.
MS. BAKER: Thank you. I'll have that marked, please.
THE REGISTRAR: Exhibit 1901.

EXHIBIT 1901: *Curriculum vitae* of Lorne Greig

MS. BAKER: And the last one is Carl Schwarz.
A Yes.
MS. BAKER: Thank you.
THE REGISTRAR: Exhibit 1902.

EXHIBIT 1902: *Curriculum vitae* of Carl Schwarz

EXAMINATION IN CHIEF BY MS. BAKER:

Q I'd just like to have you give us an overview of the steps or the components, I guess, that you took in creating the report that's now marked as Exhibit 1896 if you can just describe, in a summary way, and we'll get into it in a bit more detail, but the components or steps that you took in creating the report and coming to the conclusions you came to.

A So basically seven steps. So we first developed the approach that we were going to use for the qualitative and quantitative analysis of the evidence. Then we had a workshop on November 30th and December 1st of last year with about 30 people, both authors and reviewers of the Cohen Commission reports.

The third step was getting as much of the data as we could from those authors on the various potential stressors affecting sockeye as well as the productivity data, and organizing that data into a relational database for further analysis.

The fourth step was doing a retrospective ecological risk assessment or cumulative impact assessment based on the Cohen Commission Technical Reports that were relevant to that, and additional evidence from the PSC report on sockeye decline.

The fifth step was the quantitative statistical analysis which was in support of that synthesis of evidence looking at alternative hypotheses about what sets of stressors might have affected which life history stages and ultimately overall lifecycle productivity.

Then we wrote the technical report and revised it in response to the fairly extensive reviews, and then the last step was the addendum

on aquaculture that you just mentioned earlier.

Q Okay. And can you identify for us what your key conclusions were as a result of this work?

A Sure. So the first is that before attributing causality, you need to look at the overall pattern of change in sockeye productivity within both Fraser and non-Fraser stocks. In section 4.1 in our report summarizes the work from Peterman and Dorner and others, Skip McKinnell and so on, about what that pattern is. Because that's I think the first conclusion.

The second one is in terms of the primary factors responsible for the long-term declines in overall Fraser sockeye productivity and the 2009 low returns. So we concluded, first of all, that marine conditions interacting with climate change during the coastal migration stage were the likely primary factors for the long-term decline over the last 20 years in Fraser River sockeye productivity, and that marine conditions were likely to be the primary factor responsible for the poor returns in 2009 in both the Strait of Georgia and Queen Charlotte Sound.

With respect to the returning run of spawners from the mouth of the Fraser back to the spawning ground, climate change and en route mortality has definitely affected harvest and escapement, but not productivity measured as recruits-per-spawner, because that recruitment already includes harvest and en route mortality. It's basically escapement plus harvest plus en route mortality. So that did not affect the overall trends in sockeye productivity.

Other possible primary factors in the productivity declines include predation on adult sockeye as they come back to the mouth of the Fraser and climate change in the early life history stage from egg to smolt.

We were not able to draw any conclusion on diseases because of lack of data on the exposure of Fraser sockeye to diseases, and disease transmission from aquaculture we concluded was either unlikely or a possible primary factor depending on which of the two aquaculture reports one uses as evidence.

All the other factors we considered to be unlikely to be primary factors responsible for the

overall decline in productivity. For example, many of the freshwater habitat factors, though they may well have contributed to changes in some stocks in some years -- so, for example, delayed density dependence appears to have been responsible for some declines and productivity in the Quesnel sockeye stock in some years, but was not a primary factor responsible for the overall decline across all the stocks.

And finally, there are many gaps in existing information which make this whole process difficult, so both assessing the exposure as well as the correlation of those exposures with changes in productivity as well as having life-stage specific survival and condition information. So that led to some of the recommendations that we have.

Q Okay. Thank you. Now, you've identified that when you prepared this report, you looked at the technical reports prepared for the Cohen Commission, and you also looked at the PSC report in June of 2010?

A Yes, we did.

Q Did you do any independent research for this report?

A Well, some of the things that I just mentioned were independent in the sense that they hadn't been done before, and we weren't picking them out of the existing Cohen Commission reports or the PSC reports. So developing our approach to retrospective ecological risk assessment, that was novel, although based on existing published methods.

The quantitative statistical analyses were new and the synthesis both within and across life history stages and going across all of these reports was new research. Our recommendations really built on what was already in those reports, but we added some of our own ideas.

Q Okay. And did you independently assess the validity of any of the technical reports prepared for the Cohen Commission?

A No, we didn't 'cause we weren't asked to do that. We did carefully examine the methods that each of those authors used, and we looked at the reviews. It was the responsibility of the reviewers to review those reports.

Q The reviewers being the reports that you see attached at the end of each technical report?
A Right.
Q Okay. And then in the technical reports, if knowledge gaps were identified, would those knowledge gaps then carry forward into your reports? For example, were you asked to address any knowledge gaps that had been identified in the technical reports for the Cohen Commission?
A We were not asked to fill them, but in the sections where we discussed what we need to know better, and in our final -- I think it's section 5.2 in "Recommendations", we carried forward some of the recommendations from those reports.
Q And just kind of a background piece, I'd like to get into the report and have a figure in front of us as I ask these questions.
MS. BAKER: If you could go to page 10 of the report. Sorry, it's the actual page number, not the pdf number, yeah. There.
Q So there's a figure there on the screen which you call the "Cumulative Stress Model" and I just want to have that up there, and then ask you the question. Your report is called "Data Synthesis and Cumulative Impacts", and in looking at the cumulative impacts on Fraser River sockeye, were you able to assess first how the stressors within each lifecycle combined in a cumulative or in an interactive way to create the specific impacts on the fish?
A Actually, if you wouldn't mind, I would prefer to have page 18 up here to talk about that question.
Q Sure.
A We can come back to this page --
Q Yeah.
A -- later, but I think this is the better figure for the question you're asking within each life history stage.
Q All right. So this is figure, just for the record, 3.3-1.
A Right.
Q And it's called "The conceptual model of the life history of Fraser River sockeye".
A So this is complicated, though one reviewer thought it could be more complicated. Anyway, what we did is we went through all of the technical reports and looked at the candidate

stressors affecting each life history stage. Some of those reports were focused on particular stressors like contaminants or aquaculture or disease or predators, whereas some other reports were focused on particular life history stages, so there was a report on fresh water which looks at egg, alevins, fry to parr and smolt, things you see at the top of figure 3.3-1, and there was a report on marine conditions which looks at the post smolt part of that figure.

So we basically built up this conceptual model of the candidate stressors affecting each life history stage from those reports and from the workshop, and some of the reports, particularly those that looked at life history stages, like the freshwater report and the marine report, also looked at some of the potential interactions amongst these factors and how they could combine.

Also, the en route mortality report looked at combined interactions like temperature and pathogens, disease, harvest, all combining.

So the main thing was we tried to list all the plausible mechanisms and then consider how those might have interacted, although we don't actually have very little (sic) hard evidence on how they interacted.

Q One of the things that we were looking at, I guess, or what the title suggests is that you're going to be able to actually assess how a stressor in one life stage could impact those fish as they moved through their lifecycle. Are you able to do that, or were you able to do that?

A So only in a conceptual way or a theoretical, in the sense that the data don't really exist to sort of carry forward from each life history stage the changes in survival and condition.

What we did do, though, is in the analyses we looked at, particularly the correlational part, we're looking at factors which occur within each life history stage and considering how they ended up affecting the overall lifecycle productivity. So to the extent that there is some correlation there -- let's say that as productivity declined, some particular stressor went up or increased, you're looking at how an effect, within the life history stage, basically propagated to affect the overall life history or the overall lifecycle

productivity.
Q But what we are not able to do, I take it, is see how the sort of thousand cuts, the small assaults on the fish as they go through their lifecycle maybe have a greater impact than the whole, so you could see what a primary impact might be, but you may not be able to understand how a number of small non-lethal effects would have a cumulative effect over the life history.
A Well, you don't know that very well, except for those life history stages where you do have some estimates of survival within it, so, for example, if you take the freshwater life history stage, we have nine of 19 Fraser River stocks where we do have some estimate of survival from spawners to mostly fry, in a couple of cases smolts, and we can look at the patterns over time for that life history stage, and generally speaking, they haven't gone down. So we can say for that life history stage that the cumulative effect of all the factors operating on at least those nine stocks, at least to the fry stage for seven of them, doesn't appear to have negatively affected their survival or caused a decrease in trend and survival over the last 20 years, which is the period of interest.

So that's the power of having data that discretely summarizes the survival within each life history stage, because you are effectively looking at what's the cumulative effect of all those things, at least up till that point.

Now, there could be a delayed effect, so an animal may acquire some disease in that stage and survives fine, there's no trend there, but later on when it gets out to sea, that could end up affecting its survival. So again, you can't really distinguish that unless you have better estimates of survival at each life history stage.
Q All right. And is that something that you are able to do on the evidence available today, now?
A No. Only -- well, to a limited extent, in the sense that, as I just indicated, we have data -- for example, for the seven stocks, we measure fall fry. We know the survival from spawners to fall fry, but then after that, we're basically going from fall fry all the way back to recruitment. That's a pretty big box. That includes downstream

migration, coastal migration, returns. So we haven't been able to distinguish that with the exception of a few acoustic tag studies, but only for a few stocks here and there.

So that's the gap.

Q Okay. And that impacts our ability to understand these cumulative impacts as we're going through the life cycle.

A Yes, I think the key way to think about this is that the first thing you want to try to determine -- this is what a lot of the work in the Columbia River has done with PIT-tagged fish for example -- is in which life history stage is the bottleneck occurring, and then what are the factors most correlated with that decrease in survival.

You need some contrast, either over space or time, in those stressors to be able to deduce which of those factors are most likely.

Q And we're not able to do that on Fraser River sockeye, is that what you're saying?

A Well, we've done the best job we can with the data we have.

Q No, I'm just trying to identify that there are a bunch of data gaps that prevent you --

A Yes.

Q -- from doing that full analysis.

A That's right. You can't do it as well in the Fraser as you could in the Columbia because of the data that you don't have.

Q Right, okay. Going back to Figure 2.3-1, which is on page 10, the first one I asked you to look at.

A Yeah.

Q What does the dotted yellow line indicate in the A-1 and the A-2?

A So this is a conceptual figure. On the Y axis you have a measure of cumulative stress, and if you get to 1, you're dead. On the X axis, you have the different life history stages, so -- and these are just different pathways that an individual salmon might follow.

So A-1 is a fish that experienced lots of stress as a fry, perhaps there wasn't enough food or perhaps there were bad environmental conditions, so it almost died but not quite, and then got all the way through to the adult stage and then experienced some other stress, maybe a predator, maybe some disease, and died.

A-2 represents a situation where this was a fish that survived that early life history crunch whereas most of its brethren died, and so there was less intra-specific or within-species competition, so as they went down to the mouth of the Fraser, it actually found there was lots of food to eat because there were fewer competitors around, so there's actually a decrease in stress.

So you can have these kind of compensatory effects that occur between life history stages. They're not always necessarily additive or cumulative or synergistic as you go through life history stages.

Q Okay. And you talk about that in your report. At some point you talked about compensatory reduction and this is an illustration of what you were referring to?

A Yes.

Q Okay. Page 22 of your report, you talk about a weight-of-evidence approach that you use to assess whether a stressor or a factor made a substantial contribution to the decline. Can you explain that weight-of-evidence approach?

A Sure. So this is what we were talking about when we said we adapted a retrospective ecological risk assessment framework. So we went through basically four sets of questions here, and I'll just read them 'cause it's simpler.

Plausible mechanism:

Does the proposed causal relationship make sense logically and scientifically?

Is it possible that contaminants could harm fish? Yes.

Is there evidence that sockeye populations are, or have been, exposed to the causal factor?

So another example there would be we have some data on contaminants regarding their exposure and how that's changed over time.

Correlation/Consistency:

Is there evidence for association between adverse effects in sockeye populations and presence of the causal factor, either in time or space?

So in the case of contaminants, to continue that example, in general contaminants did not increase as sockeye productivity decreased, which suggests there is not evidence for an association. So that's the kind of question we were trying to answer.

Then the fourth category is "Other Evidence" which can be supportive, so things which say, well, are there certain thresholds which suggest that the exposure level, when above those thresholds -- in the case of contaminants, that report specifically looked at thresholds, hazard thresholds they called them, for each of the measured water quality and sediment parameters.

Then "Specificity", this is where, if there's a particular kind of effect in the population that's caused by exposure to a certain stressor, so you might say there's a certain kind of physiological response to a certain contaminant, and you can then look for that response if you have those data, or if there have been experiments on them.

"Experiments" in a laboratory or in a field are also quite helpful in confirming the causes of things, so the experiments that were done in the field, for example, on en route mortality by Tony Farrell and others, where they put fish through a kind of exercise machine and see at what temperatures they die, confirms that certain temperatures kill them.

Then "Removal":

Has the removal of the stressor led to an amelioration of the effects in the population?

Well, that relies, really, on some contrast happening in that stressor.

So those are the categories of evidence that we looked at, and then organized in that decision tree, Figure 3.3-3.

Q So that's on page 24. As we wait for this to get

settled on the screen, can you explain how you applied that weight-of-evidence approach to this tree?

A Sure. So it basically takes those questions that I just outlined and goes through asking whether a given factor or hypothesized stressor passes various tests. So the first case is, is the mechanism plausible? In almost all of the cases in all the reports, the answer to that was yes. The only exception was in the Noakes report. He felt it was not plausible that waste from salmon farms could have an effect.

Then we moved to the exposure question, which I just described, for contaminants, and here we're addressing are there data by which you can assess changes in exposure over time or over space? For many of the hypthesized stressors, we didn't have exposure data, and I should say for one of them we had no data, and that was for pathogens. So no conclusion was possible.

So the middle box, there, when it comes to exposure, we had exposure data but it wasn't likely that the fish actually got exposed to those stressors. That would be the case for something like mining or small hydro where there were so few mines or small hydro facilities within the Fraser basin that it's very unlikely that sockeye spawning and rearing habitats were exposed.

So if you get past that set of questions, you then follow the "Yes" box and you come down to, okay, so it looks like there was some exposure. Is there any correlation or consistency? I ran through an example earlier for contaminants where, in general, the answer to that was no. There was not consistency in the change in the stressor and the change in productivity.

Now, in some cases, we got through that box and down to, yes, it looks like there was some correlation that was consistent with the hypothesis, and so we moved down to the bottom box and "Other Evidence". So that's where the climate changes and changes in marine condition ended up being either possible or likely factors for some of the life history stages. They got all the way down to the bottom box.

The predators, as far as returning adult salmon, there was some exposure data that looked

some predators had increased over time, and so it looked like it might be possible but there really weren't good correlation analyses. So we ended up at the bottom without enough evidence to say anything other than it was a possible factor.

Q When you speak about contaminants in this example, are you referring to the contaminants that were measured in Technical Report 2, or are you also including the contaminants that Don Macdonald identified as being unmeasured, for example, endocrine disruptors and emerging contaminants?

A Just the ones that were measured. For things that aren't measured, we would end up in the same place as disease of no conclusion possible.

Q And again, your focus throughout was to look at primary driving factors; is that right?

A That's right. What are the primary factors responsible for driving the overall long-term declines in productivity of Fraser River sockeye, productivity over the last 20 years, and we also looked at non-Fraser sockeye to help distinguish amongst those hypotheses.

So some of those that are unlikely fell in the unlikely box to be primary factors, they could still be contributory factors.

Q And the non-Fraser stocks, was that the work in Technical Report 10?

A Yes.

Q All right. Now, what does this kind of work tell us, or this kind of analysis tell us about each life stage, then?

A So again, we're looking at the relative likelihood of each factor within each life history stage being a primary driver of the overall declines in Fraser River sockeye productivity over the last 20 years.

Q And this kind of analysis, does it allow you to tell us anything about the entire lifecycle of the fish?

A So because our primary response variable is the overall productivity of Fraser River sockeye across these 19 stocks, we are looking at correlations between the stressors and overall lifecycle productivity. So as I said earlier, it's still our conclusions, within each life history stage, do in fact still relate to the overall patterns over the whole lifecycle.

As I said earlier also, we do have data for juvenile survival per spawner for nine of those 19 stocks, so that also provides something about what happens within the life history cycle.

Q Just to go back in the report, page 14, you talk about -- there's a section in your report, 3.2, that's titled, "Unknowns, Unknowables, Knowledge Gaps and Data Limitations." I know you've talked a little bit about some of these already, but just at the bottom of the page, you talk about the evaluation of alternative hypotheses, and a couple of principles that underlie that analysis. Can you explain those principles?

A Sure. Well, the first one is sort of the general principle of science is that you can only reject hypotheses or provide evidence against hypotheses. You can't confirm that they're absolutely true, and those for which you have less evidence against them are the ones that become more likely, and the other part there is that correlation does not represent causation, and so we have to be very careful which variables we use to look at correlational patterns. There has to be a reasonable plausible mechanism before you include a given variable in a correlation analysis.

Now, if you find out that, as the example I had earlier on contaminants, that there is not a correlation, that suggests evidence against that hypothesis.

Q And in terms of data limitations in the evaluation process, were those identified by you?

A Yeah, the data limitations -- at the end of each section, we have things we need to know better, and then we summarize those in section 5.2.

Q And what are the main, the dominant limitations if you can, or the ones that sort of stood out for you as being the most problematic?

A So the first I already mentioned is that when you want to determine at which life history stage bottlenecks are occurring, it's really helpful to have information on survival through each of those life history stages, and also the condition of fish for each of those life history stages. Now, you can't get that perfectly, it would be too expensive, but we could certainly have more information than we currently have.

Then the second is, as we've just gone

through that decision tree figure, there's gaps in the information on exposures and a shortage of quantitative analyses of correlation and consistency, which make it hard to get all the way down through that tree. You know, an example would be if we had information on diseases, we could say a lot more about the likelihood of that stressor actually being responsible for some of the declines that we've observed.

Q I think that covers off most of the knowledge gap points I wanted to talk to you about. What about the unknowables? Page 16 you describe a challenge as the third challenge as unknowables. What are you trying to describe there? What's an unknowable?

A So first of all, it's hard to know exactly how a salmon dies unless it ends up in a fishing net, a predator's stomach or there's some sort of massive fish kill like happened in the Cheakamus River a few years ago with caustic soda spill.

So you can really only infer how a fish died indirectly by looking at strong contrasts across time, across stocks and across space. So ideally you have contrast in survival, and we have a lot of that because productivity trends are varied over time and over space. So then we can look at the contrast in stressors, but you're never going to know ultimately exactly how those fish died.

Even if you could measure all the stressors - and you can't - you're never going to have for all the coastal migration period full knowledge of all the predators, competitors, food supply contaminants, temperature, conditions, exactly. So that's essentially unknowable. There is incomplete information. So you're going to have to make inferences based on contrasts. And it's really unknowable exactly how all of those stressors ultimately combine to hit that 1.0 mortality part on the graph we were looking at earlier.

Q Okay. The bulk of your report is where you examine each of the different life stages, and you've broken them into five different life stages. Do you have the same degree of confidence in your conclusions for each life stage, and if you don't how would you rank your confidence in the different life stages?

A So on a relative basis, with the highest level of confidence first and going down to the least -- it would be interesting, people might debate this, but I would put from the mouth of the Fraser to the spawning ground at the highest level of confidence that got quite accurate estimates of survival and of a lot of other exposure factors, temperature and disease.

Q That's life stage 5?

A Life stage 5. Then next I would put egg to fry because we do have, as I mentioned, seven stocks where we have a egg to fry survival and two where we have egg to smolt survival. We actually have data there. Then next I would put -- that would be life stage 1.

The next would be life stage 3, the coastal migration because there are data on catch per unit effort, for example, in the Strait of Georgia which is sort of an index of abundance, and kind of like an index of survival.

Then I would put the smolt out-migration stage, life stage 2. That is from the time smolts leave a rearing lake to the time they get to the estuary. We really have very little data on that. There are a few stocks with acoustic tags, but not many.

Then the last one with the least level of confidence would be stage 4, which is the growth in the North Pacific where they're out there for a couple of years. We really don't know very much about what they're exposed to and what happened to them until they start coming home.

Q Okay. Thank you. Well, I'm going to go through your life stages starting with 1 and going to 5, not in the order of confidence. So the first one that you refer to, of course, is the egg to fry which begins at page 39 of your report.

You prepared in your errata sheet some comments on what the other evidence column means here. So the conclusions are actually on page 48. I was just identifying where the chapter or section begins. So if we go to page 48, you've got the conclusions section which is a table summarizing some of the points that we'll be discussing.

In your errata you've identified Table 4.2-1 as having some changes, so I'm just wondering if

you can relate those errata comments to the table. It's a bit difficult with only one screen.

A No, that's fine. It's probably fine to just leave the errata up, or I can read from it and you can put the other table up. Basically, as we just talked about on page 22, the other evidence column refers to the fourth set of questions in that decision tree. So when we say "no" in that table -- if you go back to --

Q Sorry, can I just --

A -- page 48.

Q Thank you.

A Yeah, oh, that's good. So when we say "no" that indicates that the other evidence was not available for the listed stressor; in other words, threshold, experiments and that sort of thing. It's hard to do an experiment on the proportion of a watershed that's been forested. You can't fit it easily into a lab. So there were not information available for those stressors where it says "no".

So "yes" means that other evidence was available. For example, on contaminants, water use, there are some lab studies that have been done. There also have been lab studies done mostly on hatchery fish for diseases, except we don't have exposure data. So "yes" means that other evidence was available for listed stressors from those reports and provided additional support for the hypothesized stressor.

"Against" means that other evidence was available and was contrary to the hypothesized stressors so, for example, there were detailed studies done on the Nechako, large hydro, which indicated that it was not likely to have had a significant effect on the overall declines in sockeye over this period of time.

Then "Mixed" means that other evidence were available, some supporting and some negating the hypothesized stressor.

Q Okay, thank you. And the "Likelihood" column, can you just explain for us what you're assessing there?

A So when we went through that decision tree, this is the conclusion that we came to regarding -- and it's in the first sentence of the caption of Table 4.2.1. It's:

> ...the relative likelihood that potential stressors encountered by Fraser River sockeye salmon during life history stage 1 (including eggs, alevins, fry, and parr), have contributed to overall declines in productivity in recent decades.

Q Okay. And contaminants, as we've already talked about earlier, when we see contaminants on this line, that's referring simply to the measured contaminants that were in Technical Report 2?
A Correct.
Q So it does not include endocrine disruptors or emerging contaminants.
A Correct. One would come to the conclusion that no conclusion was possible.
Q Pathogens, I think you've already described that there. Why do you say there's no assessment of likelihood is possible for that one?
A Could you repeat that, sorry?
Q For pathogens, why is no conclusion possible for the likelihood --
A Because there are -- it says "few data", but there are essentially insufficient data for assessing the exposure of sockeye to disease, as explained in Dr. Kent's report.
Q And I think you may have already answered this, but I'll just -- just for clarity, while these are not -- where it says that it's under the column "Likelihood", it says it's "unlikely". That's unlikely to be a primary driver, but it doesn't necessarily exclude impacts of those stressors on other life stages; is that right?
A That's right. They could be contributory factors.
Q And the next life stage is 2, smolt out-migration, and this is one where you indicated you had a lower level of confidence in the data, and why is that?
A Basically because we don't have estimates of survival from the time that smolts leave a rearing lake to the time they get to the estuary, so it's hard to know how that life history stage has changed over the period of interest.
Q Okay. So the conclusion, you did a similar table for that life stage like the other ones, and it's on page 54. Actually before I get to that table, I had a couple of other comments I wanted to make,

sorry.

Section 4.3.1 talks about plausible mechanisms. I think we've reviewed now what that means. That's on page 50. At the last sentence there, you talk about:

> Earlier outmigration could lead to a mismatch between the arrival of salmon smolts in the Fraser estuary and Strait of Georgia...

You talk about earlier out-migration leading to a mismatch between the arrival of salmon smolts in the Fraser estuary and Strait of Georgia. You talk about earlier out-migration leading to a mismatch. Is that also possible for late out-migration as well? As you excluding later out-migration?

A Both could be a problem. Any mismatch could be a problem.

Q And I think you have an errata on this page as well. The last paragraph, 4.3.2, "Exposure of Fraser River Sockeye to Stressors". The errata sheet is on the page -- perhaps it's easier if we just have the text of the document. Thanks.

You see in the fourth line down, it says:

> ...they generally spend only two months in Stage 2 migrating downstream the ocean...

A Right. That was incorrect, and that was basically -- that two-month period was meant to describe the period within which all Fraser stocks might be migrating actually came from the contaminants report. So it's obviously much less time that each stock spends migrating downstream.

Q It's more in the range of seven to ten days?

A Right.

Q Now, sorry, I'm skipping around a little bit. We looked at the table 4.3-1 which I don't think we need to spend a lot of time on that. We've identified, I take it, the same explanation for other evidence would apply on this table. It's at page 54 of the report. The same analysis under the "Likelihood" column, although it's for a different life stage; is that right?

A Yes.

Q Okay. And same for contaminants. It again

relates only to the measured contaminants; is that right?
A Correct.
Q I'll move to life stage 3. So this is coastal migration and migration to rearing areas. That begins on page 55. Plausible mechanisms that you set out at the bottom of page 55 and over to 56. We've heard in these hearings evidence of harmful algal blooms and I don't see that listed under this section. Is that a plausible mechanism as well for declines in the coastal migration and migration to rearing area phase?
A Yes, it is. That was an oversight. We do mention it later on in section 4.7 of our report, page 88, but it should have also been mentioned in this section here.
Q Okay. And then at page 64 of this section, you look at conditions in the Queen Charlotte Sound and the Strait of Georgia, and you can compare them. You did some additional analysis of data in this section; is that right?
A Yes.
Q Okay. So what was done?
A So maybe you could go to page 67. I'll just look in my report.
Q Next page.
A Actually, let's look at page 66, I'm sorry. So we assembled the data that were available for various time periods for the Strait of Georgia and the Queen Charlotte Sound, and the reason for doing that was to explore -- again, this was a preliminary analysis of which variables were best correlated with the changes in overall lifecycle productivity.

We also included information in these analyses of the spawning abundance of all these stocks as well.

So the variables that you see listed here, the last column in Table 4.4-1 shows the start of the available data. So we used -- for this analysis, between 1969 and 2004, we used anything which started before 1969, so that excluded chlorophyll measurements for both Queen Charlotte Sound and Strait of Georgia, and also average sea surface temperatures in Queen Charlotte Sound which started later, so we included the other variables, then off you go onto the next page.

There you can see the variables that were included in this time period and the ranking of the model at the bottom is the relative degree of support of that model in the data. That is, the relative ability of each model to explain overall patterns of changes in sockeye productivity across all of the -- actually there was 18 stocks we included here. We left out the Pitt.

So the top three models here all had -- actually if you could stay on the top -- the top three models all had relatively similar level of support and the top ranked model here was Strait of Georgia temperature. The second one was Queen Charlotte Sound salinity and discharge, and the third were all at the variable. So basically our conclusion from this is there isn't a clear difference in the explanatory support between conditions in those two regions over this time period.

Q Okay. Were there any shortcomings in the data available for use there? For example, were there things missing in the Queen Charlotte Sound data that you had for the Strait of Georgia data?

A Right. We were missing temperature for the Queen Charlotte Sound for that analysis and we're missing chlorophyll for both.

Q Okay. Then the next table down, 4.4-3 is the time frame 1980 to 2004.

A Correct. And now we have the temperature data. We still don't have the chlorophyll data. So it's basically similar to the previous analysis.

In this case, the top three models - you can see the ranking at the bottom there - were all including Queen Charlotte Sound temperature which is negatively correlated with sockeye productivity and salinity which was positively correlated across all of those top three models.

The Strait of Georgia models for this time period showed relatively little support in that they were lower ranked models, significantly lower.

Q Okay. And then you turn the page and you look at 4.4-4 and one page over again is 4.4-5. This is where you compare the importance of chlorophyll in the Straight of Georgia and the Queen Charlotte Sound, the importance of that measurement in explaining the productivity of Fraser River

sockeye from '96 to 2004; is that right?
A Yes, that's correct. Now, that's a shorter time period, and because it's a shorter dataset, we weren't able to compare the relative importance of the Queen Charlotte Sound variables and the Strait of Georgia variables in one analysis. Otherwise, we'd have too many parameters for the length of the dataset. So we just looked at them separately.

So within the Queen Charlotte Sound, chlorophyll was very important. It was in the top four ranked models and so that was interesting. Then within the Strait of Georgia, salinity was most important in the top-ranked Strait of Georgia models, and that was negatively correlated. So the more saline it gets, the worse it was within this time period. Again, it may be different within a longer time period.

Q And then are you familiar with the work Jim Irvine has done in relation to chlorophyll-a levels in Queen Charlotte Sound and Chilko productivity?
A Yes.
Q And how do the conclusions that Jim Irvine has drawn relate to the work that you're showing us here on Queen Charlotte Sound and the importance of chlorophyll.
A So they're generally consistent in the overall conclusions and they're somewhat different in the details. So I'll just quickly run through some of the differences. They don't really affect the consistency, but they're important.

So Jim used Queen Charlotte Sound chlorophyll for the first three weeks of April and related it to the marine survival of Chilko sockeye only. We used chlorophyll data for April and May plus other variables such as discharge, salinity, temperature and spawners to attempt to predict the overall productivity for all of the Fraser stocks, not just the Chilko.

So it's interesting that despite the fact the analysis was structured quite differently, we came up with similar results. In the six models that used chlorophyll, the productivity was positively related to April chlorophyll in all six and it was positively related to May chlorophyll in five out of the six models. So it's pretty similar outcome to Jim, different type of analysis.

Q And in this inquiry, we had some reports prepared by Dr. Beamish and others of his group and they were entered into evidence in July, in our hearings in July. Have you read those reports?
A Yes.
Q All right. And do those reports change any of your conclusions?
A No. The main conclusion that's relevant here is that marine conditions have a significant effect -- marine conditions during the coastal migration stage have a significant effect on declines in Fraser sockeye productivity, and that's consistent with what reports by Dr. Beamish and his colleagues found.

Whether that mortality occurs in the Strait of Georgia which Dr. Beamish is mostly focused on or in Queen Charlotte Sound, which Dr. McKinnell focused more on, doesn't affect our conclusion.
Q And I'd just like to go to the table for this life history stage which is similar to the ones we've already looked at, page 70.
MS. BAKER: This is referred to in the errata sheet, but please don't put the errata sheet up on the screen. We'll just ask Mr. Marmorek to explain it.
Q You see under the "Correlation and Consistency" and under the "Other Evidence" column, there's a little dash which we haven't seen before and no data. What do those mean?
A So if you recall the decision tree that I described earlier, Figure 3.3-3, the dash means that we didn't get all the way through that decision tree to that column, and therefore it didn't need to be analyzed. So in the first row of Table 4.4-6 for pathogens, there were not enough data on exposure to pathogens to merit going to -- you can't correlate data you don't have.
Q All right. For predators, to contrast with pathogens, how does the no data column then compare to the little dash under the pathogens column? What's the difference?
A So we do have some data for some predators in terms of how they've changed over time, both in fresh water and in the ocean and have included those data. However, until we did our own statistical analyses, there weren't any

correlational analyses in Technical Report 4, was it, I believe, on predators. I can't remember now which --

Q Nine.

A Okay. Oh, eight.

Q Right.

A Technical Report 8. Anyways, Villy Christensen and Andrew Trites report, that's easier for me to remember than the number.

Q Okay. The "Likelihood" column, we've already talked about what that means. I just note at the bottom of the very last line before the heading 4.47, it says:

> The conclusion is thus that it is very likely that marine conditions during the coastal migration life stage contributed to the poor returns observed in 2009.

And the "very likely" is contrasted with the "Likelihood" column that we see there for marine conditions where it just says "likely". What's the distinction you're drawing there?

A So in the table, we're talking about the overall declines in productivity over 20 years, and in that sentence that you just quoted, we're talking about what happened to the returns in 2009, for which there's a lot more evidence.

Q And then we see both "marine conditions" and "climate change" on that table, and we've heard a lot about both of those. Are they mutually exclusive or do they actually overlap in some ways?

A They overlap in a lot of ways, and that's discussed in Technical Report 9, I believe, by Scott Hinch and Eduardo Martins, so they talk about how climate change can affect conditions in the ocean in terms of food availability.

Also Technical Report 4 talks about past changes in marine conditions and temperature, and looks at future changes in marine temperatures with climate change and discusses how some of the extreme past temperature years look a lot like the expected future years, say in 2080.

So what we have is overlap there where climate change is likely to increase temperatures, and increased temperatures are likely to be bad

for food production and changing the kinds of predators that sockeye are used to, all of which is not good for Fraser River sockeye.

Now, for Alaska sockeye, a little increase in temperature can be a good thing.

Q Thank you. Now, the next life stage is 4, which you describe as growth in the North Pacific and return to Fraser, so where does this life stage begin, just physically. Is it at the end of Queen Charlotte Sound?

A I think, yeah, once the fish get up into the Gulf of Alaska.

Q Okay. So up past Hecate Strait?

A Mm-hmm.

Q Now, this one would appear to cover two different areas. There's the coastal area in Alaska but then also the North Pacific and the return home. So is that right, first of all?

A That's correct.

Q Okay.

A Until you get to the mouth.

Q Okay. So is there a difference in the amount of information that's available to allow an assessment on the return to the Fraser that's a return journey versus the growth in the North Pacific phase of their life, so two different parts to it.

A Yes, we tend to have more information once they get close to shore so, for example, in the report on page 75, we have the percent of salmon that are returning by different routes. Like we have the northern diversion, or -- in the Strait of Juan de Fuca, and so we get more information primarily from test fisheries as the salmon get close to home.

Q And this is a life stage where you feel you don't have a lot of confidence, there's not a lot of data available for you to assess; is that right?

A Yeah, it's a big blue box.

Q Okay. Your conclusion box is set out on page 79. I think we've clarified now what all of the different marks mean in this box. I won't take the time to do that.

But with respect to the different factors set out there, there's nothing for contaminants. Is that because the report by Don MacDonald didn't include marine contaminants?

A That's correct.
Q So can we assume by looking at this report that marine contaminants are a non-issue?
A No, you can't assume that. It would fall into the same part of the decision tree as pathogens or unmeasured contaminants like endocrine disruptors that you mentioned earlier.
Q The last stage is the migration back to spawn, and this stage is the stage you feel you have the most confidence in the data for; is that right?
A Correct.
Q And that begins at page 85. I think there's just an errata correction we want to make under the "Conclusions" section. The first line I think there's a correction you want to make there.
A Right. We need to replace:

> ...life history stage 2 (smolt migration from rearing habitats to the Fraser Estuary)...

With:

> Life history stage 5 migration back to spawn.

I was guilty of copying and pasting.
Q I think we've all fallen victim to that. All right. Now in this conclusion table which is on page 86, if we just scroll down, you have a different conclusion for pathogens than you have previously. In the previous life histories you've noted that there was not evidence to support many of these analyses. But here on the return through the Fraser, return to the spawning grounds, you now say that there is some data. What are you referring to there?
A So under the other "Evidence" column, as I was talking about earlier, there have been very extensive studies by Scott Hinch and Dr. Farrell and many others on the health of fish as they're making their way home to spawn, and levels of disease, physiological condition. And those are field studies, and they're quite well correlated with survival measurements from tagged fish, radio-tagged fish. So there are a lot more data for that life history stage.
Q Okay. Then under "Likelihood" column for climate change, temperatures and en route mortality, you

have two conclusions. One says "definitely" and one says "unlikely" which appear to be radically opposite. Can you explain what that means?

A Yes. So these are very important footnotes. So if you take the "definitely" first, it has footnotes B and C there which means harvest and escapement. So the en route mortality - and there's graphs in here that were drawn from the report by Martins and Hinch - shows that en route mortality has increased over time and has been very substantial particularly for the Early run and Late run sockeye.

So that affects the number of fish that get back to the spawning ground, or would, if you had the same level of harvest, and harvest has been reduced accounting for the expected en route mortality. So there's definitely an effect of climate change and temperatures on both harvest and escapement.

However, as I mentioned earlier, the way in which recruitment is measured is escapement plus harvest plus en route mortality. So it's essentially the number of fish that return to the Fraser prior to any harvest or prior to any en route mortality, so that declining productivity cannot be explained by en route mortality because en route mortality is already included in it, already included in recruitment. In fact, as en route mortality goes up, recruitment would go up. So it's unlikely to be an explanation of declining lifecycle productivity.

Q And then the addendum, which was earlier marked in these proceedings as Exhibit 1575, you have a similar conclusion table in that document, and that's at page 18 of the report, and it spills over two pages. There, okay.

Now, again, you have two contradictory notations for disease of salmon farm origin, and you'll see that on page 19 there. It says "possible" and "unlikely". Can you explain what you're talking about there and can they be reconciled?

A So our job here was to look over the work that Dill and Noakes did and look at what the implications of their conclusions were for our overall conclusions in Technical Report 6. It wasn't our job to try to reconcile them or read

the 250 references that they referred to, only 25 of which they looked at in common, by the way.

Anyway, so we basically said, well, if you took Dill's report as evidence, your conclusion would be that disease from salmon farm origin was a possible contributor to the overall declines in sockeye salmon. And if you took Noakes' report, your conclusion would be that that was unlikely.

Q Okay.

DR. BAKER: And then just one last figure before the break. The Figure 3, if you just scroll down, Mr. Lunn, follows this table.

Q What is that indicating to us?

A So as it says at the bottom, these are the mechanisms identified as possible or no conclusion possible based on Dill, 2011. So greyed out in this figure are the hypothesized mechanisms that both authors cumulatively were looking at in terms of causal pathways by which sea lice waste, escapees, and disease could potentially affect sockeye salmon from salmon farms.

So the ones that are -- that was the overall diagram, and then the ones that are greyed out are the ones which were considered to be unlikely, and the ones that remain are those that Dill considered to be possible, whereas Noakes considered all of those pathways to be unlikely.

MS. BAKER: Thank you. Mr. Commissioner, we could take the break now if that's convenient.

THE COMMISSIONER: Yes, thank you.

THE REGISTRAR: The hearing will now recess for 15 minutes.

(PROCEEDINGS ADJOURNED FOR MORNING RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: Order. The hearing is now resumed.

MS. BAKER: Thank you.

EXAMINATION IN CHIEF BY MS. BAKER, continuing:

Q The last part of your addendum I want to go to is the Table 3, which is just following this table that's on the screen. All right. And this table, I just want to identify that it replaces Table 4.7-1 that's in the main report.

A That's correct.

Q Okay. And it is a summary of all of the analyses that you've done for each life stage, basically taking the likelihood column and importing it for each life stage; is that right?
A That's correct.
Q Okay. And we would like to use the addendum one, because it includes the fish farm issues?
A Yes.
Q And that's on the following page, if you just keep scrolling up. That's the whole table. Okay, thank you. Now, moving back to the main report, Table 4.7-2, this is the very last section of your report, and I'd like you just to explain what -- well, first of all, there's an errata and we might as well clear that up right away. That's on the following table. You've got two tables here, 4.7-2 and then 4.7-3. There we go. So there's an errata correction that you wanted to make on the first column there, the header?
A Yes. So the first two columns state life stage and the first column should be stressor category i.e. Cohen Commission technical report.
Q Okay. Thank you. Now, with these two tables that we've just thrown up on the screen with no explanation yet, what are you actually showing us here? What was the analysis that was done?
A So we're looking at the relative ability of different combinations of variables to explain the observed changes in sockeye lifecycle activities, somewhat similar to the Queen Charlotte Sound/Strait of Georgia analysis we talked about previously.

So what we did for that is we carefully lined up all the explanatory factors to match the life history stages and age structure of each of the stocks, so, for example, April to August sea surface temperatures in the Strait of Georgia. And for the first table we grouped those explanatory factors by life history stage. If you could please go back to Table 4.7-2. That's great. Just to the top. Yeah, so you can see the column headings there. So we had incubation to lake rearing, outmigration, coastal migration, and so on. And the "X's" in the shaded grey cells here represent the variables that were included in that model. So M4, for example, coastal migration, includes Strait of Georgia discharge,

and so on down there.
And so we were basically curious about which of the life history stages had the explanatory factors that were most correlated with the overall lifecycle productivity, given the data that we had. And then the following table - you don't have to go there; we just looked at it - is we grouped the explanatory factors according to the technical reports. So each of the technical reports that were received, so for example, we would use just the contaminant information, or we would use just the predator information. Rather than organizing it by life history stage, we just organized it by report.

Q Okay. And what conclusions did you draw?

A So for the table we're looking at here, Table 4.7-2 -- I wonder if there's a better table to see. I guess that's the best one. Leave it there, that's fine. So for that table, the three models with the highest relative level of support - again, these are relative levels of support - included the -- the first was the model which included factors for all of the marine life history stages. The second was a model with the factors for the coastal migration stage. And the third was factors with -- was the life history stage for the return to the Fraser.
So over this time period and the data -- and give the data that we had available from 1969 to 2001, the marine phase factors appear to have the best ability to explain the patterns and productivity. And then, when we looked at the analysis by the Cohen Commission project, the model with the highest level of support included all 34 factors, so basically throwing everything into the soup. And the factors, when we looked separately, at the separate reports, the ones which came next was a model with data for predators and alternate prey and factors for the Lower Fraser/Strait of Georgia.
And so it was a bit surprising that the best model would be the one with all the factors, because the criterion that we're using to assess the relative level of support, AIC criterion, penalizes models which have a lot of variables in it.
So overall, the bottom line is that from the

lifecycle analysis, the marine phase factors appear to have the best ability and we generally -- we conclude that it's best to look at this using a lifecycle approach rather than a project approach.

Q Okay. And then page 95 of your report, which follows these tables, just before you say the relative -- in the paragraph above, The Relative Importance of Different Stressor Categories, you state, at about -- the line's nine, so part -- about halfway through that paragraph:

> The strength of any conclusion that freshwater life stages are not as important as marine life stages can only be as strong as our belief that the assemblage of variables described above is a reasonably accurate representation of the freshwater component of the life history of Fraser River sockeye salmon.

What does that mean?

A So all of these results are only as good as the data that you put into them, and for the freshwater life history stage, there really weren't many datasets available within the time we had and may not be available, period. So, for example, we had to use air temperatures instead of lake or stream temperatures as a proxy variable for freshwater conditions. So ideally, you would have a lot more data on freshwater conditions.

Q So if we accept that the freshwater life history stages are not as important as marine life stages in describing the productivity decline, does that mean that freshwater stressors are having no impact on productivity?

A No, it doesn't mean that. And first of all, you have to remember this is only one part of what we did. This quantitative analysis is supplemented by the main part of the report where we're looking at all the results for Technical Report 3 by Nelitz, et al, and Technical Report 12, and looking at all of that information. So all it means is that within this particular analysis that the freshwater indicators we had in our database were not as strongly correlated as the marine indicators with changes in lifecycle productivity.

One other thing I should point out in passing is that all of these models included the spawners, allowing for density-dependent effects in a Ricker-style model from the parent generation subsequently to the overall lifecycle productivity. So you can think of that is that if there were significant density-dependence, and there is because the coefficients for spawners are always negative in all these models, so that's carried through in the analysis. You can think of it as a freshwater event, the number of spawners that has ramifications over the entire lifecycle.

Q And looking at these models that this section is dealing with, there's a comment by one of the reviewers, Sean Cox, that there's no indication of variation in salmon productivity explained by the alternative models, and he comments that no r-squared values were provided. So I just wanted to talk to you a little bit about that. First of all, what is an r-squared value and how does it help us understand how well a model explains the data being assessed?

A So r-squared is also called the coefficient of determination, so it's the fraction of the total variability in a dependent variable, and in this case we mean the total variation in sockeye productivity log recruits per spawner over all 18 stocks that's explained by the variables that you include in a given model. So a simpler example would be you might say well, 70 percent of the variation in somebody's income, or in people's income over a sample, is made up by their level of education and their level of work experience. And so in that case, r-squared would be .7.

Q Okay. Without those r-squared values, are you able to say that one model with a certain set of variables can explain the changes in productivity better than another model?

A Yes. We can still say that one model has more support in the data than another model. The AIC criterion that we use tells you the relative level of support, which is what we were interested in, because we were looking in a retrospective way. It doesn't tell you the total proportion, the variation in productivity that is explained. And if we'd had time, we would have included the r-square in the program that we ran for this. We

did an output of the r-squareds for all the -- all the analysis. We had about three weeks from the time we got all the data to finish the report. So it was quite a crunchy time.

And so it's a good suggestion that Sean Cox made, and we could certainly go back later and do it.

Q Okay. Without those r-squared values being included, what we couldn't say, for example, is that -- well, we could say, for example, that Model 10, as you've done, describes -- which looks at marine variables, that it describes productivity better than variable -- models that look at variables in other life stages, but what we can't do is assess the percentage of the variation in the data that's actually explained by Model 10 or by any of the other models; that's what you're telling us?

A Yes, that's correct. I'll just note in passing, something that -- I looked at the Connors et al report, and although it was a very different kind of model, it didn't have as many covariates included in his long-term analysis, he had r-squareds around .7. He also included -- sea surface temperature also included spawners. So although we haven't calculated it, my suspicion is that we would have r-squared values in the same general ballpark.

Q Okay. And for the purposes of your analysis, which, as you said, was a retrospective analysis, you didn't think that r-squared values were important to have?

A It's not as critical if you're not attempting -- if you're attempting to create a model which you hope to apply prospectively; that is, to make predictions in the future, it's not as critical to have it. There's also some limitations in r-squared, because the r-squared doesn't take into account the number of parameters used to fit a model. There is an adjusted r-squared measure that does take that into account. So the AIC measure was, I think, adequate for what the purposes that we wanted to use, looking retrospectively.

I'll just point out that if you had a set of models and all of them had the same number of parameters and you used both r-squared and AIC,

it's likely that the rank order of those models would be similar, because AIC is considering to what degree the model variables explain the variation in the data.

Q All right. But couldn't say that without -- without the r-squared value, you couldn't say that the marine explains 70 percent --

A Correct.

Q So it could be better than the other ones, but it could be that the other ones are ones and marine is a two?

A No, that's right, you don't know the absolute proportions, yeah.

Q Okay. Then moving on, I'm running out of time, so I want to just go quickly through a couple things. On page 100, Table 4.8-1 sets out other factors which could potentially contribute, and you've set them out clearly, but I wonder if harmful algal blooms should be added to that list? Sorry, Mr. Lunn, if you can just keep moving it forward? Okay, there we are.

A Yes, it should be.

Q Then the last section I'd like to go to are the conclusions and some of the recommendations that are set out. So at page 104, at the top right, at the -- talking about what happened at the PSC Panel. It says:

> There was consensus among the group that a focused oceanographic and fisheries research program targeting the Georgia Strait, Queen Charlotte Sound and extending along the continental shelf to the Alaska border would [useful].

Have you given any thinking to who should participate in such a thing and how it would be structured or who would be responsible to organize or fund it?

A So that question wasn't really part of our terms of reference, but I think the first thing would be to clearly set out the objectives for the research groups, so what decisions are you hoping to inform, what level of accuracy and precision is required for those decisions, and what's the level of -- what are the scientific questions that helped to inform those decisions. So rather than

just, you know, going out and doing a bunch of research.

Logically, I think it would be led by the federal agencies responsible for Pacific salmon, so that would include DFO, NOAA Fisheries, Pacific Salmon Commission - I guess that's an international agency - and then they would get data and have participation from a whole bunch of others, so leading researchers, international organizations, like PICES, Alaska, Washington, Oregon, Idaho state fisheries agencies, First Nations, NGOs, provincial agencies, fish farmers. But it would be led, I think, by those federal agencies. That's just my, you know, off-the-cuff thinking on this.

Q And then you also talk about database improvements being needed and being identified as a problem among the researchers. What's needed, and how would it be accomplished?

A So I think the first thing is to have excellent data on Fraser River sockeye and non Fraser River sockeye productivity and stressors and to know exactly where those data came from, and then to design a database that way so that it facilitates answering the specific questions and making the specific decisions that I described earlier.

Q And who would maintain such a database and fund the effort?

A Logically, I think it would be the same agencies, the federal agencies that I described earlier. They're the ones who have the most data and they would get other datasets from other people.

Q All right. And in preparing your report, Technical Report 6, you actually created a fairly extensive database; is that right?

A Yes.

Q And is that database useful for future analysis?

A Yes, I think so. It was done for internal use, so it doesn't have a users guide and all the other things that one would want to have before it were made public. And it also would be good to add some of the stressor variables for the non Fraser stocks, but I think it's a reasonable start.

Q Okay. And then you have a set of recommendations set out beginning in -- or set out in a table, Table 5.2-1. It's on page 108, or, sorry, it begins, and in each of these -- in the column

called Comments and recommended research and monitoring activities, we see various recommendations being bolded. What is that? Why are you bolding things? What does that indicate?

A So I think we had, I don't know, was it 24 or something recommendations, something like that, and we thought it was important to highlight those which were relatively more important, recognizing that the final prioritization is something that would have to go through a quite extensive process of thinking about the decisions that need to be made and inputs to those decisions required, precision of information for those decisions.

Notwithstanding that, we looked at this and you see there's two columns there; one on explanatory importance, and one on relevance to management actions. So explanatory importance means what's the relative ability of information within each of those rows, each of those life history stages, to explain what's going on. And relevance to management actions is, well, how much would that information be used for actually making decisions on, say, harvest, habitat, hatcheries, hydro. And so we basically used those columns as a guideline for bolding certain portions.

So if you go down a little further, to the section on coastal migration, everything's bolded, because we have, from our work and from the work done by the various technical reports, concluded that that has a -- the coastal migration phase has a high level of explanatory importance. It's also highly relevant to management actions.

Q And do you have any suggestions to throw out there on how to prioritize all these different recommendations?

A So if you could go, Mr. Lunn, to page 107 down there, the questions there. So I think I've already described these, and so in the interests of time it may be easier for folks to read this than for me to read it. I think the key things are, what are the decisions, what are the inputs to those decisions, and how much information do you need?

So if you think about preseason forecast, how much precision do we want to have on that? Do we want to be able to say, "Well, things are likely to be relatively poor, average, or relatively

good," or do we want to be much more precise than that? That affects how much information you need.

And then it's also sequencing the efforts. So having certain rules, so sort of contingent rules, if we learn something then we might need to do something else. And also, you know, what are the cost-effective tradeoffs. So, for example, you need to consider how much budget you have, and you might not want to sacrifice your in-season monitoring programs to get better preseason estimates, for example. So there are tradeoffs there that have to be carefully considered.

MS. BAKER: Thank you. Those are my questions, Mr. Commissioner. So Canada will be first, and they've got a 30-minute allocation.

MR. TIMBERG: Yes, Mr. Commissioner, it's Tim Timberg, and Charles Fugère for participant Government of Canada.

CROSS-EXAMINATION BY MR. TIMBERG:

Q I'd like to start, Dr. (sic) Marmorek, with your role as a facilitator at the June 2010 Pacific Salmon Commission Workshop. And if we could have Exhibit 73 brought up. Thank you. If you could just look at the title page there, I note your name's there under "Prepared by". It says, Dr. Peterman and then yourself. Can you explain for us what your role was at the Pacific Salmon Commission Workshop?

A Sure. We were working with both the Pacific Salmon Commission, Department of Fisheries and Oceans and NOAA Fisheries, a committee, to design the workshop and -- so that committee met and discussed which hypotheses should be included. We facilitated those discussions prior to the workshop. We developed some forms, which are included in this report, for participants to comment on, evidence they felt was relevant.

At the workshop, itself, we served largely as timekeepers, and subsequent to the workshop the panel met, led by Randall Peterman, and assembled the main portions of the report, and we worked with the Panel and with Dr. Peterman to help pull all that together. There were various further conference calls with the Panel on key points that we helped to facilitate.

Q Okay. Thank you. And did you, personally, contribute any science research, yourself, to the workshop, or were you just a facilitator?
A I wouldn't say "just" a facilitator. Yes, we --
Q Fair enough.
A We did not present any independent research, and we worked to integrate the information that was presented. For example, the table that's in this report of evidence for and against, I prepared, which was based entirely on what the Panel had written, but it was just a summary.
Q Okay. Thank you. If we could then turn, to refresh our memories, we've got, at page 4 of this document, we've got the nine hypotheses. And the Panel, at the top of page 5, if we could look at that top paragraph there, so here this is under the, I guess the executive summary. It says the Panel -- I'm just reading from the document:

> The Panel concluded that the available evidence for and against each of the nine hypotheses does ***not*** point to a single cause of either the poor adult returns of Fraser River sockeye in 2009 or the long-term decrease in returns per spawner.

Do you still agree with statement?
A Yes, I do.
Q Okay. So we're not looking for one -- there's not one hypotheses out there, then, that explains what we've been seeing?
A That is correct, though I think it's fair to say that some are more likely than others to be primary causes.
Q Right. Thank you. And then continuing on with that executive summary, it says:

> Instead, the evidence suggests that multiple causal mechanisms very likely operate simultaneously and that their effects may be additive multiplicative (i.e. synergistic), or may tend to offset one another's effects.

And do you still agree with that?
A Yes, I do.
Q Thank you. And if we could then turn back to your -- and then at page 9, just to refresh our

memories, is the Table E-1, where the Pacific Salmon Commission set out their various hypotheses. If we could turn, then, to your report, exhibit -- at page 36, Exhibit 1896. Sorry, I'm at the last page. It's the last -- the second-last page of the executive summary. The pages are not numbered, unfortunately, Mr. Lunn. It's right at the very beginning. It's a section that's titled, Recommendations for Research, Monitoring and Synthesis. There we go.

And here I'm just noting that under this section you start off by saying:

> Researches at the Cohen Commission workshop agreed with the [Pacific Salmon Commission] report,

and it goes on, and that's really part of your conclusion with your report, now, they're consistent? I guess that's my main question is: Would you agree that both the Pacific Salmon Commission report from June 2010 and your paper have very similar conclusions?

A Yes, I would. I think the only distinction is that we had more information, particularly on non Fraser stocks, and also had some more information on marine conditions which slightly changed but didn't radically change the conclusions.

Q Thank you. And the membership at the Pacific Salmon Commission workshop and the scientists that worked on the -- your ESSA report, you'll agree that those are different scientists, despite the fact that you were at the Pacific Salmon Commission, the scientists at the Pacific Salmon Commission were different than the people who worked on the ESSA report?

A Yes, they are different. However, I think it's important to note that our report is a synthesis of the technical reports done by all of the Cohen Commission researchers. So I actually think of our team is including all the people who worked on those reports as well.

Q Right. And so you'll agree that despite the different scientists that were involved, they came to a very similar outcome?

A Yes. I think there's, as you would expect them on scientists, you know, some interesting arguments

between was Queen Charlotte Sound or Strait of Georgia more important in 2007, but in general people agree.

Q Okay. Thank you. I'd like to now just ask some questions about the stage 1 of your report, and I've got just a couple of questions. We'll just walk through the lifecycles, as you've put them out.

If we could move to -- back in the two pages, Mr. Lunn, to the start of the executive summary, Stage 1. One more page, please. Thank you. So here, with respect to Stage 1, you state that -- basically, you say that climate change is a possible factor with respect to the causes of decline for Stage 1, and you talk about climate change throughout the report. So I'm just wondering, what's your definition of climate change? What do you mean when you say climate change is a possible factor?

A Okay, so there's two questions there. So my definition of climate change would be the increase in greenhouse gases and associated changes in both temperatures and circulation in the ocean and other factors driven by that increased amount of heat in the atmosphere. And here we were following or synthesizing the work that Scott Hinch and Eduardo Martins had done in their climate change report in which they noted that temperature changes could have both positive and negative effects on incubation emergence in freshwater rearing. And so we carried that through as a possible factor. It had exposure in the sense that temperatures have been shown to be increasing, certainly in the Fraser, and other data showed temperature increases in many tributaries, so it remained a possible factor.

Q So when you say "climate change", are you seeing that as increased variability, or are you seeing that as an increased general temperature rising?

A Certainly both occur. With climate change in this context, for this specific section, we were relating it more to increases in temperature, because that's what Scott Hinch and Eduardo Martins had referred to.

Q All right. Would you agree that climate change manifests itself in a variety of factors, then?

A Absolutely.

Q Okay. And would you agree that climate change is not a mechanism, per se?
A A mechanism of causing mortality, is that what you mean? Or a mechanism for what?
Q Well, it's not a separate factor onto itself, that it's a combination of factors come together, when we think about climate change?
A I guess I'd describe climate change as a driving force which can ultimately affect sockeye through many different mechanisms in many different life history stages, if that's what you meant. That's certainly how I would describe it.
Q Okay. Thank you. And how is climate change, then, different from changing marine conditions?
A Well, first of all, climate change can also occur in the freshwater part of the lifecycle, not only in the marine system. So the various mechanisms that are discussed in the climate change report include both changes in freshwater as well as changes in marine systems, so Hinch and Martins talk about, for example, in the marine side climate change, changes in temperature can effect food conditions, can effect prey, can effect predators, can effect competitors. Obviously, marine conditions are strictly in the marine. They're going to be influenced by climate change, but climate change is larger than that, so changes in hydrology, for example, over freshwater, as well as changes in temperature, incidents of extreme weather conditions. Those are all climate change mechanisms which would not occur in the marine system.
Q Thank you. So then, with respect to Stage 1, you've -- going back to your executive report, you say that we feel -- and I'm reading from your report, that:

> We feel...confident in this conclusion because juvenile productivity...has not declined over...eight of the nine Fraser sockeye stocks where it has been measured.

So can you explain what you mean by that statement there?
A So in nine of the stocks we have some measure of juveniles per spawner. In seven of those stocks, it's fry per spawner; in two of them it's smolts

per spawner. And in eight of the nine, and those data are listed at the back of the PSC report, there hasn't been any trend over time in juvenile productivity, either an increase or a decrease. And in the nine, which I believe is the Gates sockeye stock, there has been an indication of some decline.

Q All right. And so based on that, then, you conclude that you can eliminate a whole suite of stressors as being a likely cause of decline as a result of this conclusion, right? And then you list these factors that we see before us: forestry; mining; large hydro; small hydro, et cetera?

A No, that's -- what you stated isn't the way we stated it. We looked at a whole bunch of evidence, particularly from freshwater -- pardon me, the Technical Report 3, in order to draw the conclusion that these habitat factors were unlikely to be the primary drivers. It wasn't only the juveniles per spawner data. There's a whole bunch of analyses in Nelitz et al, about the cumulative stress factors. And secondly, we're not eliminating it as a potential contributing factor; we're saying it's unlikely to be a primary factor driving the overall declines. It could still be a contributing factor in some watersheds, to some stocks, in some years.

Q Right. So if I understand your analysis, then, you're really focusing on juvenile productivity, but I'm wondering if you'll agree that there may also be a problem with juvenile viability, and by "juvenile viability" I mean the health broadly defined of the juvenile fish?

A That's certainly possible. If we had disease data from juvenile stages through smolt stages through adult stages, we might be able to test that hypothesis as well as condition information. We don't have that information.

Q Right. But you'll agree that it's important to study both productivity and viability; they're two separate ways of trying to understand the health of --

A Yes, I think that is important, and it's in our recommended research and monitoring in section 5.2.

Q Great. Thank you. I'd like to now move to

Canada's list of documents, Tab 2. We've listed a paper here by Petrosky and Schaller, and I note you've quoted it at page 45 of your expert report, titled, Influence of River Conditions During Seaward Migration and Ocean Conditions. Are you aware of this paper?

A Yes, I am.

Q All right. And as I understand it, this paper is about the influence of freshwater factors, primarily dams, as you talked earlier, in the Columbia River, having a delayed mortality impact on the fish when they entered the marine waters; is that a fair summary?

A It's a consideration of both freshwater factors as well as ocean conditions effecting survival rates.

MR. TIMBERG: Okay. Thank you. If this could be marked as the next exhibit.

THE REGISTRAR: Exhibit 1903.

EXHIBIT 1903: Influence of river conditions during seaward migration and ocean conditions on survival rates of Snake River Chinook salmon and steelhead, by C.E. Petrosky and H.A. Schaller

MR. TIMBERG:

Q And at the top of page 522, which is, I think, I believe the next -- I'm not sure of the page. Mr. Lunn, if you could go to the bottom, just it's 522 at the top. So, right, so the top right-hand corner, Mr. Lunn, if we could blow up that section there.

There's a quote here from the authors that I thought I would ask your opinion about. At the top right-hand corner it says:

> The NPCC noted that while we cannot control the ocean, we can monitor ocean conditions and related salmon survival and take actions to improve the likelihood that Columbia River Basin salmon can survive varying ocean conditions.

Would you agree that this statement can apply equally for the Fraser River for sockeye salmon?

A Yes, I would, although I would caution that if conditions in the ocean are really bad, the

ability to improve the likelihood may be difficult.

Q Okay. And then the next paragraph down starts with:

> Recruitment success in the ocean environment is generally believed to occur largely during the first critical months at sea...

and do you agree with that statement?

A Yes, I think that's true for most Pacific salmon.

Q Thank you. And then over the page, it states, at the top, left-hand corner, over the page, if we could look there, the second sentence says:

> First year ocean survival reflects the influence of near shore and broad scale environmental conditions, but may also be influenced by the condition of fish when they reach saltwater due to experiences in an earlier life stage.

And I presume you would agree with that?

A Yes, I would.

Q Thank you. And so this concept of delayed mortality or delayed effect, that's something we should be aware of when we're considering Fraser River sockeye salmon?

A Yes, we should.

Q And going back to my question about the importance of monitoring juvenile viability along with juvenile productivity, can you comment on what your suggestions are with respect to how to monitor juvenile viability; what should be done for that?

A Well, if we could go to our report on page 108, so the first row of that table says, for parental spawning success and incubation:

> Although an unlikely explanation of past declines, spawning success and incubation could relate to disease concerns and/or become higher priority in the future with climate change.

And then, as you follow down through there, there's various suggestions on how to monitor.

So, for example, suggestion number 4 says:

> assessments of freshwater smolt production and health for a strategically selected cross-section of stocks.

Q All right. That's helpful. So I understand there's sometimes a debate on where to put resources. Should resources be put towards counting fish at their different locations to capture their different life stations, or should debates -- or should resources be placed to study fish health to understand the health of the fish? What's your thought on those two monitoring approaches?

A I think the first thing you have to decide is what questions you're trying to answer and what decisions you're trying to make, and then, what are the inputs to those decisions, and then decide how to collect that information.

So, you know, if you're trying to make a decision on harvest, you're going to want very specific information on the expected returns, both prior to and within the season. If you're trying to make decisions on what's the long-term future of sockeye and what's happening to them, you need a more comprehensive understanding.

So my way of thinking about this is that - and it's not just my way, it's the common way of looking at things - is you first try to identify where the bottlenecks are, so where are -- where is survival declining, within which life history stage, and then look within that to try to define what the stressors are there.

So first understand the survival and condition within each life history stage, and then look within that -- those where it appears that there are problems to understand the mechanisms and stressors.

Q And so in doing that, then you would want to look at both sides, you'd want to look at the health and at the productivity?

A Yes.

Q With respect to -- I'd like, now, to have a brief conversation on pathogens. And you've stated that, in your evidence this morning and in your paper, that it's not possible to reach a

conclusion on pathogens due to data gaps; is that --

A That's correct.

Q And I'd like to compare what this report says to the Pacific Salmon Commission report. If we could go to page 5 of the Pacific Salmon Commission, which is Exhibit 73, again. And page 5, and it's the middle paragraph of the square box, and again, we're still in the executive summary. And it says, in the middle paragraph there:

> From the available evidence, the Panel also deduced that freshwater and marine pathogens (that is, viruses, bacteria, and/or parasites) are an important contributor to both the poor returns in 2009 and the long-term decrease in productivity, but again, data did not permit distinguishing further... It is conceivable that pathogens picked up in fresh water did not cause mortality until the ocean life stage. The Panel members' views on pathogens ranged from a *very likely contributor* to a *possible contributor* to the Fraser sockeye situation... Panel members believe that diseases caused by these pathogens are likely made worse by natural and anthropogenic stressors.

So would you agree that the Pacific Salmon Commission's conclusion on pathogens are considerably stronger than your paper's conclusions?

A I'd say they're somewhat stronger. If you'll note in the table, or in the passage you just read, there was, by far, the widest range of uncertainty on pathogens within the Panel from "very likely" to "possible", and our job was primarily to synthesize the work done by the Cohen Commission authors, and Dr. Kent's report essentially said there are no data.

And so I would say that we followed a somewhat more rigorous and perhaps harsher decision tree than the Pacific Salmon Commission Panel did in deciding what conclusion we would come to, as I explained earlier to the counsel for the Cohen Commission in Table 3.3-3. So with respect to diseases, without data, we were not

able to draw a conclusion, and what I found in past areas is that the range of hypotheses and degrees of belief is very large until you actually get data.

Q Right. But I understand at the Pacific Salmon Commission that there was expert evidence there, that Dr. John Winton was present, and he's a renowned expert in the United States on pathogens, and so the Pacific Salmon Commission was able to rely on his expert opinion; is that not part of why this stronger statement was made?

A Yes, I think Dr. Winton provided his opinion on it. I would say that the Cohen Commission gave him an opportunity to give a more thorough and detailed look at diseases in the work that Dr. Kent had. I mean, Dr. Winton basically went to a workshop, listened to some presentations, and then had a couple of days to work on this. Dr. Kent had a lot more time to go through this a lot more systematically and write a more detailed report.

Q Okay. Thank you. I'd like to move on, now, to some questions about Stage 4. If we could go back to your expert report, page 91, and here you are quoting from McKinnel in the second paragraph about -- and you talked about that this morning, about:

> ...biologists rarely observe death by natural causes of Fraser River sockeye at sea.

Would you agree that in a good year we have approximately 90 to 95 percent marine water mortality for sockeye salmon?

A If you're talking about from the smolt stage to recruits, yeah, marine survival rates, in a good year, are probably four to six percent or something. So I'm taking the opposite point of view.

Q Sure.

A But it comes out to the same number.

Q Right. And in a bad year, we have 98 to 99 percent marine water mortality, or one to two percent survival?

A Yeah, or worse.

Q Right. And so we're searching for a cause for an additional plus or minus five percent mortality in the marine stage to explain poor years as compared

to good years?
A Yes, I think that's correct. In a paper, which I think one of the other participants will present earlier (sic) by Dr. Hyatt, shows that you can use temperature and salinity and El Niño versus La Niña events to actually make that discrimination between good and poor ocean survival, for some stocks.
Q Right. And you'll agree that we really don't know the cause of this high mortality rate in the -- of sockeye in the marine waters?
A Well, I think we can describe the plausible mechanisms. They get eaten by other things, they die of starvation. There are other reasons by which they die. As to exactly what kills them, no, we don't know that.
Q And you'll agree that dead salmon, the mortality, are rarely found and without the bodies you can't really do biopsies or study them to determine the cause?
A Yes, I'd agree with that. And even if you had the bodies, you don't necessarily know what killed them.
Q Right. And that's part of the difficulty in testing for diseases or contaminants, because we don't have the bodies to conduct studies?
A That's correct. Although you certainly could collect fish, for example, at the trap near Mission, and you could collect fish from the Strait of Georgia, and analyze contaminant body burdens and diseases. There's certainly a possibility of doing that.
MR. TIMBERG: Mr. Commissioner, I've been given an extra 10 minutes by my fellow participant, the B.C. -- the salmon farmers, so I'll continue through till 12:30, and I may have one question upon my return.
MS. BAKER: Sorry, I think you need to finish at 12:30. I don't think there will be any time after that.
MR. TIMBERG: Well, my friend has passed me a note saying I can continue, so I will speak to him at the lunch break and I reserve the opportunity to possibly have one question after lunch.
Q If we could go to page 105 of your report. Under the section 5.2.2 Synthesis of Recommendations, you state -- you're talking about your recommendations, now:

> ...5.2-1 is a synthesis of research and monitoring recommendations, based on the Pacific Salmon Commission report, discussions at the Cohen Commission workshop, the Commission's Technical reports, and this cumulative effects assessment.

So I'm just clarifying that you started off with looking at the conclusions of the Pacific Salmon Commission and then you layered the work of the multiple reports, and then your cumulative impact assessment; is that accurate?

A That's correct. And the workshop, as it states.

Q Yeah, okay. And earlier in direct from Ms. Baker, she asked you about -- you stated that you need excellent data -- you were talking about data improvement and you suggested that we need excellent data on Fraser River and non Fraser River sockeye. Perhaps you can clarify the importance of why we need data on non Fraser River sockeye and we always talk about just the Fraser River here, and I'm curious to hear your explanation.

A The ability to test these hypotheses of what caused declines in salmon, as you stated earlier, is very difficult to do directly, because you don't have autopsies and the like, and so you're relying on indirect evidence, and what you're looking for are contrasts. You're looking for contrasts in productivity across different stocks, some higher and some lower, and you're looking for contrasts in stressors, some higher and some lower.

And so the work on non Fraser stocks is very helpful for illuminating what's the pattern that we're trying to explain. For example, Columbia River Okanagan sockeye more specifically, returned in record numbers in 2009, whereas we know the Fraser did awful. And so looking at that helps to illuminate what could be the possible differences between them, in this case, going to a different part of the ocean, going around the west coast of Vancouver Island rather than Georgia Strait and Queen Charlotte Sound. So those kinds of contrasts are enormously valuable.

Q Thank you. Could we move to page 29 of your report? This is the Fraser River sockeye salmon

productivity chart. And if we could just look at that. And would you agree that for the period 1952 to about 1990, early 1990s, the average productivity was about approximately six adult fish returned for every spawner that spawned?

A Just eyeballing, it looks a little lower than six, but around there.

Q Okay. Thank you. If we could then move to Tab 8 of Canada's list of documents. This is Exhibit 1851. And what we've done is we've updated this chart with information from the Pacific Salmon Commission. Have you had a opportunity to see this before?

A Yes, I have, this week, along with 61 other documents.

Q All right. Last week Mike Bradford spoke about this document. Would you agree that in 2010, and this is certainly preliminary data for 2010/2011, that in 2010 the productivity returned to its historic average of approximately six adult returns per spawner?

A Yes.

Q And what do you make -- and then, in 2011 it's a bit lower, it's somewhere around four. But I'd suggest -- would you agree that in 2011 that's still within the low range of its historical productivity?

A Recognizing that those are our preliminary numbers, because you don't have the full age structure, that looks to be at the low end of the historical range, yes.

Q And what do you make of this increase in productivity in 2010 and 2011?

A Well, first of all, it's good news. Secondly, as we talk about in our report, in 2008, the conditions in the Gulf of Alaska, and this comes from Skip McKinnel's report, were the coolest they'd been in about 35 years, and that seems a reasonable explanation, since sea surface temperatures are strongly related to availability of food in particular, and other things, as to why the returns in 2010 are better.

As to why the returns in 2011 are better, we'll have to look back at conditions in 2009. I know there was, I think, a La Niña event which overlapped into 2009, so there was somewhat cooler conditions, at least over the winter and so on.

So there is that variability and, you know, we're dealt a nice hand in 2008 and a somewhat good hand in 2009, but it doesn't mean you'll get a good hand in the next game of poker.

Q Mm-hmm. And so that's looking at productivity. I'd like to have the same discussion about run size. So this is showing productivity. So in 2009 we have a very, very poor run size, but in 2010 we get a historic run size. And do we need a theory that can explain both low and high run sizes? How do we understand that.

A Well, if I could answer your question by going to a figure in our report on page 37 of our report? So the total return is going to be the number of spawners times the recruits per spawner. And so these four graphs are organized by brood year. And so in the top left panel you have the returns 2002 -- 1998, 2002, so then that would be also 2006 and 2010, which you can see are dominated by the red or Late Shuswap stock.

So 2010 had the benefit of both higher recruits per spawner, as you were just shown, like around six, but also had the benefit of a pretty large cycle year in the late Shuswap stock, and you can see that they're a lot lower, the red is a lot lower in the other brood years in the other three graphs. So the total return is just the number of spawners times recruits per spawner. It's just straight math.

And when you have more spawners, even if you have the same recruits per spawner, you're going to get more returns. And that's why you can see, you know, over those -- each of those big Adams year runs, you get, you know, more recruits.

MR. TIMBERG: Thank you. I note the time, Mr. Commissioner.

THE COMMISSIONER: Thank you very much, Mr. Timberg.

THE REGISTRAR: The hearing will now adjourn until 2:00 p.m.

(PROCEEDINGS ADJOURNED FOR NOON RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: The hearing will now resume.

MR. TIMBERG: I have one final question for you, Dr. Marmorek. And, for the record, it's Tim Timberg and Charles Fugère for the Government of Canada.

CROSS-EXAMINATION BY MR. TIMBERG, continuing:

Q If you could go to page 9, the last paragraph there. It just states that:

> The scope of the present cumulative effects analysis is limited to the scope of the Cohen Commission technical research projects as a whole. Our cumulative effects analysis has been conducted within the universe of the other technical projects and the data available from within those projects. This is not a cumulative effects study of Fraser River sockeye salmon within the broader realm of all available scientific literature, research and reports.

And so my question is: Just for clarity, you'll agree that this report does not consider the testimony and cross-examination of the authors of the other technical research projects that appeared before this Commissioner?

A It doesn't consider the testimony. It does consider their reports.

Q Yeah, thank you. It's just their reports?

A Yes.

MR. TIMBERG: Thank you. Those are all my questions. And for the record, I think I've used up 11 minutes of the time that was allotted to me.

MR. PROWSE: Mr. Commissioner, Cliff Prowse, for the Province of British Columbia, and I have, I think, 25 minutes left, and I think I'll be less than that.

CROSS-EXAMINATION BY MR. PROWSE:

Q Might I have Canada's document Tab 1, please. This is an operational policy statement addressing cumulative environmental effects under the ***Canadian Environmental Assessment Act***. Have you been involved in any projects that have involved environmental assessments under the ***Canadian Environmental Assessment Act***?

A The company, as a whole, has, and I'm involved with one right now which will be, but I haven't had a lot of experience with it.

Q All right. And you've been involved in other

kinds of environmental assessments in --
A Yes, I have.
Q -- other jurisdictions? And this is the practitioner's guide, so it's basically telling people what they have to do with respect to cumulative environmental effects if the ***CEAA*** is triggered, as I understand it. This kind of assessment of environmental effects is not -- this would be within, I think, what -- in the statement you just read, would be within the broader literature about environmental effects. This isn't the kind of literature that you've considered for purposes of this report?
A Well, we did look at how cumulative effects assessment is considered under ***CEAA*** when we were designing our approach to this retrospective ecological risk assessment. The key difference here is that cumulative effects assessment under ***CEAA*** is looking forward as a project and then you're looking at what other projects can be reasonably foreseen to also be occurring in the future that might interact with the effects of the project. So that's one difference.

The other difference is that the way this operational policy statement work is, it's centred on the project as opposed to centred on the valued ecosystem component which is, in this case, sockeye. So we're looking at all the different factors that can effect sockeye and addressing it from that point of view, which some of my colleagues, like Lorne Grieg, who was on this study, have argued there's a better way to actually do cumulative effects assessment.
Q All right. And are there any conceptual differences, apart from the ones you've mentioned, between the approach you've taken and the one that's embodied in this kind of an approach that are important for the Commissioner to know?
A I think it's fundamentally different, and the task here is to look retrospectively and say, "What are all the different factors," and how they interacted to have affected sockeye, as opposed to saying, "Here is one project and how could that project, going forward, affect a variety of different environmental components, including other possible projects that might occur?"

This particular practitioner's guide I don't

think is really relevant to what the task that the Cohen Commission had to do.

Q I'm going to get this muddled up, but there's a saying, I think, in the -- if you're approaching your financial planner, that past results aren't a guarantee of future performance.

A Mm-hmm.

Q How does that concept apply to what you're just saying, because aren't we in the Commission really concerned with future performance and that's the only reason why we're interested in the past results?

A Well, I think that's a good warning, both financially and biologically, in that the sequence of ocean years which may occur in the future could be quite different from what has occurred in the past. There are many alternative futures, whereas there's only one past. The actual question that all of the main authors of these reports were asked to look at is, What's the relative likelihood of these different stressors of having influenced the past.

Now, how you best manage fisheries going forward is really a good question. That's not a question that we were addressing, nor is it a question that most of the other reports were addressing, with the exception of the ones that were looking at fisheries management.

MR. PROWSE: Mr. Commissioner, for the record, could we mark this as the next exhibit, please.

THE REGISTRAR: Exhibit 1904.

EXHIBIT 1904: Addressing Cumulative Environmental Effects Under the ***Canadian Environmental Assessment Act***

MR. PROWSE:

Q So going forward in your report, you were asked to look at research projects going forward.

A Mm-hmm.

Q And so I think it's --

A Yeah, page 103, I think.

Q Yes. In particular, there's Table 5.2-1 at page 108, Mr. Lunn. This is in the current exhibit.

So in this report, you've selected recommendations broken down by life stage and then you bolded 12 of them. And you've also -- I guess

the first question is: You've talked about, I think, having an integrated approach to all this, so that you're doing research on everything. Maybe I've misstated that. Can you explain how you would select, going forward, the research areas that you've honed in on here?

A So, first of all, it's not up to me to make that decision. And in the testimony that I gave earlier to the Commission lawyer, I outlined on the previous page - Mr. Lunn, if we go to 107 - what I think the criteria would be for further prioritizing this list. So those four questions that you see up there on page 107 are the -- represent the process that we would recommend using and the process that we've used in other projects for trying to prioritize.

Q All right. And you referred to the -- some EPA data quality objectives processes, which I take it you've utilized in other projects?

A Yes, going back to the 1980s and acid rain and many other applications since then.

Q And does the EPA have an overarching integrated scheme for their projects, or is this something -- is this really a tool that you use on specific projects within the EPA realm?

A This is more used within specific projects within the Environmental Protection Agency. That was how it was originally designed. So if you wanted to, for example, design if a river were polluted or not, that you would lay out what your decision criteria were for making that decision where all the inputs were and then design a study accordingly to fit that.

That having been said, we found it applies quite well to broader situations where you have multiple potential research and monitoring projects that you might do, and by asking the question, "What are the decisions you really want to make and what are the inputs of those decisions," certain things rise to the top as being more important than others.

Q All right. So in this, I guess, within the world of the Cohen Inquiry, then, the participants can make recommendations about how this approach would be applied and the Commissioner may choose to adopt this system or some other system or adopt these recommendations, and those would all be made

as a recommendation to the Government of Canada in this instance. Have you considered or written about different options for decision-making as to who would make these overall arching (sic) decisions? For example, there's -- some people refer to a forest - sorry, not a forest - I think a fisheries research board that I think is a past entity that had some coordinating function, but have you written on that topic as to how do you decide who should make that decision?

A No, I haven't written specifically on that topic. What I found in other projects with similar challenges is that the best way to make those decisions is to have a good dialogue between the managers who need the information and the scientists who produce it, so that the managers keep the scientists relevant to their real decision needs and the scientists keep the managers realistic with respect to what can or cannot be answered by science. So I really believe that it's a mix of the two that's required.

Q Okay. I'm going to digress from this point, simply to ask: The subject of contaminant has come up in the hearing and this morning you told Ms. Baker, a few times, that -- your comments were you've written about contaminants as based on known contaminants, as opposed to the unknown or emerging contaminants and endocrine disrupters, where simply they're not known, so you haven't considered them in your report; is that a fair statement?

A That's a fair statement.

Q I understand, from your resume, that you were involved with pulp mill standards, going back 20 years ago or so, 15 or 20 years ago; is that right?

A We weren't involved in development standards. We worked with Environment Canada and the B.C. Ministry of Environment on looking at the fate and effects of pulp mill effluents in the Fraser Basin. It was more attempting to decide what were research and monitoring priorities, and some modelling was involved with a professor from Simon Fraser University.

Q So you may or may not have a view on this, but my question is: How do we understand the evidence

we've had in this inquiry from Mr. MacDonald and others about that it should be an important project to look at, endocrine disrupters and contaminants of emerging concern, as opposed to looking backwards to the studies that you would have been involved with at that time, or the Governments were involved with at that time, about pulp mills, which I think largely succeeded in cleaning up the matters that were -- the matter of concern. Is this just a generational iterative process that, in the future it's always good to do an update and look at these things, or how would you explain that?

A Well, I think new stressors emerge and it's a good idea to do some studies to assess, "Is this a big problem or a small problem?". I don't think it should be that hard to collect some smolts at the outlet of the Fraser and examine them for contaminant burdens and get better estimates of exposure, and then do a kind of screening assessment on how large or small the problem is.

One thing I'll note in context here, and it comes back to the comments I made earlier to Ms. Baker about the pattern, is that you do, unfortunately, see these productivity declines in a lot of sockeye stocks that are from essentially pristine watersheds in the central and northern parts of B.C. and southeast Alaska. So if it were purely contaminants that were a driving factor, whatever they were, you would wonder why, in pretty uncontaminated systems, you would also be having those declines.

Q All right. Within your recommendations, the problem I want to pose to you is: We are probably living in a world of short governmental dollars, so there, I think there was, evidence in the spring that there was an anticipated five percent budgetary cut across the board, including the Department of Fisheries and Oceans. So in that context, it would seem to me that if you were able to say, "Here's the top three projects, and if we could do just these top three projects we would really come to a breakthrough in our understanding of the problems," that that would be a nice position for us all to be in. But I take it, from your table, that you think that we're in a world where there's many different possible causes

working in different combinations, different from year to year, and that a comprehensive approach seems to be what you're recommending here?

A Well, I think you also have to recognize, when you start talking about future budget cuts, there's been a huge number of historical budget cuts, and so if you say to the Department of Fisheries and Oceans, "Well, we'd like you to be able to answer all these questions when things go wrong, but we're not going to actually give you any money to do it," I don't think that's a fair way of approaching it. So my approach to this, or our team's approach to this was to say, "Here is what we think would be required to answer both management and scientific question adequately."

Now, if there aren't enough resources to do that, I think there needs to be a very careful consideration by a lot of people, as I outlined in what you see on page 107, as to what the trade-offs would be. And so I'm not going to throw out my best three guesses off the top of my head as to what would be the best, because I don't think that's the right way to make that decision. I think you'd really have to systematically analyze, what are the benefits and costs of doing each of these things and of not doing them, what are the risks of not doing them.

Q In that context, I noted in your resume that you had had some involvement with Carnation Creek research that has been done by Peter Tschaplinski and others.

A Mm-hmm.

Q Is that an example of a long-term project that has some messages about importance for understanding problems going forward?

A Well, they're looking at effects of forest harvesting on salmon and it's been a very valuable project with a long history. I think that there's always a compromise between a very intensive look at one watershed, like Carnation Creek, versus an extensive look at a bunch of watersheds, and I think you need both of those kinds of studies, because if you just look at one place, it's not going to be representative of all those places. SO the 64 stocks that Peterman and Dorner looked at give us a very broad look, but not very deep, and whereas the work that, say, is done on

Carnation Creek or on the Kehoe River by Bruce Ward, gives a much deeper look at, you know, a few places. You can't afford to do that everywhere, but I think you need both.

Q But, in particular, you need a long time series of data to --

A Correct.

Q So it's important to keep it going year after year?

A Absolutely.

Q I wanted to ask you, briefly, about work you've done for the Province of British Columbia -- that your firm has done for the Province of British Columbia to do with sensitive watersheds and a watershed evaluation tool. Can you just explain that to the Commissioner to the extent you're aware of it?

A Sure, I've been involved in that work. Going back to the '90s, there was something called the Forest Practices Code, and every watershed that was scheduled for harvesting had to go through a watershed assessment procedure. And then around 2000 they decided that they didn't want to make forest companies do that for every single watershed, so they developed this watershed evaluation tool which was basically a method of rating the relative sensitivity of different watersheds, and then those that had the most important fisheries or fish populations, and they could be either from a number of harvested fish or from an endangered species or a species at risk point of view, or were most vulnerable, because steep-sided watersheds and other geomorphic features made them more vulnerable to the forestry, that those would be designated as fisheries-sensitive watersheds, and so the Province has gone through and identified those.

And then we've been working with the Ministry of Environment at a sampling design and, instead of monitoring protocols that could be used both with remote sensing information as well as field protocols to try to assess the relative sensitivity and the current status of watersheds, both before and after forest harvesting, and some of those methods are just being tested in the field actually next week.

Q Thank you. Now, I think, judging by the exhibit

number of your resume, that you were probably a co-author on the Technical Report 3?
A That's correct.
Q And so I had a very high overview level, I recall that Katherine Wieckowski did an approach to the Wild Salmon Policy that was sort of a shortcut tool. I know I'm grossly oversimplifying. Do you recall that?
A Well, we'd have to get out Technical Report 3, if you actually want to look at it. But basically, she looked at a number of existing status assessments for the 36 conservation units and while that work was ongoing there was another report, I think the first was Pestal et al, and then there was another report by Grant et al, and so sort of compared the two and looked at how the two different status assessments turned out. And most of them didn't actually change very much. So I don't know what you mean by a shortcut. It was basically using the information that was available to provide some kind of status assessment for the 36 conservation units.
Q Have you been involved in work on the Wild Salmon Policy, apart from that -- apart from that report?
A We have done some work. We did some work for Pacific Fisheries Resource Conservation Council on the Wild Salmon Policy, looking at, I think they call it, other ecosystem values, so essentially other animals that benefit from eating salmon and what the implications are of different escapement policies that way.

We've also done work on the Wild Salmon Policy with the Department of Fisheries and Oceans on the habitat side, in terms of habitat indicators, and co-authored a document. I think Heather Stalberg was the first author on that.
Q All right. So Strategy 4, as I understand it, is intended to deal with some form of integrated management. Have you, first of all, done any work or writing on that? Probably you may have done that on the PFRCC document?
A No, we're not involved in Strategy 4. I don't remember right now all the numbers in the Wild Salmon Policy, but I think it was 2 and 3, rather than 4. One of my colleagues, Mark Nelitz, may have gotten involved in some of that work.
Q All right. Commission Counsel, this morning,

marked three documents, documents 1892/93/94, which were papers by Welch and Parsons and Blackbourn and MacKenzie and others; are you familiar with those?

A I'm just trying to remember which -- these are...?

Q These were miscellaneous papers. They --

A Are these the ones with the pesticide use, or --

Q No, no.

A No? Which ones?

Q This is on sea surface temperatures and --

A Oh, I'm sorry. Sorry, Welch and Parsons.

MS. BAKER: (Inaudible - off microphone).

MR. PROWSE: All right. All right.

A Sorry, no, I'm familiar with some of the work by David Welch, but I'm not familiar with the -- oh, sorry, these ones that were marked this morning? No, this is the first time I'd seen these.

MR. PROWSE: All right. I have no further questions, Mr. Commissioner.

MR. HOPKINS-UTTER: Good afternoon, Mr. Commissioner. Shane Hopkins-Utter, representing the B.C. Salmon Farmers Association this afternoon. And by my count, after Mr. Timberg had some of our time, I count that we have 20 minutes remaining.

CROSS-EXAMINATION BY MR. HOPKINS-UTTER:

Q Dr. Marmorek, today I'm going to ask you some questions about modelling, generally, and the first thing that I'd like to ask you about is the use of benchmarks. Mr. Lunn, if you could please pull up Tab 2. This is Technical Report 1896 at pdf 29, page 13. The second paragraph, beginning with, "Rocket science".

A Oh, I like this paragraph.

Q I do, too, which is why I wanted to ask you about rocket science being used as a benchmark. Maybe you could just start us off with that, because I think after a very long time in hearings and being lawyers, we've come to appreciate the complexity of this, but I think your words will capture it best.

A Well, I should mention that this particular paragraph had two completely opposite reactions to it. Randall Peterman asked if he could use it in an address to the American Fisheries Society, and Sean Cox said it was nonsense and should be

removed. So given that range of benchmarks for this benchmark paragraph, the point here is that there is a lot of variability, particularly in the ocean, where it's not well monitored and the interactions can vary from year to year, so it's extremely difficult for fisheries scientists to predict what the recruits per spawner are going to be from a given parent generation.

And so the metaphor here, and I think Sean Cox's objection was, "There's no place for metaphor in science writing," is that a rocket scientist would have to deal with continuing changes in the sort of ground conditions from year to year and the ability to monitor that variation.

I don't know if it's really a benchmark in this context for science, I just think it means there's a lot of difficult -- I should mention that Sean Cox said, in his review, that fisheries scientists were not smart enough to launch rockets, so that's the counter view here.

Q So in your opinion, would you prefer the weight of evidence leaning toward Peterman's assessment?

A Understandably.

Q Moving on, well, given the complexities that you've just spoken of, I understand that models are generally used to be a type of representation of reality. Is that something that you would agree with, a mathematical representation of a reality?

A It's a simplification of reality for the purposes of answering some question of interest. And the famous saying is that "All models are wrong and some are useful."

Q Well, you also noted, earlier, that the best model that you were able to come up with was, I think you used the word, surprisingly, was the one that actually included the most factors, so the most real world factors. Was that --

A Actually, no, that wasn't true. That was in one case. We went through several different analyses. That was true for the case where we were looking at combining or using all the Cohen Commission projects as away of organizing information. When we organized it by life history stage, actually, models which use less information proved to be more informative.

Q You also mentioned that, and I think it was in

that same context, the AIC can penalize for having too many variables. Is that one of the weaknesses of AIC?

A No, it's not a weakness at all; it's a strength. Historically, biologists tended to throw all the things they'd monitored into the computer and, you know, they'd run 100 variables and five of them would come out significant and they'd say, "Oh, these must be important," and that would happen just if you threw random numbers in. So the stress of the Burnham and Anderson approach, who prompted AIC in the 1990s, was to make very specific hypotheses and test only variables related to those hypotheses and to, in fact, penalize models which threw more variables in but didn't get any better explanation of the data.

So essentially you try to get the best possible explanation, in this case of sockeye productivity, with the least number of variables.

Q From my recollection, is that the r-squared you should be seeing some improvement as you go?

A The AIC measure is a measure which includes the likelihood of the data given the models and the measure of how many variables you use, so that's different from the r-squared that we talked about earlier.

Q Mr. Lunn, if you could pull up Tab 12. This is Spanos, and I know I'm going to mispronounce it, so I'll just us the AIC and the Reliability of Inference: Model Selection Versus Statistical Model Specification paper published in 2010.

Are you, in fact, aware that there are criticisms of the AIC, as evidenced in this paper?

A Yes. And since we received those, which was five o'clock Friday, we actually had Carl Schwarz, head of statistics at Simon Fraser University -- he was the chair of the Simon Fraser statistics department, look over this paper, and we also looked it over.

MR. HOPKINS-UTTER: Mr. Lunn, could we mark that as the next exhibit?

A Would you like me to comment on that paper?

Q Yes, yes, absolutely. Sorry. Before I forget.

A So the two points in the paper that Spanos et al -- Spanos raises here, the first is that AIC approaches:

> ...give rise to unreliable inferences, primarily because their choice within a [prescribed] family of models (a) assumes away the problem of model validation, and (b) ignores the relevant error probabilities.

Q So if those two faults are committed, then it will give rise to unreliable inferences; is that what the paper is saying?

A That's what the paper is saying, so I don't agree with it for a number of reasons. First of all, these AIC statistical methods have been used in the scientific literature over the last 15 and 20 years, and as described in Burnham and Anderson, 1998; and, secondly, the -- if you go through the process I just described earlier, whereby you define your hypotheses based on biological theory and examine the relative level of support for each model, using AIC and other criteria, goodness of fit measures of how well the model fits, examining residuals and doing other things, then it's a quite valid approach to do, and we did that, we applied it appropriately.

And I'll just say that while there are parts of that Spanos paper that Dr. Schwarz agreed with, there were other parts which he did not agree with at all.

Q Dr. Marmorek, I hate to interrupt, but I do have limited time, unfortunately, and I was just wondering if you were, in fact, aware of the criticisms that some others make. You have stated on the record that you disagree with them, however.

A Correct.

MS. BAKER: Mr. Commissioner, the witness was in the middle of an explanation. My friend put the article to him. The witness is allowed to explain whether he agrees with it or not agree with it. I mean, he wants to have this marked as an exhibit. He at least needs to allow the foundation to be met or not met.

THE COMMISSIONER: Carry on.

A I think the key point, here, is that if you carefully decide which hypotheses you're testing and then look at various diagnostic information, including AIC and other measures of goodness of

fit, that they're wholly appropriate, and that the criticisms outlined in this paper are not a problem.

MR. HOPKINS-UTTER: Thank you for your answer. Sorry for trying to interrupt.

Mr. Lunn, could you please take us to Exhibit 1896, Technical Report 6, at pdf 116 -- oh, I'm sorry, could we please mark that as an exhibit, Mr. Registrar.

THE REGISTRAR: Yes, that will be 1905.

MS. BAKER: Again, Mr. Commissioner, the witness actually did not adopt anything or accept anything in this journal article, so I'm not sure that it should be marked as an exhibit.

MR. HOPKINS-UTTER: Mr. Commissioner, I would suggest that, as has been the practice throughout these Commission hearings, I've asked questions of the witness on the paper, he recognized it, he acknowledged that he gave it, and he gave his opinion on it.

THE COMMISSIONER: Very well, we'll mark it as an exhibit, thank you.

THE REGISTRAR: So marked.

EXHIBIT 1905: Akaike-type Criteria and the Reliability of Inference: Model Selection Versus Statistical Model Specification, by Aris Spanos

MR. HOPKINS-UTTER: I'm sorry, what was the number for that?

THE REGISTRAR: 1904 -- or, I'm sorry, 1905.

MR. HOPKINS-UTTER: Thank you.

Q Dr. Marmorek, I note this table 4.8-1, it says:

> Other factors potentially contributing to the decline of Fraser River sockeye salmon that were not considered within the spectrum of Cohen Commission technical reports.

And the list includes competition with pink salmon, hatchery fish, increased predation. So is it not true that there are a number of other factors that potentially contributed to the decline that just weren't being considered in the Commission reports?

A Well, we mentioned one this morning of harmful

algal blooms, and my sense is - I'd have to check - that virtually all of the other factors that were considered by the Pacific Salmon workshop in June of 2006 -- sorry, 2010, are listed here. So in terms of primary factors that might be explaining the overall decline, I don't think there are any others. I welcome your suggestions.

Q I'm not the scientist, unfortunately. May we move to pdf 124, Mr. Lunn, of the same report. A little further down the page. Actually, at the bottom of the page, I believe. Thank you.

So this is the cell downstream migration to estuary. It reads, on the right-hand column:

> We do not know the survival rate of smolts during their downstream migration, or when they arrive in the Fraser estuary (vital to understanding potential mismatches between arrival times and marine plankton blooms).

And you give some recommended activities. The next page, Mr. Lunn. At the top, under numbers 7 and 8.

So is it true that the bold highlighting indicates what is a high priority in this table?

A Those are the ones which we thought were, if you had to divide them roughly in half, those were the ones that we thought would be in the -- at the higher level priority. Others might disagree.

Q And number 8 reads:

> Estimates of the size and health of smolts arriving in the Fraser estuary (e.g. pathogens, contaminant body burdens, lipid reserves),

That's not in bold text, is it?

A It's not in bold text, but it's still there, which means it's still something we think is worth doing.

Q And are you aware that on August 24th Dr. Miller - I'm going to paraphrase what she said - but Dr. Miller gave evidence that the signature or, sorry, the parvovirus may be coming from the freshwater environment due to the higher prevalence in smolts as they leave freshwater, entering the marine environment. Would that testimony suggest that

this number right here should be considered a higher priority?
A I think it goes back to deciding, as I said at least two or three times, what the decisions are you want to make and what are the most important inputs to those decisions, as I was just speaking to your colleague about the data quality objectives process. So the relative importance of genomic studies versus all these other things need to be examined in the context of what you're going to do with that information for what decisions.
Q Mr. Lunn, page 66 of this report -- I'm sorry, pdf 66, page 50. No, you actually had it on the screen. Under 4.3.1, the second sentence beginning with:

> Nelitz et al. (2011) point out that sockeye salmon smolts are cued to migrate towards the ocean in response to changing environmental conditions, which includes responding to day length, lake springtime temperatures...

I'll skip down:

> Earlier outmigration could lead to a mismatch between the arrival of salmon smolts in the Fraser estuary and Strait of Georgia, and the timing of plankton blooms that are essential for growth and survival in Stage 3 (coastal migration).

Do you agree with this assessment that climate change can, in fact, affect the timing of salmon outmigration and availability of plankton blooms?
A Yes, I do, and with the clarification that was mentioned earlier, that it could be that you arrive either too early or too late.
Q So one way or the other --
A Mm-hmm.
Q -- the smolts could be entering at a different time and the, I guess, the environment hasn't caught up, at that point, with their new timing?
A Or the environment has changed in a different direction. They both could be changing in different directions for different reasons.
Q Mr. Lunn, could you please take us to page 63, paper page 63 of this report, second paragraph

down. Dr. Marmorek, would you agree with the statement that you have here, the second sentence:

> This suggests that there is strong evidence for a direct impact of climate change on sockeye salmon.

A So I think earlier I was asked about the overlap between changes in marine conditions and changes due to climate, and I think the key, here, is that climate change can and has increased sea temperatures and is likely to increase them more so, and that changes in sea temperatures affect prey, predators, competitors, in various ways. So I think for here we're talking, now, about the coastal migration stage, which is why we concluded that this was a likely factor, both marine conditions and climate change.

Q Thank you. And on the next page, Mr. Lunn, page 64, under Conditions in Queen Charlotte Sound versus the Strait of Georgia, I'm just going to note, briefly, Peterman et al, that's the PSC report, Exhibit 73:

> ...concluded that it was "very likely" that physical and biological ocean conditions inside [Strait of Georgia] during this life stage had been a "major factor",

And they rated this as "likely" in contributing to poor ocean returns in 2009. And the Cohen Commission rated these similar conditions in the Strait of Georgia as being "likely". Would you agree that this, in fact, confirms that both of those workshops concluded that it was either very likely or likely that the Strait of Georgia played a role or overall for 2009 returns, that is, in fact, what this said?

A I think that's what the workshop said, and then I think the work that was done subsequently in the reports was a lot more thorough, in terms of the work by Dr. McKinnel, at looking at the conditions, actually, within both the Strait of Georgia and Queen Charlotte Sound. And there's other work that's been already provided by Dr. Beamish which provides further evidence.

So I think we got -- we made a lot of

progress, iteratively, amongst the different workshops, but also in the reports and articles.

Q So those environmental conditions, then, at play seems to be, from my non scientific background, a significant role?

A Correct.

Q And isn't it also true that the top-ranked model that you discuss at the bottom of page 65 was, in fact, for the Strait of Georgia? I'm sorry, "The analysis of this time period showed," -- I'm reading 65, Mr. Lunn, at the bottom:

> ...that there is support for both [Queen Charlotte Sound] and [Strait of Georgia] models – the top ranked model was for [Strait of Georgia], the second for [Queen Charlotte Sound], and the third was the global model, including both regions.

So that is in fact, true, that the Strait of Georgia was the top-ranked model for this particular model -- analysis, I'm sorry?

A Well, I think you have to put this model work in context, right? We took the data that was provided to us, and the main purpose, just as I said earlier, our main purpose was to see if we could disprove some of the hypotheses that came out of the Cohen Commission reports, particularly on marine conditions. So if it had turned out that marine conditions, for example, the variables that were in there had no support, whatsoever, in the productivity measures for sockeye, that would make us wonder, "Hmm, I wonder what's going on?" So the fact that it came out that both Strait of Georgia, on one case, and Queen Charlotte Sound were good at, let's say, relatively good at predicting sockeye salmon productivity and that we could not reject that hypothesis, it doesn't mean that these models are right. As I said earlier, all models are wrong, some are useful. This was useful in the sense that it allowed us to not throw away some of the conclusions that we'd already gleaned from the other Cohen Commission reports. It didn't contradict the conclusions we'd already came to by synthesizing the information in those reports.

Q And in my final minute, I would just like to ask

something that you just brought -- well, you just brought up. Why is it significant to attempt to disprove hypotheses rather than to prove theories?

A I think the -- it's the basic approach of the scientific method, which is that as long as events occur in a way that is consistent with a theory, you can't reject it. But if an event then came along that was contradictory to that theory, you would then be able to reject it. So if you went along and said, "Well, the last 10 years this seems to be correct, therefore it must be true," what would you do in year 11 when you found out you were wrong? So that's why it basically comes to the idea that you can only disprove hypotheses, and those which have failed to be disproven over a long period of time gradually become accepted.

MR. HOPKINS-UTTER: Thank you very much, Mr. Commissioner, those are my questions.

MR. McDADE: My name is Greg McDade, I'm counsel for the Aquaculture Coalition.

CROSS-EXAMINATION BY MR. McDADE:

Q Doctor (sic), you've probably had the most challenging job of the various report writers, as you had to put them all together.

A By the way, I appreciate everyone calling me "doctor", but it's really just "mister", so I thought you might want to correct that.

Q I'm sorry.

A My initials are "D.R."; my parents strategically named me that way, but...

Q Well, D.R., I think it's -- you've noted quite clearly in your addendum report, and that's where I'm going to focus, is I think that's Exhibit 1575, that the reports of Dr. Noakes and Dr. Gill were somewhat of an anomaly in this matter in that they were fairly significantly different from each other, they came to opposing conclusions. And I'm interested in how you attempted to resolve that matter. As I understand it from your report, you didn't really attempt to weigh those reports against each other or to choose who was right.

A Well, maybe we could go to -- Mr. Lunn, if we could go to page -- the Table 2 in page 18 and 19, I think that's informing this discussion. So we went through the decision tree, the retrospective

ecological risk assessment tree, and admittedly, neither Dill nor Noakes nor any of these authors had used that approach in organizing their reports. But we basically pulled out -- we mimed those reports to say, "Well, where do they differ and where are they similar?"

And so if you look down the mechanism column, you know, across waste, escapees, sea lice, disease - salmon origin, disease -- those are the four, really, they basically all agreed that there was a mechanism. Noakes did not agree that there was a plausible mechanism by which waste could affect sockeye salmon.

When it came to exposure, they agreed that there was unlikely to be exposure to a significant fraction of sockeye salmon for either waste or escapees. They disagreed on sea lice, and they disagreed on disease.

When it comes to correlation consistency, they felt there was no correlation or consistency for waste, escapees and sea lice; they all came out the same way, Noakes and Dill, but they disagreed on disease. So essentially, as you said, it wasn't our job to reconcile them. Our job was to look at what the implications would be if you used one report versus the other report for our overall conclusions.

So what it comes down to is they came to unlikely conclusions by somewhat different pathways in our decision tree, but nevertheless, the same conclusion and -- with the three first rows there, but in the last row, in disease, came to different conclusions. So we basically just carried -- said, "Well, if you accept Dill, then this is what you would conclude, and if you accept Noakes, this is what you can conclude."

Q And I'm going to focus, for the rest of my examination, on the question of disease.

A Okay.

Q And disease arising from fish farms. Now, if we go to page 14, the top -- sorry, one page earlier that that. Yes. So if we highlight the last half of the first paragraph there:

> ...Dill's [2011] examination of further evidence led him to believe that disease transfer from salmon farms is the most likely

mechanism of concern that could explain the negative correlation between salmon farm production and sockeye productivity described by Connors...

So we have Dr. Noakes saying one thing and Dr. Dill saying another. You don't evaluate who's more likely to be correct?

A That's correct. We do recommend that we actually get some disease data.

Q Yes.

A And actually, so did Noakes and Dill.

Q Yes. But if Dr. Dill is right, that disease transfer from salmon farms is a most likely explanation for the negative correlation between salmon farms and sockeye productivity, that may be the explanation for these long -- or at least one major explanation for the long-term decline in the productivity?

A I don't think Dill said that salmon farm production was the most likely factor causing the decline of sockeye productivity - we'd have to go to his report - but I don't believe he ever said that it was most likely. I think what he said is that of the various mechanism by which salmon farms might affect salmon productivity, disease is the most likely causal pathway. That's quite a big difference.

Q Yes. And that's part of what the challenge we have before us, is that in the absence of empirical evidence, or empirical proof of these pathways, you're left with plausible hypotheses or plausible mechanisms, right?

A Yes, that's correct.

Q And in the difficult task that the Commissioner faces here, I think it's fair to say that there's not a lot of empirical evidence about wild salmon catching disease, whether it's from farms or any other source, that's something that you've identified as that lack of empirical evidence?

A That's correct. We identified it as a gap and as a need to be filled.

Q Right. And so if one hasn't done any studies on these questions, one's not going to find any empirical evidence?

A By definition.

Q And as you also, I think, it's fair to say, it's

very, very difficult to actually prove the cause of disease in the wild population of fish, because the dead fish disappear before you could test it?

A I haven't actually said that. I think that if you went, and as we say in the conclusions to this report, if you went out and measured the incidents of diseases in areas close to or far away from fish farms or before and after sockeye pass fish farms, you would be able to get some useful information. The greater the level of contrast in exposure, the better.

And just as an addition to that, when the Okanagan First Nation was considering reintroducing sockeye into Skaha Lake, they went through three years of disease studies and had assistance from DFO and others and measuring diseases in Columbia River sockeye versus diseases in fish in Skaha Lake, and they found out there really wasn't much difference, and so they proceeded with that experiment.

So I think you can gather information on disease and make sensible decisions, if you get the data.

Q So it's possible to design studies that would show these links, if they're there?

A I think so.

Q And that's not really rocket science, is it? That's a pretty obvious way to do it, isn't it?

A It is good fishery science. Now, there are some wrinkles in that if you find a high instance of disease in a population and then you later find very low instance of disease in the population, as Michael Kent said, that could either be because the disease disappeared or because the fish that had the disease died. So it's tricky. And I think it's useful to use some of these acoustic tagging information, where it's possible to actually get a much better estimate of exposure.

Q But if I accept that evidence, that it's feasible to design studies that would tell us something about -- on an empirical level about disease, the plain and simple fact is, to the best of your knowledge, nobody's done those studies?

A To my knowledge, and also to Dr. Kent's knowledge, in his report.

Q Did you run across any information to suggest why someone wouldn't have done such an obvious study

over the 20 years that fish farms have been in place?
A Well, I know there have been studies done on pink salmon, and I also know that it's been a challenge to get collaboration amongst the various groups to undertake such studies.
Q And so let me just wonder aloud about the wisdom, perhaps, of going and looking for empirical evidence when no one's done the studies. That would be a pointless exercise, wouldn't it?
A I think it's worth looking for whatever evidence exists and doing -- making the best judgments that you can, given that evidence, and we were relying on Dr. Kent's summary of disease information to conclude that there wasn't much information and, therefore, that no conclusion was possible about disease. So we were assuming that he had scoured what was available.
Q Yes, and let's accept that as a given fact, that Dr. Kent is right, that no one has done any empirical studies and so it's not possible to find those kinds of empirical proof. That's a reasonable statement, isn't it?
A I think we're just repeating the same thing I just said, so...
Q Okay. Well, let me say this: In science, when you haven't done the studies that are necessary to establish the empirical connection, that doesn't mean the harm doesn't exist?
A Oh, that's correct, something could be happening that is not good for you or for the fish and we haven't done a study to detect it, yes.
Q Well, and science -- there's a body of science that talks about risk or likelihood of something existing that isn't based on direct empirical evidence, that's common in science, that you have evidentiary studies but you also have theoretical studies?
A There are theoretical studies, but in the absence of evidence there are many possible theories as to what's going on. I've seen many examples over my career where people argued vociferously over some particular parameter or mortality rate, and when they actually got the data there wasn't much to talk about anymore. So I think the simple answer is, go out and get the data, because otherwise there's a very wide range, just as we see here

between Dill and Noakes' conclusions, in the absence of information, and all there really was, was Connors' estimate of total farm production and Noakes questions to the degree to which that represents diseases, it's a very indirect indicator, it's still the only indicator that he had that was available to him. So I think the answer is to get the information.

Q So if there's no empirical evidence one way or the other, it would certainly be wrong to say that diseases coming from fish farms are not the cause of the 2009 sockeye decline? You simply have no proof of that at all, do you?

A Okay, so now you've asked a question with respect to one year's poor returns, namely 2009, and asking a question, if you don't have any empirical evidence, are you able to reject salmon farms as a cause of that decline? So I would argue that based on the fact that the difference between 2009 returns and 2010 returns was something like a 14 of 15-fold change in recruits per spawner, that it's pretty unlikely that there was a 14 or 15-fold change in the amounts of disease occurring between the 2009 returns and the 2010 returns. In other words, I would say it's pretty unlikely that the main cause of the variation between those two years was due to salmon farms is much more likely, as we've said in our report, that it was due to marine conditions, specifically temperatures and lack of circulation and the like.

This is not to say that salmon farms have had no effect. As we've said several times today, things which are not the primary factors responsible could still be contributing factors.

Q Well, and when one talks about cumulative impact, which is the title of your paper, if disease combined with bad ocean conditions causes mortality, that would be a direct effect, wouldn't it; it would be a cumulative effect?

A It's possible that disease and marine factors could combine. As for when we talked about disease earlier and because we said, "Well, because we have no data, no conclusion is possible," I think you still need to come back to that and say, "In the absence of actually having data on the exposure of sockeye to that stress, you're not able to draw a conclusion as to its

relative likelihood of being a primary factor."
Q So doesn't disease, by its nature, when it becomes epidemic, isn't it episodic?
A It can be. There are some other diseases which enter into populations and stay in those populations for long period of time. They don't necessarily always go up and down.
Q Yes, but it's not uncommon a disease can have a very significant effect in a given year and not so much in the year before and the year after?
A That can happen.
Q Yes.
A I agree.
Q So you can't really draw those conclusions even from 2009 and 2010, can you?
A So your argument, as I take it, is that in the absence of any disease information, but based on the fact that some diseases go up and down, that diseases could be responsible for the 15-fold fluctuation in recruits per spawner between 2009 returns and 2010 returns? I guess, in the absence of any information which would show one way or the other that there were massive outbreaks of diseases, you couldn't reject that. It seems unlikely, though, in that you would think that if there were massive outbreaks of diseases you would have heard something about it from the fish farmers and you would have read -- seen something about it in the database that has been collected, admittedly only for a very short period of time.
So I think looking at that data, which showed basically no trends in diseases, the work that -- the database that Korman -- Josh Korman put together --
Q Did you read the --
A -- becomes a bit of a stretch to say that the -- how likely it was that there was a sudden big disease that nobody detected.
Q Did you read the cross-examination of the Project 5 and Project 1 reports?
A Parts of it. It's pretty long. I didn't read all of it.
Q Were you aware that there were diseases found in that database that were -- that 60 percent of the time were identified as unknown or open?
A Yes, I read that portion.
Q That's a pretty significant fact, isn't it?

A Again, we're talking -- yeah, I believe that -- I think it was something like it went from two percent to five percent in the total number of fish, or something like that, wasn't it? So that seems a fairly small --
Q The question is --
A -- it seems a fairly small proportion, to me. That doesn't mean to say -- I don't think you can say anything about the amount of disease that existed prior to them monitoring for disease in fish farms, you know, so prior to 2002 we don't have a very good estimate, and I think that Brendan Connors did the best he could to use the salmon farm production as a proxy indicator, if you will, for disease.
Q So let me ask you this. In the debate between Dr. Noakes and Dr. -- or Dr. Noakes' criticisms of Dr. Connors --
A Oh yes.
Q -- methodology, you're familiar with that?
A Yes.
Q Where do you stand on that? Was Dr. Connors right or wrong?
A So the truth is somewhere in between. Some of the points that Noakes made, this is Exhibit 1538, which I looked at last week, was farm production used by Connors is not an adequate proxy variable for disease, and I agree that it would be much -- I agreed it would be much better if there were farm-specific levels of production and a much longer time series of disease, and also that he didn't have to aggregate the data to avoid -- for proprietary reasons. So I think it would be much better if there were more detailed information, but historical data doesn't exist. I think it was reasonable to use that as a proxy measure, just like sea surface temperature is a proxy measure for a bunch of other things, food production.
Some of the other criticisms, IHN was not detected prior to 2003. Well, we don't actually have good disease data prior to 2003. If we did, we'd use it. BKD was more of a problem for Pacific than Atlantic salmon. Well, Pacific salmon were mostly used before in the earlier time period, so it's not unreasonable to assume a proportionality between production and disease, if there were disease. We don't know that there were

disease. But it's not an unreasonable proxy indicator.

And then, I think there were some other criticisms. We said pink salmon may influence Fraser River sockeye salmon, although there is no strong evidence to support this assumption. Actually, there is. In the PSC report, Appendix C-E16, Greg Ruggerone's analysis is quite strong correlative evidence of pink salmon effects on Fraser sockeye. Noakes said that Connors did not account for density dependence because he didn't use residuals from the stock recruitment curves, but he did actually -- he didn't use the residuals, but he included spawners, so he does -- and all of his models include density dependent effects.

So all in all, I didn't think the criticisms from Noakes about Connors' work, rather, were sustainable. I think there are certainly weaknesses in the historical dataset, and it would be much better if there had been per farm production data and actual disease data going all the way back to the 1980s, but it didn't exist.

Q But there's nothing wrong with Dr. Connors' methodology, given the data he had to work with?

A I didn't see anything wrong. I thought he was quite careful in the way he went through his work.

Q So let me ask you about something -- I -- well, can I put Exhibit 1482 on the screen? Can you just blow up the abstract part of that?

There is a body of literature that's been introduced, some of it which has been introduced as exhibits into this Commission, which established that aquaculture facilities are, in theory, an ideal place for disease to generate and emerge. You're familiar with that body of literature and certain --

A I haven't seen this paper before. I have heard about those ideas. I'm not as familiar with the literature as either Dr. Noakes or Dr. Dill are.

Q Well, as you point out, Dr. Noakes didn't really look to this body of literature, and Dr. Dill did.

A What we pointed out was that there were only 25 references amongst the 250 that they had in common, and we also recommended that some independent scientists actually work on reviewing all this literature.

Q In the absence of empirical evidence, because the studies just haven't been done, if you have evidence from other places and you have theoretical evidence and plausible hypothesis upon a biological level that fish farms are likely to be ideal breeding grounds for disease, shouldn't that be relevant in assessing the risk? Isn't Dr. Dill right to refer to that kind of literature?
A I think it's reasonable to look at that other literature in terms of assessing the risk. In terms of evaluating how large that risk is, as I said earlier, until you have data, the range of tangible hypotheses is really large. So I don't think it's that difficult to collect that data and, therefore, rather than making inferences entirely based on evidence from other places, I think it would actually make sense to get the data.
Q But in the meantime, until you've done some empirical studies, if you have scientific evidence that a particular activity is potentially harmful, how do you take that into account in terms of whether, for instance, how to site -- whether to site fish farms in the middle of wild salmon migration routes? How does one evaluate risk in the absence of empirical evidence?
A Well, I think that you try to use information on past locations, in this case fish farms, and observe what has happened to animals moving past them. You try to gather all the information that you can and make your best judgment. And I think that, you know, some of that's what the Cohen Commission's doing overall here, is trying to make their best judgment based on incomplete information on a number of factors, including fish farms.
Q So, I mean, to be -- to use a metaphor, if you have an explosives factory that hasn't blown up for three or four years but creates a risk, does it make sense to site it in downtown? Or would you send your children to a school next to an explosives factory? Isn't risk a factor to be considered, even though you lack empirical evidence, and isn't that what Dr. Dill was doing?
A Well, I think there's pretty strong empirical evidence that explosives explode, and I don't think there's quite as strong empirical evidence

that --
Q That fish farms cause disease?
A -- that fish farms have caused disease in sockeye salmon, and so I think it's reasonable to combine what evidence you have and make your best judgments, just as Dr. Dill did, and just as Dr. Noakes did, as well. They made different judgments, you know, based on the evidence they looked at. I think it would be valuable to have other independent scientists look at it. I still would argue that, you know, if it took you 10 years to get this information, okay, maybe you can make a judgment now, but if it takes you one year to get the information, why not just go out and do it?
MR. McDADE: Mr. Commissioner, I note the time. I have about four minutes left. We can either take the break now or --
THE COMMISSIONER: Carry on.
MR. McDADE: What's that? Continue?
THE COMMISSIONER: Carry on.
MR. McDADE:
Q Now, let me just change gears for a second, because I was struck by the logical wisdom by what you said when you said it makes sense to look for the bottlenecks, in terms of the life history stage, and then look for the stressors within that particular bottleneck.
Now, if we were to apply that approach here, am I correct in hearing you that one bottleneck you had identified is the early marine stage, or the coastal migration stage in terms of the life history stage?
A Yes.
Q And when one looks at that bottleneck and the stressors that have -- are new in the environment since this long-term productivity decline in 1992, I hear your report talking about climate change or marine conditions as one stressor that may have changed. Are there any others that shout out at you on the coastal migration phase?
A Well, clearly, fish farms are one candidate stressor and they were included in our conceptual model. I wonder if I could quickly get, Mr. Lunn, if you could go to page 34 in our report. Just by way of answering this question, I think it's really important, as I said at the beginning, to

think about the overall pattern that it is we're trying to explain, and it's not only the pattern of decline in the Fraser stocks, these are non Fraser stocks.

Now, they've also, if you look at the Southeast Alaska stocks and you look at the Yakutat stocks and you look at the Central Coast stocks, which have very minimal exposure to fish farms, they've also shown declines. So this isn't to say that fish farms could not have effected Fraser River stocks, but I don't think there is sufficient -- I don't think fish farms are a sufficient explanation for the pattern of decline in sockeye, generally, between Washington and Southeast Alaska.

Q So the fact that the Okanagan and Columbia stocks did well in 2009, when the stocks that migrated up the inside passage, that would be a relevant fact to you, too, wouldn't it?

A Yes. And the work that Kim Hyatt's done shows that there were very different temperature conditions on the outer side of the west coast of Vancouver Island where those stocks were going than occurred in the Strait of Georgia, in 2007, which was the migration year for those smolts.

So what I'm pointing to is it's not just fish farms that differ between the inside and outside, there's also man other oceanographic variables that can differ.

Q Can we go to - I'll just finish off in a minute or so here - can we go to page 23 of the addendum report, 1575. I just want to identify -- sorry, numbered page 23. Yes thank you.

You've made two pretty strong recommendations, as I see it here. In the third line below the bold headings, you say that the -- there are three categories of high priority data which need to be incorporated into the database. One, is fish health in farm salmon; two, is water quality in the vicinity of salmon; and three, is wild sockeye post-smolt survival estimates before and after passing salmon farms. Now, I think we've discussed that already.

A Yes, I think so, yeah.

Q And these you describe as high priority, because they're potentially different if the answers are positive?

A I think it's necessary to get data to test alternative hypotheses.
Q And further down the page you acknowledge the recommendation of Drs. McAllister and Carruthers, that one idea would be to experimentally manipulate the intensity of salmon farming by having fallow years and seeing what the outcome would be. Now, that seems to make sense. Do you think that's appropriate to the risk?
A I think that creating as strong contrasts as possible in both space and time has been shown in many other environmental stressors to be the best way to try to find a signal. Now, that having been said, I'd just add one additional thing, is you should do it in a way that doesn't get confounded with pink salmon. So if you did it every odd year, fallowed every odd year, that would really screw up your experimental designs. You might want to do it one out of every three years, get it out of sync. So you basically want to separate the signal from the noise.
Q Can we have Exhibit 1573. Now, there's one place -- 1563, I'm sorry. That didn't look at all -- there's one place on the coast where the salmon migration migrates to a very narrow place and where there's a great number of fish farms. Would that be the most sensible place to do that experiment?
A I haven't really thought about it before. I think if you could create contrasts between the highest level of exposure and the least, that would be the most informative. So I'm not sure, in this diagram, whether the salmon going along Johnstone Strait would be less exposed than the salmon which are going inland through some of those areas where we've got lots of pink dots, but I think you'd have to think about that and try to get as much contrast in exposure as possible so you could test the hypothesis.
Q Well, in 2007, the Chinooks, those farms that were growing Chinooks in that area where removed and weren't present in 2008. Don't we, in fact, have the fallowing -- if that's true, that's the nature of the fallowing experiment you might do, and we might see what the difference is in returns in 2009 and 2010?
A Oh, I don't think you'd want to look at returns,

because then you're integrating all sorts of other information. What I would suggest is that you'd want to look at acoustically tagged fish so you could tell exactly where they went and what exposure they had, and then look at diseases in co-migrating fish. Maybe you can't look at the disease without sacrificing the fish beforehand, but I think you want to look at disease and health of those fish. Returns have every other factor influencing them as well, so I don't think it would be a very good test to just look at returns.

MR. McDADE: Thank you. That's my time, Mr. Marmorek.

A Thank you.

THE COMMISSIONER: Thank you, Mr. McDade.

THE REGISTRAR: The hearing will now recess for 10 minutes.

(PROCEEDINGS ADJOURNED FOR AFTERNOON RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: The hearing is now resumed.

THE COMMISSIONER: Mr. Leadem.

Mr. LEADEM: Good afternoon, Mr. Commissioner, good afternoon, Mr. Marmorek. My name is Tim Leadem. I represent the Conservation Coalition, groups such as the David Suzuki Foundation and other environmental groups in these proceedings.

CROSS-EXAMINATION BY MR. LEADEM:

Q I want to commend you firstly for a very readable report.

A Thank you.

Q I come down on the side of Dr. Peterman and like the analogies, because sometimes if we can analogize correctly, we can usually understand concepts which are often difficult, and so I find that it's very useful to try to do that. So if at any time during your answers you want to use metaphors and analogize, please do so. And I also happen to like colourful language, so you can insert that, as well.

I want to begin by drawing your attention to the workshop that you facilitated back in November 30th of last year. I think it was a two-day workshop. And my understanding is, is that you facilitated a workshop at which all of the

scientists who were preparing reports for the Commission, the expert reports or technical reports, as we've come to call them, assembled and over a two-day period addressed a number of concepts, discussed the reports amongst themselves and was a fair exchange in information, as scientists are very often capable of doing. Is that correct, do I have that right?

A Yes, that's correct. Now, this was November 30th, and December 1st, and so the draft reports for each of those studies were in varying states of completion. So Dr. McKinnell's report was already done, but others were still in process, so there were varying stages.

Q One of the tasks that you did during that workshop was to focus upon the 2010 PSC symposium that was the year before in June of 2010, and then you asked some of the participants at the workshop in November of 2010 to comment on the PSC workshop. And I want to just go with you to those results. I think they're at the tail end of your report.

A Yeah, that's right, the last two pages.

Q Actually, 1896, Mr. Lunn, and if we could go to PDF 362. You state there in Appendix D that:

> Workshop participants were asked to examine the PSC Report...

This is the one that Dr. Peterman and yourself I think were the facilitators of at SFU in June of 2010; is that right?

A Well, Dr. Peterman was the head of the Science panel and the lead author on the report, and we worked as facilitators and assisted.

Q So if I look at this table, I think it's in two sections, there are a number of alternative hypotheses to explain the 2009 decline. And what I think, if I'm reading this report correctly, the top bar, the one in grey is the PSC and then the workshop that you facilitated in June -- or sorry, in November would be in yellow; is that right?

A Yes, that's correct. If I could just make a couple of clarifications. First of all, for each of these hypothesized factors in the leftmost column in this Appendix D, we're looking at both the overall trend over the last 20 years, as well as the 2009 low returns.

The other thing to mention is that the PSC report, we had the workshop and then the panel met and considered their recommendations over a series of conference calls and exchanges of emails. Whereas the workshop that we did for the Cohen Commission on November 30th, December 1st, we had maybe a couple of hours at which we had subgroups meeting to explore these ratings. So there really wasn't as much time --

Q Yes.

A -- or thought given to it.

Q Right. I understand that. We heard evidence also from I think it was Dr. Rensel from -- who had testified to this Commission with respect to harmful algal blooms that he was part of the PSC workshop, but I think there was a contingent from the United States that left, so they didn't get a chance to vote. Do you recall that?

A That actually isn't the way it happened at the PSC workshop. What happened was that the -- we met in subgroups and got input from those subgroups on particular hypotheses, and I was actually in the group that Dr. Rensel was in that dealt with harmful algal blooms and contaminants. That information was brought back to the Science panel and it was actually Brian Riddell who wrote the chapter dealing with contaminants and harmful algal blooms. So there's a lot of consideration that went into that, actually, by the PSC Panel. Not so in the Cohen Commission, but it was considered in the PSC group.

Q All right. What I want to focus on is "Marine ecology", the fourth hypothesis down on the left-hand column. And it was broken down into two discrete areas, the Strait of Georgia and then outside the Strait of Georgia. And so if I compare those for the overall, I take it that the dark shading meant that there was emphasis to be placed on that topic by the PSC workshop; is that correct, do I have that right?

A Right. So a major difference between the PSC workshop is at the PSC workshop we heard a lot about conditions in the Strait of Georgia, and the panel was convinced by those presentations that the Strait of Georgia were more important than the conditions outside of Georgia Strait, both for the overall changes, as well as the 2009 poor returns.

Now, the Cohen Commission, particularly Dr. McKinnell, presented other quite convincing evidence that there were weird things going on in 2007 in Queen Charlotte Sound, as well, and that the Strait of Georgia wasn't quite so bad. And so those scientists at the workshop came up with stronger weighting on conditions outside of Georgia Strait.

Now, there's been further work done since then, which seems to indicate that both are important, so, you know, the truth may be simply that.

Q Somewhere in the middle.

A Yeah.

Q So if you were looking at marine -- the marine ecology to substantiate a hypothesis for the long-term decline, as well as the 2009 decline, you really wouldn't want to dissect out the Strait of Georgia and simply focus upon that singularly. You would really want to look upon the total marine environment and include Queen Charlotte Sound as well.

A Yes, I would.

Q if I could now ask you to turn to PDF 304, these pages are unnumbered. I think what I'm going to do is go back into the workshop and some of the reporting out that I think that you did of that workshop. I gather this is your work, Mr. Marmorek?

A Sorry, we're talking about the workshop that was done for the Cohen Commission? Yes.

Q Yes.

A We wrote the report from that workshop.

Q Okay.

A Yes.

Q So under the heading "Research and Monitoring Recommendations", and if I could just flip the page to PDF 305, and the second full paragraph on that page, I just want to take you there because this is one of the themes I've been pursuing throughout the body of work that I've been doing through this Commission. You say there:

> One of the resounding issues throughout the workshop was researchers' difficulty in obtaining and understanding data from the existing databases. Considerable effort

should be spent building and maintaining an integrated database, with focused research and monitoring goals in mind.

And then you go on to say:

The database should include the historical sockeye data with clear metadata as well as data from current and future monitoring.

And:

In order for this to be useful to scientists, it would need to be regularly updated and maintained.

So I'm just going to stop there and see if I can expand a little bit on that, because all the scientists who participated were complaining to you, I gather, about the lack of consistency in the data and the ability to use data sources from different areas and be able to make them compatible. I'm not a mathematician, so I'm paraphrasing very roughly. So is that basically what the problem was, or is it a little bit more complex than that?

A Well, I wouldn't say that all the scientists were complaining about this. I think one of the difficulties which existed was assembling all the productivity information on sockeye and getting that into a consistent format. The different stocks -- within a given stock there are different life history patterns. Some fish spend one year in fresh water, some spend two, they spend varying amounts of time in the ocean, and you have to line all of that information up if you're going to do these correlative analyses. And then the stressor information also comes from a whole bunch of different databases.

So, you know, I think the Cohen Commission has really catalyzed quite a bit of good effort that way. And the database that we assembled for this study, although it just was an internal database, it's structured in a way that could be built upon. So I think we're making progress on both getting the metadata together, as well as the stressor information and the sockeye information.

Obviously that would need to be updated every year, and you're not going to have a budget like the Cohen Commission does every year, so this has got to be made part of common practice.

Q So in terms of any recommendations with respect to the data, can you help us at all in terms of how accessible this database should be, whether scientists who study in this realm should have access to it, whether there should be some limits on it, who gets to control it, who gets to put data into it. Do you have any recommendations with respect to those kinds of topics?

A I think that past experience elsewhere has indicated that the people who are closest to collecting a particular kind of data. So let's say for example, the Okanagan First Nation and DFO collect Okanagan sockeye data, are the best ones to organize that information and then say we're happy with our analyses for this year and put that out, and then get that in a common framework. And then if there is some update to that, they discovered that there was an error, then they can update that information. I think then you can feel fairly assured that that information has been carefully checked, and then put into a centralized or integrated database, which I think should be publicly accessible.

There are examples like that in the Columbia Basin, there's something called StreamNet where there's public access. There's also the Columbia Basin Fish and Wildlife Authority has a publicly accessible web accessible set of information. The key thing is it has to be carefully checked before it goes in there.

And as far as being able to do analyses on the data, provided that it's -- that data has been quality checked, I don't see any reason why anybody shouldn't have access to the full suite of data, and that will stimulate different kinds of analyses, which I think is healthy. The key thing is that what goes into it has to be carefully checked and so there has to be one group that's responsible for assuring that it is good quality.

Q Mm-hmm.

A That's very important.

Q Yeah, I remember that acronym, garbage in, garbage out, so that if you're not putting in very

reliable data, then you're obviously not going to be able to get the results that you want at the end of the day from being able to analyze that data and to synthesize it and have it form the basis of reports.

A Yes, and the other thing is the metadata, which is the data about that data, that says this is how it was acquired. These are the sampling methods. These are where the data came from, all those other details, extremely important.

Q Now, in addition to some of the workshop that focused on PSC, there was a full range of discussion about some of the projects that were ongoing, and I want to take you to some of the minutes --

A Sure.

Q -- of that meeting that occurred, as well. If we can go to PDF I believe it's 327. Now, at that time my understanding as the group Counterpoint was actually going to file a report on "Status of DFO Management and Science" and as it turned out that report never did get filed; is that right?

A That's what I've heard, but I haven't been tracking the thousands of documents that have been filed.

Q It certainly wasn't one that you analyzed when you came upon your cumulative effects analysis at the end of the day?

A No, it wasn't, because it didn't pertain to describing the different stressors that we were analyzing.

Q And I'm just going to take you to some of the discussion that ensued when Edwin Blewett was presenting on behalf of this topic. If we can just go to the next page, PDF 328, please, Mr. Lunn. And at the bottom you'll see that there's some discussion. There's a discussion, that's Dr. Skip McKinnell from PICES, is that right, who is --

A Yes.

Q -- saying:

> Did you consider looking at the number of primary publications by DFO authors? You will likely find that the amount of science done as a proportion of the actual work they do has been shrinking over time. It would be

> useful to see a graph on the number of published articles on Fraser sockeye where DFO scientists were the senior authors.

And then that discussion is then followed up by Dr. Peterman, and that continues on to the next page. Do you recall that discussion?

A Yes, I do.

Q And that's a fair or accurate representation of the discussion that ensued about that topic, is it not?

A Yes. My recollection is that Edwin Blewett did include some information at least on scientific publications in the executive summary of his report, but I don't know if it was exactly what Skip McKinnell was asking for.

Q And similarly the next topic at PDF 330 was "Diseases and parasites" and this was a presentation by Dr. Kent, was it?

A Yes.

Q The lead author for that. And then if you follow through to PDF 331, there is then a discussion that ensues. I imagine that what happens, Dr. Kent - I wasn't there, and would have loved to have been there, but I'm glad you did not have lawyers there, quite frankly - but essentially, as I understand it, someone presented the topic for about 15, 20 minutes and then there was an opportunity for discussion from the attending scientists of that person, is that --

A That's correct.

Q -- basically the framework. So the discussion that ensued after Dr. Kent presented his paper was mostly between Dr. Reynolds and Dr. Kent, that that would be Dr. John Reynolds, who was one of the outside reviewers and a professor at Simon Fraser University; is that right?

A Yes, that's correct.

Q And so he says at the conclusion of the presentation, he says "How specific" -- or he asks a question:

> How specific are these pathogens to specific species of salmon? And is it a good idea to restrict the scope to known cases involving sockeye?

And Kent says:

> I don't exclude any from the list. Where sockeye are less susceptible to disease, I would put it as a moderate risk assessment.

And then Dr. Reynolds says:

> Is that a good criterion to use? If studies show sea lice can infect other species of salmon, it might be useful to tackle it head-on, to take sea lice out of the picture.

So once again the discussion carries on for several pages after that, but essentially that would be an accurate representation of the discussion that ensued following Dr. Kent's presentation, would it not?

A Yes. We had a very good recorder who taped the proceedings and then transcribed them, so I think this is an accurate description of the free-ranging discussion. I guess what I would say is that it's a free-ranging discussion at a workshop, and I think in general you need to look at all the information in detail, you know, to make decisions, rather than a five-minute discussion.

Q Yes, I appreciate that. On page PDF 332, I just want to focus on a comment that Dr. Kent made in response to Dr. Rick Routledge, who was one of the outside reviewers. He's also a professor at SFU, is he not?

A Yes, he is.

Q And about one-third of the way down Dr. Routledge asked:

> Is there any evidence of vectors for disease to consider?

And then Dr. Kent says:

> You can show in lab studies that *Lep*...

And that would be the louse, the salmon louse, *Lep*.

A Mm-hmm.

Q

> ...can jump from adult fish. Some pathogens

for example are transmitted via leeches but can also transfer through the water.

And he goes on to say:

Could sea lice be transmitting disease?

He asks himself that question.

In freshwater, there have been increases in snail-borne disease due to increasing numbers of snails.

And then he goes on to discuss a whirling disease. And then Dr. Reynolds chimes in and says:

A recent paper showed sea lice do jump from host to host in the wild. Male *Lep* jumped from pink to coho smolts.

So that once again was a flavour of the discussion that occurred with Dr. Kent and Dr. Reynolds and Dr. Routledge concerning the vectors, the possibility that disease could be vectored, as you will, by something such as *Lep;* is that fair to say?

A Yes, that's correct, and in the conceptual diagrams that we were looking at earlier from our addendum, that causal pathway is represented.

Q And one final topic that I want to focus on, if we can go to, I think the next page, PDF 333, Don MacDonald, who was the author of, I think, the technical report on contaminants, then presented, and he presented his findings and then there was a brief discussion that ensued following his report, mostly with Dr. Peterman, Dr. Routledge, who once again that followed the presentation of Mr. MacDonald's report, was it not?

A Yes, that's correct.

Q I just find this whole process to be rather invigorating and enlightening, unlike this kind of a situation where it's very controlled, where I get to ask the questions and hopefully you get to respond. When scientists meet, it seems to be a free-ranging debate. And so I think at the end of the day, one of the things that I've been proposing is that there be some way to move

forward on the science that needs to be done. You've identified, for example, some specific scientific research areas that you would like to see followed. And you've identified those in bold, and you've identified them with respect to lifecycles and life stages of the salmon. And others have preceded you to the podium and obviously they also have specific research topics that they would like to see done; more often than not it happens to coincide with the area of expertise that they happen to be involved with.

But I guess I want to come back to this. If we're going to move forward in terms of the science, and the ability of science to really grapple with this issue of what's behind the decline of the sockeye, or how can we ameliorate the condition of the sockeye, what kind of apparatus can you envisage being brought forward to see that that work is conducted and carried out?

A So first of all, I think it's important to point out that our recommendations in section 5.2 were not just our suggestions --

Q Yes.

A -- or my ideas. These were built, as we said earlier, on the work that came out the Pacific Salmon Commission workshop, as well as the recommendations of all the Cohen Commission researchers. So and as I was saying earlier to the Cohen Commission lawyer, I think the first step is to decide what are the key management decisions that need to be made, and what are the scientific uncertainties that affect those management decisions, and then to have a dialogue between managers and a subset of scientists from DFO, NOAA Fisheries, Pacific Salmon Commission, plus others, you know, academia, but a smallish group, a manageable group, to winnow down that list to set forth a sequence of studies that are cost-effective for answering specific questions and to put in place the infrastructure that will maintain those studies for a sufficient length of time so, as was mentioned earlier, that we have a continuing time, which I think was with the lawyer from the Province.

So, you know, as I was asked earlier by the Commission lawyer, that's my opinion how to set

that up. There may be a better way, there may be other people who have better ideas about that, but that wasn't within our terms of reference to decide, you know, or recommend that kind of structure. It just seems logical, given who has the mandate. In the United States NOAA Fisheries does, in Canada DFO does, and both on the PSC, so those seem like logical leaders to tackle it.

Mr. LEADEM: All right. We'll come back to that concept tomorrow. I think we're at the end for today, Mr. Commissioner.

MS. BAKER: Thank you, Mr. Commissioner. I wonder if we can organize our timing for tomorrow. We'll be starting at 9:00 and ending at 3:00, and I was going to propose that we could maybe go from 9:00 to 10:30 and take a break at 10:30, and then go from whenever we come back from that break to 12:30, just the one break in the morning. Would that be acceptable?

THE COMMISSIONER: I guess my answer is we'll see, but that sounds like not an unreasonable proposal, Ms. Baker. But just before we break I just wanted to ask a question to clarify something that Mr. Leadem asked the witness. And I must apologize. I'm not sure that I -- it's possibly Exhibit 1896. It was the paragraph dealing with database, Mr. Leadem, that you were asking the witness about. And I just -- is that Exhibit 1896?

Mr. LEADEM: That was 1896.

THE COMMISSIONER: And it might have been -- was it page 305, possibly?

Mr. LEADEM: It was PDF 305, I believe.

QUESTIONS BY THE COMMISSIONER:

Q I'm sorry, PDF 305 -- I apologize, that's probably it. Yes.

A Sorry about the pagination, that's our fault.

Q No, that's fine. I'll just take a moment just to ask you this, and I'm not being facetious, but what do you mean by "database" in the context of this paragraph?

A So an organized form of data includes for this problem all the information on the numbers of spawners from each stock, the age structure of each of those stocks, so how many return as three-year-olds, four-year-olds, five-year-olds, how

many years they spend in freshwater, the proportions and so on, which can vary by year. So and then that's for both -- that would be for both Fraser stocks, as well as for non-Fraser stocks.

And then the information on various stressors, which we were discussing earlier with respect to the analyses that were included in section 4.7 of our report. So for example, I'll just find the section here, page 93, not PDF page but the actual page, yes, this table. That's an example of the kind of stressor variables which would be helpful to include in such a database.

And a database, a relationship database is an organized framework which links information by stock, by year, by location, by type of information, by variable name. There's a nice description of it in Appendix 3 of our report. It's a very structured way of organizing information as opposed to what generally exists, which are a whole bunch of different spreadsheets that are different for different stocks and in different locations and for different stressors. And what we had to do was basically grab all that information and organize it into this structured framework and relate them all. So that's what we call an organized database.

Q And that's what you're speaking of.

A Yes.

THE COMMISSIONER: You can follow up tomorrow with that, Mr. Leadem.

Mr. LEADEM: Thank you, Mr. Commissioner.

THE COMMISSIONER: But I appreciate the answer. Thank you very much.

A Thank you.

THE REGISTRAR: The hearing is now adjourned till 9:00 a.m. tomorrow morning.

(PROCEEDINGS ADJOURNED TO SEPTEMBER 20, 2011 AT 9:00 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.

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.

Karen Hefferland

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.

Pat Neumann