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

Commaissaire

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

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

le jeudi 21 avril 2011

APPEARANCES / COMPARUTIONS

Wendy Baker, Q.C. Maia Tsurumi	Associate Commission Counsel Junior Commission Counsel
Mitchell Taylor Jonah Spiegelman	Government of Canada ("CAN")
Boris Tyzuk, Q.C. Clifton Prowse, Q.C.	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. ("BCAUEW")
No appearance	Rio Tinto Alcan Inc. ("RTAI")
No appearance	B.C. Salmon Farmers Association ("BCSFA")
No appearance	Seafood Producers Association of B.C. ("SPABC")
No appearance	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")
Don Rosenbloom	Area D Salmon Gillnet Association; Area B Harvest Committee (Seine) ("GILLFSC")

APPEARANCES / COMPARUTIONS, cont'd.

No appearance	Southern Area E Gillnetters Assn. B.C. Fisheries Survival Coalition ("SGAHC")
Christopher 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")
No appearance	Western Central Coast Salish First Nations: Cowichan Tribes and Chemainus First Nation Hwlitsum First Nation and Penelakut Tribe Te'mexw Treaty Association ("WCCSFN")
Brenda Gaertner	First Nations Coalition: First Nations Fisheries Council; Aboriginal Caucus of the Fraser River; Aboriginal Fisheries Secretariat; Fraser Valley Aboriginal Fisheries Society; Northern Shuswap Tribal Council; Chehalis Indian Band; Secwepemc Fisheries Commission of the Shuswap Nation Tribal Council; Upper Fraser Fisheries Conservation Alliance; Other Douglas Treaty First Nations who applied together (the Snuneymuxw, Tsartlip and Tsawout); Adams Lake Indian Band; Carrier Sekani Tribal Council; Council of Haida Nation ("FNC")
No appearance	Métis Nation British Columbia ("MNBC")

APPEARANCES / COMPARUTIONS, cont'd.

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

TABLE OF CONTENTS / TABLE DES MATIÈRES

EXHIBITS / PIECES

EXHIBITS FOR IDENTIFICATION/PIECES POUR L'IDENTIFICATION

Vancouver, B.C./Vancouver
(C.-B.)
April 21, 2011/le 21 avril 2011

THE REGISTRAR: Order. The hearing is now resumed.
MR. HARVEY: Good morning, Mr. Commissioner, good morning, Dr. Peterman.
DR. PETERMAN: Good morning.
MR. HARVEY: Chris Harvey, for the Area G Trollers, continuing.

RANDALL PETERMAN, Recalled.

BRIGITTE DORNER, Recalled.

CROSS-EXAMINATION BY MR. HARVEY, continuing:

Q Would you agree that Dr. Carl Walters is very highly regarded in the field of fish population dynamics?
DR. PETERMAN: Absolutely, yes.
Q Yeah. With a reputation extending well beyond the Pacific Region, I think; is that correct?
DR. PETERMAN: On yes, for sure. Worldwide, I'd say.
Q And he also seems to have the somewhat uncommon human attribute of being able to admit his own past mistakes? I don't know if you've noticed that.
DR. PETERMAN: Occasionally.
Q I want to -- you've got a binder, I think, in front of you with documents that I gave notice of, and at Tab 14 there's a document by Pestal -- Pestal, Ryall and Cass.
MS. BAKER: You might want to just name off the titles, because he hasn't got the --
DR. PETERMAN: Is that the 2010 version?
MR. HARVEY: No, it's the 2008.
DR. PETERMAN: Okay, great. Brigitte, did you hear that, the Pestal et al 2008 paper?
DR. DORNER: Yeah. I don't have that here, actually. I have the 2010 paper.
DR. PETERMAN: Yeah, that's right. I don't know whether we downloaded the wrong one, but I think that you can go ahead with the questions. I think we'll have a pretty good idea what's in there.
Q It's Exhibit 398, so perhaps we could have that

brought up. Yes. I just wanted to ask you about a passage at page 0019. This discusses the 1987 rebuilding strategy. It says, at the top of the page:

> DFO formed a task force in 1987 to develop a plan for increasing the average Run Size of Fraser River sockeye to at least 30 million fish. Specific objectives were to:
>
> maximize production,

et cetera.

DR. PETERMAN: Yes, I see it.

Q And then it goes on to state, perhaps without reading the whole thing, we can just drop to the bottom of that last paragraph in the middle, the last three or four lines:

> The Task Force felt it was too risky to try and achieve the same level of production across all four cycle years. Instead they recommended that exploitation rates should be reduced experimentally on the off-cycles for some stocks to learn about the mechanisms of cyclic dominance.

And then the second bullet point down states:

> Rebuilding would require reductions in harvest rates to 65-70% within four years (i.e. 10-15% percentage points less than historical levels,

et cetera. It says on the next page, page 12, in the top bullet point, that:

> Rebuilding should take 12-16 years with an adjustable escapement schedule that varies with run size.

And this, I think, describes the beginning of what is sometimes referred to as an experiment in the -- with the goal of rebuilding stocks, but an experiment which involved putting more spawners on the spawning grounds in some years; is that correct?

DR. PETERMAN: Yes, I think that's correct.
Q What I wanted to ask you is if you have done, or if you're aware of anyone else having done, a retrospective on that experiment in order to determine the results?
DR. PETERMAN: Well, if by retrospective you mean evaluating whether it succeeded in increasing spawner abundance, then yes, I think that the data we present in our report show the spawner abundance definitely has increased over time and it's been in several documents. If --
Q Yes.
DR. PETERMAN: Is that what you mean, or do you mean evaluating it in terms of changes in catch --
Q No, I mean, if it is an experiment, which it seems to be, it's part of the process of these experiments, and I think they're sometimes called "adaptive management experiments". It's part of the process, a necessary part of the prospect (sic) that there be a retrospective analysis after the period of the run --
DR. PETERMAN: Right.
Q -- correct?
DR. PETERMAN: Okay.
Q So where is that retrospective analysis, or does it not exist?
DR. PETERMAN: Well, as far as I know, it doesn't exist, except some version that Carl did, Carl Walters, that is, on his own. I'm not sure when it was, maybe sometime in the last year, that I read about it in the transcripts of the hearing at this place, on the 9th of February or the 10th of February and he was talking about how he had passed it on to DFO and had asked a few other people to do it. But other than that, I have not seen it, I've only heard about it.
Q All right. There's a fairly recent PowerPoint presentation that he did at Tab 8 of the binder. Now, you don't have the tabs, so what I can say is that that says, yes, on the title page, Where have all the sockeye gone? Carl Walters, Fisheries Centre, UBC.
DR. PETERMAN: Yes, okay, I have that here.
Q Okay. And on the ringtail version of that, at page 0019, and I don't know if you have the ringtail version or not.
DR. PETERMAN: Okay, well, just tell me what's on it

there.
Q There's a number of pages. I'm just going to flip through. It starts with, "There is clear evidence of an upper limit". Oh yes, it's on the screen. If you look at the screen in front of you, you'll see the page in question. So do you have that page that's on the screen?
DR. PETERMAN: Yes, thank you, I do.
Q Yes.
DR. PETERMAN: Yeah.
Q So just to move through these, this one indicates his view:

> There is clear evidence of an upper limit (carrying capacity) for smolt production from Quesnel, Chilko, Shuswap Lakes.

DR. PETERMAN: Mm-hmm.
Q The next page states, in the bold print at the top:

> There is an overall negative relationship between productivity and spawner abundance, as is typical in stock-recruitment relationships.

DR. PETERMAN: Mm-hmm.
Q And the next page:

> Survival declines prior to 2003 can be explained largely by density dependent effects related to increases in spawner abundance.

And then the next page says:

> Models with delayed density dependence fit data better than Ricker model, particularly for 1990-2004 brood years.

And then the next page:

> Has there been "overescapement"? Declines in Chilko recruitment at high spawning stock since 1990.

And then, finally, the following page reads:

What changed when Chilko spawners increased?

He has three bullet points:

- Spawners increased in 1990
- Freshwater survival dropped immediately
- Inverse relationship between freshwater and marine survival in recent years

I'm sorry, I did want to go one more page. The next page reads:

> The monster Adams run of 2010 was produced by an intermediate spawner abundance.

It says:

> And the Quesnel stock has also shown maximum recruitments at intermediate spawner abundances.

Does that seem to encapsulate Carl Walters, I don't know whether we call this the retrospective, but it encapsulates his current views, so far as you're aware?

DR. PETERMAN: Well, I guess it does. These are slides he made up of his interpretations of the data, so...

Q Okay. Is that consistent with --

MS. BAKER: Mr. Commissioner, it's an unfair question, really, because he's asking the witness whether this encapsulates what Carl Walters is thinking, when he's already said he hasn't talked to Carl Walters and he only knows about the theory from what he reading a transcript. So, I mean, he can say this is a document, he can say, if you tell him Carl Walters did it, maybe he did, but I don't know what he can do beyond that.

MR. HARVEY: What I was leading to, and --

MS. GAERTNER: With due respect, Mr. Commissioner, I support Ms. Baker's views on this, and I also don't even know when and how this document was produced, what year it was produced, for who it was produced. There's nothing that suggests how to put this document into context.

MR. HARVEY: My questions were going to the retrospective, and I actually hadn't asked the

question I wanted to ask, which was:
Q Is this, to your knowledge, the Carl Walters' retrospective analysis, or do you not -- or do you know?
DR. PETERMAN: No, I don't know. I suspect this is part of it, but again, from what I saw in the transcripts from that February 9th or February 10th hearing, I think he went beyond this.
Q Yes. Yes, he certainly did. This doesn't do anything more than outline certain points.
DR. PETERMAN: Mm-hmm.
MR. HARVEY: Mr. Commissioner, I wonder if I could have that marked, and if my friends object, I would ask it be marked for identification. If they don't object, I would ask it be --
MS. BAKER: I think it should be marked for identification, because it hasn't been seen by the witness before, so he can't do much more than say, "I read it on the screen as you took me through the pages."
MR. HARVEY: Well, with respect, this was in my disclosures.
MS. BAKER: It may have been in your disclosure, but it doesn't mean the witness can identify it.
MR. HARVEY: I see, all right.
THE COMMISSIONER: We'll mark it for identification purposes, Mr. Harvey. I'm not sure, is it fair just to call it a document with the title without knowing more about it? So it's just --
MR. HARVEY: Yes.
THE COMMISSIONER: -- Where have all the sockeye gone? It appears to be a Carl Walters document from the Fisheries Centre at UBC. So whatever the next identification letter is.
THE REGISTRAR: It will be for identification Y.

MARKED Y FOR IDENTIFICATION: Document titled, Where have all the sockeye gone, by Carl Walters, Fisheries Centre, UBC

THE COMMISSIONER: Thank you.
MR. HARVEY: Now, I'm wondering, Mr. Lunn, if we could have the transcript of February 10th back again. Yesterday, I was looking for a passage by Mr. Woodey -- Dr. Woodey, I'm sorry, at page 46.
There's a statement right at the top of the page. This follows a discussion, and just so we

don't mix it up, he was talking about a more specific experiment in 2001 and 2002. He says, at the top of the page:

> And it's the summer-run fish that became the experiment, under my terminology, that is, the escapement levels of particularly Quesnel Sockeye in 2001 and 2002 were very large and, thus, the "experiment" has shown that the over-escapement, that I term over-escapement as, has resulted in disastrous results for the Quesnel Sockeye...

That seems to be Dr. Woodey's view on that, and I want to ask whether you agree, or are those -- do you agree with that characterization that the experiment with respect to spawner levels in 2001 and 2002 for the Quesnel could be termed "disastrous" in terms of productivity loss?

DR. PETERMAN: No, I wouldn't use the term "disastrous".

Q All right. But you do accept that that set off a long-term decrease in productivity?

DR. PETERMAN: Well, it appears to be coincident with the decrease in productivity, yes.

Q Yes.

DR. PETERMAN: Whether it was responsible for it is, of course, another question, because there were several things happening simultaneously out there in the world.

Q Yes. But this is the -- this is the run that fits pretty much perfectly with the Larkin density dependence model, does it not?

DR. PETERMAN: Yeah, the Quesnel stock is the one that we found has the greatest support for -- pardon me, that the Larkin model that represents delayed density dependence across generations of spawners is the model that best fits the Quesnel data.

Q Yes. And the 2001, of course, is the cycle year for 2005 and 2009; 2002 is the cycle year for 2006 and 2010. The effects drove the dominant year cycle in 2009 down below what should have been the subdominant in 2010, did they not?

DR. PETERMAN: Well, I don't have the numbers in front of me, so I can't say for sure, but I think I remember reading that.

Q Yes. You discussed the Quesnel and the long-term

productivity loss. How many cycles do you think it will take the Quesnel system to recover from that long-term productivity loss?

DR. PETERMAN: Well, I don't really know how one would define "recover". I'm looking at, well, I mean, there's several ways to define that. "Recover" in terms of spawner abundance, or total annual recruits or harvests per year or what measure are you using?

Q Total annual recruits. In other words, get the run back to the size it had been built up to in the late '90s.

DR. PETERMAN: Okay, and your question was, "How long will it take to get back there"?

Q Yes.

DR. PETERMAN: I can't say.

Q All right.

DR. PETERMAN: No, there are too many factors affecting what productivity is occurring, and I just refer you back to the discussion yesterday where there seems to have been some major change across many systems in something --

Q Yes.

DR. PETERMAN: -- that's driving productivity down.

Q Yes.

DR. PETERMAN: So it's hard to forecast how long it would take the Quesnel to get back to those high levels of total recruits that were found in the 1980s. The peak is in the 1980s, by the way, in the Quesnel.

Q Thank you. All right, while we're on this transcript, there's something more, I think, about the experiment that began in 1987, the so-called rebuilding policy, and that's at page 62. There's a passage here by Dr. Walters, starting at line 22. Dr. Walters says:

> DR. WALTERS: The key mistake I believe we made came out in a paper by Jeremy Collie and I, and Randall Peterman, in 1990, and that's when we sort of officially recommended the off-cycle rebuilding experiment and talked about how to do that in terms of the timing groups. In that paper, we did a formal decision analysis, did a kind of cost benefit/risk analysis-type calculation of whether it was worth pursuing the experiment, because there

would be immediate losses in fishing and so on.

And we overtly discounted the possibility of strong delayed density dependent effects. We said, "We just don't believe the Larkin model, we don't believe the delayed effects could be so large." And had I known about and had we looked at the Gilhousen order - I guess it wasn't out quite then - if we'd looked even more carefully at Ricker's older work and seen the violence of the original cyclic behaviour of these populations, I'd have taken Jim Woodey's warnings a lot more seriously. We'd have left the Larkin model in our decision analysis and it would have very likely told us that the downside of potential loss of the experiment exceeded its potential benefits.

He's referring, there, to a paper that you jointly authored with him.

DR. PETERMAN: Mm-hmm.

Q Do you agree with those comments?

DR. PETERMAN: Well, frankly, I don't, literally, because I don't recall the discussion at the time, so overtly saying, "Well, we're going to recommend this rebuilding plan and it's very clear that that's what should be done," I think that came out as kind of a side topic from the paper.

Q All right.

DR. PETERMAN: And I think the fact that the Larkin model was not included was based on the evidence available at the time.

Q Yes. At any rate, you would agree with me that if you were redoing any experiment that had to do with the Quesnel run, you would certainly want to incorporate the Larkin model?

DR. PETERMAN: Oh yes, absolutely.

Q Yes.

DR. PETERMAN: You're talking about redoing the experiment -- if you were reconsidering an experiment now?

Q Yes.

DR. PETERMAN: Absolutely, yes.

Q Yes. Now, when -- just moving to another subject. When you were asked, yesterday - I've forgotten who asked the question;, I think it was counsel for Canada - about how you explained the 2010

returns - that wasn't quite it - but you were asked about the 2010 returns, and I think you agreed that they were inconsistent with the long-term trend you identified?

DR. PETERMAN: Yes, that's right. It appears that the productivity for the brood year that led to the returns in 2010 was unusually high --

Q Yes.

DR. PETERMAN: -- given the record of the past two decades.

Q Yes. And I think - I don't have the transcript - but I think you explained it in part by saying there was good escapement in 2006?

DR. PETERMAN: That was part of the reason why there was a large return.

Q Yes.

DR. PETERMAN: Yes. So you take the number of spawners times their productivity returns per spawner and you get a very large return in 2010.

Q I see. But by "good" do you mean reduced escapement more in line with the levels that the old, pre-1985 fishery commission worked with?

DR. PETERMAN: No, I was referring to a moderately large escapement - and I don't remember the numbers - for the total --

Q All right.

DR. PETERMAN: -- Fraser escapement is what I was referring to.

Q We have the numbers here, I think, in the Pestal and Cass document, the 2010 document that I think you have.

DR. PETERMAN: Okay, which tab number was that, again?

Q That's Tab 15, Pestal, Huang and Cass, 2010. And starting at page 99, and if we could go to the bottom of the page of 99, this is for the Quesnel --

DR. PETERMAN: Okay.

Q It's at the top of the page. And we can see 2001 and 2002, the very large escapement numbers that were discussed earlier. In 2005, there was a large escapement, but 2006, it was very -- considerably less than 2005; do you see that?

DR. PETERMAN: Yes, I do. 723,000, is that what you're referring --

Q Yes.

DR. PETERMAN: Right.

Q And it's that number that you would have been

referencing with respect to your comments about good escapement in 2006?
DR. PETERMAN: No, I was referring to the 2006 escapement for all of the Fraser stocks.
Q All right. Well, we'll take -- well, I can't do them all, but I'll take the major ones. These are the -- you accept these numbers, 90,000 effective spawners, 90,415 effective female spawners, 2006, for the Quesnel?
DR. PETERMAN: Well, I guess it was in the Pestal et al report, so --
Q Yes.
DR. PETERMAN: -- that's all I know.
Q All right. The next page is the Chilko, and the bottom of the page there shows the Chilko effective female spawners for 2006 at 261,967.
DR. PETERMAN: Yes.
Q Do you see that?
DR. PETERMAN: Yes, I see it.
Q And just moving up the column, that's less than all the numbers between 1990 and 2001, is it not?
DR. PETERMAN: It appears to be, yes, except for the 2002 -- oh, yes, you said between --
Q 1990 and 2001.
DR. PETERMAN: And 2001, yes, that's right.
Q All right. And the other large stock, of course, is the Shuswap. That's on page 102. The numbers there for 2006, effective female spawners, 2006, 1,170,725, do you see that?
DR. PETERMAN: Yes, I do.
Q Substantially less than 2002.
DR. PETERMAN: But much larger than any of the surrounding years from 2004 --
A Yes.
DR. PETERMAN: -- to 2008.
Q And almost exactly what the 1954 escapement was in that area; do you see that?
DR. PETERMAN: Yes.
Q 1954. And that was the previous record high return for the Shuswap, correct?
DR. PETERMAN: 1954?
Q 1954.
DR. PETERMAN: No, doesn't look like it. I see '58 larger, I see '82 larger, 1990's larger.
Q I'm talking about recruits.
DR. PETERMAN: I beg your pardon, I was looking at the column "effective female spawners".

Q Oh yes.
DR. PETERMAN: Recruits are on the right side. I see.
Q Yeah, but -- yeah. And recruits is what the whole system is aiming for, isn't it?
DR. PETERMAN: Yes, in most senses, yes.
Q Yes. All right. Now, moving to, actually, the central point you make in your paper about the decline in productivity, and I'll direct my question to the 2009 run, the decline in productivity attributable to residual factors. Is there any way you can separate the decline in productivity in the 2009 run attributable to the Larkin model of delayed density dependence from the decline due to residual factors?
DR. PETERMAN: Probably not quickly.
Q All right.
DR. PETERMAN: I guess the raw data that we have would demonstrate that. Let's see, I'm trying to think. Brigitte, where could we find that most quickly in the appendices? It would be the residuals, I guess, wouldn't it, rather than the Kalman filter value?
DR. DORNER: Yeah, I think Appendix 2.
DR. PETERMAN: So you're asking specifically about Quesnel again, are you?
Q Yes, and I'm looking -- well, I was actually looking -- I mean, Quesnel was meant to be the dominant run, I think, but if we look at the overall run size in 2009 and ask, what is attributable -- what portion of the decline is attributable to the Larkin model of delayed density dependence; what portion is attributable to residual factors, is there any way we can determine that?
DR. PETERMAN: Yes. So if you, Mr. Lunn, if you could go to Appendix P-2, please? It's the fifth page in, lower left corner.
MR. LUNN: Did you say fifth page?
DR. PETERMAN: Fifth page, yes. So you should see Quesnel there. No. I guess the page number's wrong. There it is, lower left corner.
THE REGISTRAR: Dr. Dorner, if you have a point to make, just speak up, please. Sometimes we do not realize --
DR. DORNER: Oh, I was just going to say I don't think we can do that for 2009, because the data only goes to 2004.

DR. PETERMAN: Oh, that's --
MR. HARVEY: Oh, I see. Okay.
DR. PETERMAN: Wait a minute, now.
Q The blue lines on the one we're looking at is the Larkin model, isn't it?
DR. PETERMAN: Yes, that's the Kalman filter value.
Q Yes. All right.
DR. PETERMAN: But Brigitte's point is valid that --
Q That you can't --
DR. PETERMAN: We only go to the 2004 brood year, that the last -- if you look at the bottom two series, the highly variable lines, those are the residuals from the Larkin model in the grey "X's and the residuals from the Ricker model in the red "+'s". And so they stop in the 2004 brood year.
Q I see. All right. You can say, generally, though, that what's happening is that - I think this is the thrust of your paper - that both are contributing?
DR. PETERMAN: Both delayed density dependence effects and other factors --
Q Yes.
DR. PETERMAN: -- not related to spawner density, yes.
Q Yes. And with respect to the portion of the decline attributable to the residual factors, the cause is basically unknown?
DR. PETERMAN: That's correct.
Q It may be climate change related?
DR. PETERMAN: Maybe.
Q Maybe.
DR. PETERMAN: It may be lots of other things, too.
Q In other words, it may be something that we can't do anything about?
DR. PETERMAN: It's possible, yes.
Q And something we can't do anything about that is making the marine ecosystem more challenging for the smolts entering it?
DR. PETERMAN: Well, that seems to be --
Q Right.
DR. PETERMAN: -- the most likely hypothesis.
Q Yes.
DR. PETERMAN: But as I said yesterday, it's conceivable that there's something going on in freshwater --
Q Yes.
DR. PETERMAN: -- that doesn't lead to mortality until the fish are in the marine environment.

Q Yes. But in either event, you would agree, I think, that it becomes important, very important, that fishery managers do everything they can to ensure that the smolts entering the marine ecosystem are as large and healthy as possible

DR. PETERMAN: In general, we've seen that larger smolts tend to survive better in the ocean, so yes.

Q Yes. Because once they enter the ocean there's quite a gauntlet in the Strait of Georgia that they have to run with degraded food web and predators, and that sort of thing?

DR. PETERMAN: Well, we know that the mortality rate of juveniles is highest in the period between when they leave the lake, in the case of stocks where you estimate the smolts, and in the first year to year and a half of ocean life.

Q Yes.

DR. PETERMAN: So where it is exactly, I wouldn't necessarily pin it down to the Strait of Georgia, but there's high mortality going on early in their life history --

Q Yes, all right.

DR. PETERMAN: -- post-lake.

Q And would you accept this, that what is within the control of fishery managers is, to a certain extent, at any rate, keeping the right balance in the freshwater ecosystem between the biomass of sockeye fry and the carrying capacity of the rearing lakes?

DR. PETERMAN: Well, it's certainly affected by the escapement goal, if that's what you mean, but as you know, there's imperfect control over the fisheries and over en route mortality, so it's not possible to hit the escapement target perfectly by any means, anywhere.

Q Yes. But you would agree that the fishery managers can and should exercise their best efforts in that regard?

DR. PETERMAN: Yes, of course.

Q Okay. You, I think, did not accept Dr. Woodey's characterization of the Quesnel system decline as disastrous, but you would agree, at any rate, that it was most unfortunate?

DR. PETERMAN: Well, it was a substantial decrease.

Q Yes. Something that should be avoided if at all possible?

DR. PETERMAN: Yes.
Q I'm curious, therefore, why you did not include as a recommendation in your report that fisheries managers avoid so far as possible the large numbers of spawners that led to the density dependence declines in the Quesnel system.
DR. PETERMAN: Well, Brigitte might have some additional thoughts on this, but we felt that our report was purely a science report and that we did not have any -- a role to recommend how management should be done. As you well know --
Q Yes.
DR. PETERMAN: -- management decisions are made on the basis of complex management objectives that take into account multiple stakeholders, multiple frameworks for timeframes, short-term versus long-term. And so the objectives behind escapement goals attempt to take those things into account as well as the risks associated with the given level of escapement and the given level of fishing, therefore.
Q Yes. So you didn't consider it your role as a biologist to make any recommendations in that regard?
DR. PETERMAN: That's right.
Q All right.
DR. PETERMAN: Brigitte, did you want to add anything to that?
DR. DORNER: Well, yeah. The goal of our report was to look at trends in all the Fraser sockeye stocks, and this is just one particular stock that in many ways is unusual, so it certainly didn't seem to be the purpose of the report to make recommendations about particular stocks.
Q I see. Okay, well, thank you. Now, there's something that I've been wondering, and I'll just put it out for your comment, it's pure speculation, but I'm wondering if it's -- if there's a certain political incorrectness in the biologist community, a certain political incorrectness feeling about suggesting that excessive spawning abundance should be avoided in the sense that you're getting -- you're being opened to the criticism that you might be letting economics interfere with your biological judgment?
DR. PETERMAN: Well, I assume by using the word "you" you're talking about us, the biologist community

in general?
Q Yes.
DR. PETERMAN: Yes, that's what I thought. Okay. So I can tell you that the field that Brigitte and I work in is full of biologists who do take into account other measures of success than simply the number of fish on the spawning ground. So there's a whole fisheries -- a stock assessment community, for example, where our research is geared towards taking into account multiple objectives and evaluating, quantitatively, what the trade-offs would be among the fishing benefits, the First Nations benefits, whatever other benefits might be, in addition to meeting escapement goals. So I wouldn't say that's a fair characterization --
Q All right.
DR. PETERMAN: -- of the community as a whole that I'm familiar with and that I work in.
Q All right. Well, thank you for that. Would you agree that the conservation goal of sustaining a fishery resource over time requires taking into account the detrimental effects of excessive spawner abundance, and by that I mean excessive -- in excess of the carrying capacity of the freshwater ecosystem?
DR. PETERMAN: Yes.
Q Okay. You're familiar, I guess, with the concept of the ecosystem-based approach to fisheries management?
DR. PETERMAN: Absolutely.
Q Does that approach require taking into account also the food web upon which the sockeye juveniles depend?
DR. PETERMAN: Yes.
Q And avoiding what one witness here described - I think it was Ken Wilson - he referred to a salmon centric approach, an ecosystem-based approach would avoid a salmon centric approach?
DR. PETERMAN: I guess you could phrase it like that, because you're expected, in an ecosystem-based management context to consider the broader system that is affected by whatever actions you're taking.
Q Yes. And you've heard, of course, of the precautionary principle, we all have. Would you say that the precautionary principle, properly applied, requires that precautions be taken to

conserve the food web upon which sockeye juveniles depend?

DR. PETERMAN: Well, now, I'll let my technically correct standards slip here a bit and say, yes. There's a big difference between the precautionary principle and the precautionary approach, but we won't go there. So, in general, I think what you're getting at is, given uncertainties, a prudent manager or a set of managers would take into account the potential impacts on the dynamics of the ecosystem that support the salmon.

Q Yes.

DR. PETERMAN: For sure.

Q Yes. The same principle that we think of as being applied to salmon has to also be applied to the microscopic organisms that the salmon rely on for survival?

DR. PETERMAN: To the extent they can be documented to effect the survival of salmon, yes.

Q Yes, all right. Now, I just want to do some housekeeping matters here and mark some exhibits in my binder. At Tab 3 is a document which actually I put to Karl English, but I think I omitted to have it marked.

MS. BAKER: It's marked as Exhibit 727 already.

MR. HARVEY: Oh, is it? Thank you. 727, thank you.

Q Perhaps I could ask you: Are you familiar with this? It's A Habitat Based Evaluation of Okanagan Sockeye Salmon Escapement Objectives, published by Fisheries and Oceans Canada.

DR. PETERMAN: No, I'm not familiar with it. I scanned the abstract.

Q Okay. At Tab 4, there's one of your papers jointly published by Karin Bodtker --

DR. PETERMAN: Yes.

Q -- and Michael Bradford. That, I find, is a lot of useful information in it. Just to read some of the opening words below the abstract, for example:

> Historically, management goals for escapement in populations of salmon...have been estimated using stock-recruitment analyses, habitat-based models, or both. Stock-recruitment analyses required data series gathered over at least a decade, while a relatively short-term study might be sufficient for a habitat-based model

(Koenings et al. 1993; Hume et al. 1996). Habitat-based models have been used to estimate escapement goals when the S-R -

-- that's stock recruitment --

- data are limited,

and that seems to describe the field of how carrying capacity is determined; is that correct?

DR. PETERMAN: The habitat-based models? Yes.

Q Yes.

DR. PETERMAN: The habitat-based models are an indirect way of estimating carrying capacity for the lakes, right.

Q Yes. At page 009 of this there seems to be some useful graphs. You discuss, here, the carrying capacity in terms of effective female spawners per hectare; is that correct?

DR. PETERMAN: That's right.

Q And if we look at the bottom two graphs, one for the Shuswap and one for the Chilko, one axis -- is this the Y axis, the vertical axis?

DR. PETERMAN: Yes.

Q It deals with adult recruits per hectare. And then across the bottom the horizontal axis is effective female spawners per hectare. This shows an optimum and then a decline. Can you just describe what this --

DR. PETERMAN: Yes.

Q -- is meant to depict?

DR. PETERMAN: Okay, so what we see here, first of all, are the historical data in the black dots, solid black dots in both graphs that relate the adult recruits per hectare to the effective female spawners per hectare. And the reason we divide it by per hectare of the lake surface is so you can compare across lakes.

So within the Shuswap graph on the left, item (d) there, you will see the curve that's fit to those data.

Q Yes.

DR. PETERMAN: A solid line curve with no points associated with it, and that's the spawner recruit curve, in essence. The two curves delineated with "X's" and triangles are the estimates of the distribution of estimates of target escapements

based on -- well, the triangles are based only on the data from the spawner to recruit and spawner to juvenile analyses. So that's the estimate of where you get the maximum number of smolts.

Q Yes.

DR. PETERMAN: Okay? The X's delineate distribution of our estimated target escapement via another method that's called the Bayesian PR method, so it takes into account the photosynthetic rate of the lake and the various uncertainties associated with the steps in the calculation of carrying capacity of smolts.

Q Yes. And generally this shows that as you increase the number of effective female spawners you -- beyond a certain point, the apex there, you have quite a dramatic decline in adult recruits per hectare?

DR. PETERMAN: No, actually, it's a bit confusing, probably, but this -- take the triangle one only. All that triangle does, or that shape of function does that's delineated by the triangles, is it describes the probability distribution that the escapement gives rise to maximum smolts is at that value. So, for instance, it's most probable that about 55 to 60 million effective -- pardon me, 55 to 60 effective female spawners per hectare --

Q Yes.

DR. PETERMAN: -- will give the maximum smolts.

Q Yes.

DR. PETERMAN: Whereas it's very improbable that 150 effective female spawners per hectare would give the maximum of smolts.

Q I see. So it's analyses such as this that tell you what the optimum escapement should be?

DR. PETERMAN: Well, again, it's only based on assuming that your definition of "optimum" is the escapement level that produces the maximum number of smolts.

Q Yes, and if you want to -- the qualification would be if you want smolts that are healthy and well nourished and a good size by the time they reach the sea, you might have to back it off a little?

DR. PETERMAN: Right.

Q All right.

DR. PETERMAN: Yeah.

Q So there's less competition for food?

DR. PETERMAN: Yes.

Q Yes, all right.
DR. PETERMAN: Although, I should qualify that, having said yes. This analysis with the PR method does take into account body size, so it's looking at trying to maximize the smolt biomass. So I'm sorry, I have to correct what I said. I think it's the smolt biomass, if I recall correctly, that's maximized, rather than the smolt numbers. So that would -- smolt biomass would take into account the introduction between too many fish and too small or too few fish and very large ones.
Q Yes. Yes, because if you just take numbers you might have a great crop of stunted --
DR. PETERMAN: Exactly.
Q -- fry that are weak, yes, all right.
MR. HARVEY: Could we mark this, please, as the next exhibit?
THE REGISTRAR: Exhibit Number 751.

EXHIBIT 751: Article by Karin Bodtker, Randall Peterman and Michael Bradford, titled, Accounting for Uncertainty in Estimates of Escapement Goals for Fraser River Sockeye Salmon Based on Productivity of Nursery Lakes in British Columbia, Canada

MR. HARVEY: Okay.
Q The next tab, Tab 5, deals with factors effecting sockeye salmon returns to the Columbia River in 2008. There's a reference in this paper, at page 26, to one or more of your papers. This is published by the American fisheries authority. At page 26, do you see, the top paragraph:

The significant correlation between sockeye SARs -

-- I think that's their term for --
DR. PETERMAN: Yes, it's their term for the recruits per spawner.
Q Recruits per spawner.
DR. PETERMAN: So SAR stands for smolt to adult --
Q Smolt to adult.
DR. PETERMAN: Wait a minute --
Q Recruits per spawner. We would use "RS" our --
DR. PETERMAN: Yeah, that's right, yeah.
Q

- in both rivers -

-- they're talking about the Columbia Basin --

- is evidence that returns in 2008 were most likely influenced by factors downstream of Bonneville Dam. This result was similar to Peterman et al.(1998), who found that covariation in the survival characteristics of sockeye salmon was highest amongst stocks that resided in close proximity to each other.

et cetera, et cetera. Is this a paper that you're familiar with, this -- not your paper, but this NOAA fisheries analysis of 2008 returns in the Columbia?

DR. PETERMAN: I'm not very familiar with it, no.

Q I'd like to just look at page 28. Well, first of all, let's -- first of all, I'm sorry, could we look at page 3? It's a convenient map of the system, so we all know what we're talking about here. This is the river system that starts, I guess, on the Washington/Oregon border, if my geography's right, and goes up through a number of dams, which over the years impeded passage, the northern run goes up into Lake Osoyoos and Lake Okanagan, and then there's another branch goes up into the Columbia River into B.C., and then there's another branch that goes well off into Idaho and Snake River. So that's the system that ends up, in two of its branches, in Canada.

But at page, what was it, 28, is the conclusion, which I found interesting. They're talking about their good returns in 2008, and they say:

In summary, the results discussed here provide a consistent pattern to explain the large return of adult sockeye to the Columbia River in 2008. Based on these results, we conclude that the factors responsible for the high return largely acted on fish downstream of Bonneville Dam and during the marine component of their life cycle, and not in the river upstream...

So these authors seem to think that the marine component of the life cycle of these 2008 stocks was advantageous, whereas I think your analysis, which did not include the Columbia, comes to an opposite inference; would that be correct?

DR. PETERMAN: Well, 2008 was certainly included in the period where the general trend was downward in productivity for the stocks -- most of the stocks that we looked at, yes.

Q Yes.

DR. PETERMAN: But I would have to look at each individual residual for 2008 in each stock to see whether there was an unusual --

Q Yes.

DR. PETERMAN: -- upward bump.

Q Yes. Is there a tendency in this field of fish population dynamics amongst the scientists examining the data if they can't say for certain there's a factor in the freshwater they tend to say, "Oh, must be in the marine environment," is that a common scenario?

DR. PETERMAN: Well, I guess it's a matter of logic. So if you have the data on abundances in freshwater at some point and you find that there's no explanation of the change in the overall lifecycle survivor rate from spawners to recruit that's associated with the change in the freshwater environment, then by deduction you would say, "It must be in the remaining part of the lifecycle."

Q Yes.

DR. PETERMAN: So I don't think that's illogical.

Q And basically when one expresses that conclusion, one can never be proven wrong, because it's very difficult to determine what goes on out in the North Pacific?

DR. PETERMAN: Well, you could be proven wrong if someone else comes along and finds out that, well, in fact, it was a freshwater pathogen that was picked up in the juvenile stage that didn't cause mortality until the marine environment.

Q Yes, all right. Just one final reference here, at page 5, if I may. Page 5 relates to freshwater production at the top. It says:

> The Osoyoos/Skaha Lakes system is more productive and is about 5 times larger than

Lake Wenatchee (Mullan 1986). Consequently, natural smolt production is higher in Lake Osoyoos than in Lake Wenatchee -

-- gives the weights --

Smolts leaving Lake Osoyoos are also larger on average than those leaving Lake Wenatchee.

Those are the sort of smolts that you would expect to do well in the marine environment, are they not, the ones leaving the Osoyoos system?

DR. PETERMAN: Well, in comparison to Lake Wenatchee.

Q Yes.

DR. PETERMAN: Is that what you mean?

Q Well, yes, I guess so. They clearly do better than the Lake Wenatchee product.

DR. PETERMAN: There's a huge range in body size of smolts across stocks in the west, so I can't say how those would compare with all the others.

Q Yes.

DR. PETERMAN: But compared with the Lake Wenatchee, apparently, yes, they're five times larger.

Q Does this paper illustrate the reasons supporting your recommendation that there be more communication and discussions with those in charge of fisheries management in the U.S.?

DR. PETERMAN: Well, certainly this is an example of one of the many documents that probably wouldn't have passed our way if this hadn't come about through the Commission. These internal government documents are what we call the grey literature and they're often lagged in their availability by considerable time or maybe even buried in a way that we can't find them.

Q Except this isn't a ringtail document, I found this on the internet.

DR. PETERMAN: Yeah. Well, like I said, unless we all sit scanning all the government websites --

Q All right.

DR. PETERMAN: -- it's hard to keep up with what gets put there, and not everything gets put on the websites, I'll tell you.

MR. HARVEY: I wonder if we could mark this as the next exhibit, please.

THE REGISTRAR: Exhibit 752.

MS. BAKER: Before it's marked, I'm not sure -- the

basis for marking it was the witness hadn't seen the document before and didn't agree with what was in it except to agree that the words said that they -- what they said. So I'm not really sure what the basis of marking this an exhibit is.

MR. HARVEY: Well, it adds, usefully, to the database to be considered by this Commission.

THE COMMISSIONER: Sorry, what is the exhibit number, Mr. Giles?

THE REGISTRAR: This would be 752.

EXHIBIT 752: Paper by Northwest Fisheries Science Center, titled, Factors effecting sockeye salmon returns to the Columbia River in 2008

THE COMMISSIONER: Thank you.

MR. HARVEY: Now, I just -- there are just two more I want to refer to. One is -- no, one more. That's at Tab 9. This is a ringtail document, and you've been given notice of this, relating to Rivers and Smith Inlet. There was some discussion I had with you, yesterday, about the reasons for an increase in productivity in the late '90s. At page 002 in this paper, just below the graph, on the right-hand side, and it says:

> However, total returns declined dramatically in 1994,

that's after -- perhaps I should have read the top four lines just above the graph:

> Total sockeye salmon returns generally increased over the same period, setting records of over 800,000 and 900,000 fish as recently as 1991 and 1992, respectively.

And then it notes the dramatic declines in '94. And then reduced escapements. Is this a paper that you had considered when we discussed Rivers Inlet yesterday?

DR. PETERMAN: Yes, I had read through it.

MR. HARVEY: All right. Could that be marked, please, as the next exhibit.

THE REGISTRAR: Exhibit 753.

EXHIBIT 753: DFO Science, Stock Status Report, Rivers and Smith Inlet Sockeye, dated January 1997

MR. HARVEY: Those are my questions, thank you.

QUESTIONS BY THE COMMISSIONER:

Q Dr. Peterman, as Mr. Harvey is finishing, I just thought I'd ask you just to go back to an answer you gave. I don't, and I apologize if I'm not directly articulating the scope of the question he asked you, but I just want -- he gave you a general question and you gave a general answer, so fair enough. But in terms of how discussions around escapement fit with conservation, he directed you to the role that biologists that might play, or the view that biologists might have about escapement and how to express their views on escapement and the trade-offs that he mentioned around the economics surrounding those kinds of discussions.

But I'm not sure I fully understood your answer. I'm not sure I fully understand what role biology plays in escapement, and I say that because escapement is part of the management process that I've heard about for many weeks.

DR. PETERMAN: Right.

THE COMMISSIONER: And a great deal of involvement by managers who might be biologists as well as non-biologists in the escapement strategies, whether you call it "target" or "goal".

I just want to make sure I understand the role that biologists play with respect to this term called "escapement", and if you could also explain to me whether delayed dependency is a strictly scientific phenomenon, or whether it also -- there's an intersection between management and science with respect to that phenomenon.

DR. PETERMAN: Okay, sure, I'll try to answer those questions, and maybe I'll start with the last one first, and I would say that -- and Brigitte, of course, can add to these comments.

I would say that with respect to delayed density dependence, where it intersects with management, is that if, as we've suggested in our report, in the Quesnel system there is an effect

of previous year spawner abundances on the productivity of this year's spawners, then the setting of the escapement goals for all those previous years should take into account their potential subsequent effect.

And so -- and the effect on productivity comes into the management in the sense that the managers are trying to meet many objectives that are over and above simply meeting the biological escapement requirements, namely, what benefits can we get out of the fish in terms of economic social benefits. So the managers are considering multiple objectives from multiple parties, as you probably well know. So that's where that intersection comes in.

With the delayed density dependence, it certainly would affect the scientific advice that goes to the managers. So, as you probably already have heard, also, in all the agencies, the scientists really have a very defined role, the biologists, to provide that scientific advice to the managers and, I hope, interact with them to make sure the managers understand the advice, and the uncertainties in the advice, in particular.

So where the notion of escapement comes in, from a biologist's point of view, is saying, "Okay, what abundance of spawners would be required to meet particular management objectives?" So it's not possible to answer the question, "What should the escapement target be," without some objective. I know you mentioned in some other hearing where I was, it's like a business. You have to have an objective. You have a clear target that you're moving towards, and a way of measuring it quantitatively to see how close you are to reaching that target, right?

So if you set an escapement goal and say, "The only thing we're interested in is maximizing the commercial fisheries dollar revenue from this stock," you would probably come up -- well, almost by definition you would come up with a different target number than if you were interested in maximizing the First Nations benefits from the salmon, or the recreational benefits, or you've probably heard about the nutritional benefits to the ecosystem. Those are all different objectives. And the weighting that the managers

might place on those probably differ from person to person or place to place, or even time to time, for that matter.

So in that context, the biologist's role is really to take into account what they've learned about what the objectives are to help provide the appropriate scientific advice and the uncertainties in that advice.

Q Now, again, with apologies to Mr. Harvey, because I don't think I can rearticulate what he asked you, but in terms of understanding the spatial analysis you did and the results that you have brought to this Commission, is it necessary for those who look at those results to understand not only the science that is happening around those results, but also these objectives that you're discussing, as to how they might have had some impact on those results, for example, Alaska, State of Washington, or perhaps other jurisdictions?

DR. PETERMAN: Mm-hmm. I would say that our results would not be influenced by the objectives, because what we're trying to focus on here was the biological index of productivity, and solely that. So we're trying to ask, what were the temporal patterns and the spatial patterns of changes in recruits per spawner, or the various measures that we had of that. And I'd like to just draw back to the analogy that I started the hearings with, the auto plant. So as you all know, different companies that make autos might have slightly different ways of doing things, doing business, they might have slightly different objectives, different markets they're trying to reach, and so their targets for what they might have in terms of number of workers on the plant floor will influence not only how many cars they put out, but maybe even the number of cars put out per worker.

So that would be influenced by the introduction of robotics, for instance, to increase the efficiency per worker. That's going to change that productivity measure, which is the same thing as things we might see effecting the recruits per spawner in the salmon world.

In the salmon world, the only management actions that I can think of that would affect that would be, as I mentioned yesterday, the influence

of hatcheries, lake fertilization, spawning channels. Those sorts of things are intended to increase the survival rate in the early part of the life history, with the assumption that that's going to carry through to the end of the life history and bring back more adults.

So Brigitte, I don't know if you have anything to add to that?

DR. DORNER: No, I just reiterate that if we had just looked at abundances then, yes, the management decisions would affect that, but since we looked at productivity I think it's very little effect, apart from those exceptions that Randall just mentioned.

THE COMMISSIONER: Thank you, Dr. Peterman.

DR. PETERMAN: Okay, you're welcome.

THE COMMISSIONER: Mr. Harvey?

MR. HARVEY: Mr. Commissioner, could I ask a follow-up question to that?

THE COMMISSIONER: Yes, of course.

CROSS-EXAMINATION BY MR. HARVEY, continuing:

Q Dr. Peterman and maybe Ms. (sic) Dorner, you would have something to say on this, too. I didn't understand, Dr. Peterman said that the -- with respect to the clear goals and targets, if the target is maximizing commercial fisheries, then that would lead to one set of decisions, or something like that, but you have a different number, if you're maximizing recreation or First Nations.

I've assumed, and it's inconsistent with this answer, I've assumed that the maximum sustained yield of sockeye, in the sense of the maximum that our freshwater system can support and the marine environment can support --

DR. PETERMAN: Mm-hmm.

Q -- would satisfy all objectives, the commercial fishery, the First Nations, and the recreation, because all are interested in their -- in increasing the abundance of the sockeye runs so that they have, if they have a certain percentage share or whatever, they can increase their share, their numbers; is that correct?

THE COMMISSIONER: Sorry. Ms. Gaertner?

MS. GAERTNER: Mr. Commissioner, maybe this is a useful

time. I heard Mr. Harvey say that -- yet last week, in a question to Karl English, that everyone in this room agrees with maximum sustainable yield, and I think this question, again, flows from that misunderstanding. I was quite surprised, and when my clients heard it, they were quite surprised, and they have asked me to clear the record. That is a misunderstanding on the part of Mr. Harvey, and I think that that might be useful for him to consider in his questions --

MR. HARVEY: All right.

MS. GAERTNER: -- and his approaches.

MR. HARVEY: Well, that's all the more reason why Dr. Peterman should explain what he means by his answer.

DR. PETERMAN: Sure. Okay. Well, I guess, from my perspective, yeah, the concept of maximum sustainable yield is that there's an available surplus, if you will, to what the biological requirements are for replacing the spawners, and that could be allocated in various ways. But, if you get down to the details of the practical implementation of that concept, you're going to have the problem of mixed stock fisheries.

In the allocation of catches among the different user groups, let's just stick with the three largest ones, the commercial, First Nations and recreational, might be dependent on what mix of stocks you have at various levels in the mixed stock fishery, and that would be influenced by the escapement targets on each of those systems.

So I guess that's what I was thinking of.

Q So you're thinking more of the place of harvesting, is it? Because --

DR. PETERMAN: For the timing.

Q And assuming that First Nations would only wish to harvest right on their doorstep, and similar with recreational fishers up the river would want to harvest in that point rather than moving downstream, or --

DR. PETERMAN: Well, no, I'm just thinking that different stocks come in at different times and at different places, of course, and so they're vulnerable to different fisheries, accordingly. So a manager would take those different aspects of the total returns that might be part of MSY and consider that complexity.

Q So in the details there might be some variation, but generally, surely, the maximum number of returns, for example, the 2010 return was far more beneficial to all user groups than the 2009 return?
DR. PETERMAN: Certainly, yes
MR. HARVEY: All right, I think that's pretty much it.
THE COMMISSIONER: Thank you, Mr. Harvey. Ms. Baker, did you want to take the break now?
MS. BAKER: Sure.
THE COMMISSIONER: We could do that, sure.
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.
MR. ROSENBLOOM: Thank you very much. My name is Don Rosenbloom and I appear on behalf of Area D Gillnet and Area B Seiner.

CROSS-EXAMINATION BY MR. ROSENBLOOM:

Q Dr. Peterman and Dr. Dorner, I have a series of questions that I will naturally direct them to Dr. Peterman, but, Dr. Dorner, at your end if you wish to answer, make a contribution in respect to any of the answers being given by your colleague, Dr. Peterman, obviously I invite your analysis and opinions.
DR. DORNER: Okay. I shall do that.
Q Thank you. Dr. Peterman and Dr. Dorner, in reviewing the studies, scientific studies that have been commissioned, or the projects that have been commissioned by this inquiry, by this Commission, in terms of scientific analysis, can you tell me whether any of the subjects from Project 1 through to 12 are focused on the issue of carrying capacity of watersheds, of nursery lakes? And I invite you to look at the preface to your report, Exhibit 748, the first page of the preface. Because I don't see it. And I want to ask you a few questions that arise from that situation, if indeed you do not see a scientific study being done in respect to that issue.
DR. PETERMAN: I'm not sure whether the third one

wouldn't do that. If it's going to be covered anywhere, I would think it would be there. That's the "Fraser River freshwater ecology and status of sockeye Conservation Units". But so I haven't seen the report. I don't know if it's done yet, so I'm not sure what they did.

Q Well, you've been part of a collective of scientists that were authors of these papers.

DR. PETERMAN: Yes.

Q Who have talked out things in a workshop. Do you recall the matter of carrying capacity of water systems to be part of the discussion at that workshop?

DR. PETERMAN: Quite frankly, I don't remember. That workshop, yeah, it was a one-day workshop -- or, no, I guess it was two days, but I don't remember the carrying capacity issue coming up. That's not to say it wasn't discussed.

Brigitte, do you remember at all?

DR. DORNER: Yeah, no, I don't think so, the beginning, I don't think it was discussed.

Q And if I am wrong in my cursory review of these papers, I invite Commission counsel to stand up and correct me that indeed one of these papers, paper 3 or otherwise, is dealing with this issue. Assuming that it isn't, is this not a critical and important issue obviously in terms of population dynamics or production measures for this Commission, and indeed at the end of the day for managers of the resource?

DR. PETERMAN: Well, yes, knowing how many fish a freshwater system can produce is obviously part of the scientific knowledge that goes into providing the advice to managers, but I wouldn't say it's the only one. Clearly there is other major issues of a biological nature. But, yes, I would agree that having a good estimate of the carrying capacity is important.

Q Yes. And so if it isn't the subject of investigation by the Commission up to this point in terms of scientific assignments to these scientists, do you believe that it is important that the Commission elicit evidence in respect to that question?

DR. PETERMAN: Well, I'm wondering whether the answer to your question might come out as a side product of other studies, even though there wasn't a

specific topic aimed at that. So the kind of evidence that we were just going through before the break on estimating the escapement that would give rise to maximum number of smolts, or maximum biomass of smolts, pardon me, would be heading in that direction, and that there have been those analyses.

Q But you would agree with me that the subject deserves something other than peripheral treatment, for obvious reasons?

DR. PETERMAN: Sure.

Q Yes. And you, sir, in particular, have a special interest in carrying capacity. I understand that you have co-authored a study at least in the marine environment in terms of carrying capacity; is that not correct?

DR. PETERMAN: Yes, it related to density-dependent growth on the high seas, interactions among salmon, yes.

Q And so you, better than anyone, understands the significance of carrying capacity issues and thresholds in terms of carrying capacity in the realm of harvest management.

DR. PETERMAN: Well, I don't know if I understand it better than anyone, but I'm familiar with it, as is Brigitte, I think.

Q Of course. Now, my learned colleague, Mr. Harvey, has covered a great deal in respect to issues of common interest to a number of us at this Commission, but I do have a few added areas to cover in respect to escapement issues. Firstly, there's been a focus from time to time in this inquiry whether over-escapement has led to a catastrophic event, and that has -- we've been drawn into that debate, in part because of the paper that Drs. Walters and Riddell did in 2004. Yesterday some evidence was put to you in terms of transcript of Dr. Walters and Dr. Riddell regarding their feelings today about it, in light of information they have acquired since 2004.

Putting aside the issue of whether over-escapement can lead to catastrophic result, you would agree with me that it is terribly relevant whether over-escapement might simply be detrimental to productivity in terms of the sockeye salmon, obviously. In other words the threshold need not be catastrophic. But surely it

is terribly important biologically to determine whether or not an over-escapement can be detrimental to productivity. I think I state the obvious, do I not?

DR. PETERMAN: Yes, yes, yes. I think I would agree with that.

Q Then in terms of your findings about Quesnel, and it may be best to go directly to your report so we understand the context. As I understand it, from your analysis, and I want to go into the methodology of your analysis in a moment or two, you say in part at page 45 of your report, again Exhibit 748:

> To summarize our analysis of density dependence, we conclude that although there is evidence of both simple and delayed density dependence for many Fraser stocks, our results do not support the general hypothesis that efforts to rebuild Fraser populations in recent years may have resulted in "over-spawning", thereby causing substantial declines in productivity for these stocks. The only exception to this generalization is the Quesnel stock, which shows evidence of both delayed density dependence and patterns of spawner and recruit abundance that are consistent with the hypothesis that recent declines in productivity are attributable mostly to increased spawner abundance.

Let's accept for a moment your findings here, and your opinions as stated in that paragraph. Even if it is your opinion that only the Quesnel stock shows the phenomenon that we're talking about here, that in itself is very consequential, is it not, in terms of biological analysis, because it is a warning or an alarm that goes off in terms of what happens when there is a significant over-escapement. Do you agree with that?

DR. PETERMAN: Well, I would say it's an alarm that goes off that says any biological analysis of future returns from the Quesnel system should take into account these interactions across brood years.

Q Yes. And it is an alarm that should go off

generally in terms of reviewing harvest management with other systems, too, especially systems where there's a delayed density dependence.

DR. PETERMAN: Well, yes, if there is delayed density dependence. But as we said in our report, there's no evidence of that for anything other than the Quesnel stock at the moment. I mean, it could be ten years from now we have new data, might come to a different conclusion.

Q Well, and I'm speaking beyond at the moment. I'm talking about the future of harvest management. It is surely an issue.

DR. PETERMAN: Yes.

Q Thank you. Now, Mr. Lapointe, I believe it was Mr. Lapointe -- or you obviously are very familiar with him, it is Dr. Lapointe? I apologize.

DR. PETERMAN: No. No.

Q Mr. Lapointe. Mr. Lapointe came before this inquiry back in December and I want to inject into our discussion with you today an opinion that he made at that time. And I'll be referring to a transcript of January the 19th -- if I said last year, his testimony was January the 19th of this year, and at page 36. And before I go to the direct quote, basically what he has warned about in his testimony is that the consequences of over-escapement can have a detrimental effect, not only on sockeye salmon, but on other fishes within the system. And I want to put the passage to him (sic), and I want your comments about it.

DR. PETERMAN: Okay.

Q And it's at page 36 and it is at line 25. It was an examination of Mr. Lapointe by my learned friend, Mr. Leadem. At line 25 he said:

> I think we do anticipate that if we put a really, really large number of predators, which is what the sockeye -- the sockeye are when they're in their lakes, the juveniles, in that ecosystem it's going to have an impact on that ecosystem.
>
> Quesnel sockeye is a perfect example. Quesnel sockeye impacts of that -- of this build-up of the Quesnel run have not just impacted the number of fish that came back in 2006. The Kokanee population in Quesnel Lake

has collapsed. The large trout population in Quesnel Lake has [fundamentally] also been impacted.

Unquote, and it goes on from there. You'd have no reason to dispute the concern that Mr. Lapointe makes in respect to a fallout of consequence to other fishes within a watershed when there is a certain over-escapement.

DR. PETERMAN: Well, no, I don't disagree with his concern.

Q I didn't expect you would. Thank you. Now, moving --

DR. PETERMAN: Brigitte, did you want to comment at all on this?

DR. DORNER: No, same thing here. Of course.

Q Thank you. The next thing I want to deal with is methodology in terms of the approach that you have taken with this paper. And what I understand from the paper, and forgive me, I'm not a scientist.

DR. PETERMAN: Sure.

Q Thank you. Is that you have applied a filter called a Kalman filter in doing your analysis And in applying this filter you have placed a caveat, and this is my term, not yours, on the application of the Kalman filter. And I refer to page 25 of your report, and I'm approximately halfway down the top paragraph:

> The Kalman filter then attributes to "noise" the part of the time series variation (in recruits per spawner) that does not conform to the patterns allowed by the interaction of the observation and system models. Because our knowledge of the properties of the signal and data errors is imperfect, the model specified in the Kalman filter is necessarily also imperfect, and the Kalman filter therefore sometimes filters out some of the short-term variation in the signal, i.e., true short-term variation in productivity, and may also let some of the noise pass. In practice, this means that major peaks and valleys in productivity may sometimes appear "smoothed out", or conversely, that the filter may fail to remove blips that distract from the overall pattern.

And you go on from there. My question is this, sir, and in fact to Dr. Dornan. You have participated in various workshops with other scientists over the last year and a half regarding where your paper was going, and in particular in the application of the Kalman filter, have you not.

DR. PETERMAN: Yes.

Q And in particular, sir, you participated in a science review, in fact to be more specific about it, it was the Scientific Advisory Committee meeting last June, I believe. Do you recollect being a participant in that?

DR. PETERMAN: Is that the one where I provided a review for the Sue Grant et al 2010 --

Q Yes.

DR. PETERMAN: -- pre-season forecast for Fraser sockeye?

Q I believe so. And to try to get to the quick here, will you agree with me that you have learned through your participation in that forum and in others that there is controversy within the scientific community as to whether or not the Kalman filter is appropriately utilized or whether it indeed imposes a significant bias that should disqualify it from application.

DR. PETERMAN: Ah. Okay. So I'm not sure specifically what you're referring to, but the controversy might be related to the fact that we're assuming something about the underlying structural form of the relationship between spawners and recruits, which is true of the Ricker model and any other model, for that matter.

The bias that you refer to might have to do with the fact that Carl Walters claimed, and he was on speakerphone from Florida -- no, I think he was at UBC at the time, actually, because this meeting was in Nanaimo. But he was pointing out that in fact the Kalman filter does not show as rapid of a decline as in fact might be happening in nature, that it was lagged. And I agree with that.

Brigitte and I were just talking about this the other day, that if you look back at our 2000 paper, the one I published with Jeff Grout and Brian Pyper, we showed by a simulation that in fact if you specify a true known change in

productivity in the model, and then you generate some data that the Kalman filter uses to estimate that change, the Kalman filter lags behind what the true change was. Is that the bias you're talking about?

Q Yes. And would you agree with me that Dr. Walters in conversation with you basically suggested to you that the Kalman filter was badly biased towards underestimating the severity of declines in productivity that some stocks have suffered.

DR. PETERMAN: Right. That's what he said, and that's what I'm saying we saw in -- I wouldn't say grossly underestimating the severity, but it's definitely underestimating that when the decline started, and but not necessarily how rapidly it was declining.

Q Right. But you were accepting generally of his scientific concern?

DR. PETERMAN: Yes. As I said, we published basically that result in 2000. But I don't know, Brigitte, you look like you've got something else to add there.

DR. DORNER: Yeah, two things. When he was making that remark and also with the 2000 paper, you were predicting, whereas we are just reconstructing.

DR. PETERMAN: Ah, right.

DR. DORNER: Bias is quite as bad that way because we're doing the backwards moving. The other point I would like to make is that we looked at the also the residual time series and various averaging methods for the residual time series and basically didn't see any substantial differences in the trends that we reconstructed. So as far as we are concerned, the issue is pretty much a red herring as far as these particular data are concerned.

Q As a result of what you heard in that workshop, did you make any modification to the study that is now before us here, or did you not take that into account, the critique, for want of a better term, that Dr. Walters brought to your discussions?

DR. PETERMAN: Well, we did take into account in terms of interpreting the data, as Brigitte was describing. We checked to see whether our Kalman filter estimates really reflected what the residuals were showing. And remember the residuals are less -- well, it's just a different measure of productivity. But we did not modify

the method for estimating these Kalman filter productivities, or, pardon me, the Kalman filter is an estimation method. We did not change that estimation method, there's no way to change it, there's only one way to do it.

And so the fact is that, as Brigitte said, we then checked, I guess, plots of the residuals, what we were seeing in the smoothed Kalman filter estimates to see whether there was any major discrepancy, and we did not see it. So we have confidence that the results shown by our Kalman filter smoothed trends are the best estimate of the actual trends that are available.

Q I don't want to take too much more time, but you would agree with me that the Kalman does not properly represent abrupt changes like those documented in what's upcoming Report 4 to this Commission.

DR. PETERMAN: Yes. Well, that again we showed that in our 2000 paper that if we specified something like a step function, where -- which is sort of like we saw in the 1976, '77, when the ocean changes dramatically and very quickly to a new level of productivity, then the Kalman filter will lag behind in representing that, just like the residuals would. In fact, the Kalman filter responds faster to that change the residual measures.

Q Yes.

DR. PETERMAN: That's one of the things that we showed in that paper. That the reason we used the Kalman filter in several papers subsequent to that 2000 paper to reconstruct the historical trends in productivity was it was shown to be, by a simulation analysis, the best method to track changes, regardless of whether they were quick changes or slowest. But it's true that when you have a step function, something changes dramatically, you're going to lag behind estimating that change.

Q And because the filter does not properly represent those abrupt changes we're just talking about, it's replacing the changes with estimated smooth trends. Would you agree with that.

DR. PETERMAN: Yes.

Q Yes. And the importance of these biases is that they invite misinterpretation of the productivity

data correlations between this data and various possible causal factors. Do you agree with that?
DR. PETERMAN: No, not quite. Because it turns out that when you go to ask about correlations with something like a time series of contaminants, you don't use the smooth Kalman filter. You use the unsmooth Kalman filter values, and that might seem like a technical detail, but it is an important one. And so the unsmooth Kalman filter values are actually more variable than the smooth ones, and they're a better characterization for the type of analysis you're showing, or asking about now.
Brigitte, did you want to add to these last few comments I made? I didn't get back to you.
DR. DORNER: For the correlations you would also use residuals in addition to the Kalman filter values to just confirm your results.
DR. PETERMAN: Mm-hmm.
Q Thank you very much. Now, Dr. Peterman and Dr. Dorner, you report that relatively few stocks still fit the Larkin model, correct, in terms of delayed density effects, and after correcting for productivity trends and using this Kalman filter that we're talking about, would you agree that you failed to note that these relatively few stocks that represent -- that we're talking about, that do fit the Larkin model, represent the vast bulk of the total sockeye production. Do you agree with that?
DR. PETERMAN: Well, I guess we didn't say that explicitly but anyone who's familiar with the data will know that.
Q Right. So I'm not faulting you for it, but you would agree with me that the three stocks that you identify as fitting appropriately within the Larkin model with the delayed density dependence, are the major stocks in terms of production for the Fraser River.
DR. PETERMAN: No, sorry, not true. The late Shuswap is the major producer in the whole system and it did not have the Larkin model fit best, as I recall here. I'm just looking through my notes.
DR. DORNER: There was no clear evidence either way.
DR. PETERMAN: For the Shuswap.
DR. DORNER: Yes.
DR. PETERMAN: So in other words, the Ricker and the Larkin models fit equally well for the Shuswap.

DR. DORNER: Within the four AIC points.
DR. PETERMAN: (Indiscernible - overlapping speakers).
Q But the others are major producers, obviously.
DR. PETERMAN: Yeah, the Chilko is definitely a major producer.
Q Yes.
DR. PETERMAN: As is Quesnel.
Q Yes, thank you. Now, I want to move on very briefly. You speak in your paper of a major new finding, and that is that you have brought before us a correlation or analysis to show that the decline in the sockeye stock of the Fraser is common to declines taking place in Washington State and Northeast Alaska, and you say that's a new finding. I'm surprised it is a new finding. And let me ask you these series of questions briefly. Surely the Alaskans were totally familiar up until recently with what was their productivity or recruit per spawner index for their watershed within the Northeast.
DR. PETERMAN: Actually, okay, so by "Northeast" I think do you mean North-eastern Pacific or Southeast Alaska?
Q I'm sorry, Southeast Alaska.
DR. PETERMAN: Yeah, yeah, you're right. Okay.
Q Thank you.
DR. PETERMAN: So, yes, but, you know, believe it or not, I actually got those data on the Southeast Alaskan sockeye and the Yakutat Peninsula sockeye from an Alaska Department of Fish and Game Biologist named Doug Eggers, just fortuitously at a meeting he was here in Vancouver. I can't remember which meeting it was, and he told me he had just compiled these data. And I said, "Oh, wow, that would be wonderful if we could have those data for our analyses, too." And he gave them to me. And I said, "Well, what have you observed in the data?" He said, "Oh, the productivity's going down dramatically in most of those stocks." I said, "Oh, that's really interesting." And that -- that sounded like he had just found that out himself.
Q Well, I don't want either you nor I to cause an international incident, but surely, they're not asleep at the switch up in Alaska if there is a dramatic decline in their productivity within a major portion of their territory, of their

watersheds.
DR. PETERMAN: Ah. It may be a major portion of their territory, but it's not a major portion of their stocks.
Q Yes.
DR. PETERMAN: So those stocks are actually very small in abundance compared to the Bristol Bay stocks, which are doing fantastically well --
Q Yes.
DR. PETERMAN: -- over the last decade.
Q So you're saying because the productivity generally, the volume of the abundance within the Southeast is a small portion of their total productivity in Alaska, they may not be paying a great deal of attention, or more to the point --
DR. PETERMAN: That's right.
Q -- not being terribly concerned about it; is that right?
DR. PETERMAN: Well, they might not have -- apparently, from what Doug Eggers implied, or he may have said it explicitly, that he was -- not recompiling, he was compiling these data as if it was the first time to really fix up the whole dataset. Not saying the first time the data had ever been looked at, but as you may know from talking to other biologists in other hearings, people go back and make various corrections at various times. They learn something new from their recent sampling about where the adults were at various times. So they correct their age structure and they correct their stock identification and reallocate some of the fish among stocks differently than they had before. So I sort of assumed that's what was going on when he said, "I've just compiled these data for all these small stocks," because he'd done that new analysis.
Q What about Washington State? Surely they were well aware --
DR. PETERMAN: Oh, yeah.
Q -- of these issues.
DR. PETERMAN: Oh, yes, I'm sure. Lake Washington is a very well-tracked system down there.
Q Yes. And what has been the response in Washington in terms of both state and federal government to a dramatic decline in productivity there? We know what the response is here, in terms of the government of the day appointing this Commission.

What has been their response to this, if it is as dramatic as you now report?
DR. PETERMAN: I can't tell you what their response is. But to be honest, the sockeye there in Washington are a very minor player compared to coho and chinook.
Q All right. And then we come to British Columbia, non-Fraser. You again say the same pattern observed in terms of the rest of the province. Is it to the same dramatic degree as we're dealing with in the Fraser, and if so, might this Commission have, if given the terms of reference, really been looking at the entire province?
DR. PETERMAN: Well, that's an interesting point. Brigitte and I have talked about this in the past. It seems rather ironic that the problem that stimulated the establishment of the Commission was the Fraser River poor returns in 2009, which were represented, or well-known to be part of a longer-term trend, when in fact if the other biologists in the region had been asked to put their data forward, it might have been clearer that this was a wider problem.

And in fact we were disappointed that we weren't asked to come to speak to this Commission earlier, for that reason. We thought, well, let's get the scientific facts first correctly laid out as to what has happened, both for the Fraser and elsewhere, to see is it unique to the Fraser or is it not? And that would help frame the questions that the Commission is addressing more succinctly, or maybe more appropriately.
Q But you appreciate that the Privy Council's terms of reference for this Commission --
DR. PETERMAN: Yeah.
Q -- is limited to the sockeye. But I guess what --
DR. PETERMAN: No, I know that.
Q -- you and I are speaking about is that if one were redrafting the terms of reference today, knowing what we know from your report today, in fact the issues we're focussing on for the Fraser might also be very much a -- could very much be a matter of focus for the entire province.
DR. PETERMAN: Yes, definitely.
Q Thank you.
DR. PETERMAN: Brigitte, did you want to add to that, at all?

DR. DORNER: No, I agree.
DR. PETERMAN: Okay.
THE COMMISSIONER: Oh, you've just lost her there.
MR. LUNN: She's still on the phone.
MR. ROSENBLOOM:
Q You have also spoken in testimony and also with your report to the Harrison, and if we could learn things from the Harrison.
DR. PETERMAN: Mm-hmm.
Q The Pitt River Watershed, I understand there's an enhancement program which maybe skews the --
DR. PETERMAN: That's right.
Q -- analysis there. But there is a wild stock there, isn't there.
DR. PETERMAN: Yes. And in fact I had trouble figuring out what portion of the Pitt sockeye comes from the hatchery releases, as opposed to the wild stock. And I went back and forth a few times with the DFO biologist there. But there is, I would say, the majority of these fish are hatchery derived.
Q but I gather some are wild.
DR. PETERMAN: Yes.
Q And I gather that they, too, have had an impressive productivity index, as opposed to most of the other stock we're talking about in the (indiscernible - overlapping speakers).
DR. PETERMAN: Well, impressive meaning upward trend?
Q Yes.
DR. PETERMAN: Yes, that's right. That's shown in our Figure 9. Did you want to look at that or just --
Q I don't need to right now.
DR. PETERMAN: Okay. All right. Fine.
Q So in fact if we have lessons to learn from the Harrison, we may have lessons to learn from the Pitt wild stock, too, don't we, or might we.
DR. PETERMAN: Well, it's always worth asking when, I guess, Mr. Leadem was asking yesterday or pointing out that sometimes outliers are very important, if I remember who made that point, and so those are outlier stocks in our time trends and so certainly it's worth looking at them more carefully to find out why are increasing in productivity where the rest of them are either constant or going down in productivity.
Q All right.
DR. PETERMAN: Absolutely.

Q And I taught at the law school for 20 years and I taught my students never to ask questions they don't already know the answer, but I'm about to break that rule --
DR. PETERMAN: Okay.
Q -- in a series of questions about Pitt River. In respect to the Pitt River, am I right, unlike the Harrison, the Pitt River sockeye do not migrate out as fry.
DR. PETERMAN: As far as I could tell, that's correct. I asked that question specifically of Michael Lapointe at the Pacific Salmon Commission and I think that was the answer that I got.
Q So where with the Harrison one might focus in part on their what I'll call unique behaviour, in terms of migratory movement and timing of migratory movement, with the Pitt they appear to have a more traditional dormancy in the nursery areas?
DR. PETERMAN: That's right.
Q Is there anything that comes to your mind that distinguishes the Pitt stock, again wild stock, from generally the rest of the stock of the Fraser?
DR. PETERMAN: No, I didn't pursue that, and I don't think Brigitte did, either. I think she left it to me, actually. So I was focusing on the fact that once I found out that it was mostly a hatchery stock, I kind of discarded it from further consideration because we were trying to focus mostly on what are the wild stocks doing.
Q When you bring forward figures about the Pitt stock and reproductivity, in other words, your biological index of productivity, are those figures based upon wild stock of the Pitt River, or wild and enhanced stock?
DR. PETERMAN: It's the latter, wild and enhanced.
Q And so you are unable to distinguish in your work a wild from enhanced in terms of the productivity issues?
DR. PETERMAN: For the Pitt River, that's correct. We were not able to get those numbers separately.
Q I see. I come to my last area of cross-examination and that relates to your recommendations. And we hear loud and clear from you what I'll call a frustration that there hasn't been more of a uniform approach to data collection within inter-jurisdictional because of common

interests and so on and so forth. And you suggest obviously that the Commission consider recommendations that would bring about a more meaningful database for all you scientists up and down the coast from Washington, maybe even Oregon, all the way up to Alaska. In reviewing that and knowing more about political science than I do about biological science, does it make sense that there be an international commission established, much like the Pacific Salmon Commission, that would be mandated to do just what you recommend in your set of recommendations. In other words, that it not be left on the table for this Commission to simply make recommendations and then that might lead to the Government of Canada, DFO making a phone call to Alaska, and so on. But that there be a more formal structure such as similar to a Pacific Salmon Commission, so that that international commission would drive the objectives that you speak about and drive the research in a focused way. What's your comment to that?

DR. PETERMAN: Well, again, Brigitte might have some additional comments. But I'll start with the notion that I don't really favour setting up a whole new institution just for this relatively simple step of getting people to coordinate their data collection and data storage and quality control. That --

Q But doesn't somebody have to drive this?

DR. PETERMAN: Oh, certainly, but I would imagine that with appropriate amendment to the Pacific Salmon Commission's terms of reference or the North Pacific Anadromous Fish Commission's terms of reference, that might be possible to do within existing institutional structures.

Q Well, the Pacific Salmon Commission's jurisdiction is limited to the Fraser, isn't it?

DR. PETERMAN: I know that. Yeah, that's why I said an amendment would be required.

Q I see. So rather than invent a new commission --

DR. PETERMAN: Right.

Q -- you believe that one of these two bodies you've just mentioned might serve that purpose if they received the necessary statutory and international amendments.

DR. PETERMAN: Right. But in fact now that I think of

it, the Pacific Salmon Commission is not such a bad place to start with that amendment, because its jurisdiction includes looking at interceptions of B.C.-bound salmon in Alaska. There's lots of issues with chinook, for example, so that's the institution that's already dealing with the U.S.-Canada issues under the Pacific Salmon Treaty. So I would think this would indeed fit.

Q But, Dr. Peterman, would you not agree with me that whatever body it is that drives this has to have representation from the State Government of Alaska, obviously, and the Pacific Salmon Commission would not currently be structured to have that input, would they.

DR. PETERMAN: Well, that's true. I don't think they have an explicit separate representation, but that's a U.S.-Alaskan thing, as you know, it's whether the Alaskans like to be represented by themselves or by the Government of the U.S.

Q But wherever this goes, the driver has to be a body that has representation of the state interests in Alaska, the state interests in Washington State, the Federal U.S. Government, the Canadian Federal Government through DFO and possibly the Provincial Government of British Columbia. You would agree with all that, wouldn't you?

DR. PETERMAN: Sounds like a good idea. Yes.

MR. ROSENBLOOM: Thank you very much. I have no further questions.

DR. PETERMAN: Thank you.

MR. LOWES: Thank you. J.K. Lowes for the B.C. Wildlife Federation and the B.C. Federation of Drift Fishers.

CROSS-EXAMINATION BY MR. LOWES:

Q Dr. Peterman and Dr. --

DR. PETERMAN: Dorner.

Q -- Dorner, sorry.

DR. PETERMAN: D-o-r-n-e-r.

Q Sorry, Brigitte. I'm afraid I'm going to be a little bit repetitive, but I think in the interests of time and efficiency, it's better to be a bit repetitive than to try and cut and paste. So excuse me if you have been --

DR. PETERMAN: Okay.

Q -- if you have been asked some of these questions. At first my main task here, Dr. Peterman, is to try to establish the parameters of your report, what you tried to do and what you didn't try to do. Now, I'm suggesting, or if I suggest that your report is essentially contextual in this sense, that you provide a context for the examination of hypotheses about causes and potential solutions, or mitigating matters, rather than provide a hypothesis yourself; is that correct?

DR. PETERMAN: That's true, except for our analysis of the delayed density-dependent hypothesis.

Q Yeah, we'll get to that in a minute. But essentially the -- what you're doing here is you're providing a context for the investigation of hypotheses.

DR. PETERMAN: That's correct.

Q Yes. And essentially that context is provided by your discovery, if I can put it that way, or your presentation of a general downward trend in the smoothed out residuals of the Ricker and Larkin models.

DR. PETERMAN: Yes. Those residuals did show the trend, but not nearly as clearly as our Kalman filter estimates.

Q Yes, I understand that. And is "residual" another word for "anomaly"? Are the --

DR. PETERMAN: Yes.

Q Yes. So and that general downward trend is, according to your studies, irrespective of both abundance and geography?

DR. PETERMAN: I'm not sure what you mean by irrespective of abundance.

Q Well, you deal with a trend in production ratio rather than -- so you're not concerned with abundances. You're concerned with the ratio of spawners to recruits.

DR. PETERMAN: Or the ratio of recruits to spawners, yes, that's right.

Q Recruits to spawners. Yes.

DR. PETERMAN: That's right, yes.

Q Now, is it my understanding that for the purpose of your analysis and indeed the key to your methodology, you accept the Ricker and Larkin models and their underlying theory of population dynamics?

DR. PETERMAN: Yes. Any time you fit a model to data, you're assuming that the model's correct.
Q Right. And then you in fact take the underlying theory of population dynamics that's reflected in the Ricker and Larkin models out of the equation to see if there's something else that -- there's another story; is that right?
DR. PETERMAN: Exactly.
Q Right.
DR. PETERMAN: We're removing the effects that are represented by those models from the data to see if there's some other process or processes, plural, changing productivity over time.
Q Right. And that something else could be either an alternative to or additional to density-dependent effects.
DR. PETERMAN: That's right.
Q And in the case of the Quesnel, in your opinion, it's clearly additional?
DR. PETERMAN: Additional...
Q To the density-dependent effects, the downward trend.
DR. PETERMAN: No. In the Quesnel case we're saying that there is no downward trend in productivity if you incorporate the assumption that there's interaction between brood lines, which is what the Larkin model does. So that's why for the Quesnel, we ended up with the Kalman filter estimate of productivity showing a constant value for the last decade or so, I don't know --
Q Sorry, I think we're at cross-purposes here.
DR. PETERMAN: Okay. I think so too, yes.
Q I think that my question was essentially that what you're looking for is trends that are not explained by either the Ricker or the Larkin model. You're looking for trends --
DR. PETERMAN: Not like anomalies from what the Ricker and Larkin models would show.
Q Right.
DR. PETERMAN: Yes, that's right.
Q And your ultimate recommendation, as I take it, is for improvements to data to measure and analyse that general trend that you've identified.
DR. PETERMAN: Well, our recommendations include that, yes.
Q Well, that's the gist of your recommendation, isn't it? That this is significant -- this is a

significant analysis and significant information and it ought to -- and it needs a consistent database, and it needs a --

DR. PETERMAN: Yes.

Q -- long time series.

DR. PETERMAN: Yes.

Q And I'm recommending that steps be taken to acquire those.

DR. PETERMAN: Yes.

Q That's the crux of your recommendation.

DR. PETERMAN: Yes.

Q Now, that analysis and that recommendation is not inconsistent with the existence of local factors in addition to the general widespread trend that you've identified. You'd agree with me?

DR. PETERMAN: Well, in fact, yes, absolutely, and that's a very good point to bring up. That the reason that these trends are not identical is that there are local processes that affect one stock but not another.

Q Yes.

DR. PETERMAN: And so definitely the case that you have to recognize there is different scales of processes. We're implying that there's some large scale processes operating and then there are more local scale processes on top of that.

Q Yeah, and you've used the analysis of recent financial collapse.

DR. PETERMAN: Right.

Q And you've said that there are some general statements that you can make and there are obviously some specific ones. Is that correct?

DR. PETERMAN: Yes.

Q And depending on the particular stocks in your portfolio, some of the declines in those stocks may be better explained by local causes and some may be better explained by the general trend.

DR. PETERMAN: Yes.

Q Is that right?

DR. PETERMAN: That's right.

Q And so what you're dealing with ultimately, I suggest, is what you're recommending is the priority of research effort. You're saying that the general trend research into that should be given some priority.

DR. PETERMAN: Yes, that's right.

Q Yes. And again that's not inconsistent with the

exploration or research into specific hypotheses about local problems.
DR. PETERMAN: That's correct.
Q Now, you've obviously reviewed the transcripts of Drs. Woodey, Riddell and Walters' evidence in February the 9th and 10th; is that correct?
DR. PETERMAN: That's right, and I think Brigitte has, as well.
Q Yeah. And they have a pretty clear hypothesis about delayed density effects and the effect of over-escapement on those effects; is that correct?
DR. PETERMAN: That's the way I read the transcripts, yes.
Q Right. And you wouldn't want to be taken as discouraging their pursuit of that hypothesis.
DR. PETERMAN: No, of course not.
Q No. And indeed when you have three heavyweights like that in the -- in the biological sciences world, you would recommend that they pursue that hypothesis.
DR. PETERMAN: Well, I'm not sure what you mean by pursue the hypothesis. If you mean continue to do analyses on it, then, yes, that's --
Q That's what I mean.
DR. PETERMAN: Yes.
Q Yeah. And indeed you, I guess, or at least let me ask you this question, will recall that both Drs. Riddell and Walters talked about the significance of data, recent data, available to them, in terms of analyzing that hypothesis; is that correct?
DR. PETERMAN: Yes, they mention that.
Q All right. And some of that data is recent in the sense that it's recent events, that is, the 2009 and 2010 returns, is that correct, do you recall?
DR. PETERMAN: I'm not sure which recent data they were talking about.
Q Well, I'm going to suggest that they were talking about two different things. One was the apparent recent discovery of the Gilhousen report.
DR. PETERMAN: Oh, yes.
Q Which I understand shows data on returns and escapements from 1892 to 1944. Do you know that?
DR. PETERMAN: I've read about it in the transcripts. I do not know that document.
Q You haven't seen it.
DR. PETERMAN: I've not seen that document.
MR. LOWES: Yeah. Perhaps we could call up Exhibit

418, please.
MR. LUNN: Certainly.
MR. LOWES: And you can take it from me that -- well, you'll see that from the title it states the "Estimation of Fraser River Sockeye Escapements from Commercial Harvest Data, 1892-1944". Would you go to page 92, please. I'm afraid my eyesight's not very good. And if you would go to the last paragraph which states this. Could you highlight the -- bring up the last paragraph?
MR. LUNN: You can use the screen on the other side, too.
MR. LOWES:
Q
> These escapement estimates allow the calculation of total annual run sizes and of the production from various sizes of spawning populations in the historical period of the developed fishery. They also allow study of the phenomenon of quadrennial dominance during that period. However, such studies are beyond the scope of the present report and will be left for others to address.

And would you agree with me that if the others are Dr. Walters, Dr. Woodey, Dr. Riddell and even you, that certainly the suggestion of the author is a good one.
DR. PETERMAN: To pursue future research on those data?
Q On those data.
DR. PETERMAN: Yes. With a caveat, and the caveat is that people should not assume that the world is stationary.
Q No.
DR. PETERMAN: That is, we should not assume that conditions back in the 1800s to 1944 for rearing and in freshwater and the marine conditions are the same are they now.
Q Absolutely.
DR. PETERMAN: Okay.
Q But that's a pretty valuable dataset.
DR. PETERMAN: Apparently. Again I haven't seen it.
Q Well, you wouldn't disagree with if it is what it purports to be, that it would be a useful dataset.
DR. PETERMAN: Yes.
Q Yes. You would disagree or you wouldn't?
DR. PETERMAN: Oh, no, I mean I would not disagree.

Q Thank you. And the other, of course, new information is the events of 2009 and 2010.
DR. PETERMAN: Mm-hmm.
Q And I think you said that at the moment their implications are unknown but certainly over time they may have significant implications for your analysis and the analysis of others.
DR. PETERMAN: Yes.
Q Yes. Now, would you agree with me that cyclical dominance, if natural, is a dramatic example of delayed density effects?
DR. PETERMAN: That's one of the ramifications, yes.
Q Yes.
DR. PETERMAN: Well, let me qualify that, and Brigitte might want to add here, too. Any time you describe a biological phenomenon with a set of words, you have to assume something about the magnitude of that effect. So density dependence has a range of effects.
Q Yes.
DR. PETERMAN: From small to large.
Q Yes.
DR. PETERMAN: Same with delayed density dependence.
Q Yes.
DR. PETERMAN: And so I think with that qualification, I'll agree with what you said, but we're assuming here that we're talking about strong delayed density dependence across generations.
Q Yes.
DR. PETERMAN: Okay. Brigitte, did you want to add anything to that?
DR. DORNER: Well, yes. Cyclic dominance isn't necessarily caused by delayed density dependence. There are other mechanisms that could cause that, and vice versa, delayed density dependence can occur in situations where there isn't necessarily cyclic dominance.
Q Yes. I won't refer to the transcript, but I just recall asking Dr. Walters what was the relationship between cyclic dominance and delayed density of effects. And I'll take you to the transcript if you need to, but he essentially said that cyclic dominance was an example --
DR. PETERMAN: Right.
Q -- of delayed density effects. You would agree with that.
DR. PETERMAN: Yes. And that's what Brigitte was

saying, as well.
Q Good. Now, in your report at page 13 you describe -- no, not page 13, sorry. Well, let me deal with page 13 firstly. Well, in the interests of time, I won't.
In your report at page 8 you briefly set out four complementary approaches to understanding the hypothesized processes to explain the decline in the Fraser River sockeye production.
DR. PETERMAN: Yes.
Q Is that right? And you essentially applied two of the four.
DR. PETERMAN: Yes, the first two only.
Q Right. Now, I take it that an example of -- let's take the example of density dependent effects and delayed density effects, I take it that an example of, or at least a comparison of, or a tracking of cycle years for various stocks or groups of stocks, and statistically analyzing those, that would be an example of approach number 3?
DR. DORNER: If I can just jump in here, we did do a number 3 for delayed density dependence.
Q I understand.
DR. PETERMAN: Yes, that's right.
Q So the answer to my question is yes.
DR. PETERMAN: Yes, that's right. Thanks for that clarification. This has always been a subtle discrepancy between our sweeping statements of what we did and did not do.
Q All right.
DR. PETERMAN: Because the delayed density dependence is the only hypothesis that we examined.
Q I understand. And an example of (4) field experiments would be increasing or decreasing escapements on particular stocks; is that correct?
DR. PETERMAN: Well, that isn't what we had in mind.
Q No.
DR. PETERMAN: I think we were thinking more in terms of well, if there's a mechanism such as a parasite load or a pollutant that you do experiments with those potential morality agents present and absence, and compare the results.
Q Well, what I have in mind is, and again I won't take you to the transcript, but Dr. Walters speaking directly to the Commissioner and in his words indicating a -- not a desire particularly, but the possibility of, quote, pushing the

escapements around to see what happens.
DR. PETERMAN: Yes. Well, that's part of Carl's philosophy about active adapted management.
Q Right. And that would be an example of approach number 4.
DR. PETERMAN: Yes.
Q Yeah. Now, except to the extent to which you've been pushed off your -- or out of the four corners of your report, it's fair to say that you're looking at the salmon, the sockeye salmon as a biological, what would -- a biological group, or a biological -- you're not looking at it as a resource in terms of its particular uses for human beings.
DR. PETERMAN: That's right. We were focusing on biological processes.
Q Right. And would you agree with me that -- and again, as you said, that what you're really talking about in your bottom line of -- of where the focus should be on analysis is one of priorities. Now, going back to your financial analysis, assume for the moment that there is a general downward trend in your whole portfolio, and there's a specific hypothesis for your best stocks. Another way of determining priorities would be to focus on your best stocks, wouldn't it?
DR. PETERMAN: Focus on them in what sense?
Q On seeing whether there's a local problem.
DR. PETERMAN: Oh, yes. Sure. That's part of the picture, part of the analysis.
Q And again this is with respect to your answer to Mr. Harvey and to the Commissioner's questions. You're not purporting in your report to give management advice.
DR. PETERMAN: No, that's right.
Q And especially with -- and you're in particular not purporting to give advice with respect to escapements.
DR. PETERMAN: No.
MR. LOWES: No. Thank you.
DR. PETERMAN: Thank you.
MS. BAKER: Mr. Commissioner, this is a natural place to break, and we have two questioners for the afternoon.
THE COMMISSIONER: Thank you.
THE REGISTRAR: The hearing is now adjourned until 2:00

p.m.

(PROCEEDINGS ADJOURNED FOR NOON RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: The hearing is now resumed.
THE COMMISSIONER: Ms. Gaertner?
MS. GAERTNER: Mr. Commissioner, it's Brenda Gaertner for the First Nations Coalition, and as you can see, I'm here alone today.

CROSS-EXAMINATION BY MS. GAERTNER:

Q I want to start first by extending my gratitude to Dr. Peterman for the commitment to salmon that your résumé reflects and your choices to work on behalf of the salmon.
DR. PETERMAN: Thank you.
Q You will appreciate that my clients consider Western scientific approaches as being a useful place at the table, but not necessarily a decision-maker at the table. You understand that distinction, don't you?
DR. PETERMAN: Yes.
Q And you'll also appreciate that they were comforted when I reminded them that you were trained as an ecologist, because as an ecologist, you're trained to do systemic or systematic approaches in looking at salmon, aren't you?
DR. PETERMAN: Yes, that's right, and Dr. Brigitte (sic) had that training at her Ph.D. level as well.
Q Yes. And I was going to turn to you, Dr. Dorner, again. I'd like to thank you and thank you for actually participating today. It sounds like you've been suffering from a cold or something like that over the last two days. I think you can rest assured that, like others, most of my questions are for Dr. Peterman and that there are a couple of areas where I think the expertise in your work will come in. So you can rest a little during the call if that's possible for you.
I just want to let you know where I'm going.
DR. PETERMAN: Okay.
Q And I want to start by saying that I'm going to touch upon some of the causes of decline that are consistent with your finding and ask you to go a

little further than where you went on the report, if that's going to be possible.

I'm going to pick up some concerns or observations around the correlations between the Fraser River sockeye salmon and Bristol Bay. I'm going to take you to both your report and the previous report by Dr. English on that.

I would be remiss not to talk about and have somewhat of a discussion on that which we call delayed density dependence and distinguish that from over-escapement and cyclic dominance.

I'll then go to the Quesnel system and ask you to look at some of the more recent numbers on the Quesnel system and help us understand that in the context.

DR. PETERMAN: Okay.

Q I'm going to need your help to respond to some of the themes and approaches that we keep hearing about in this room on some of these topics.

MS. GAERTNER: And then finally, Mr. Commissioner, I'm going to ask him to bring his expertise to you on issues of uncertainties, risk assessments and other precautionary principles that I know, Dr. Peterman, by a review of your résumé, you have quite a bit of expertise.

Q Is that correct?

DR. PETERMAN: Yes.

Q And then finally I'll take you to your recommendations. I have an hour and 15 minutes, and I'm going to stick to my time period. With any bit of luck, I'll finish before that.

One of the strong conclusions that appears from your report that I think is worthy of emphasizing, and I want to give you an opportunity to speak about it, is that your observations are that this decline began in the mid to late '80s, and that it was sharply identified in the '90s or more in this decade. Is that a fair way of doing a total summary of it, that the decline in productivity is not a recent phenomenon but it's something that began at least in the mid to late '80s?

DR. PETERMAN: For many stocks, yes. There are certainly exceptions within the Fraser system to that statement, but, yes, there --

Q What are the exceptions for the -- within the Fraser?

DR. PETERMAN: Well, let's see, I was just trying to find the right figure. I think it's Figure 9, right, Figure 9, page 49. Just a slight change to the general statement. If you look at the Fraser Summers, you'll see that the productivity went up there in the 1980s for those, and then they dropped back down again. If you look on the screen --

Q Yes, I see that.

DR. PETERMAN: -- you see that there, right? Okay. And so with that caveat -- and I suppose that's also true slightly for the Birkenhead over there farther to the right in the Fraser lake group, but in general, there's been a downward trend in productivity for many of these stocks since the 1980s, yes. Certainly in the 1990s.

Q All right. So, then, now I'm going to take you to another key finding of your report, is that this decrease in the productivity of sockeye stocks are occurring over a much larger area than just the Fraser River, and that the patterns of decline are not unique to the Fraser River, and you then go on to say at page 3, and then over at page 10 again, that there's a larger factor at play and you state a "shared causal mechanism" or a "shared mechanism".

I'd like to drill down a little bit with you on this notion of a shared causal mechanism. Does that mean we can rule out factors that are unique to portions of the Fraser River, like over-fishing in the lower Fraser if that's somehow somebody's concern or any of those types of things? That's not causing this large trend. That may be something that -- as Mr. Lowes described, might be something that, if it even existed, that's a local issue or a stock-specific issue, but it's not relevant to these large trends.

DR. PETERMAN: Well, I would generally agree with you, but of course there's the caveat that there is a non-zero probability that you could have a series of independently operating processes in many different watersheds that just happen to cause a decrease in productivity in all those watersheds simultaneously. It's very unlikely in our view, but technically it's possible.

Q I'm going to stress that you would present it if it was something of concern, that it's so unlikely

as --
DR. PETERMAN: Yes.
Q -- to be unreasonable.
DR. PETERMAN: Yes, that's right. Brigitte, do you want to add to that?
DR. DORNER: No, I agree.
Q And then you went on further and you said there was a coincident -- you suggest there could be a coincidental combination of processes. That's another series of words that you used at page 3. Could you tell us what you meant by that?
DR. PETERMAN: Oh, well, that's I guess what I was just referring to. Let's just hypothetically say pollutants were the problem in the Fraser system causing the decrease in productivity, and maybe predation by Steller sea lions was the cause of the problem in the central coast stocks, and west coast Vancouver Island stocks had a parasite. If all those processes were affecting the sockeye salmon population simultaneously, it would look like there was some shared source of a single mechanism or a single set of mechanisms, when in fact there were three quite different mechanisms, not operating in all those areas.

So that's why we're saying there's a coincidental convergence of mechanisms that is possible, but we think it's very unlikely.
Q Dr. Peterman, one of the ways that I've been trained with First Nations with people that have been fishing this river for many, many decades and hundreds of years, as we try to get practical about our observations sometimes, and so I think that that's --
DR. PETERMAN: Sure.
A -- difficult sometimes for scientists. But it's quite unlikely, very unlikely that it's a coincidental combination of processes --
DR. PETERMAN: Yes.
A -- that are explaining this trend.
DR. PETERMAN: Yes.
A It wouldn't be worthwhile to spend significant resources trying to find something like that.
DR. PETERMAN: Oh, no. But on the other hand, any attempt to look at what is causing the decline in each of the populations would probably pull out whatever evidence there was that might be unique to particular systems, as well as getting out what

might be the shared sources of variation.
And I want to emphasize the plural there, "shared sources". So I don't know whether you were correct in what you read. Did we write "mechanism", singular? If so, we meant "mechanisms", plural.

Q Okay. I think that's useful. And you went on in a number of places in your report to talk about something that, again, my client has raised with the Commissioner as a serious concern, which is cumulative impacts.

DR. PETERMAN: Uh-huh.

Q And you would also agree that that could also be something that's shared amongst all of these different stocks.

DR. PETERMAN: Certainly.

Q So it could be a single phenomenon happening to all of them so, for example, a predator that everybody is being exposed to up in the Gulf of Alaska, if all of these stocks get to the Gulf of Alaska; is that correct?

DR. PETERMAN: Yes, that's one possibility.

Q Or it could be the same thing all throughout the whole system. Is that another possibility?

DR. PETERMAN: Yes.

Q All right. Do you have any hints? I heard you use that word a little bit yesterday, or hunches as to what is the unifying cause?

DR. PETERMAN: Well, this is a loaded question. Of course I have hints, but no evidence definitely.

Q Well, you're respected in this field, Dr. Peterman, and I think your observations of what would be reasonable and what could be possible could be very helpful to us here.

DR. PETERMAN: Right, well, thank you for that vote of confidence, but I would say I always like to rely on the facts. I know that you'll be hearing from another group sometime in the future, Project 6, I believe it is, the work led by Dave Marmorek on cumulative effects where he is pulling together data analyses on the different possible mechanisms that could be affecting these fish.
So with that caveat that we haven't -- Brigitte and I have not actually looked at the data. I guess this shared variation over a large spatial scale is something that scientists have reported on before, but on a much smaller scale.

So I'm one of the co-authors of several papers on this topic, in fact, and what we had looked at in the past -- and this is just something we've looked at, I'm not saying this is the cause here -- was the influence of ocean conditions, for example. Just a second. I think Brigitte was talking there.

We looked at the influence of ocean conditions early in the life of juveniles as they entered the sea and we found that there was a shared response to increased sea surface temperature by fish south of the Skeena and Nass, namely, a negative response in terms of their productivity. So when sea surface temperatures were above normal, it led to below normal survival rates for those fish, whereas from the Nass, Skeen and north, increased sea surface temperatures, when the juveniles hit the ocean, were associated historically with improved recruits per spawner.

So that's an example of a mechanism that would have larger spatial scale than just a particular watershed. But it is just an example, and quite frankly, there are many different processes operating out there.

I know I should clarify one thing. Those temperatures that the fish encounter in the ocean are not anywhere near their thermal fatal -- or lethal limits. Nowhere near it. So the sea surface temperature is just an indirect index of the dynamics of the ocean system which reflect upwelling of cold nutrient-rich water feeding the food base, attracting predators, whatever it might be. So I just want to make sure there was no misunderstanding there in the listeners.

Q It's not necessarily the temperature of the water that's affecting it directly, but much more the changed environment and what it comes with.

DR. PETERMAN: That's right, and the temperature is an index of that changed environment, that's right.

So anyway, that's just one example. But, Brigitte, you might want to add to some speculations here, but I can just say that most everything that we did not rule out, you could figure out a possible mechanism of a large scale that could generate the results we've seen.

Q All right. At page 3 of your report, and in your recommendations, of course, you are suggesting we

look at the post-juvenile stage.
DR. PETERMAN: Right.
Q And that certain stressors such as pathogens that are non-lethal in fresh water cause mortality later in the sockeye life history. I'm wondering if you would also agree that there could be pathogens in marine waters --
DR. PETERMAN: Oh, yeah.
Q -- that have the same effect that show up later in the sockeye life history and that that actually could read fresh water or marine pathogens.
DR. PETERMAN: Absolutely.
Q So it would be accurate to revise that part of your report to reflect both of those?
DR. PETERMAN: Let's see. Exactly which sentence are you looking at now?
Q It's page 3 of your report. It's the summary of where your --
DR. PETERMAN: Right. So -
Q -- conclusions --
DR. PETERMAN: -- does the line say:

> ...freshwater habitat degradation, contaminants, pathogens...

And so on?
Q Yes, and there's -- hold on, let me find the actual quote. I think I might have the wrong page number. This is much more general. I apologize for that. That's me not checking my notes once more. It is there, but what I am hearing from you is the intention to make sure we look at both fresh water and marine pathogens.
DR. PETERMAN: That's right.
Q That could be affecting fish during -- the fish could be exposed to it and then it could be affecting it later in its life cycle.
DR. PETERMAN: Yes.
Q Sorry about that, Dr. Peterman.
DR. PETERMAN: It's okay.
Q I apologize for my error in reference.
Just before I turn to the correlation questions that I have for you, I wanted to make sure -- and Mr. Commissioner, if this sounds so basic, please apologize (sic), but I think there's something useful for me to learn, if not others in this room.

Yesterday when you began your testimony, Dr. Peterman, you went quickly to say that it's long-term trends, not really annual cycle variations that managers should be interested in or most interested in.

DR. PETERMAN: Mm-hmm.

Q We didn't go back to that yet, and I wondered if you could explain to me carefully -- because my clients are very interested in abundance and they've been somewhat trained by scientists to look at abundance numbers and all of those things, so I want to get a sense of what you were saying to us all when you reflected how important it is to be looking at these longer-term productive trends.

DR. PETERMAN: Sure. Well, I guess all I was trying to point out was that it's easy to get caught up in trying to explain the year-to-year variation in any index that's relevant to your decision-making. That kind of fine-grained look at the data can sometimes make it easy to miss the longer-term trend. So you might be looking at year-to-year variation on something that's going up at ten percent per year, or something like that, and you miss it. I doubt if any biologist would miss it, but they might.

So that was the key point there, is that this Kalman filter estimation of the productivity trend is trying to figure out what is the main signal that's being given by the salmon survival rates, and if that signal is it's a deteriorating condition, namely, survival rates are going down, productivity is going down, we should pay close attention to that.

But the problem when you have very noisy data that is highly variable from year to year on top of a trend, it's sometimes a long delay before you see the trend because of the year-to-year variation. I think most people have experienced this sort of thing in everyday life.

Q And the other one, and we've heard you speak about it quite a lot, is this notion of stationarity and the --

DR. PETERMAN: Yes.

Q -- idea -- and particularly the problem that we're asking science to predict the future by observing the past; is that correct?

DR. PETERMAN: Mm-hmm.
Q That's a bit of a challenge for you as scientists, isn't it?
DR. PETERMAN: Sure.
Q And if I've got this right, generally - and this is a general comment - that escapement models and MSY and determining those types of things are actually based on stationarity and are primarily based on abundance records, if I've got that right.
DR. PETERMAN: Most of them are, yes, and that's why we went to this Kalman filter estimation procedure because it does not assume stationarity. That's the major advance. It purposely says let's allow for non-stationarity in productivity, if it exists. And if it turns out that the data doesn't reflect it, well, then, you're not forcing it to show up in the output from that Kalman filter.

But what we've done through the simulation testing that we published back in 2000, it showed that this method, the Kalman filter, is better at tracing changes, namely non-stationarity in productivity, than previous methods that assumed stationarity.
Q Thank you. I think that's very helpful as an overall. I'm now going to take you to the correlation work you did and in particular to Figure 11 at page 53, and your note to the figure on page 54.
MS. GAERTNER: Then, Mr. Lunn, shortly thereafter, I'm going to go to page L-2, so if you could have that ready.

Now, I'm going to summarize what I heard so far so that we can go on from there. We've got the blue are becoming bluer and the red are becoming redder. That's the simplest way of doing that.
DR. PETERMAN: Mm-hmm.
Q And it's therefore the negative correlation amongst the Fraser stocks and other B.C. stocks is getting stronger, and -- are the positive correlation, and the negative correlation with Bristol Bay and Alaska is getting stronger; is that correct?
DR. PETERMAN: Yes, if you could be --
Q Good way of summarizing?
DR. PETERMAN: You just corrected your first negative

to be a positive, yes.
Q I did it wrong? Yeah, no, I did. I just corrected that --
DR. PETERMAN: Yeah, I know you did.
Q -- negative to be a positive.
DR. PETERMAN: Okay, that's right.
Q Is that right?
DR. PETERMAN: So the positive or weak positive correlations among B.C. stocks seem to have become stronger over that period up until 2004 that we looked at, and the correlation between B.C. and Western Alaskan stocks has become more strongly negative. That's right.
Q Now, what you're doing is observing a pattern, not explaining a pattern by doing this; is that correct?
DR. PETERMAN: Yes.
Q That's all you're doing in this component, right?
DR. PETERMAN: Absolutely.
Q All right. And that's an extremely important thing for those of us that aren't scientists to keep in mind; is that correct?
DR. PETERMAN: Yes.
Q All right. And I want to take you to page L-2 of Dr. English's report which is Exhibit 718. When we were working with this report, we noticed something that seems to go -- it's another observation around the relationship between Bristol Bay and Fraser River stocks. This is done on a ratio of annual returns to the average returns, so it's not quite exactly the same times (sic) of comparison you're doing.
MS. GAERTNER: L-2 -- oh, it's page L-6, Figure L-2, sorry, Mr. Lunn.
Q Now, I'm not going to -- I'm going to have you explain this a little bit better than I'll do, but when we looked at this, there's certain stanzas of interpretation one can have that suggests that as the Fraser River stocks go up, the Bristol Bay stocks go down, and as the Bristol Bay stocks go up, the Fraser stocks go down. I just wanted you to take some time with that graph, because I'd ask you to comment on that and whether those observations are correct, and if they are, how they reflect upon the pattern that you observed that's reflected in your correlation graph.
DR. PETERMAN: Okay. Well, I haven't seen this before,

but I can readily interpret it, I think. So the annual returns in the solid line for the Fraser are some smooth -- probably moving four-year average, I would guess, annual returns.

Q Yes.

DR. PETERMAN: Okay. And I think the dashed line is probably not labelled at all.

Q Yes, the dashed line is Bristol.

DR. PETERMAN: That's what I thought. So the Bristol Bay sockeye total run -- again, is this a four-year moving average or five-year? It doesn't matter which one it is. It's a moving average of some sort. Yes, it looks like they're out of phase, so that in the early period, say, prior to 1985,'86, it's clear that when Bristol Bay abundance is above its mean, B.C. sockeye are below their mean.

And, by the way -- oh, no, sorry, this is Fraser only.

Q That is Fraser, yes.

DR. PETERMAN: The solid -- yeah, okay, right. That breaks down a little bit in the '80s and '90s so that there's more corresponding above-average pattern in both of them, and then they become opposite again starting in the late '90s, so they're out of phase.

So, okay, now I've just described that and, I'm sorry, I missed the question about it.

Q Well, I hadn't quite got there.

DR. PETERMAN: Okay.

Q It's helpful to us, so we're on the same page there. So assuming that's an accurate depiction of the numbers for those fisheries, that doesn't necessarily reflect a long-term climatic change that's going on, that that's much more cyclic in its nature and potentially a four- or five-year cyclic nature, would you agree with me on that observation?

DR. PETERMAN: Yeah, I think this is reflecting what you see in the cyclic dominance phenomena as the larger signal. So that's the high degree -- high amplitude variation --

Q That's the --

DR. PETERMAN: -- in those functions.

Q That's the Fraser runs, though --

DR. PETERMAN: That's right.

Q -- which are much more cyclic dominant than the

Bristol Bay, right?
DR. PETERMAN: Mm-hmm.
Q So it's much more -- the relationship to each other, which you call a negative correlation in yours --
DR. PETERMAN: Right, yeah.
Q -- at a broad trend, appears from this map to also be somewhat more narrow in that it's not just a broad trend, it appears to look like it has a trend within its regular cycle.
DR. PETERMAN: Mm-hmm. Well, I'll bet we're using the same data, so in fact - or very similar data - so I'll bet that if you plotted these datapoints on an X/Y scatter plot so you had the Fraser total abundance on the "X" axis and the Bristol Bay abundances on the "Y" axis, you would get a tendency for a negative correlation. This is something that we did actually in a paper Fred Wong and I published in 1984, even prior to that period, most of it not shown on this graph. There tended to be an inverse relationship between abundance of Bristol Bay sockeye and abundance of -- let's see, was it all B.C. sockeye? I think that paper was all B.C. sockeye.
Q So what does that tell us, Dr. Peterman?
DR. PETERMAN: Okay, well, there are several things. Well, first of all, I just want to go back and try to clarify what's in this graph, because there are two possible interpretations of that horizontal line which is at 1. It could be that Karl English fit the long-term trend line to the data, and he could have fit the trend line to each, the Bristol Bay data separately from the Fraser sockeye data.

And then he's trying to show here the trends above and below that trend line, although he could have also just calculated the long-term average abundance, and then shown here the deviations above. So I think it's the latter based on the "Y" axis label, the ratio of annual returns to the average return.

So, having said that, yes, there is an association between these abundances in the two regions, and it is consistent with what we have observed in our study, to the extent that the productivity in one area has been going down in recent years, in the last decade or so, namely from Yakutat Peninsula, southeast Alaska, on down,

and it's been going up in the western part of Alaska.

Q So does this suggest that there might be a shared food supply, a competition between a shared -- I mean, that may be simplistic, I know, but it's one of the things that could come from this kind of relationship.

DR. PETERMAN: That's one possibility for sure, that they could be sharing food supply and they do overlap in space and time in the Gulf of Alaska, as I pointed out last year -- I mean yesterday. It wasn't that long ago.

Q It did feel like a long time ago.

DR. PETERMAN: That's right.

Q It has felt like a very long day, that's for sure.

DR. PETERMAN: Right. But there's another possibility, and that is that the oceanographic conditions that are driving these total abundances, if they are driving them, are simply quite different in the northern part of the region where the Bristol Bay fish are separate from the B.C. fish, and that would be in the early part of their ocean life. We know that the early part of the ocean life is where most of the mortality occurs in these fish during their total life span.

So there are many possible mechanisms here behind this inverse correlation that you see here in front of you, and common food supply is one, a different set of oceanographic processes in the regions being another, but being driven by a common forcing variable, the climate.

So, yes, Brigitte, do you want to add to that at all?

DR. DORNER: No, that sounds about right.

Q All right. That's helpful. We're going to pick that up a little bit when we get to your recommendations also.

DR. PETERMAN: Okay.

Q It's very helpful to have your observations on what might be going on there.

Maybe I'll just make sure I've got this right. It is a fair observation, taking your correlations and this chart, that it -- while climate change may be causing this, it's unlikely, given that this is a very four-year cycle (sic).

DR. PETERMAN: Right. Okay, but, now, wait a minute. No, I don't agree with that. Let's look on

different time scales here. So you're focusing on the variation in abundances about every four years.
Q Yes.
DR. PETERMAN: Four or five years. What we were trying to do was to look at the longer-term trend --
Q Yes.
DR. PETERMAN: -- underneath year-to-year variation or four-year to four-year period of variation. I would say that take a different lens to these data and look at the data since 1993, say, and is there a trend in there? Just fit a linear trend to the data and I'll bet you'll see it's a downward trend for the B.C. sockeye -- for the Fraser sockeye, pardon me.
Q Okay.
DR. PETERMAN: So, yes, there's variation across four-year blocks in that, but I think it's on a downward trend.
Q All right. So they are consistent with each other and your --
DR. PETERMAN: What's consistent --
Q -- observations --
DR. PETERMAN: Yes.
Q -- and these are consistent.
DR. PETERMAN: Yes.
Q Yes, I wasn't suggesting they were inconsistent.
DR. PETERMAN: Yes.
Q I was more looking to see whether it helps us to look for causation.
DR. PETERMAN: Aha, I see.
Q That's what I'm -- I'm not looking for -- I didn't think they were inconsistent.
DR. PETERMAN: Okay.
Q I was wondering whether or not those two together help us use limited resources to look in the right places for potential causation.
DR. PETERMAN: Certainly.
Q And what would that be?
DR. PETERMAN: The resources --
Q How do these two observations, coming together in their consistency, help us to refine where we're looking?
DR. PETERMAN: Well, again, the four-year pattern that you see here is something that's persisted for decades in these stocks, and we're saying that if you look at the longer term trend that these

short-term patterns are superimposed upon, suggesting there's something underlying the mean annual returns -- in our case it was the returns per spawner -- that we should be looking at. So I guess what that tells us is we should be looking for processes that are having a persistent effect on decreasing survival rates across space, decreasing as in decreasing over time.

Q For example...?

DR. PETERMAN: You take any of the hypotheses that we do not rule out and you can postulate any of them.

Q All right. So that --

DR. PETERMAN: So increasing the abundance of Steller sea lions, increasing abundance of fish predators of salmon, increasing contaminants, increasing pathogens, you name it. Any of those could conceivably explain what we've demonstrated to be the case, that decreasing productivity. But I say potentially, and until we look at the data, Brigitte and I certainly can't say which of those hypotheses are plausible, but that's I hope what you're going to here when Project 6 is reporting in.

Q All right. Thank you. I'm going to move on to my next topic, then. That was helpful.

DR. PETERMAN: Okay.

Q Which is that which, in scientific terms, is called delayed density dependence. Now, I just want to start with an overall observation, which is that often in this room the term "delayed density dependence" and "over-escapement" seems to be used interchangeably. They are not the same term; is that correct, Dr. Peterman?

DR. PETERMAN: That's correct.

Q And could you explain the importance of the distinction between them?

DR. PETERMAN: Well, delayed density dependence is a mechanism. It's a description of an assumed link, a biological link, between generations of spawners and the success of a given generation of spawners whereas over-escapement is a term given to the number of spawners relative to some reference point.

Q Which could be very specific to a particular goal or objective of --

DR. PETERMAN: That's right.

Q -- a particular harvester or otherwise; is that

correct?
DR. PETERMAN: Exactly. The comparison point for the escapement levels is something that you could say is determined by management, so you could say it's over-escaped compared to what they wanted, or it's over-escaped compared to some biological or economic metric that you come up with. It's a very open-ended term, that "over-escapement" one.
Q We're going to get to the Quesnel run in a few minutes, but in your work in here, you were very much looking at delayed dependency dependence and not this notion of over-escapement; is that correct?
DR. PETERMAN: That's right, yes.
Q And the next thing that I wanted to raise just as a common definitions almost, it's common knowledge that there are cyclic dominant stocks within the Fraser sockeye. It's also - and you mention this in your report - that it's also, for example, the Bowron, that cyclic dominance can become transient.
DR. PETERMAN: Right.
Q And that it's quite likely and possible that stocks become cyclic very strongly, and then they might revise that, might not be cyclic for a while, and then they may return to cyclic patterns; is that correct?
DR. PETERMAN: Yes.
Q And I know this might sound self-evident, but that's likely salmon responding to something.
DR. PETERMAN: Oh, undoubtedly. The Bowron pattern that you cited is a case where there wasn't cyclic dominance for many years, and then there was a cyclic dominance pattern, and I can't remember how long the period was, maybe a decade or a decade-and-a-half, and then the cyclic dominance pattern dissipated again in response to some pressures, and it could be natural or human-induced or both.
Q And so it could be the salmon coping or thriving or potentially even evolving in response to a particular mechanism that they're experiencing during their life cycle; is that also fair?
DR. PETERMAN: Yes.
Q And from an ecologist's perspective, one cannot assume that there's anything wrong with that.
DR. PETERMAN: With changing the nature of the cyclic dominance or...?

Q With the salmon changing the nature of their cycle, like it's a natural phenomenon.
DR. PETERMAN: Yes. Yes.
Q There's nothing bad about it, per se.
DR. PETERMAN: No.
Q It's actually perhaps one of the things that makes them wild as distinct from a grown fish; is that correct?
DR. PETERMAN: No, I wouldn't say that because there's lots of wild fish that don't show cyclic dominance.
Q One of the things. I said "one of the things". One of the things that shows the wildness of the salmon. They're responding to circumstances we don't understand. They're doing it at patterns we cannot predict.
DR. PETERMAN: Sure.
Q They're alive and well as a result of that for centuries. Do you agree with all of that?
DR. PETERMAN: For those that show cyclic dominance, yes.
Q Yes. It's likely that cyclic dominance is useful to them.
DR. PETERMAN: Well, I won't anthropomorphize like that, myself, but...
Q It helps them to sustain themselves, doesn't it?
DR. PETERMAN: I'm not sure.
Q Likely?
DR. PETERMAN: It's just a phenomenon of their population dynamics. Just like insect populations go through outbreak periods and non-outbreak periods. It's part of their natural dynamics.
Q Given that it's either a coping, thriving or potentially evolving mechanism, you'll agree with me that right now science doesn't know which one it is. Science doesn't know why, all of a sudden, a stock might become more cyclic dominant for a while and then let it go, or any of those. That's an unknown in the scientific world right now; is that correct?
DR. PETERMAN: Yes.
Q And the other is this notion - and we've heard during the evidence of Dr. Walters his response to it - but there's also increased synchronicities that occur between the stocks and amongst the stocks during -- over long periods of time; is that correct?

DR. PETERMAN: Actually, I did read about that in the transcript from those hearings on February 9th and 10th, and I have not looked at that synchronicity myself.
Q Okay.
DR. PETERMAN: So I can't attest to that. Brigitte, have you looked at that at all?
DR. DORNER: Nope.
MS. GAERTNER:
Q Now, if I understood Doctor -- some of Dr. Walters' comments during his testimony, it appeared that he had strong concerns about the Adams and the Quesnel travelling together through a similar cyclic pattern. That wouldn't necessarily be a concern from an ecological perspective, would it be? I couldn't think of any ecological reason why there would be that concern. Can you?
DR. PETERMAN: An ecological reason? Well, I guess the only -- okay, so if what you mean by Adams and Quesnel going through the cyclic dominance pattern together, so if they had the same dominant cycle years, for example?
Q Yes.
DR. PETERMAN: Then the only ecological problem would be if they end up competing for limited resources and it detrimentally affects both stocks. So that is a possible reason why that would not be -- you'd see decreased survival rates perhaps in one or both of those populations.
Q So likely the cyclic dominance wouldn't continue to be this synchronized over a long period of time if that was a problem.
DR. PETERMAN: I'm not sure, because we really still don't know what causes the cyclic dominance.
Q Perfect. Let's go to the Quesnel stocks. I'm going to need to take you through a couple of figures. I'll just preface my questions with respect to the Quesnel stock, just to let you know, that from a First -- my clients up in the upper rivers, in particular those whose territory is the spawning grounds of the Quesnel are located, have a difficulty saying there's anything called an over-escapement in most recent years in the Quesnel runs, so I just want you to know that that's the perspective of the questions that I'm coming from.

DR. PETERMAN: Okay.
Q And you'll agree with me or confirm that Quesnel's not really a single stock, it's a system. There's numerous lakes and like the Early Stuarts, there's dozens of different spawning populations and that a lot can go on, and we've got more than one conservation unit in the Quesnel system. Is that correct?
DR. PETERMAN: Yeah, at various scales of resolution, you could break these groups, the 19 populations into smaller groups for sure.
Q And if I heard the evidence correctly, both yesterday and today -- and I want to fine-tune this to make sure I've got this right. There's no evidence yet to support the conclusion that delayed density dependence caused the catastrophic response of the Quesnel in 2009.
DR. PETERMAN: That's right. We did not show any evidence of that.
Q All right. So then I want to go to the Quesnel data and I have to take you to two documents. I did my best last night to see if I could do this all in one, Dr. Peterman, but it doesn't appear that I can get you the numbers of the Quesnel all in one document. So I'm going to take you to Exhibit 399 and I'm going to take you to page 99 of that. You saw that earlier today.
DR. PETERMAN: Which document is it? Okay, the Pestal et al, yes, okay.
Q And it's the escapement --
DR. PETERMAN: The 2010 Petal et al document?
Q Yes, and at page 99 is the numbers for Quesnel from 1948 through to 2008.
DR. PETERMAN: Mm-hmm.
MS. GAERTNER: Then, Mr. Lunn, if you can call up also at the same time Exhibit 420. Exhibit 420 is the -- I think the DFO calls them "The Near-Final Escapement Estimates for 2010" for the summer run sockeye salmon.
Q You'll find at page 3 of that actual document, a description of Quesnel. I'm taking you there to get two numbers, if I may.
DR. PETERMAN: Okay.
Q They're going to give us the 2009 spawner return and the 2010 spawner return.
DR. PETERMAN: Okay.
Q But, please, if it's useful to you, read that full

paragraph for yourself as it relates to Quesnel and the results there. But as I see it, in 2009, we've got a spawner number of 149,467.

DR. PETERMAN: That's in the Pestal et al document?

Q No, that's in the -- we don't -- Pestal et al takes us only to 2008.

DR. PETERMAN: That's the brood year, yes. Okay.

Q Yes. And so 2009 --

DR. PETERMAN: Return, yeah.

Q -- we've got the returns of spawner returns, not full-run returns but spawner returns of 149,467.

DR. PETERMAN: Okay.

Q You'll see that on the fourth line down of that paragraph.

DR. PETERMAN: Yes.

Q And in 2010 on the second line, you have the number 249,376, and again, that's spawners. And so if I've got that right, I'd put that down under the third column of the Pestal et al numbers. So after the year and after the run, I've put that 149,467 as the spawner return and 249,376.

DR. PETERMAN: Well, the only issue there is I'm not sure whether the document on the left has numbers of spawners in effective female spawner units or whether it's total spawners.

Q I don't have those other numbers. I don't have the effective females or the recruits. I only have the spawner numbers for you. That's what --

DR. PETERMAN: Right, okay.

Q Okay?

DR. PETERMAN: I'm cautioning you, then, the third column in the Petal et al document is effective females.

Q Oh, sorry, I meant the second column. I counted the year as the first, "Year", "Run", and then "Spawners".

DR. PETERMAN: Oh, okay. All right.

Q Okay?

DR. PETERMAN: Yes.

Q And so I've got for 2009, I put 149,467 and for 2010 I have 249,376.

DR. PETERMAN: Right.

Q All right. So where I want to go with that, having talked about -- if I can find my questions -- a number of -- obviously Quesnel is a cyclic dominant system.

DR. PETERMAN: Mm-hmm.

Q And we see that cyclic dominance all the way through those numbers, and by 2001 and 2002, we start getting some stronger returns to the Quesnel, and as I understand it, that's partly due to the management decisions around the Lates, but that's an aside, I suppose. Let's keep going.

So we've got those returns, and then in 2005, we have the dominant return and that's a smaller return. That would reflect perhaps the delayed density dependence that you're talking about. But that likely is one of the first places we start seeing that from an abundance perspective. Is that a fair way of observing that?

DR. PETERMAN: No. I'm sorry, I can't --

Q Can't follow that?

DR. PETERMAN: And I don't agree with the way you interpreted it.

Q Okay.

DR. PETERMAN: You say that's the first place where we see the delayed density dependent effect.

Q Well, we're building up until then.

DR. PETERMAN: Right.

Q So we're not suffering from anything causing less -- like we're building. We've been building all along for quite a few years.

DR. PETERMAN: But, as I said before in response to your question about defining the term "delayed density dependent", it is a mechanism and we've documented that it exists in the Quesnel data and we use the entire dataset with Quesnel to establish that. So I don't think we can say, well, it didn't really appear until a certain year. I don't think that's correct.

Brigitte, do you want to expand on that or agree or...?

DR. DORNER: No, that's exactly correct.

Q All right. Then I'm going to go to the next step which is that delayed density dependence clearly isn't a problem for Quesnel. It's how they -- it's been existing for a very long time and in fact we had a building occurring. What I want to point out particularly is that in the 2009 and 2010, we're rebuilding quite quickly on the spawner -- 2009, of course, not. In 2009, every stock in the Fraser suffered from lower returns. But in 2010, we've got a 95 percent increase, or so, from the previous year.

DR. PETERMAN: Well, the -- oops, from the previous year? You mean 2010 is 95 percent greater than 2009?
Q No, no, sorry, from the one, two -- fourth year before.
DR. PETERMAN: So 2006?
Q 2007. Have I got this wrong now?
DR. PETERMAN: Yes, I'm afraid you do have it wrong.
Q Okay. Let's --
DR. PETERMAN: So 249,000 is the return in 2010 which is about one-and-a-half times what it was in 2006, maybe. I haven't got my calculator here, but something like that.
Q So at the end of DFO's summary of Quesnel:

> Spawning success for the Quesnel system in 2010 is 95.4%, well above the long term system average of 84.4%.

What does that mean?
DR. PETERMAN: Okay, sorry, just -- oh, it's the last sentence. Let me read this in context, please. I need to read the sentences before.
Q Please. Please, of course.
DR. PETERMAN: Okay. This is a completely different variable that they're presenting in this last sentence. I believe spawning success has to do with the egg retention rate, so in other words, if you have 100 females and only 95 percent of them lay their eggs and the other five retain them for various reasons, I think that's what they're referring to as a spawning success rate. So that's how they get the effective female spawners numbers. They take the number of females that are estimated in total, and then they take off the proportion of eggs that are not deposited. I believe that's what that last sentence refers to. Spawning success otherwise would not make any sense to me.
Do you have any thoughts on that, Brigitte?
DR. DORNER: That's how I interpret it as well.
Q Okay. I guess the other comment that I'd ask you to consider and respond to is when my clients saw these numbers and when I reviewed them, they were somewhat quite relieved of course when they saw the 2010 responses or numbers in that it appears that Quesnel is rebuilding quite significantly

from 2010, and shows the rebuilding again as far as 2006. So in a very local comparative, it's improving already.
DR. PETERMAN: I see what you mean.
Q Do you agree?
DR. PETERMAN: Well, if you're comparing the numbers of spawners, 249,000 in 2010 with the number of spawners in 2006, which is 169,000, then, yes, I would agree 249,000 is greater than 169,000.
Q And we're far away from any concerns about over-escapement.
DR. PETERMAN: No, I wouldn't agree with that statement because we just talked about how I defined over-escapement.
Q We're far away from any issues about over-escapement as it relates to the Quesnel. If you just --
DR. PETERMAN: I have no --
Q If you simply look at --
DR. PETERMAN: -- idea.
Q -- the productivity that's reflected in the earlier numbers.
DR. PETERMAN: Yeah, but again, it depends on what your objective is. Maybe in a particular year the objective might be to have the population shift its dominant cycle years. So they say, "We want to fish the heck out of this stock and knock the escapement way down in order to shift the dominant cycle year." Maybe that's the objective, in which case you measure how many fish escaped relative to what your target was at that point, and decide whether it's over-escapement.
Q If your objective was the long-term sustainability of the sockeye so that this generation and seven generations from now could fish it, then it appears that we've got a little bit of a rebuilding occurring compared to the catastrophe in 2009 --
DR. PETERMAN: Mm-hmm.
Q -- and that the Quesnel seem to be responding.
DR. PETERMAN: I would say that the first part of that is likely true because the optimal escapement from the standpoint of maximum sustainable yield is undoubtedly greater than 249,000 fish. I don't know what the number is, but I'm sure it's greater than that.
Q Now, this -- this is a risky question and I'm

going to ask it. Thursday of a long weekend, what the heck, eh?

DR. PETERMAN: Mm-hmm.

Q How do we, as lay people, understand the difference between data that supports observations that a cyclic run might be changing or adapting or returning to strong cycles and that which we see with Quesnel right -- like how do we tell the difference between a changing cyclic dominant pattern and over-escapement if you were to call it that -- or, no, I won't go to over-escapement. That's not a comparative. But do we see that pattern and not be worried about it, per se, but appreciate that it simply reflects a pattern that the sockeye are moving towards in response to a natural environment?

DR. PETERMAN: Okay. Well, I think what you're asking is, first of all, how do we detect whether there is a change in the cyclic dominance pattern.

Q Yes.

DR. PETERMAN: And, as you can well imagine, with something like these data, it's going to take some years of data subsequent to 2010 before you can realize whether there has been a change in the cyclic dominant pattern. So instead of the dominant year, say, going back 1985, '81, '85, '89, those are clearly dominant cycle line. And if that starts to shift, you might need several years after that before you see it.

Now, you had a second part of the question which was to do with whether you would be worried about it? Sorry, I didn't quite remember what you said.

Q Whether there would be any concerns associated with it.

DR. PETERMAN: Well, I suppose there are all sorts of reasons by one might be concerned in terms of the impact on the timing of events. I know you heard earlier in this series of hearings about pre-season forecasts, so when you have a change in the cycle dominant pattern, you're likely to have quite large forecasting errors, and so that might be a concern for users of the salmon and their planning.

Aside from that, the biological concerns, say, from the standpoint of conservation, I don't think it would matter whether the dominant cycle

year is the 1985 series or the 1986 series.
Q Thank you. I understand there -- moving onto a slightly different topic now. I understand that there are estimates of the number of juveniles in Quesnel Lake from the 2005 spawning, which is about 52 million, and that this is not an unusual number. I'm suggesting that there's nothing unusual in the first part of the freshwater life, but the eventual return for juveniles into Quesnel Lake was very small in 2009.
DR. PETERMAN: Mm-hmm.
Q Now, for the nearby Chilko run, the smolt abundance was in excess of 70 million for the 2005 spawning, was very high, and the adult return was also very poor. Is it accurate that these two lines of evidence suggest that the 2009 return was an event that affected both populations in either the late freshwater or marine stages?
DR. PETERMAN: Okay, I'm sorry, I lost the thread of your argument there, because I was trying to look up my numbers for the Quesnel fry --
Q All right.
DR. PETERMAN: -- and what happened to them. Could you just take me back to that start --
Q It's my understanding that the estimates of the number of juveniles in the Quesnel Lake for the 2005 spawning was about 52 million, and that that's not an unusual number. It's not particularly small for the size of the spawning population suggesting nothing unusual at the first part of the freshwater life.
DR. PETERMAN: Okay.
Q But the eventual return was very small in 2009.
DR. PETERMAN: Mm-hmm.
Q Now, for the nearby Chilko run, the smolt abundance was in excess of 70 million for the 2005 spawning. Some might consider that a high number. And the adult return was also very poor.
DR. PETERMAN: That's right.
Q So is it accurate to say that these two lines of evidence suggest that the 2009 return was an event that affected both populations in either the late freshwater or marine stages?
DR. PETERMAN: Yes.
Q And this would seem not to support the suggestion that over-escapement in Quesnel in 2005 or the delayed effects of earlier years impacted the 2009

run?
DR. PETERMAN: I flinch at that word "over-escapement" again.
Q Well, and I use it carefully in that way.
DR. PETERMAN: Yeah. Well, again, over-escapement is only an appropriate word when you defined it relative to some management objective or some stated objective.
Q All right. So the suggestion of the three -- I think it's roughly three million spawners in Quesnel in 2005, and that's the -- no, it's --
DR. PETERMAN: No, you said it was --
Q So one million, sorry.
DR. PETERMAN: Okay.
Q It was too much. That's been suggested a number of times here.
DR. PETERMAN: Has it?
Q Yes.
DR. PETERMAN: I see. Okay, I wasn't aware of that.
Q So this would seem to suggest that that is not an over-escapement into the Quesnel in 2005, or that the delayed effects of the earlier years impacted the 2009 run at all.
DR. PETERMAN: Well, no, hold on now, because you see there is a possibility that you get delayed mortality within a brood class, so this word "delayed" is an adjective now in a different context than delayed density dependence, which is looking at delays across years.

Think about what might happen if you have a very large spawner abundance in one year such that the fish are so crowded in the lake and they get very poor food supply, they're more vulnerable to stresses, they become more susceptible to pathogens, but then those pathogens are on the fish but they don't cause mortality until after they're enumerated. In the Quesnel case, the fall fry or in the Chilko case, for departing smolts. So that's a possibility.

So that's why I wouldn't necessarily go as far as you did and say it's not the effective spawner abundance in the fresh water, it must be after that. Do you see what I'm saying?

Q So the effects of spawner abundance could occur before the late fresh water -- before the late fresh water.
DR. PETERMAN: Yeah, the effects will obviously have to

occur before in terms of starting the mechanism going, but the mechanism might not manifest itself until later in the life of the fish.

Bridget do you want to add to that?

DR. DORNER: Yes.

DR. PETERMAN: Brigitte's got her hand up, good.

DR. DORNER: Yeah, to answer that question more conclusively, we've also need to (indiscernible - connection cutting out).

DR. PETERMAN: Oops.

DR. DORNER: Sorry, I'm getting a lot of feedback here and I'm trying to somehow eliminate that.

We'd also need to know how big the smolts were. It may well be that the number of smolts that are coming from Quesnel, the usual number but if the size was particularly small, then it might have predisposed (indiscernible - connection cutting out).

Q I don't have that information so I'll have to move on.

DR. PETERMAN: Yes, unfortunately, neither do we. What I do know, though, is that the Chilko smolts were about average in size as I recall. There were many more of them than normal, but they were about average in size rather than what -- we really would have expected them to be smaller than average size. So the Chilko smolts seem to go out to sea in a fairly healthy state.

Q All right. I'd like to take you to Exhibit 73 next which is the synthesis of evidence from the workshop in June of 2010, and I want to take you page 44 and then over to page 85 and 86. In that report we hadn't had the work that you had completed for the Cohen Inquiry, and so I want to make sure that I've understood a couple of things that are said there and give you an opportunity to see whether or not they would be revised based on the work that you've done in this report.

Maybe I'll just take you to their proposed research on page 86 and 87. So this was at a time in which the working hypothesis was that there could be delayed density dependence in many of the stocks in the Fraser River that were influencing productivity. So at pages 86 and 87 was the recommended proposed research that came out of that work. When Ken Wilson and Dr. Riddell and Dr. Woodey and Dr. Walters was here, I went to

this proposed research because it seemed to be a reasonable response to concerns about delayed density dependence. I just want to know, now that we know what your report says, are we looking at this only as it relates to Quesnel? Is that the only place we actually have to do this kind of research and, if so, is there anything that we don't have to do anymore, or are these still relevant areas of research that could be useful for better understanding the delayed density effect in Quesnel.

DR. PETERMAN: Well, it's a good question. I guess I would say, yes, based on our results that we report here, the Quesnel is the most likely candidate for there having been delayed density dependent effect, so if you're going to learn anything about what the mechanisms are, that's the place to look. Given the limited resources for doing research of this nature, I'd say start there with Quesnel and don't study all the other stocks that cyclic dominance or other cases if you're only interested in understanding the delayed density benefit on them.

I want to emphasize that on pages 86 and 87, these are proposed research topics for that particular hypothesis.

Q Yes.

DR. PETERMAN: They're not for the entire set of hypotheses.

Q Yes, absolutely.

DR. PETERMAN: I know you know that, but I want to make sure everyone else knows that.

Q And that's how we've been working with those recommendations. These are not the common recommendations of the report for sure.

DR. PETERMAN: Right.

Q These are just as it relates to that. And I just want to go to recommendation 5 on page 87 then.

DR. PETERMAN: Mm-hmm.

Q Because I'm not quite sure what you meant by that.

DR. PETERMAN: Mm-hmm.

Q Again. Given my clients' perspective that we respect the salmon rather than tell them how to return and do anything other than that, what were you saying with respect to item number 5? I'm not saying you weren't being respectful, don't get me wrong on that one.

DR. PETERMAN: Yeah, sure.
Q I didn't mean that at all, if that's how you interpreted that.
DR. PETERMAN: No, I know what you meant.
Q I'm just curious about what kind of management strategies you were suggesting.
DR. PETERMAN: Okay.
Q Because I appreciated your comments yesterday on how those types of things can have cultural, social, economic and other implications that would have to be carefully considered, and so what were you talking about?
DR. PETERMAN: Okay. Well, this point 5, if I could just read it out:

> Contrasting measurement strategies should be applied to different stocks over enough time to observe a response.

So what that's referring to is this concept of active adaptive management that Carl Walters developed many years ago. The idea is that if you want to learn more about delayed density dependence, try changing a pattern of spawner abundances in the successive period of four years so that you get an alteration of where the high magnitude competition occurs rather than having it occur on what was the dominant year before. Maybe change it to a different year or maybe have high abundances every other year rather than have it one out of every four or two successive years out of every four.

So that's what -- contrasting in this ecological context usually means create comparisons and try to make them extreme so that you observe the response clearly amid all the noise that might be in the data.

Q I want you now to apply the precautionary principle to that recommendation, Dr. Peterman, and suggest what you might want us -- I mean I get very worried about that type of option in a very vulnerable and a very unpredictable time, that somehow humans are going to move in and start telling salmon how they should return.
DR. PETERMAN: Well, I completely respect your viewpoint, and I'll say two things about it. First of all, of course, any management strategy

where you're trying to -- pardon me. Any set of management options that you're evaluating, where you're trying to manipulate the system, have to be evaluated in a broad context. What might be the possible ramifications, and what might be the possible responses? One of the responses might be that the food supply dynamics in the lake are going to be completely messed up if we go and change the pattern of large numbers of spawners. So, sure, the precautionary approach would be don't take undue risks if you aren't sure what's going to happen.

But, to be quite frank, and I mean this respectfully, we've all been subjected to experiments, management experiments for the last, I don't know, 50, 60 years.

Q Who's the "we" in that?

DR. PETERMAN: People.

Q Okay.

DR. PETERMAN: So we have just not had a carefully designed experiment in the standard scientific sense where you are carefully controlling and manipulating things in order to learn how the systems work. Instead, there have been many other objectives out there that have led to choosing certain management options and those objectives are perfectly legitimate.

So I just take the subtle point, which may be too subtle to really emphasize here, that we have been subjecting the fish and the people who rely on the fish to experiments in the past. I just wouldn't call them very well designed experiments.

Q And one of them, including the one that I understand Dr. Walters talked about in the '80s and into the '90s, was trying to actually change the cyclic runs and that that has failed.

DR. PETERMAN: Mm-hmm. Right.

Q And so we wouldn't want to be trying to do that at this point in time, given the unknowns we're working with and the catastrophic responses that we're experiencing. Would you agree with me on that?

DR. PETERMAN: You'd want to consider very carefully what the outcomes of that last experiment were before embarking on another one.

Q Especially given the unknowns in the marine environment and what the salmon are responding to

if you add up all those on --
DR. PETERMAN: Sure, that's right.
Q So as it relates to those recommendations, the one that we have to take extreme caution on, and in fact exercise a lot of precaution, would be as it relates to - I mean, all of them, of course - but the recommendation number 5 has its difficulties.
DR. PETERMAN: Definitely.
Q All right. I'm going to change the order in which I'm doing something 'cause I'm very cautious (sic) of the time. So I want to take you to a paper that you wrote, Mr. Peterman, called "Possible Solutions to Some Challenges Facing Fisheries Scientists and Managers."
DR. PETERMAN: Mm-hmm.
MS. GAERTNER: Mr. Commissioner, this is the topic that is a little bit further off from his direct report that you have in front of us, but one that was in his résumé. Dr. Peterman was kind enough to point it out to me when I asked him about some of the challenges of communicating uncertainties and precautions amongst managers.

You know, instead of me taking you through this report, I think it would be much more effective for Dr. Peterman to give us an overview, a short overview of what this article tells us, particularly as it relates to mixed stock fisheries and the types of challenges associated with communicating models and uncertainties to various different First Nations and other harvesters and other managers and all of the complexities associated with that.
Q As I reviewed your report, you weren't particularly - or this article - you weren't particularly focusing on any particular fishery.
DR. PETERMAN: No.
Q But when I read it in the context of this inquiry, it appeared that the Fraser River sockeye salmon seemed to exemplify some of the most significant challenges that you identify throughout this, and if you'll agree with me on that, I think it would be useful, and then I'm going to highlight some of the communication issues that you go to at the end.
DR. PETERMAN: Okay. How long do I have?
Q You have about five minutes.
DR. PETERMAN: Okay, right. So I'll underline one

thing that you mentioned, that is, this does not refer specifically to salmon, but it certainly is relevant to it. This is a paper from a keynote address I gave at a conference in Europe in 2003, and so it was much more suited to broad level questions on dealing with uncertainty in fishery science and management.

I guess the first main point in this article was to highlight the fact that we have at least four major sources of uncertainty that we need to deal with when doing fisheries management and fishery science feeding in advice to managers. The first is natural variability. Natural variability includes both long-term trends like we've been describing in the productivity, as well as short-term variations around those trends.

Then there is what we call "observation error" or "measurement error" according to some people, so we don't have perfect information on the number of spawners. We have pretty decent results when we apply various methods, but they're not perfect. Same with total adult returns. We have samples from the catch and we have certain reporting mechanisms, but there's variation between what's actually caught and what is actually appearing on our final records. So there's observation error.

Then there's another source of error in these fisheries management systems that's very important, and that is, when we're trying to understand the dynamics, we're making assumptions when interpreting the data about how the system actually works. So this has come up before where we tend to rely on the Ricker model and the Larkin model for sockeye salmon as the two alternative views of the world. Those are probably wrong, but those are the best we've got. So that's another source of uncertainty and it's called structural uncertainty. It's where you don't really understand the two structures of the underlying system because we don't have enough information.

Then the final source of uncertainty in the management system is that even if you had perfect information of the first three types, no natural variability, perfect observations and perfect understanding of the underlying dynamics of the system and you chose the appropriate management

action to meet your objective, there would be then what we call implementation uncertainty or outcome uncertainty. That is, you apply regulation but guess what? The harvesters are going to go out there and they're going to apply it in space and time in some manner that will probably slightly deviate from the regulations, because they get higher catchability than they expect at a given time or lower catchability than they expect due to environmental factors maybe. Fish are too deep compared to normal or something like that.

So there's some uncertainty around reaching these spawning targets for instance. If that's the objective, you'll notice you look historically at the data. The actual escapements deviate from the targets. Sometimes they're above, sometimes they're below.

So what this paper starts out with is this notion that there are four key sources of uncertainty at a minimum that should be followed through all the way to the management advice, and from there to the management decision-making that takes those uncertainties into account. So that process of taking those uncertainties into account in the scientific advice is called the risk assessment phase. There are various methods to do that, that I won't go into.

When that scientific advice is presented to decision-makers with the uncertainties fully described, one of the challenges is to adequately communicate that uncertainty in a way that's meaningful to the decision-makers. As you can imagine, most decision-makers are not quantitatively trained in the fisheries field and so they don't have experience necessarily in understanding probability distributions and how to interpret them.

So part of our job, as scientists, to do risk assessment, is to portray those uncertainties in a practical manner that makes sense to the decision-makers.

Then we get into the risk management phase, so what I just described as the risk assessment phase, risk management phase is where the decision-makers come to some evaluation step where they start to look at what are the chances of various types of outcomes occurring, given the

four sources of uncertainty that I've described that are out of their control. These days, that is, in the last five to ten years, most fisheries management agencies around the world have recognized that objectives need to be specified in terms of probabilities. That is, they recognize they're not going to exactly meet some target. What they want to do, though, is to have a certain probability of meeting the target, some acceptable level of probability or, if it's not a target, if there's something they want to avoid, an objective would be we want a greater than 70 percent chance that the population is going to fall below some number. That's another probabilistic objective.

So that's an element of the risk management step that's critical, is defining the objectives that recognize uncertainties explicitly. Then there are various steps that managers have to go through obviously in making decisions about trade-offs. So they might say, well, we've got multiple objectives to consider here -- this is just hypothetical now in general -- but they might say, all right, we don't want the spawners to fall below some number, say 4000, with greater than 70 percent chance. We also don't want the recreational catch to fall below some number with more than 50 percent chance. Same thing with First Nations, same thing with commercial catch. So you might have multiple objectives like that.

Well, I can tell you from experience that there are very few cases where you can meet all of those objectives simultaneously with one clear action. There have to be trade-offs made. So part of the assessment process that scientists do is say, well, what if you relax the required probability of meeting each of your multiple objectives, or any one of them, pardon me. Would that allow you more scope in what management actions you can choose? It usually does.

So our job as risk assessors is to map out that space of trade-offs that the decision-makers would need to go through in coming up with a final decision. So I guess, in a nutshell, that's what that paper is all about.

Q All right. And then I'm going to take you specifically to what's page 1339 of the paper, on communication.

Commissioner Cohen has heard a fair bit of evidence so far about the challenges associated with communicating some of this complex scientific data to First Nations in particular, I'm going to focus on, and the challenges associated with that communication are not a singular event. They've been going on for quite a while now.

DR. PETERMAN: Mm-hmm.

Q I see from your review of the communication some very strong comments about needing to take concerted effort by managers, scientists and what you call stakeholders through ongoing involvement in interaction and analysis to improve mutual understanding. That's found at the bottom of page -- the first column on 1339.

DR. PETERMAN: Right, mm-hmm.

Q And then you go over on page 1340 to talk about specific problems of communication in the perceptions of risk.

DR. PETERMAN: Mm-hmm.

Q And risk assessments are of course something that is happening actively in some components of Fraser River sockeye management now as you're aware, right?

DR. PETERMAN: Yes. Yes.

Q And, again, and I'll leave it for those to read and not take too much time, because I'm not challenging any of your recommendations there, but you conclude - and I want to give you an opportunity to think of it - we need many more innovative approaches to facilitating two-way communication.

I wonder if I'm correct in understanding that two-way communication includes framing questions for scientists, framing how the data is collected, framing -- understanding how it's presented and interpreted. It's not just two-way communication once all the data has been collected and analysis is presented, but rather that two-way innovative approach to communication really needs to happen as you're beginning to ask the questions or frame the questions of the scientists. Would you agree with on that?

DR. PETERMAN: Well, ideally, yes, and I think the challenge is to get everybody involved in a collaborative process right from the start. It's a time-consuming business.

Q But an important part of the business if we're going to have science help us in making critical decisions in the management of the Fraser River sockeye, would you agree with me?
DR. PETERMAN: Absolutely, yes, that's right.
Q And in fact, it's not something that you picked up on your recommendation, but it's clearly something that you think is important in the ongoing involvement of science in the management of Fraser River sockeye is to ensure that there are very useful and well-developed communication systems, they're innovative, iterative and continual so that people can build the expertise and use it and understand what science can have to offer; is that correct?
DR. PETERMAN: Yes, I agree with that.
Q All right. I'll take you now to your recommendations and I just have a couple of questions, Mr. Commissioner, and then I'll be finished. I am five minutes over.
I wanted to just speak to recommendation 2 and 3 -- oh, yes, I do want to mark that exhibit. Thank you.
MS. GAERTNER: Could I have that article marked as the next exhibit?
THE REGISTRAR: Exhibit number 754.
MS. GAERTNER: Thank you.

EXHIBIT 754: Article titled "Possible solutions to some challenges facing fisheries scientists and managers", June 2004, authored by Dr. Peterman

MS. GAERTNER:
Q A couple of things you said in your testimony, why would agencies not want to share their data?
DR. PETERMAN: Well, it's not so much agencies as individual scientists, so as you may know, research scientists in DFO and research scientists in other agencies, National Oceanographic and Atmospheric Administration have, as part of their role, to publish their research and they're rewarded on the basis of the innovativeness and quality of their research. So good quality publications are one index of that. Sometimes people don't like to share the data until they're published. So that's what I was --

Q Which is often quite a bit later than the data has become final -- the data is final, it's now in an analysis and then papers get written, but the data could be available much earlier than that.
DR. PETERMAN: Yes.
Q So again, why wouldn't agencies want to share their data? You're talking about hard data in recommendation number 2, aren't you? You're not talking about analyzed data, right?
DR. PETERMAN: Well, it could be either or both. Well, we refer to both. You need raw data before you start analyzing, but often you want to have some processing of the raw field data, you know, that might come in on a daily basis and then you roll it up in to an annual number, for instance. Most people aren't going to care about the daily data. They're only going to care about the annual numbers.
Q Now, I take it that you went only as far as agencies in Canada and the U.S. and didn't speak about First Nations or stakeholders. Was there a reason for that? Presumably anybody collecting useful data should be involved in this, and anybody with important contributions to the management of salmon may be useful in the participation of recommendation 2 and 3?
DR. PETERMAN: In the recommendation 2 and 3, yes. I say in our own case, we knew who had the data that we needed to do the kinds of analyses and those are the agencies that I described before, the four of them. But you're right, that anytime - especially these days - we're getting additional information coming in, it should be treated in the same way, no matter who it's coming from. The same quality control process, same rigour as any other dataset.
Q All right. Finally with respect to recommendation 5, a couple of people have asked you this question, and by my observations, you managed to avoid answering it. So I'm going to ask it in a little bit -- more directly. You know, one of the things that a friend of mine observed about the ocean is it's -- while it may be cold, it's not a fridge. You just can't open the door and check out what's in it in less than five minutes. It takes a long --
DR. PETERMAN: Right.

Q -- time and it's difficult to figure out what's there and how the -- all that it's influencing. People ask, "What do you recommend we do?" And your response was, "Well, how much money do you have?"
DR. PETERMAN: Mm-hmm.
Q And, you know, what's my budget? What can we do best?
DR. PETERMAN: Right.
Q Let's not look at hard numbers, but let's say you had a minimum budget and then you had a maximum budget and you could do everything. What would you be recommending? Like if we had a very minimum budget, as is quite likely given the -- if we were relying solely on Department of Fisheries and Oceans, we might not have a large budget to do this. Perhaps if we can get good collaborative work with other organizations, we can get a bigger budget. But where would we start as a minimum --
DR. PETERMAN: Mm-hmm.
Q -- and where would we go if we had a lot of money?
DR. PETERMAN: Okay. So now this is specifically with respect to recommendation 5, I understand, which is talking about getting a better understanding of:

> ...salmon migration routes and timing during out migration, as well as their residence in the marine environment.

Is that right?
Q Yeah, and --
DR. PETERMAN: Okay, so this is only talking about recommendations for research for that topic?
Q Well, let's talk about anything as it relates to the marine -- so you could take 4 and 5 because you're talking about out-migration in 4.
DR. PETERMAN: Mm-hmm, okay.
Q This is all marine research.
DR. PETERMAN: Okay. All right, sure. So, again, Brigitte, I'm sure you'll have something to add to what I say, so you can think about that.
Well, let's take an example of what's going on right now and how that could be augmented. So as far as I understand, there are only two research projects being carried out by DFO on marine processes related to salmon. One is the

work led by Dick Beamish in the Strait of Georgia, and the other is led by Marc Trudel, T-r-u-d-e-l, Marc with a "c", M-a-r-c. Those are focused on sampling fish at a particular time and place.

In the case of Marc Trudel, looking at a larger set of physical and biological variables as well, zooplankton, currents, salinity, temperature in the ocean, that sort of thing. Marc Trudel does his work off the north end of Vancouver Island, in Queen Charlotte Sound, in Queen Charlotte Strait, but over a very limited area. As I said, Dick Beamish is in the Strait of Georgia.

So if we really want to understand what is going on in the marine environment with these fish, I think we need to have a lot better coverage of where these fish are at various times, and that may require some tagging of a great extent, much larger than has been done for years, decades in fact, to find out where these fish are and what is happening to them.

So I expect that with a minimum budget -- I don't know whether by "minimum" you mean something larger than we have now?

Q Sure.

DR. PETERMAN: Okay. So it would be nice to see the kind of program augmented that Marc Trudel is doing, for example, off the north coast of Vancouver Island. I think we've seen examples of what progress can be made. I think I mentioned yesterday the Bering Sea project. In the Bering Sea, they have a tremendous amount of effort and it's totally out of scale with anything we can do in Canada in terms of costs, but it shows that if you have a concentrated effort from a large number of scientists, physical oceanographers, climatologists, zooplankton biologists, phytoplankton biologists, fish biologists, all working together at the same time and place, you can gain a lot of information quickly.

Right now, as I understand it, you've got Marc Trudel and whatever few colleagues he has along on his trips sampling as many different things as he can in the limited time available and I think that's a great place to start, to get more scientists involved if we're looking at pathogens, predators, whatever hypotheses you want to check

out.
So I guess that would be what I would expand recommendation 5 to say. So, Brigitte, do you have anything to add to that?
DR. DORNER: I feel a little bit out of my depth, because I'm not up to date on what modern sampling methods can and cannot do, but I would agree that the first priority would be to understand where the individual salmon are actually going, not just as an aggregate but actually by stock and have that resolved a little bit more temporally.
DR. PETERMAN: Mm-hmm.
DR. DORNER: The other thing that I would like to point out is that it would be really helpful to know more about ocean conditions, in particular in Canada. I know that in the U.S., there's a lot more known about connections between large-scale phenomena and fine-scale phenomena, and usually those data stop at the border because Canada didn't sample the same kind of things.
DR. PETERMAN: Right.
DR. DORNER: I think it would be helpful to look a little bit into that and see what we could do in terms of just continuing the things that have been done further south.
THE COMMISSIONER: Mr. Gaertner, I think we'll take the break --
MS. GAERTNER: All right.
THE COMMISSIONER: -- for ten minutes, and then we'll come back and then we'll adjourn at 4:00. Thank you very much.
MS. GAERTNER: Okay. Thank you.
THE REGISTRAR: The hearing will now recess for ten minutes.

(PROCEEDINGS ADJOURNED FOR AFTERNOON RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: Order. The hearing is now resumed.
MS. GAERTNER: Mr. Commissioner, I have no further questions of the witness. I just have one housekeeping matter that I need to attend to.
And if Mr. Lunn, if you could call up Exhibit 413? Last week, when I was examining Dr. English, I discovered, to my chagrin, that this exhibit referred to a memo of Dr. Woodey of 1996, as being attached, but when we looked at the exhibit it

didn't have that memo attached.

We have now found that memo in ringtail, Canada's ringtail No. 059755, and that's a memo from Dr. Woodey to the members of the Fraser Panel, regarding changes in stock groupings. You'll recall those questions of Dr. English, perhaps. That will feel like months ago, as -- it was last week, but the -- I couldn't refer to that memo, because it was not yet in evidence. However, it had been and should have been attached to Exhibit 413. And so what I'd like to now do is have it marked as an exhibit. It could be marked as Exhibit 413A, as a separate exhibit, because it was not marked at the time Exhibit 413 was entered.

THE COMMISSIONER: All right.

MS. BAKER: Mr. Commissioner, the memo which is referred to by my friend was not attached to the document that was shown to the witness at the time this exhibit was marked, so it's not that there was an oversight in the exhibiting process, it actually wasn't attached to the memo. And in ringtail the exhibit doesn't have this memo attached as part of the ringtail document, so they are different documents. It could be that this is the memo, probably is the memo that was referred to in what is now marked as the exhibit, but in terms of process, the exhibit is what it is and we can't now, after the fact, now say, you know, there is a copy of this memo somewhere attached to a document prepared by Ken Wilson and therefore that now becomes the exhibit, because that wasn't the exhibit as it was presented in the hearing.

I don't have any -- I'm not saying that that's not a valuable memo from Mr. (sic) Woodey, but that's not, in fact, the document that was put to the witness, so I do have some concern about changing our exhibits midstream like this.

MR. LEADEM: Mr. Commissioner, sorry, I don't mean to interrupt, but it was me who tendered that evidence into the record through my witness, Mr. Wilson, at the time, and it was my mistake for not including that memo. My intent was to always put in the entire memo plus the appendix. And I was not able to find the appendix in ringtail in sufficient time to do so.

My understanding, now, is that we can put the

whole thing in and then it will make sense, because there was some material referenced in the memo that references this particular appendix.

So it makes some sense to do as Ms. Gaertner is suggesting so that we have the complete record.

MS. BAKER: I hear my friends but, unfortunately, that wasn't the document that was put to the witness, and so the document, if it had been before the witnesses, may have generated other questions or other evidence, and we can't, unfortunately, go back and recreate what didn't happen.

THE COMMISSIONER: Well, I think the solution, Ms. Gaertner, is to mark it for identification purposes for today, and to allow Ms. Baker and yourselves, participants' counsel, to discuss what you are proposing, which is to have it marked as part of Exhibit 413. And if a resolution cannot be arrived at between counsel, then you can make further submissions to me and I will deal with it.

MS. GAERTNER: All right.

THE REGISTRAR: It will be marked as Exhibit -- or, I'm sorry, a document, letter Z.

THE COMMISSIONER: Z for identification, then, thank you.

MR. REGISTRAR: That's correct.

MARKED Z FOR IDENTIFICATION: Memo from J.C. Woodey, Pacific Salmon Commission, to L. Loomis and A.F. Lill, Fraser River Panel, dated April 18, 1996, re: Assessment of the classification of stocks to stock group

MS. GAERTNER: Thank you, Mr. Commissioner.

THE COMMISSIONER: Thank you.

MS. GAERTNER: And those are my questions for today.

THE COMMISSIONER: Thank you very much.

MS. FONG: Mr. Commissioner, Lisa Fong, for Heiltsuk Tribal Council. Mr. Lunn, if you could assist us by pulling up Exhibit 345, Appendix 5?

MR. LUNN: Certainly.

MS. FONG: And at that appendix are the management areas for Central Northern British Columbia.

CROSS-EXAMINATION BY MS. FONG:

Q Drs. Peterman and Dorner, I'm just going to pick up on Ms. Gaertner's question regarding your

Recommendation Number 5, you stated that there needs to be more research on salmon migration routes that have (sic) been done than in more than a decade. Now, do you know, looking at this map for reference here, if there's Fraser River sockeye salmon migration through areas -- Management Areas 7 and 8, either migrating out or returning to spawn?

DR. PETERMAN: Well, most certainly returning to spawn, and I assume the juveniles go out through there, too, yes.

Q Okay. And Dr. Dorner, do you have anything to say about that?

DR. DORNER: That would be my assumption as well.

Q Thank you. And both of you, on what basis do you have that belief? What kind of research has been done; are you able to tell us?

DR. PETERMAN: Training and logic. That is, if there are spawners in Area 8, they probably come in through Area 8 to go to spawn -- I'm sorry to be facetious, I don't get what you're getting at. Of course, if there are spawning populations in those areas, they have to have migrated through those waters, both as juveniles and as adults.

Q And so when you say that there needs to be more research on salmon migration routes, I guess what I'm wondering, because what I'd understood you had said to Ms. Gaertner was that there hadn't been a lot of research done --

DR. PETERMAN: Yeah.

Q -- in over a decade? Are you aware whether there has been any research done on the migration route through 7 and 8?

DR. PETERMAN: No, I'm not aware of that research. There may have been, but I'm not aware of any.

Q Okay.

DR. PETERMAN: Do you know, Brigitte?

DR. DORNER: I do not know.

Q And are you able to say to me that any research that's done on salmon migration routes, so here not specifically 7 and 8, but the research that's done, is First Nations traditional knowledge about where and when the salmon have been harvested, is that relevant to that sort of research?

DR. PETERMAN: Absolutely.

MS. FONG: Thank you, those are my questions.

DR. PETERMAN: Oh, okay. Thank you.

MS. BAKER: Thank you, Mr. Commissioner. I just have a couple of points of reply and then we can close, hopefully before 4:00.

RE-EXAMINATION BY MS. BAKER:

Q Mr. Harvey and Ms. Gaertner both took you to Exhibit 399, which is the 2010 Pestal document with the spawner recruit numbers for all the stocks at the back, and Mr. Harvey focused on Chilko and Quesnel and Lake Shuswap, and Ms. Gaertner looked at Quesnel, I think, in particular.

In your work, you looked at recruitment and spawner numbers for all stocks in all years, right?

DR. PETERMAN: Yes.

Q Okay. And is there any -- is it scientifically valid to draw conclusions from individual recruit numbers for individual years and individual spawner numbers for individual years as pulled out in isolation, virtually?

DR. PETERMAN: Well, it depends on the question you're asking. If you're asking the specific question, "What was the success rate for the spawners in 2005 that led to the returns in 2009 in the Fraser system," then, yeah, it would be legitimate to pull out those particular data. But if you're trying to generalize and say, "What happened to that cohort also happened across all space and all time," no, then it's not legitimate.

Q Or, this is the best number for all purposes and for -- that you -- or number of recruits that you -- or, excuse me, number of spawners that you see in one year will be the best number for all time for all spawners?

DR. PETERMAN: Oh no, no, of course not.

Q Okay. Both Mr. Harvey and Mr. Rosenbloom talked to you about escapement setting and density -- delayed density dependence. I just want to ask you, is it your understanding that the Larkin model, which is used in the escapement setting process currently in use, the FRSSI process, is a model which accounts for delayed density dependence in the stocks that show cyclic dominance?

DR. PETERMAN: Yes, that's right, that's what I

understand, the FRSSI process includes the Larkin model.
Q And that model accounts for delayed density dependence --
DR. PETERMAN: That's right.
Q -- and stocks that show cyclic dominance?
DR. PETERMAN: That's right.
Q All right. Mr. Rosenbloom also asked you some questions about the carrying capacity of nursery lakes, and you agreed that that kind of information and research was important and was good to have?
DR. PETERMAN: Yes.
Q For the work -- for the research to be done to understand the carrying capacity of nursery lakes, I take it is that research that would require specific primary research on those lakes, time series, data, other kinds of physical research on those lakes?
DR. PETERMAN: Well, interestingly, it doesn't necessarily require time series data, at least not a lot, not like we're talking 30, 50 years. One of the innovative things that the DFO group did at Cultus Lake, lead by Ken Shortreed and Jeremy Hume, was to develop this PR method, the photosynthetic rate method, that could be used to estimate the rearing capacity of the lakes for juvenile salmon by taking relatively few samples in a few years.
Q But it would require samples to be taken from the lakes over a couple of years?
DR. PETERMAN: Oh yes.
Q So it couldn't just be done by looking at paper and data already available for the bulk of the nursery lakes we have in the Fraser system?
DR. PETERMAN: Well, if they already have the data on those lakes, yes; if they don't have the data, no.
Q Do you know if the data is in existence right now?
DR. PETERMAN: Well, yes, they have -- the Hume et al 1996 paper in the *Canadian Journal of Fisheries and Aquatic Sciences*, I think, is a good summary of the lakes for which they had such data.
Q At that time?
DR. PETERMAN: At that time. And they probably -- well, I shouldn't say "probably". It's likely that they have some data, at least, since then, but I couldn't tell you what.

Q Okay. Would the carrying capacity of nursery lakes explain the productivity decline across the stocks that we've seen in the last 10 years?
DR. PETERMAN: Are you asking, has the carrying capacity decreased?
Q No, I'm asking whether research, that kind of research, would help us explain the decline in productivity that we're looking at in the Commission.
DR. PETERMAN: Well, I'm going to say, no - Brigitte might have a different view - because I think what we have suggested is it's very likely that what has happened to the change in productivity, recruits per spawner, that is, the entire lifecycle, is not so much due to what's gone in the freshwater system, with the exception of the Quesnel Lake system --
Q Right.
DR. PETERMAN: -- and that we should probably be putting research priorities elsewhere.
Q Right.
DR. PETERMAN: That's my opinion. Brigitte, do you want to add to that, or subtract?
DR. DORNER: I fully agree with that. I wouldn't -- if it was a matter of money allocation, that's not where I would spend my money.
Q This is directed to both Dr. Dorner and Dr. Peterson, but primarily maybe to Dr. Dorner, because you've been on a screen and it's hard to get a word in edgewise. Is there anything that you wanted to have an opportunity to respond to that you've been questioned on over the last two days that you'd like to speak up about now before we end the session?
DR. DORNER: I can't think of anything right now. I think Randall did a pretty good job, overall.
Q And Dr. Peterman, same: Is there anything that you need to clarify that has been left?
DR. PETERMAN: No. If you're referring to things that might have been said in error or things that I wish to add, probably not. I've sufficiently repeated myself that you're all tired of hearing it. So thank you for the opportunity, though.
MS. BAKER: Okay, thank you. Those are my questions. I think we're done for today. Oh, Mr. Taylor has something that he wants to add.
MR. TAYLOR: I realize I may be pushing my luck, it's

10 to 4:00 or five to 4:00. The exhibit that is now Z, I think if counsel had three minutes we might be able to pin this down, or I could do it with your -- with the Commissioner, in front of you, right now, if you want? But I don't know what they're going to say.

THE COMMISSIONER: I'm sorry?

MR. TAYLOR: I have an idea how we can deal with that.

THE COMMISSIONER: Let's hear it.

MR. TAYLOR: Do you want me to just blurt it out?

THE COMMISSIONER: Absolutely.

MR. TAYLOR: The issue -- no one seems to be opposed to that document going in as an exhibit, the issue seems to be the mechanics of exhibits. It seems to me that if we put it in as it's own exhibit, with its own number, and in the title of the exhibit, which would include what it is, a letter of such and so, we also add "been an attachment to" -- "been an attachment to the original version of Exhibit 413," or whatever the other one is, and that seems to solve, to me, Ms. Baker's concern about the integrity of the record. I can see her point in that you want to be clear that 413 existed in a particular form when it was put to the particular witness it was, but it also seems to deal with what both Ms. Gaertner and Mr. Leadem are saying.

THE COMMISSIONER: Mr. Taylor, the only reason I'm -- I'm not saying that's not a good idea. That could very well be the resolution for this. My only concern is not all counsel are here today, and I just want to make sure that if any other participant's counsel have a few on this that they have an opportunity to express it before we necessarily take the step you're suggesting. And I'm not saying it's an eminently sensible way to go, but I think perhaps an opportunity for all counsel -- maybe, perhaps, none of them have a view on this, but at least give them the opportunity.

MR. TAYLOR: Yes, good point. And so I'll just leave that, perhaps, with Commission Counsel, that if they want to float that with everyone, that's certainly something I subscribe to.

THE COMMISSIONER: All right. Well, I appreciate it, and that may be the answer, but as I see, I just want to err on the side of giving them an

opportunity.
MR. TAYLOR: Thank you.
THE COMMISSIONER: Thank you very much. Thank you, Ms. Baker and Ms. Tsurumi, for your conduct of this session, and to Drs. Peterman and Dorner for your paper and for your willingness to stick around for two days while lawyers had an opportunity to cross-examine you. Thank you very much for that.

I want to wish those participants' counsel who are here, and those who are not here, as well, a very happy long weekend. I know you'll all use it to the best of your advantage to R&R a bit; you have all earned it a great deal.

I must say that, as you know, it's written into our terms of reference that the part of the Commission's mandate is to encourage broad cooperation amongst the participants' counsel and stakeholders, and it's been my experience, since we started this process, that for the most part you have all done what you can do to assist me in that regard, and I am forever grateful that you are making those efforts, because that is a very important ingredient of this Commission. And as I say, from my observation deck here, there may be things going on behind the scenes that are things that counsel have to do to represent their client's interest, I've been around long enough to understand that, but at least from my observation point, I'm very grateful for the cooperation you've shown in this hearing room, and I thank you for that very much.

Enjoy your weekend, and I believe we're adjourned, now, till May the 2nd, at 10:00 a.m.; is that -- at 9:00 a.m.; is that correct?
MS. BAKER: I don't think we've communicated that to everybody, yet, but word will be going out this afternoon that we're going to start early on May 2nd, because we are not able to sit on May 3, so we're going to try and get an extra hour in on May 2 by starting at 9:00.
THE COMMISSIONER: In any event, we're proposing to commence at 9:00 a.m. on May the 2nd. Thank you all very much.
THE REGISTRAR: The hearing is now adjourned as indicated.

(PROCEEDINGS ADJOURNED TO MONDAY, MAY 2, 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.

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

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