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

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

Public Hearings

Audience publique

Commissioner

L'Honorable juge /
The Honourable Justice
Bruce Cohen

Commissaire

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

le mercredi 20 avril 2011

APPEARANCES / COMPARUTIONS

Wendy Baker, Q.C. Maia Tsurumi	Associate Commission Counsel Junior Commission Counsel
Mitchell Taylor, Q.C. Jonah Spiegelman	Government of Canada ("CAN")
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. ("BCPSAC")
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")
John Gailus	Western Central Coast Salish First Nations: Cowichan Tribes and Chemainus First Nation Hwlitsum First Nation and Penelakut Tribe Te'mexw Treaty Association ("WCCSFN")
Brenda Gaertner Leah Pence	First Nations Coalition: First Nations Fisheries Council; Aboriginal Caucus of the Fraser River; Aboriginal Fisheries Secretariat; Fraser Valley Aboriginal Fisheries Society; Northern Shuswap Tribal Council; Chehalis Indian Band; Secwepemc Fisheries Commission of the Shuswap Nation Tribal Council; Upper Fraser Fisheries Conservation Alliance; Other Douglas Treaty First Nations who applied together (the Snuneymuxw, Tsartlip and Tsawout); Adams Lake Indian Band; Carrier Sekani Tribal Council; Council of Haida Nation ("FNC")
No appearance	Métis Nation British Columbia ("MNBC")

APPEARANCES / COMPARUTIONS, cont'd.

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

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THE REGISTRAR: Order. The hearing is now resumed.
MS. BAKER: Good morning, Mr. Commissioner. Today we have Project 10 being tendered and Dr. Randall Peterman and Dr. Brigitte Dorner are here to testify. But before we get started with that, there's two housekeeping matters.

The first one is the marking of the Policy and Practice Report that was circulated to all participants on April 1, and it is titled "Overview of Fraser River Sockeye Salmon Net and Gross Escapement Data", and so I'd like that marked, please as the next PPR.

THE REGISTRAR: That will be PPR number 10.

PPR10: Overview of Fraser River Sockeye Salmon Net and Gross Escapement Data, April 1, 2011

MS. BAKER: Thank you. And then Mr. Taylor has a correction to make with one of the exhibits marked earlier.

MR. TAYLOR: Mr. Commissioner, this deals with Exhibit 176, and I believe you would have two copies of that. It is the *c.v.* of Heather Stahlberg on your desk there, one marked "Old", one marked "New". With the Commission counsel's cooperation and other participants' cooperation, we have generally been putting in *c.v.*s with personal information redacted. And by that I mean their home address, home phone number, and that sort of thing, which invariably is at the top of the *c.v.*

The *c.v.* of Heather Stahlberg got in as Exhibit number 176 with her personal information there. And the proposal, and this has been circulated to Commission counsel and all participants, is to take Exhibit 176, which has her personal information, you can see it at the top, and take that exhibit out, and instead put in the copy which should be with you marked "New", which you can see has three points of redaction, which are the pieces of personal information, the work phone number is left in, and it is otherwise

exactly the same document. And so I am seeking leave to take out the old and put in the new as the Exhibit 176 going forward, and the Commission's website would be amended accordingly.

THE COMMISSIONER: Yes, thank you very much, Mr. Taylor.

MR. TAYLOR: All right, thank you. So I can take it that we have a new --

THE COMMISSIONER: Yes.

MR. TAYLOR: -- Exhibit 176.

EXHIBIT 176: *Curriculum vitae* of Heather Stahlberg (replacement with personal information redacted)

MR. TAYLOR: Thank you. And that is it. And with that, when it comes time -- I forgot to introduce myself, Mitchell Taylor, and with me is Jonah Spiegelman, and Mr. Spiegelman will be the counsel examining the witness when it comes to that today.

MS. BAKER: Thank you. And for the record it's Wendy Baker for the Commission, and with me is Maia Tsurumi.

So we are dealing with Project 10 today, and as I identified, I would like that project to be marked as the next exhibit, and then we will go through and have the witnesses sworn, and go through their qualifications, if I could.

THE REGISTRAR: That will be marked as Exhibit number 748.

EXHIBIT 748: Cohen Commission Technical Report 10 - Fraser River Sockeye Production Dynamics, February 2011

MS. BAKER: Thank you. Could the witnesses be sworn.

THE REGISTRAR: Good morning, witnesses.

THE COMMISSIONER: Microphone, Mr. -- there you go.

RANDALL PETERMAN, affirmed:

BRIGITTE DORNER, affirmed:

THE REGISTRAR: Mr. Peterman, would you state your name, please.

DR. PETERMAN: Randall Peterman.

THE REGISTRAR: Dr. Dorner, would you state your name.

DR. DORNER: Brigitte Dorner.
THE REGISTRAR: Thank you. Counsel.
MS. BAKER: Thank you.

EXAMINATION IN CHIEF ON QUALIFICATIONS BY MS. BAKER:

Q We have provided *c.v.*s for both witnesses to all parties, and I would like to go through those first. So the first one is the *c.v.* for Randall Peterman. Thank you. This has a date on it, February 9, 2011, and I would like to just review with you, you currently are the Canada Research Chair in Fisheries Risk Assessment and Management at Simon Fraser University; is that right?
DR. PETERMAN: Yes.
Q And you have a Ph.D. in Zoology from the University of British Columbia?
DR. PETERMAN: Yes.
Q Your *c.v.* is extensive and I am not going to obviously take you through all of the papers and articles and other publications and work that's set out in your *c.v.*, but I would like to highlight some that are relevant to what we're talking about today. You're an author of a publication entitled "Cycles, stochasticity, and density dependence in pink salmon population dynamics" that was published in 2010?
DR. PETERMAN: Yes.
Q You are also an author of an article entitled "Historical trends in productivity of 120 Pacific pink, chum, and sockeye salmon stocks reconstructed by using a Kalman filter", also published in 2008?
DR. PETERMAN: Yes.
Q Also in 2008 you're an author of an article entitled "Uncertainties in population dynamics and outcomes of regulations in sockeye salmon (*Oncorhynchus nerka*) fisheries; implications for management"?
DR. PETERMAN: Yes.
Q 2007, an article entitled "Recruitment and survival of Northeast Pacific Ocean fish stocks: temporal trends, covariation and regime shifts"?
DR. PETERMAN: Yes.
Q You have done work on the "Use of the Kalman filter to reconstruct historical trends in productivity of Bristol Bay sockeye salmon"?

DR. PETERMAN: Yes, that's right.
Q You have also -- and that's an article that was done in 2003.
DR. PETERMAN: Yes.
Q Back in 1995 you and Carl Walters and Mr. Korman did an article entitled "Empirical and theoretical analyses of correction of time series bias in stock-recruitment relationships of sockeye salmon"?
DR. PETERMAN: Yes.
Q You have prepared a paper in a refereed conference proceedings titled "Pacific Salmon Environment and Life History Models"?
DR. PETERMAN: Yes.
Q And that was in 2009.
DR. PETERMAN: Yes.
Q And in fact, Dr. Dorner was a co-author on that?
DR. PETERMAN: Yes, that's right.
Q You also did a paper in a refereed conference entitled "Evaluation of methods to reliably track changes in productivity of fish populations that arise from climatic change".
DR. PETERMAN: Yes.
Q That was in 2004.
DR. PETERMAN: Yes, that's right.
Q And back in 1991 you did a similar paper for a conference, a refereed conference, "Density-dependent marine processes in North Pacific salmonids: Lessons for experimental design of large-scale manipulations of fish stocks."
DR. PETERMAN: Yes.
Q You have many other articles and relevant pieces of work which I'm not going to take you to. I will just note that you have provided advice to the Department of Fisheries and Oceans, correct?
DR. PETERMAN: Yes, that's right.
Q The Pacific Salmon Commission.
DR. PETERMAN: Yes.
Q The regulatory bodies in Alaska and in Washington with respect to fish management.
DR. PETERMAN: Yes, that's right.
MS. BAKER: Mr. Commissioner, I'd like Dr. Peterman to be qualified as a Fisheries Biologist with expertise in fish population dynamics and ecology and risk assessment.
THE COMMISSIONER: I don't see anyone wishing to cross-examine him on those credentials, so he shall be

so qualified. Thank you, Ms. Baker.
MS. BAKER: Thank you. Now I'd like that *c.v.* marked, please as an exhibit.
THE REGISTRAR: Exhibit number 749.
MS. BAKER: Thank you.

EXHIBIT 749: *Curriculum vitae* of Randall M. Peterman, February 9, 2011

MS. BAKER: And then Dr. Dorner, I'd like to turn to your *c.v.* Do you have that, Mr. Lunn?
MR. LUNN: Yes. I'm bringing it up.
MS. BAKER: Okay, thank you.
Q Dr. Dorner, you have a Ph.D. --
THE COMMISSIONER: Excuse me, Ms. Baker. Is it possible to have Dr. Dorner on the screen along with the *c.v.*, or is that --
MR. LUNN: Yes, I'll see.
THE COMMISSIONER: That's great. Thank you very much.
MS. BAKER: Wonders of computers.
Q Dr. Dorner, you have a Ph.D. in Resource and Environmental Management from Simon Fraser University?
DR. DORNER: Yes.
Q And you have worked with Dr. Peterman over many years in fisheries matters; is that correct?
DR. DORNER: That's correct, yes.
Q All right. And your experience includes working with dynamics in management of Pacific salmon, including comparative analysis of time trends and productivity?
DR. DORNER: That's right, yes.
Q And you identify on your *c.v.* a number of software development projects, which would include NCEAS, which is the salmon-and-climate-change model. This is a simulation model for exploring the relative abilities of alternative salmon monitoring strategies to detect and track climate-induced and human-induced changes in salmon productivity.
DR. DORNER: Yes.
Q You also have worked on a development tool entitled CLIM2, which is a closed-loop management strategy evaluation of Pacific salmon dynamics and management?
DR. DORNER: Yes.
Q You have also published a number of articles that

are relevant to the work you've done on this report, including in 2009 an "Evaluation of performance of alternative management models of Pacific salmon (Oncorhynchus spp.) in the presence of climatic change and outcome uncertainty using Monte Carlo simulations"?

DR. DORNER: Yes.

Q In 2009 you and Dr. Peterman were both authors of a paper titled "Statistical models of Pacific salmon that include environmental variables"?

DR. DORNER: Yes.

Q And in 2008 you and Dr. Peterman were both authors of a paper titled "Historical trends in productivity of 120 Pacific pink, chum, and sockeye salmon stocks reconstructed by using a Kalman filter".

DR. DORNER: Yes.

Q And your work includes working with statistics and simulation modelling; is that correct?

DR. DORNER: That's right, yes.

Q And reviewing various quantitative methods in analyzing fisheries?

DR. DORNER: That's right, yes.

MS. BAKER: I'm not going to take you through all of the work set out in your *c.v.* but, Mr. Commissioner, I ask that Dr. Dorner be qualified as an expert as an Ecologist and an expert in quantitative methods and statistics in simulation modelling.

THE COMMISSIONER: Yes, again seeing no one wishing to cross-examine, I will qualify her in those fields.

MS. BAKER: Thank you. If I could have her *c.v.* marked as the next exhibit.

THE COMMISSIONER: That *c.v.* will be marked as Exhibit number 750.

EXHIBIT 750: *Curriculum vitae* of Brigitte Dorner

MS. BAKER: Mr. Commissioner, for my questions, Dr. Peterman will be taking the lead and Dr. Dorner will jump in where she feels that she's got some additional points to add, and then for the questions from the participants, they will answer those questions as directed or as they feel they can best be answered. So I will start with Dr. Peterman.

EXAMINATION IN CHIEF BY MS. BAKER:

Q First, just to overview the purpose of your report, you identify in the "Executive Summary" that your report presents data and analyses to assist in contributing to our understanding of the possible causes of reduced abundance and productivity of Fraser River sockeye.

DR. PETERMAN: Yes, that's right. We were asked to compile data on sockeye populations along the West Coast to compare them with the trends in Fraser sockeye.

Q And as you have just identified, your focus was on productivity trends, not only in the Fraser but also in the Pacific Northwest, including Washington and Alaska?

DR. PETERMAN: That's right. We collected data on 64 different populations, ranging from Washington up through Western Alaska, and we estimated some changes in productivity. Nineteen of these stocks were from the Fraser River.

Q Right. And if we can then please turn, Mr. Lunn, to page 15, Figure 1. This is a map that shows where the populations were located. All right. So did you want to review...

DR. PETERMAN: Yes. So you can see the solid dots there are the ocean entry points for the juveniles as they migrate to the sea from the freshwater habitat. And starting with number 1, that's Lake Washington. And the Fraser River stocks are numbers 2 through 20, and we went up through the West Coast of Vancouver Island, 21-22, Northern B.C., or Central and Northern B.C., Skeena and Nass, and 26-27, and then on out to Western Alaska.

Q Thank you. Productivity is a key term in your report, and I think it would be helpful if you could explain what you mean when you refer to the word "productivity".

DR. PETERMAN: Yes, okay. Well, before I do that, I should just point out that we gathered data from all these populations going back to as far as 1950, and for the Fraser stocks and for the Bristol Bay stocks, we had almost 50 years of data for each of them. For the populations, they generally tended to be shorter datasets, 25 to 35 years.

I should point out also we did not have data for 2010, because they were not available at the time we finished this analysis.

So you asked about the measures of...

Q What is productivity.

DR. PETERMAN: Oh, what is productivity, right. Well, productivity is simply a measure of how successful parents are at producing offspring that mature to come back to the coast, and it's analogous to the what you might think of in business, where you've got measures of productivity that are commonly produced for manufacturing plants, for instance, so many cars produced per worker per week. That's a measure of productivity.

Q And in the work that you did for this report, how did you measure productivity? What measures of productivity did you use?

DR. PETERMAN: We had three of them, and I'll just list them first and then I'll explain them. So they were the recruits per spawner, so adult return to the coast prior to onset of fishing, produced per spawner. Another was what we call the residuals from what we might expect in the way of recruits per spawner, and then the third was a measure of productivity called the Kalman filter. And let me explain those now.

So the first measure, number of adult returns per spawner is shown in Figure 2. So if, Mr. Lunn, you could bring that up, please. It's on page 22. So on the left side, if we could just see the left axis there, please. Right.

So what we have on the left side is an example for the Quesnel Lake stock. The type of data we received from all the agencies, that's the total number of spawners, "S" in red, over time, and then the total number of adult recruits that were produced by those spawners in blue triangles. So for each of the populations we had this time series, and as I said, the time series differed in length, depending on which population you had, but for the Fraser they were about this long for almost all of them.

The next, to the right, shows you the measure of productivity that I'll talk about first, which is recruits per spawner. So it's simply dividing the total of adult recruits by the number of spawners that produced them. And what you can see

there for the Quesnel is that there's been the decreasing productivity of recruits per spawner since the 1980s.

Okay. And then the second measure is the residuals in recruits per spawner -- we can keep on this figure. And so what that residuals measure is, it's what is left over from fitting a Ricker model or a Larkin model. I understand that you've heard about the Ricker model and the Larkin model in the past, as ways of describing quantitatively the number of adults produced. And so what you do for each population like the Quesnel here, is you fit the data of recruits as a function of spawners, and that allows you to then remove the effect of spawner abundance, and what is left over is the environmental effect.

So again with the factory analogy, we've got so many cars produced per worker per week. Some weeks are better than others. You have more cars produced because of certain increase in efficiencies or decrease in inefficiencies one week to the next. Those decreases from what you would expect from the long-term average are the residuals in productivity.

So if we can go to figure 3(a), please, we can look at an example of these residuals, and that's page 26. Great. So if we just look at the left-hand one, please.

Okay. So what I show here on the bottom are a couple of the highly variable lines, the red and grey lines, and the red pluses are the residuals from fitting the Ricker model. You can see that in the label up above there. So this is the time series showing some years where there's an above-zero number, greater than expected productivity, and when it falls below the zero line, it's less than expected productivity. And what is expected again is what is based on the fit of the Ricker model.

We also did the same thing for the Larkin model, which you remember includes a delayed density-dependent effect across generations of spawners, and that shows a very similar trend in this case to the Ricker model, except for the years past about 1993, and we'll come back to that later on.

So in a sense these residuals show you the

effects of environmental processes independent of the spawner abundance processes, so the conditions are more favourable in some years than others, as you can see.

The third estimate is the Kalman filter estimate, and that's shown in the top two lines in this figure, the blue triangles and the solid red dots. So again the red is from the Ricker model and the blue is from the Larkin model. Both of these were fit with what's called a Kalman filter, which just removes some of the high frequency variation, the noise, if you will, and it tries to extract the underlying trend. And the long term underlying trend is what's really most important to the fisheries mangers, the users of fish, and the general public.

So what we want to do with this third measure is to remove the sources of noise to get at that underlying trend. And as you can see, the underlying time trend is much clearer in the Ricker case, the solid red dots, than the red pluses in the series below. And the same thing with the Kalman filter Larkin model results, which are the blue triangles. It's a much smoother trend, clearer trend, than what you get with the grey Xs down below, which are the residuals.

So those are our three measures of productivity: recruits per spawner, residuals from the expected value, and then the Kalman filter values. We calculated all three measures of productivity for all 64 populations.

Q All right. In your report you go through a section describing the methods that you used in a lot of detail, which I'm not going to take you to today in any detail. I'm going to actually begin today with reviewing the "Results and Discussion" portion of your report, which begins on page 33.

DR. PETERMAN: Okay.

Q And the first sort of topic, I guess, is:

> Evidence for delayed density-dependence and the hypothesis that high spawner abundances may be responsible for declines in Fraser productivity.

So that was the task was to look for that evidence. The first analysis that you discuss

relates to spawner and recruit abundances alone without fitting any model, as I understand it. For this analysis, did you look at all of the Fraser River stocks and all of the other stocks, the other, to make up the 64 you described, or did you just look at data on Fraser River alone?

DR. PETERMAN: We just looked at the Fraser River, the 19 stocks.

Q All right. And in your first paragraph under the heading, you refer to a paper from Walters et al. Now, that paper has been marked as Exhibit 417 in the proceedings, and I wonder if that can be made available.

DR. PETERMAN: Well, I don't particularly need to refer to it.

Q Oh, I just want to make sure that the Commissioner knows which report we're talking about.

DR. PETERMAN: Oh, okay.

Q If we can just put that up and then take it away again.

DR. PETERMAN: It's the one from the -- yes, that one right there.

Q Yes.

DR. PETERMAN: Mm-hmm.

Q Okay. So this is a paper which was discussed by Drs. Walters and Riddell earlier this year. I think we can put that away now that we've identified what it is. That paper is referenced in your first paragraph. How did your work relate to the work that was done earlier in Exhibit 417?

DR. PETERMAN: Well, we found the same thing as the Walters et al 2004 paper. We particularly looked for evidence of catastrophic decrease in abundance subsequent to large spawning stocks. And just like Walters et al 2004, we did not find much evidence of that at all. In fact, we found that the adult recruits came back in fewer numbers than the number of spawners, that is, below replacement, in only seven percent of the years across all the 19 Fraser River stocks, all across the 50 years approximately of the data.

So looking more closely at those cases where the number of recruits did come back at fewer than the number of spawners, none of those cases followed an extremely large spawner abundance that led to a chronic low abundance or a stock collapse. So we came to the same conclusion then

qualitatively as the Walters et al on the basis of a bit more data.

Q Right. Out of all the stocks that you reviewed, did any individual stocks show a relationship between high spawner abundance and low recruits?

DR. PETERMAN: Yes, but the relationships were generally fairly weak.

Q And did you do any further statistical analysis in addition to just looking at spawner recruit abundance to determine whether there was a relationship?

DR. PETERMAN: Yes, we did. So the analysis that I just described simply looked at the time series of data on spawners and recruits, and this next analysis to look at delayed density-dependence was based on fitting the Ricker and Larkin models again, and comparing how well they describe the data. Now, as you may recall from previous hearings, the Larkin model includes the effect of spawner abundance in one year -- pardon me. It includes the effect of spawner abundance in previous years on the productivity of offspring from spawners in this year. So let me give you an example.

So if you have a spawning population 1990, then what the Ricker model does is it just looks at the effect of spawner abundance in 1990 on the productivity of that population, in other words, the recruits per spawner is a function just of those spawners in 1990. The Larkin model, though, looks at the number of spawners in 1987, '88, '89 and '90 as possible contributors to changes in productivity from the 1990 spawning.

So that's the conceptual difference between the Ricker and Larkin model. So we fit those two models separately to the data and what we found was that the Larkin delayed density-dependence hypothesis only appears relevant to the Quesnel sockeye stock in the Fraser.

So if we could look at page 37, please, Mr. Lunn, from line 10 in the middle paragraph down, I think you'll see this described.

So that was the general conclusion, that it really appears that the Larkin model was only relevant to the Quesnel. There's a little more subtlety to this, though. It turns out that for the Chilko, Quesnel and Stellako stocks, the

Larkin Kalman filter model fits the data better than does the Ricker model. However, for two of those stocks, the Chilko and the Stellako, the Larkin Kalman filter model still shows a decreasing time trend over time in productivity, just like the Ricker model does. So in other words, they're coming to the same conclusion that there's a decrease in productivity for the Chilko and Stellako. So that means that there is some factor or factors other than spawner abundance in any of the years that's causing the productivity to go down. Because the Larkin model takes into account the effect of spawner abundance across generations, but it still shows a decrease in productivity in the last decade or so. So that means something else is causing that decline.

So from a practical standpoint it does not matter for these two stocks, Chilko and Stellako, that the Ricker or Larkin model fits best, because they actually describe the same time trend, downward time trend in productivity. The only exception then to this is the Quesnel stock, which shows that there's no time trend in the environmental factors.

So if we go back to figure 3(a), if you wouldn't mind, that's on page...

Q 22.

DR. PETERMAN: 22?

Q 26.

DR. PETERMAN: 26. So what you see there, this is the Kalman filter results at the top. You see that the blue triangles for the Larkin model show that the productivity has stayed more or less the same, whereas the Ricker model, the red dots, shows it going down since the 1980s. So what this is saying is that the Larkin model, since it includes cross-generation spawner abundance effects on productivity, is able to explain what the Ricker model was not able to do by just looking at spawner abundance effects in a given generation.

So this means that for the Quesnel stocks shown here, there is evidence that the cross-generation effects of spawner abundance is important, but it's only true for the Quesnel. It's not true for any other Fraser sockeye population.

Q And does that conclusion change your view on the

similarity of your results with the results found by Walters et al in 2004?

DR. PETERMAN: Not really, no. I think qualitatively the results are the same. It's just this one stock is an exception. And we're asking the question about evidence for delayed density-dependence in two different ways in the two analyses. One is a very simple-minded analysis where it asks how often did the number of recruits come back at fewer than their spawning population parents? That was the first analysis I showed, which is what the Walters et al 2004 paper did, and we agree with them. And in this case we're using a little more sophisticated way of asking, is there evidence for delayed density-dependence across generations of spawners. And frankly, I would probably go with this source of evidence as having more weight than the earlier evidence that I just presented.

Q All right.

THE COMMISSIONER: Ms. Baker, I thought Dr. Dorner had something she wanted to add, but I could be in error on that.

DR. PETERMAN: Brigitte, did you want to say something?

DR. DORNER: I would just like to add that there were several other stocks for which the Larkin model was likely better than the Ricker model, but for none of those stocks it really mattered either in terms of time trends whether you took the Larkin model or the Ricker model.

DR. PETERMAN: Right. Thanks for that clarification. So what she's referring to is when I said that only the Chilko, Quesnel and Stellako stocks clearly showed the Larkin model was better, it means they were the only ones that showed a statistically significant, significantly better fit than that Ricker model. Whereas Brigitte's saying there are many stocks which there was just a slightly better fit of the Larkin model to the Ricker, but in practice it doesn't matter.

Q And one of the conclusions, or in relation to the Walters et al 2004 paper, the question was whether there was evidence of catastrophic decrease or collapse in recruitment per spawner at the highest spawning, at highest spawning stocks. Was that something that you would say is evidenced in Quesnel, or are you just saying that there is a

relationship, a density-dependent relationship that you can see in Quesnel?

DR. PETERMAN: No, I would say the second. So we're only seeing evidence for the delayed density-dependence through fitting the Kalman filter Larkin and Ricker models and comparing their trends.

Q Okay. So you wouldn't say that you've seen evidence of a catastrophic collapse --

DR. PETERMAN: No.

Q -- of that stock.

DR. PETERMAN: It's highly cyclic, as everyone knows, with the cyclic dominance phenomenon.

Q Okay. And then on page 45, you set out your conclusions on the section, and if I can just summarize what -- or you could just summarize, what were your overall conclusions with respect to the delayed density effect in high spawning levels on the productivity declines we've seen in recent years in the Fraser system?

DR. PETERMAN: Well, I guess what we can say is that we conclude that although there's evidence that there's two kinds of density dependence, just the standard ones shown by the Ricker model of the effect of abundance of spawners in one year on the productivity of their offspring, and that there is delayed density-dependence for the Quesnel stock, it does not seem to support the hypothesis that efforts to rebuild the Fraser populations in recent years has led to over-spawning in a way that caused substantial declines in productivity. In other words, there doesn't seem to be evidence from this work that supports the concern that the increased spawner abundance has caused the decrease in productivity over time, except perhaps for the Quesnel stock.

Q Okay. On page 45 you then begin the next section of your report, and this looks at the:

> Comparison of productivity patterns across sockeye populations.

And I wonder if we could just start by having you identify what the purpose of this analysis was.

THE COMMISSIONER: Ms. Baker, I wonder if I could just interrupt just very briefly, just so I just get an understanding of Dr. Peterman. You said this

orally, but on page 1 of the "Executive Summary", Dr. Peterman.

DR. PETERMAN: Yes.

THE COMMISSIONER: Just so I understand it. You say in that final sentence about:

> ..."productivity" is the number of adult returns produced per spawner, where "spawners" are the fish that reproduce for a given sockeye population in a given year, and "adult returns" (or "recruits") refer to the number of mature adult salmon resulting from that spawning that return to the coast --

DR. PETERMAN: Right.

THE COMMISSIONER:

> -- prior to the onset of fishing.

I just want to be clear, you're not talking there about the number of out-migrating stock.

DR. PETERMAN: Right.

THE COMMISSIONER: And the other thing is you're not talking about those who actually return to the spawning grounds.

DR. PETERMAN: That's correct. Yes. So the adult recruits are used here to measure productivity because it's prior to the sources of mortality, starting with the onset of fishing, and then you've heard, I think, about en route mortality as the fish are migrating up the river. It's before that. So what you're trying to ask is how successful has a group of spawners been in producing maturing offspring. And "maturing offspring" is really the key phrase here. Maturing adults or what we're calling recruits, prior to the onset of fishing.

THE COMMISSIONER: Okay. And when you say "to the coast", what exactly do you mean by that?

DR. PETERMAN: Okay. Well, so what that is estimated as is the sum of the catches, the sum of the spawners, the sum of the estimates of the en route mortality, that is, as the fish migrate upstream, we've got these various ways of estimating mortality. So those three components added together estimate the number of recruits that return to the coast prior to the onset of fishing.

THE COMMISSIONER: I see. Okay. So you're adding that together.
DR. PETERMAN: Absolutely.
THE COMMISSIONER: All right.
DR. PETERMAN: Yes. Good question.
THE COMMISSIONER: Thank you very much.
MS. BAKER:
Q All right. So I was moving to this next section:

> Comparison of productivity patterns across sockeye populations.

And asking if you could just state what the purpose of this analysis was in this section, and then we'll get into it in some detail.
DR. PETERMAN: Sure. Well, we had three purposes for examining these data across the 64 sockeye populations.

First of all, we wanted to accurately describe these trends in Fraser River sockeye productivity that was the genesis or that were the genesis of this Commission, so that we get a clearer picture, not just what's in the news, but just really describe what has happened to the Fraser sockeye.

The second, we also wanted to determine whether the decreasing trend in the Fraser sockeye abundance and productivity was shared by other sockeye populations on the West Coast. In essence we wanted to ask, is this a unique phenomenon to the Fraser, or is it widespread, and if it's not unique, how widespread is it?

And the third purpose was to generate these productivity estimates of three kinds that I've mentioned to provide to other researchers that are working on the various hypotheses and testing the hypotheses for the Commission in the 12 projects that are going on.

So we did that. We provided all these data to all those other researchers, and I believe you've heard from some of them, and there are others yet to come.

So if we could have a look at Figure 9 on page 49, please. Okay. So what this figure shows is the Kalman filter estimate of productivity, which you will recall is the one that filters out the high frequency noise and looks at the strong

underlying trend, or the signal that we're most interested in. And what we show here is the Kalman filter trends for the best fit model. So it depends on which stock you're talking about, whether it's the Larkin model or the Ricker model, but the key point that I want to make here is that we have these consistent time trends in productivity across many populations.

So if we look at the top left graph, it's the Early Stuart. You see a decreasing productivity, a general trend going from mid-1960s downward. And so this index value is a bit hard to explain. But what it is, is we've scaled all of these time trends in productivity to the same base point, so that we can compare trends across populations. Some populations are much more productive than others and you wanted to be able to compare the trends visually by having them on the same scale. So that's what we have here. And so you can think of the zero point as being the average of a given time series and the deviation above the time series is higher than the average productivity, and below zero being below the average productivity.

So going beyond, then, to the Early -- pardon me, the Fraser Early Summers on the right, top right, you will see that there are quite a few populations there that show the same trend, starting at about 1970, downward trend in productivity for all of them except for the Pitt. The Pitt is the purple triangles, and the Pitt is different probably because it's been affected by hatcheries strongly since the 1960s. So I don't think it's as comparable to the others that are more or less wild populations.

So if we could go to the next graph on the lower left, please.

Q Sorry, before you leave that.

DR. PETERMAN: Oh, yes, sure.

Q The one that is a flat line on zero, could you explain that.

DR. PETERMAN: Oh, yes, thank you for pointing the flat one, there are two of them actually, they're Gates and Raft. And that flat line simply says that the Kalman filter estimate was not able to distinguish any strong trend, and so it just said, well, it must all be due to noise, we'll just put in a flat

line, and so a lot of high-frequency variation, either due to natural causes or observation error in the data. So any time you see a flat line on these Kalman filter series it just says the estimate is that there's no trend over time in productivity.

Okay. Now, if we could go to the lower left, please. Okay. So this is the Fraser Summer group, and I assume you've all heard about the four run-timing groups, so I don't need to explain that. Okay, good.

So we have four stocks here and you'll notice that three of them, all except the Quesnel in green pluses there, all of the other three show this consistent time trend since the late '80s, early '90s, downward time trend in productivity. But most of them showed a similar time trend increasing from the '60s to the late '80s, early '90s. So this time trend is different from what you've seen in the other run-timing groups, but it's similar to the extent that the most worrisome period to us, the last decade, is also showing this decreasing productivity.

And then over on the far right, please.

Q Sorry, did you want to comment on Quesnel; you had excepted Quesnel.

DR. PETERMAN: Oh, yeah, well, Quesnel, I'll just point out again, this is the time series we saw before, it was in a different colour, but it's the flat, relatively flat time series in productivity estimate, which we attribute to the -- well, the fact that it's flat as opposed to decreasing is attributed to the delayed density-dependent phenomenon that the Larkin model represents.

Oh, and I should point out, by the way, that the headings there, with the stock names, Quesnel, Stellako, and so on, if they're in red it's the Ricker model that's the best fit model. If it's in the black it's the Ricker model.

Q Sorry, you just called them both the Ricker model.

DR. PETERMAN: Did I?

Q Yes.

DR. PETERMAN: The red one -- thank you, that's very good. The red ones are the Larkin model best fit, and the black ones are the Ricker model best fit. But as Brigitte pointed out, there's just a very slight difference in all these cases between the

Ricker and the Larkin model trend, so we decided not to clutter up these graphs by showing you all the options. The only one that's really different significantly in this sense we're talking about here today is Quesnel.

Okay. So if we could have the graph to the right, please. Here's the fourth run-timing group now. In this group we have two stocks, again showing the decrease in productivity since the late '80s, that's the Birkenhead and the Cultus. And then many of them are showing constant trend in productivity.

And the outlier here that's really important that we'll come back to later is the Harrison stock, which shows a considerable increase in productivity since the mid-1980s. And that's a very important clue to perhaps what's going on with these Fraser River stocks, because it turns out to have a different life history than the other stocks.

The Harrison go to sea as fry rather than as smolts. They don't rear in the lake, and they have a different, we think, a different out-migrating pattern through the Strait of Juan de Fuca rather than through Johnstone Strait, and I'll come back to this later.

Q So that looks at the Fraser stocks.

DR. PETERMAN: Yes.

Q On page 51 you have similar trends set out for some non-Fraser stocks, and I wonder if we can just review those.

DR. PETERMAN: Yes, please, could we look at that page 51, please. Okay. So now here we have all these non-Fraser stocks.

Actually, Mr. Lunn, could we go back to Figure 1 on page 15, please, this is just the map.

Okay. So just to refresh your memory now, where all these non-Fraser stocks are that I'm going to show productivity trends for in a moment. So Lake Washington obviously to the south of us. There are two stocks, 21 and 22, on the West Coast of Vancouver Island. The Central Coast, 23-24, and then -- oh, and 25, pardon me, Atnarko Lake, and then the Skeena and Nass stocks are 26-27. So those are the northernmost B.C. stocks. Then we get up into southeast Alaska, 28 through 32, and another region that you'll see is called the

Yakutat, and that's stocks 33 through 37, also part of Alaska. And then later on I'll be showing you the Bristol Bay stocks, which are 55 to 62, over in Western Alaska.

Okay. So if we could go back then to Figure 10 on page 51, please.

All right. Now, it's very significant what we found here, that if you look at these time trends, again scaled, these are the Kalman filter time trends in productivity estimates.

Washington in the top left graph, the red dots, shows a decreasing productivity pattern that's extremely close to what we see for the Great Central and Sproat Lake stocks, which are on the West Coast of Vancouver Island.

Q And why are those grouped together?

DR. PETERMAN: Those are grouped together because they all migrate out along the West Coast of Vancouver Island, from the best of our knowledge as juveniles from Lake Washington, go out through the Strait of Juan de Fuca, migrate along the West Coast of Vancouver Island, and go past where the Great Central and Sproat Lake fish go in, as well. And you can see a very strong correlation in their temporal patterns, particularly in this period of greatest concern to us, since the late 1990s. Okay.

And then if I could get the next ones on the right, please, at the top. Okay. These are the Central Coast stocks now. Three of them show strong trends downward in the '80s and '90s, but then a rebounding in the late '90s to high productivity. In particular, those are the Long Lake and Owikeno Lake stocks, or what are otherwise known as Rivers Inlet is the same as Owikeno Lake, and Long Lake is Smith Inlet. So those two populations for some reason had an increasing productivity in that late '90s, early 2000 period.

The Atnarko Lake stock had a small increase during that time, but then a decrease. So again, though, the important point is the last decade or so -- or not the last decade, the last six years or so for all these populations showed a decrease in productivity.

Okay. If we could go now to the bottom left one, please.

Q To the North Coast B.C.?
DR. PETERMAN: Yes, North Coast of B.C. So these are two well-known stocks, North Coast, Skeena and Nass, also showing downward trends, particularly for the Skeena since the late '80s, but Nass not really showing a consistently until the late '90s, early 200s.

And then to the right of that, please. These are the first Alaskan stocks now. This is Southeast Alaska, and one of them, the Speel there, solid green dot, green pluses, just shows a flat trend, but all of the others except for Chilkoot show a decreasing trend, as well, this time starting a little bit earlier in the early 1990s, or late '80s. The Chilkoot actually shows an increasing productivity, just like the Central Coast B.C. stocks did.

Okay. And then finally, going a little bit further north along the coast, the Yakutat stocks. Again, several of them are flat, or two of them are flat, and three of them show this decreasing productivity trend, similar to what we saw earlier.

So what I think is pretty obvious from these graphs is that we are not looking at a phenomenon that's unique to the Fraser. It appears, particularly in the last decade, that a decrease in productivity has occurred in these other populations, as well. So to us, to Brigitte and me anyway, it seems that while it's possible that independently operating processes in these non-Fraser regions could have caused a decrease in productivity, it seems very unlikely that that would have happened consistently over such a large area, up the coast to the Yakutat region in Southeast Alaska.

So we don't have any direct evidence of mechanisms obviously causing these changes, but we would suggest that based on this comparison of time trends from non-Fraser with Fraser stocks, that this widespread phenomenon of a decreasing trend is more likely due to some shared process of some type.

Q You also, in the appendices to your report you have done analyses on stocks across all these 64 stocks, and although you haven't shown it in the body of your report, you also have some similar

tables or figures in relation to Bristol Bay, and I wonder if we should just look at those as a comparison as well.

DR. PETERMAN: Yes, definitely.

Q So if I can -- unfortunately the appendix pages aren't numbered, but if you turn to Appendix P3 and you count in 17 pages, Mr. Lunn will have it on the screen which will make it easy for people to see.

DR. PETERMAN: There it is right there.

Q Yes. But it is the 17th page into Appendix P3.

DR. PETERMAN: Right.

Q And at the bottom half on the left of this page you'll see the Bristol Bay South and North, a similar trend analysis.

DR. PETERMAN: Yes. Exactly. Okay. So you've undoubtedly heard about the Bristol Bay sockeye in comparison to B.C. sockeye, and it's important to note that these stocks, not only are they extremely abundant, but compared to the B.C. sockeye stocks, but they also had different time trends. So I highlight these because they're very well documented, they go back to the 1950s, and these Kalman filter time trends again from the best fit model show quite a different pattern from the rest of the stocks that we've just been looking at.

You will notice that there's a tendency for an increasing productivity since the early '90s onward, with perhaps one exception, and that is the Kvichak stock. And the Kvichak stock is one that we'll undoubtedly come back to later. This used to be the world's largest sockeye population, but it's productivity has gone down dramatically. But you'll notice that all the others have shown this increasing trend, particularly since the early to mid-1970s, when there was a regime shift, oceanographic conditions became much more favourable.

And then in the Bristol Bay North group, which is really the Western Bristol Bay stocks, you see this consistent upward trend in productivity, again since the early 1990s, in direct contrast to what we've seen in the Fraser sockeye, and many of the other B.C. sockeye.

Q When you refer to that term "regime shift", I think that's a term that --

DR. PETERMAN: Ah.
Q -- means something specific to people in your scientific community, but maybe you can explain what that means.
DR. PETERMAN: Okay, sure. Well, there's strong evidence that beginning in 1976-1977, the winter, that the wind circulation patterns changed substantially, which changed the oceans currents, which changed, which led to an increase in productivity of the food supply for the salmon. And so what we see in pink salmon in particular in the north, as well as in sockeye salmon in the north, that you have this increase in productivity. Number of recruits produced per spawner increased dramatically, which led to an increase in total abundance of adults, increased catches, and in some cases increased spawners, as well.
Q All right. So it's an environmental regime, a decadal regime.
DR. PETERMAN: Yes.
Q As opposed to a management regime.
DR. PETERMAN: Yes, thank you for that question, I forgot that. Yes.
Q Thank you. All right. Now, the next analysis that you describe in your report is a correlation analysis of productivity patterns across stocks, and if we turn to page 53 of your report you have a figure that explains some of this. I don't know, the colours seem to wash out quite a bit. I don't know if there's anything that can be done.
DR. PETERMAN: Okay. Well, if maybe we could blow it up a bit, it might help, if it's possible to blow it up. I'm not sure.

Okay. So what this table does is it visually summarizes the pair-wise correlations among the time series, common to -- pardon me, the residual time series of productivity for these different populations.

So let me try to walk you through this. So if you look across the top you'll see all of the stocks arranged from south to north, except somehow Washington got placed above Fraser. But there are all the Frasers, first four columns, four Fraser run-timing groups, then Washington, and then it works up the coast: Barkley Sound, Central Coast, North Coast, all the way up to

Bristol Bay and then farther, AYK is the Arctic-Yukon-Kuskokwim. And then along the left side you'll see exactly those same labels in the same order.

So if we look at one cell, for example, the cell for the Central Coast, so it's the leftmost column, Central Coast and Fraser Early Summer, where those two cross, if you could just point to that Mr. Lunn, please, with your -- with the mouse.

Q And I wonder, I just want to suggest that people may want to look at their hardcopy of the report, because so many of the gradations in colour are washed out on the screen.

DR. PETERMAN: Okay.

Q If you look at the paper you can see many more shades.

MR. HARVEY: Sorry, what page is that on?

MS. BAKER: It's page 53.

DR. PETERMAN: Okay. So if we look at the cell there that, Mr. Lunn, if you could put your pointer on the Central Coast and Fraser Early Stuart, the far left column there, yeah, that one right down there, right where the little pointer is.

What that cell shows visually is the average correlation between each pair-wise comparison between the Central Coast stocks and the Fraser Early Stuart stocks. So there are several Fraser Early Stuart stocks. There are several Central Coast stocks. So you take each pair of those time series of productivities and you correlate them. Each pair has a correlation value. You take the average of all those correlations and that average is shown in colour here.

So if you look at the legend on the right in dark blue it's correlation 1, which means they're perfectly synchronized, and lighter shades of blue means they're positively correlated, that is, they're going in the same direction, but they're not as strongly correlated as they would be if they were perfectly correlated. And then at the other end of the scale, dark red is opposite trends, so the correlation is negative.

So if you look at this pattern here, what you'll see is that there tends to be mostly blue in the lower left triangle of this table, which is the correlations among the B.C. stocks and the

Washington stock. So that lower left, about eight-cell wide and eight-cell tall, yes, that triangle there, it's all blue, or shades of blue with a few whites in there.

So one thing that's very important to ask, though, is does this correlation pattern stand up through time, or does it shift. So these results that you're looking at right now cover the entire time series from 1950 to 2004 brood years.

Now, if we go to Appendix 7, which is on page 126, starts on page 126, we can look at how this correlation pattern changes through time.

So this is the first time block, 1950 to 1985. You see more or less the same trend as what we just looked at, although slightly weaker. There are less blue cells, less positive correlations in that lower left triangle among the B.C. populations than we saw in that long-term average.

So what I want to do now is just flip through three slides in a row. We'll do this one, which is 1950 to '85 - and not yet - but the next one will be 1985 to '95, and then the final slide will be 1995 to 2004. So we're just going to look at timeframes and see how the correlation pattern changes.

So, Mr. Lunn, could we do that, please. This next, okay, there's the '95, '85 to '95, and then the next one is '95-2004. So I don't know if it's possible to get those all in the same position so we can just flip from one to the other, but it's really much -- yeah, okay, there we go. Sorry, Brigitte, you're off --

Q Brigitte's gone, poor Brigitte.

DR. PETERMAN: Brigitte's gone for the moment.

Okay. So let's start back at the original one. So if you look at the colours down in the lower left corner of this table, we'll step through to the next period from '85 to '95, and then from '95 to 2004. And you'll see particularly in the last period there's a much stronger positive correlation than there was in the previous period, which means the stocks are becoming more synchronized in their trends in productivity than they were in the earlier period. Another thing you'll notice is that in this latter period the blue positive correlations appear

farther to the north than in the earlier period.
So, Mr. Lunn, can we go back to the first one again and just flip through the three. So middle period, final period. So you see there's much stronger positive correlation now in those comparisons between the B.C. stocks and the Alaskan stocks than we saw before.
And you'll notice something else happen then, which is that you get a much stronger negative or inverse correlation between these Alaskan stocks and the B.C. stocks up in the top two rows. So they become much redder than in the first period.
So if we just go back to the first one again. Didn't have the AYK data prior to the 1970-something, so we didn't have that one. But next period, please, and then final period. So the final period again much more red, meaning much stronger negative correlation.
So whatever is happening is causing the time trends in productivity among the B.C. stocks to become more strongly positively correlated in the recent decade compared to the past, and more negatively correlated with what's going on in Western Alaska, Bristol Bay, in recent years compared to the previous.

Q Thank you.

DR. PETERMAN: Okay? So I think that's it for these slides. So if you could go back, then, to the main text, please.
So just to describe, then, what we're saying in words, this really reinforces our conclusions from the visual comparison of the time trend plots that I just showed you a little while ago, that there does seem to be strong evidence of positive correlation among the stocks in B.C., Southeast Alaska and the Yakutat region, particularly in the recent decade.

Q Okay. The last piece in your report is looking at productivity patterns through different life stages, and that's set out beginning at page 57 of your report. Why did you do this analysis. What were you looking at across life stages?

DR. PETERMAN: Okay. Well, we had the luxury of having some populations where we had the juvenile abundances as they were going to sea. So rather than the analyses relying totally on the link between spawners and the adult recruits, which has

been the subject of the discussion to this point, we had a few cases where we could compare the productivity in the first life stage from spawners to juveniles migrating to sea, and then from the juveniles to the adults. And then we could ask, where is this decrease in overall productivity happening from the spawners to recruits? Is it in the early life stage, or is it in the later life stages?

Q And how many Fraser River stocks do you -- did you have freshwater data on that would allow you to do this analysis?

DR. PETERMAN: Nine.

Q Okay.

DR. PETERMAN: So if we could maybe show how the comparison was done by moving to figure 12, please, on page 58. Okay.

So this is a set of figures all for the Chilko. It turns out that Fraser sockeye only has one population for which we have good time series of juvenile abundance, and that's the Chilko. The Chilko smolts are those fish that stay over winter one year in-lake, and then go to sea the following spring.

So on the left side you see there the time series of the abundances at the start and end of each life stage, and all we need to do is look at the figures on the right column.

So in the top right column is the number of smolts per spawner, and so that's labelled "J/S" in the top right corner, juveniles produced per spawner. And what you see here for the Chilko -- pardon me, the label on the "Y" axis there, next to the number says "Number of smolts", that's not correct. It should read "Juveniles produced per smolt".

And what you see in particular here is there is a decreasing productivity in that freshwater phase, from the 1960s through to the late 1990s, on average a downward trend. But since the late 1990s, the productivity in Chilko Lake has gone away up. And that says that, well, if that had followed through to the adult stage, you'd expect big increase in Chilko recruits per spawner.

But that didn't happen, because it's what shown in the next graph below, there was a decrease in the number of recruits produced per

juvenile in that period, from the late '90s through 2004. It was more or less constant from when smolts go to sea until they come back from the 1960s, maybe a increasing a bit even, to the 1990s. But then it went down dramatically.

So you put those two life stages together, and we see the series we saw before, which is recruits per spawner - that's down below now, please - and because of this huge spike in the 1989 brood year, spawning year, we can't see the trend too well, but just the change in the freshwater survival productivity going up was not translated through to increasing productivity of recruits per spawner, at least not to the degree observed in that first life stage.

Q Were you able to do this analysis for any non-Fraser stocks?

DR. PETERMAN: Yes, we did, but unfortunately only four of the non-Fraser stocks really had juvenile abundances of any use for this type of analysis, as I say, reasonably long time series. And those stocks were Skeena, Lake Washington, and the two lakes on the West Coast of Vancouver Island, Great Central and the Sproat.

And I want to point out here, by the way, that these other stocks for which we have juvenile data include a substantial amount of time in freshwater. So although we can say for this Chilko stock we're looking at right now, that the second life stage looks at the survival rate from the day when they leave the lake to when they come back to the coast as adults, prior to the onset of fishing, it's mostly in the marine water. There's about an eight- to 12-day period that the Chilko smolts migrate from the lake down to the ocean. But for the other populations that I'm going to talk about here in the Skeena, the Nass -- pardon me, not -- the Skeena, Lake Washington, and the Great Central Lake and Sproat Lake, there's a longer period in freshwater.

So the juvenile abundances do reflect some other period that we can't really separate out what happened between when they were measured as fry in the lake, for instance, and when they were entering the ocean.

Q So for those four stocks that you just mentioned, the juvenile data is fry data, as opposed to smolt

data.
DR. PETERMAN: That's right.
Q Okay. And for the Fraser River stocks --
DR. PETERMAN: Oh, well, no, sorry. Skeena is smolts also.
Q Okay.
DR. PETERMAN: But there's a long way between Babine Fence and saltwater.
Q Okay. And for the other, for the Fraser stocks, we talked a little bit about Chilko, which is you have smolt data to work with.
DR. PETERMAN: Right.
Q On the other Fraser stocks, I understand that there's fry data that you're using --
DR. PETERMAN: That's right.
Q -- as well.
DR. PETERMAN: That's right. And so the fry are estimated the winter before they become smolts, so there's this extra year of mortality in freshwater that's reflected by those estimates.
Q And what was your conclusion from this analysis, and I'm looking at Figure 13 on pages 59 and 60, which seems to set out your conclusions.
DR. PETERMAN: Yes, if we could have a look at that, page 59, please. Okay. So what we see here, then, is the spawner-to-juvenile survival rate in the left-hand column for three of the four run-timing groups in the Fraser.

The top left is the Early Stuart, and you see spawner-to-juvenile productivity has gone up since 1990, but just to the right of that is the juvenile-to-adult survival rate, and that's gone down. And that's what's led to the latter life stages, the juvenile-to-adult stage is what's led to that decreasing recruits per spawner time trend that we looked at earlier for the Early Stuart.

Similarly for the Early Summer runs in the middle two panels there, we have the time series for the spawner-to-juvenile. Now, in those two cases, Gates and Nadina, those also have decreasing time trends in productivity, at least until mid-1990s when Nadina went back up. And both of them, though, show a decrease in the juvenile-to-adult stage in that last decade or decade and a half, at least.

And then at the bottom we have two panels there for the Summers, and the spawner-to-juvenile

stage is flat or constant for all stocks except for the Chilko, we just looked at that, which is the increasing trend there. And on the right-hand side, though, we see that the juvenile-to-adult productivity has gone down for all three of those stocks.

And the fourth one, it's not terribly critical to look at, the fourth run-timing group.

But basically what we see here is that there appears to be a decrease in juvenile-to-adult stage since the 1990s, that has led to the overall decrease in recruits per spawner that we've observed in most of the Fraser sockeye.

Q All right. So I just want to do some overall conclusions with you. What, first of all, did your work contain -- or how much of your work was a quantitative analysis of causal mechanisms for the decline?

DR. PETERMAN: Yes, that's right. Thanks for asking that. We did not do any quantitative analyses of mechanisms causing the trends that you observed. We didn't have the mandate to do it. Our contract said describe the historical trends in productivity of these stocks as a basis for other researchers to test their hypotheses with our output. So what we can say, though, is based on these spatial patterns, I think we can at least hint at some of the hypotheses that might be more viable than others, but I'll...

Q Did you do a quantitative analysis of the delayed density-dependence hypothesis?

DR. PETERMAN: Yes, that's what we looked at earlier.

Q Yes.

DR. PETERMAN: We had our conclusions within the Kalman filter Larkin model was that there is strong evidence that the Quesnel stock demonstrated delayed density-dependence.

Q Okay. I'm just asking that because when you answered my question, you said you didn't do any quantitative analysis, and I understood that the delayed density analysis was quantitative.

DR. PETERMAN: Oh, yes, thank you. Yeah, yeah, yeah, you're right. You're right. I was thinking no quantitative analysis to explain these time trends by using data say on sea surface temperature, food supply contaminants or other things like that.

Q Okay. All right. So what do the shared

productivity patterns that we see in your time trends tell us about causal mechanisms, anything?

DR. PETERMAN: Well, as I hinted at before, to us, and Brigitte might want to jump in here, but to us it seems like there's a much greater chance that there's some shared trend across these populations to varying extents, than that there's some near coincidence of independently operating factors causing a downward trend in productivity of all these stocks simultaneously. It's possible that there's a coincidence. We can't deny that. But it seems unlikely over such a large spatial scale that that would happen.

So it seems like there would be processes operating on a larger scale that would be affecting these populations simultaneously. And these could be things such as oceanographic patterns driven by climatic processes. And in fact I'll just stop there at that one, because there is evidence, other evidence that's been published by many people, including my own group, that shows there is some spatial coherence, some spatial positive covariation among populations in their productivity. But what's been documented before in the literature has not been to this large of a spatial extent. It's never been looked at before.

so there's something that's changed. In our analyses that we published in 2002, we saw a much more constrained positive correlation spatially, but being published in 2002 we had data only up to the late 1990s. Now we've got almost another decade of data where from what you've seen here, appears there is much stronger trends than in the past, and the downward trends.

So that idea of some kind of large scale climate-driven, perhaps, oceanographic processes might have explained this. But I should re-emphasize that you can take a magnifying glass to these trends that we've been looking at and you can say, well, actually they're not the same; they're different. For instance those Central Coast populations had an increase in productivity in the mid-1990s, late 1990s, whereas the Fraser stocks generally did not, although there's a few exceptions there. The Fennell did. And you could say, well, they're not the same. Sure. But I'd

say, and all hypotheses should be, why is it that on average they all had lower productivity at the end of the period than at the start. I think there is a trend that we should be looking at, and those who are testing hypotheses about various mechanisms should be taking into account this very large spatial pattern that seems to be shared.

So just to get back to this coincidence idea. It is possible that you could argue, for instance, well, contaminants have become so pervasive that now they affect all these populations from Washington right through to Southeast Alaska. Sure, that's possible, but I bet that when you hear from Dr. MacDonald in his study, you'll probably find that, well, the contaminant levels are different in different watersheds, and in different estuaries, and in different coastal systems. So that may be the case. It may be the case that pathogens are widespread and it's affecting all of these populations now, and we have no evidence of that, of course, in our analysis. So it could again be a mere coincidence. The same with predators, any of these types of mechanisms are conceivable. But to us, until we see the data, we would suggest that people take a close look at the large-scale pattern as the defining characteristic of what it is they're trying to explain these trends with. It should be a phenomenon that's got a large spatial scale.

So if I could, could we go back to just looking at that Harrison case. This is a very important clue we have. So it's Figure 9 on page 49, please, Mr. Lunn. Yes, it's the lower right corner. If you could just blow up that one, please.

I mentioned this in passing before. So this is the Kalman filter time series again of productivity estimates, and it turns out that that Harrison stock might provide a clue as to what is different about the stocks that are going down from those that are staying constant in productivity, or even going up.

The Harrison fish, unlike all the others in the Fraser, are sockeye that go to sea as fry. They don't overwinter in a lake. So they also apparently rear for two to three months in the

Fraser River estuary before going into the Strait of Georgia proper.

They're also there in the Strait of Georgia longer. Dick Beamish has found them in the northern strait as late as September when most of the other sockeye from the Fraser River, the smolts, are gone.

And then there's one tantalizing hint of evidence that the Harrison juveniles go to sea via the Strait of Juan de Fuca, the south of the island, rather than going through the north. But it's important to note that despite that observation, everyone will jump on that and say, oh, they're going a different route, so that must be -- they've exposed themselves to something completely different. Well, don't forget, the West Coast of Vancouver Island stocks, Great Central and Sproat Lake, as well as the Washington Lake stock, showed a completely different pattern. Those latter three stocks, Washington, Great Central and Sproat, showed a decreasing time trend in productivity, not an increasing time trend in productivity like Harrison. So maybe it's not what goes on, on the West Coast of Vancouver Island.

I'm talking too much probably, so go ahead.

Q That's fine. Can you rule out any mechanisms based on your work?

DR. PETERMAN: Well, I guess, based on what we've seen, yes, we probably can, but very few mechanisms can we rule out. I would say that life stage comparison and analysis that we had for the nine sockeye stocks in the Fraser system for which we have juvenile abundance data showed that in seven out of the nine cases where we had some decreasing time trend in productivity, most of those showed a decrease in the juvenile-to-adult stage, but not in the juvenile-to -- or pardon me, to the spawner-to-juvenile stage. So that suggests to us that there is probably little effect of what goes on in freshwater on that overall time trend in productivity, with one exception, and that is a hypothesis that perhaps the juveniles of the Fraser sockeye are picking up something in freshwater, like a parasite, or virus, or bacterial disease, that doesn't manifest itself as mortality until the fish get out to sea after

they're enumerated as juveniles. So that's possible. But again, we can't speak to that. That will be up to others who are investigating that pathogen hypothesis.

So with that caveat, we'd say that it seems unlikely again over the large spatial extent that we've seen these decreases in productivity that it would be due to shared variation in freshwater processes, because it would -- those freshwater processes would have to occur in all those stocks, from Lake Washington right up through Southeast Alaska, to the same extent and at more or less the same time.

So again it's possible that there's been some outbreak of some disease that we don't -- or some pathogen that we don't know about, leading to disease and mortality, but we doubt it, based just what I'm -- based on the spatial scale, and just being an ecologist and knowing how different these watersheds are.

In fact, just as an aside, those of you who haven't heard this before, the stock assessment biologists in the past often used watershed-specific parasites to identify which stock was which in the mixed stock catch. So they would pick up the fish, they'd sample the fish and say, oh, look, here are these fish. They have this particular parasite. It's only found in this lake and not in the other lakes, so we know that's lake "X". So again, that's just a bit of an aside, saying how likely is it we're going to have the same pathogens all along the coast, operating simultaneously. Okay.

So that's the first hypothesis I think we can, if not rule out, put at a very low probability, is this is a freshwater event.

Second, we can probably rule out delayed density-dependence as the shared source of downward-driving trend in productivity. Like I said before, it's definitely true for the Quesnel stock that delayed density-dependence seems to have occurred, but it does not seem to have played an important role in any of the other stocks we've looked at, and we fit the Larkin model, by the way, to all 64 populations, not just the Fraser. So I think we can rule out the delayed density-dependence argument for explaining this shared

time trend.
The third hypothesis that we can rule out is this en route mortality. So as we were discussing earlier, the en route mortality is what happens to the adults as they enter the river system and migrate up towards their spawning grounds. Some of them are dying, particularly in warm water conditions, high flow conditions, and that is not an explanation for the decrease in productivity that we've had -- that we've described here today. Because as I explained to the Commissioner, the estimate of adult recruits are taking that en route loss into account. So the en route loss estimates are added to the spawner abundances, which are again added to the catches to get the adult recruits. So just by definition, the recruits, the change in recruits per spawner cannot be attributed to the en route mortality directly.
And so that's not to say, of course, that en route mortality isn't important in determining spawner abundance. Obviously it is, and it has been in several years, where the mortality has been as high as 95 percent, apparently.
So clearly that's an important phenomenon that will affect the total abundance of recruits over the long term for some of these stocks, but it doesn't -- the en route mortality does not again affect the productivity measures that we've looked at, recruits per spawner.
So I think those are the only three hypotheses that we can really rule out.

MS. BAKER: Mr. Commissioner, if I can keep going, I have a couple of questions on his recommendations, and then I'll be finished, if that would be all right. Thank you.

Q I want to move to the "Recommendations" section of your report, and they're very clear. I'm not going to take you to all of them. There's just two I want to ask you a couple of questions about, and those are recommendations 2 and 3. Sorry, they begin on page 65 and --

DR. PETERMAN: 66.

Q -- well, 2 is on 66.

DR. PETERMAN: Oh, yes. Yes, okay.

Q And 3. So starting with recommendation number 2, you make a recommendation that there should be,

that first of all you say there's a need for agencies in Canada and the U.S. to coordinate research activities through a working group. Can you explain that a little bit further and explain how you see that being set up?

DR. PETERMAN: Sure. So in case you're not aware of it, the research that goes on, on sockeye salmon, well, for any salmon, for that matter, tends to be quite separate in the U.S. and Canada. But I think what we've shown here today is a reminder that maybe we should be looking at sockeye biology in a little more broad scale than we have in the past. So we've recommended that there be some more coordinated research that would permit scientists to answer questions about large-scale processes that we describe. If instead everyone is in their little channel, their little box, and they're not looking at what's going on around them, they might miss some bigger picture topic that's really important.

So this coordination obviously happens informally through conferences and the literature, but not on a day-to-day basis. And so we're proposing that some coordination between the U.S. and Canadian agencies be done in terms of their sampling programs, what kind of data they're collecting, how they're analyzing the data, how they're sharing the data. And obviously this kind of group would need to be set up with the agreement of the four relevant agencies, Department of Fisheries and Oceans, Washington Department of Fish and Wildlife, Alaska Department of Fish and Game, and the Pacific Salmon Commission, so and any other groups, for that matter. I mean, these days we're using data from all sorts of stakeholders, as well, so I would say they should be involved to the extent possible.

And I think that it's going to be a difficult task to maybe convince these parties that some shared activities are in their benefit, but I would hope that seeing this kind of result about large-scale shared patterns would prompt them to do that.

Q And would the creation of the kind of working group you're talking about involve significant resources?

DR. PETERMAN: Probably not. I think it would be

fairly minor. In fact, I could see a few initial meetings being required among the international groups, and then maybe an annual meeting, and constant contact, of course, electronically. But I wouldn't see it as a big cost, no.

Q Okay. And then recommendation 3 relates to recommendation number 2.

DR. PETERMAN: Yes.

Q And it talks about developing and maintaining well-structured databases for storing, verifying and sharing data across the regions. So maybe you can just give us a bit of an explanation how is data that's relevant to salmon research and management currently stored and shared by the four agencies that you've just described?

DR. PETERMAN: Yes. Well, this recommendation was put in here in response to my frustration, quite frankly, at getting the data that we required for this analysis. It turns out that not only are each of the agencies storing their own data on Excel spreadsheets, which is what you'd expect, that is, that they would store them separately, not necessarily on Excel spreadsheets, but even within an agency the spreadsheets were compiled in different ways by different people in different offices, or even down the hall from one another within the same office. So this was one thing that I felt compelled to comment upon.

So the issues that I see in terms of the data quality and accessibility are that, well, first of all, the data quality are good. In terms of being able to come up with reliable estimates of adult returns and spawners for the populations that we've described, I think the data quality is good. However, we found quite a few cases where the spreadsheets that were sent to me were poorly documented, or had internal inconsistencies, or they even had wrong numbers. And the reason I know about wrong numbers is I did some calculations myself from the columns that they put together in various ways, and this is rare, but it happened.

There was poor documentation to the extent that I had to phone up some of the biologists to find out, okay, what did you really mean by this column heading? And as you probably know when you use spreadsheets, you try to put in some little

succinct column heading in the small space you've got available to you, which is not usually enough to make it totally unambiguous to someone who did not develop the spreadsheet, like me, or any other user. And in fact these labels sometimes were ambiguous to the point where I found out that after a phone call, oh, yes, you should be using so-and-so's values for the adult recruits, not mine. That is that spreadsheet, I'll send it to you. It's another spreadsheet and it's called "M" there, where the column "K" that's labelled "adult recruits" in mine, don't pay attention to that one, which is shocking, to say the least. I wouldn't pass my students with that kind of approach, but...

And then the internal inconsistencies came up where I found out that there were some calculations across age structures that were not done right, and so some numbers farther down in the spreadsheet were not consistent that were shown farther up.

And notations, now, this is a minor point it sounds like to you, I'm sure. But there are three different types of age notations in salmon biology, and even among people in the same agency, I saw all three. So surely they can agree on one. And in fact, I think all agencies should agree on one.

So this is just a bit of the few examples of what I saw as some lacking of coordination in putting together these data, and that certainly won't facilitate sharing data across agencies. So if we can fix that problem up, it will help make the data available more widely, and, Mr. Commissioner, I would argue that if these databases had been standardized much sooner and had been made more available more widely sooner, it would probably have been clearer that there were these shared trends going on before now. It can't be guaranteed, but I suspect that would have been the case.

Q Is this situation that you've described, I take it, it can be improved and what's the -- what are the resources required to do the improvements that you've talked about?

DR. PETERMAN: Well, I'm not expert on formal relational databases, Brigitte is, I suppose. But

really what is required here is that a formal standardized relational database, like Microsoft Access, or maybe some other non-Microsoft product preferably, would enable people to have a central point where they have quality control, quality assurance processes that they use to insert their data, and to change any data.

And I say change the data because this often happens in salmon biology. You will see frequently a dataset, say, for the Skeena River sockeye is my favourite, where people will do corrections to the estimates of adult recruits at some later time, because now they've got a dataset available from Alaska about their interceptions of Skeena-bound sockeye that they didn't have before. So they'll go in and correct their estimates. Well, if person "A" corrects his or her estimates, but person "B" who's using the old dataset doesn't get those corrections, they might not even hear about them, they'll do analyses and they'll come up with different answers.

So that's why we need a centralized database. There has to be a strong requirement for metadata, metadata are basically descriptions about the data: where did they come from, what sampling methods were used, who did anything to change them from the original, what qualifications are there on their use, or cautions. So I think every data -- pardon me, data from every stock should be put into this kind of database in standardized formats. And this is, to my mind, an obvious thing to have done and it hasn't been done.

Q And does it require a large amount of resources to do this work?

DR. PETERMAN: Well, I can't speak to that for sure, but I doubt it. I think you need some person as the end person who's controlling the database, and I don't know how you facilitate people at different regions getting access and input and doing quality control, but this must have been done many times over in many different kinds of institutional settings.

Q Right. And is there an overall efficiency that would be gained from putting in that investment?

DR. PETERMAN: Oh, yes, absolutely. Efficiency is probably the key phrase there. Well, and the other one is sharing standardized data.

Efficiency would be increased because you wouldn't have this problem with people going back and forth, figuring out why their analysis was different from someone else's and finding out, oh, my data weren't updated since you updated yours.

MS. BAKER: Thank you, Mr. Commissioner. Those are my questions.

THE COMMISSIONER: Thank you.

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

(PROCEEDINGS ADJOURNED FOR MORNING RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: The hearing is now resumed.

MR. SPIEGELMAN: Good morning. For the record, my name is Jonah Spiegelman, counsel for the participant, Government of Canada. I just have a few questions for these witnesses this morning. The Commission counsel's direct took care of most of the clarifications I wanted to make on this report.

CROSS-EXAMINATION BY MR. SPIEGELMAN:

Q And I will begin with a little bit of exploration on the concept of productivity and why you chose to use that for this report. And I guess the most basic point that I would ask you to comment on is that, notwithstanding your choice of productivity as the measure to evaluate for the report, would you agree that overall abundance of fish is really the measure that's of most significance to the users of the resource?

DR. PETERMAN: Yes, absolutely. But in order to understand the changes in abundance, one needs to know not only the changes in spawners but how effective they are at producing adults so that productivity measure is critical.

Q Thank you. In the time series that you used to do your analysis, you stated previously that the 2010 data was not available at the time that you prepared your report. I'm just curious if you can comment on whether the 2009 return data was part of those time series?

DR. PETERMAN: Yes, we had 2009 return data.

Q Notwithstanding that it wasn't incorporated in your analysis, can you say what the 2010 data

might have done to the analysis or the trends that you generated?
DR. PETERMAN: Well, from what I've seen, and I guess these are unofficial estimates, there was apparently about 29 million adults that came back to the Fraser in 2010. And if you look at the number of spawners that produced those adults, it goes back to the recruits-per-spawner measure of about six, which is about what we observed throughout the 1960s and '70s. And so that productivity wasn't particularly unusual. It happened to be on top of a very good spawning run in 2006. So the two combined, going back to the historical average productivity with the relatively high spawner abundance, meant that we had very high total returns.
Q Right. And you described briefly today, and also in your report, the Kalman filter technique, which I understand is used to sort of smooth out the trends taking away the high and low points. Is that a fair general estimation or description?
DR. PETERMAN: Exactly, exactly.
Q Given that smoothing of those trends, would you expect that, for example, the low year in 2009 and high year in 2010, might have been smoothed out as anomalies in the analysis that you have done?
DR. PETERMAN: Well, as we just went over, we didn't have any 2010 data.
Q Right. But had you had that, would the smoothed trend maybe not dipped quite so sharply across all of these time series?
DR. PETERMAN: Well, yes, it probably would not have dipped, continued to be as low, if we had added the 2010 data in there. But that would only have been true for whatever Fraser sockeye stocks had unusually high values. I have not yet seen the stock-by-stock breakdown of the total returns from 2010.
Q At a general level, is it possible that the Kalman filter technique and the smoothing of the trends that it achieves could result in some anomalous years looking like trends once it's smoothed, especially if those anomalous years are at the end of a time series?
DR. PETERMAN: Yes, actually the end of the time series is the part of the dataset that we're least confident in. And that's true of any kind of

analysis like this where you're estimating trends and that's just the nature of the information. So it is true that what we're doing with the Kalman filter method is smoothing out some of those bumps, some of the deviations, above average productivity from below average productivity, that happens on a year-to-year basis. And I can say, though, that we did some simulations - well, we published them 11 years ago - where we set up a computer model where we generated what is known as the "true" time trends in productivity. We generated some data from those that represented the type of data we get out in the field from catch and the spawner statistics. And then we asked with the Kalman filter, what representation of the "true" trend do we get? Is it good or is it a poor representation? We found out that compared to other methods that were available, the Kalman filter method was the best one, estimating the "true" time trend in productivity. So it is definitely not perfect - no method is perfect - but it is the best that we've got.

Q Right. And just picking up on one point you made, if you had the least confidence in the end of a time series and your primary conclusion is that the end of these long time series is where you see these sharp declining trends, does that in any way weaken the conclusion of your report, or can you comment on that?

DR. PETERMAN: It might weaken it a bit but not much. I think the extent of those decreasing trends that we saw, that is, they're at least a decade long in most cases, would not be affected by this lower confidence of the most recent data point. By the most recent data points, I'm talking about the most recent, two, three, four years. Brigitte, do you want to add to that at all?

DR. DORNER: No, that's exactly what I would say, that yeah, the last couple of data points have the least influence on what the Kalman filter shows. And yes, it lags behind a little bit but that affects only a few years. So a 15-year trend would not be explainable by, you know, anomalies in Kalman filter estimates.

Q Okay.

DR. PETERMAN: Good question.

Q In general, would you attribute the difference

between the 2009 and 2010 years to be noise in the data or is there something that we can valuably learn from examining that distinction?

DR. PETERMAN: Well, as I said a few minutes ago, if my understanding is correct, that we had about 29 million sockeye returning to the total Fraser watershed in 2010, then that is a dramatic change in recruits-per-spawner compared to what we've seen in the last decade. That value ranged about -- it was about ten -- or pardon me, it was about six for the decades, '60s and '70s, and then has dropped down continuously since then. So to have it move up from where it was close to one, as I recall, up to six is a dramatic shift. So I wouldn't call that noise.

Q Right.

DR. PETERMAN: But one year does not make a trend so I think that's obvious, too, right?

Q Yeah. Do you think that those who are searching for causes of the decline and looking for causal mechanisms should not focus too much on the long-term trends but really focus on year-to-year variation? Is there value to those distinct and stochastic events that may help explain what's happening?

DR. PETERMAN: Well, that's a good question. I guess this is a matter of scale, that is, what is one interested in here, in this Commission? I suspect it is more the long-term trend than the year-to-year variation around the trend. In order to understand causal mechanisms in salmon population biology or almost anything, it is useful to have a wide range of conditions in which you've observed the purported disturbance and the response, the response being productivity here and the disturbance being something like predators or pathogens or contaminants. If you have a narrow range of conditions for the contaminants, for instance, you won't be able to test that hypothesis very well. So to the extent that there's been some wide range in conditions, as we have had in spawner abundance, for instance, that helps us delineate some confidence in certain hypotheses.

Q Thank you. I'm going to move away from the Kalman filter issue and to ask a couple of questions about the use of the scale graphs that we had a

look at earlier today.
DR. PETERMAN: All right.
Q And my understanding of how those graphs are scaled is that essentially the amplitude of whatever trend you've identified in the data has been standardized to fit the size of the graph that you have?
DR. PETERMAN: Exactly. Well --
Q And so maybe not exactly.
DR. PETERMAN: Yeah, I mean that's, okay, good enough for now anyway. Keep going.
Q Would you agree that to a lay reader, just looking at your graphs, that the presentation of your results in that manner could be potentially misleading in that one may assume that the decline in a given stock, as compared to another stock, is similar when, in fact, the magnitude of that decline is quite different?
DR. PETERMAN: Okay. In terms of the magnitude, absolutely, you're right. Okay. So maybe, Mr. Lunn, if we could go to Figure 9 on page 49, please? And let's just take as simple a graph as possible. How about the lower right one, the Fraser Late. Okay. So remember that what this is now, this is scaled values. So we've taken these Kalman filter estimates of the productivity and put them on a scale that would make all the stocks comparable so that we can say, yes, they've got a similar trend, or, no, they don't in terms of about when the trend starts upward or starts downward.

The reason we did that is, let's assume that we're back one step before we scaled the data, and we had just the raw estimates of Kalman filter values. So the Cultus stock, the blue triangles there, maybe in the Kalman filter estimate the parameter might be a value of 2 at the start of the series in 1950. And it went down from 2 down to 1.2, something like that. Okay? Whereas, let's say the Birkenhead had a value up more like 3 at the start of the series, so it would have been way higher, and it went down to 2.5, just as an example. So you wouldn't be able to see as well what the shared variation was, visually, as you can now when we see these scaled numbers. However, it is true that the magnitude essentially is from maximum-to-minimum for each of these

patterns.
So the absolute range in productivity might be smaller. The absolute loss in productivity for the Birkenhead stock over the time trend might have been smaller than that for the Cultus in absolute value. But it makes it harder then to see where the time trends are occurring because we had those graphs -- well, we've got those graphs actually in an appendix, if you want to see them, Appendix P-1 and P-2. But do you want to go there or not?

Q Well, I was going to point out that those -- that those unscaled values are in the appendix and ask you to agree that for those investigating the causes, it's important to go beyond just the body of the report and tend to see how the relative magnitude -- like that data is in there.

DR. PETERMAN: Absolutely.

Q And it's important to go past the main body and into those appendixes in order to get the full picture?

DR. PETERMAN: Yes, well, thanks for pointing that out. That is true for the data that we sent to all the people who were working as contractors for the Commission on these various hypotheses. We sent them the raw data of Kalman filter estimates, absolute values, not scaled.

Q Okay.

DR. PETERMAN: So they've got those for sure. But that's a good point. For anyone else that didn't receive the data directly from us, along with an explanatory note, yes, they should be aware that those raw values are in Appendices P-1 and P-2.

Q And is it possible and, if so, are there any examples of scaled values and graphs that show very similar trends over time where one population has a declining trend in productivity but is still above replacement value, whereas another population that has a declining trend in productivity but has dropped below replacement value, which would have implications for the sustainability of that stock?

DR. PETERMAN: Yes, absolutely. So the scaled values will not show that difference. You'd have to go to the raw values in Appendices P-1 and P-2.

Q Okay, thank you.

DR. PETERMAN: And you'll see in some of those cases

that the time trends will go below value 1, which means the recruits are not replacing the spawners that produced them. But those are all back there in the back. And in most cases, you don't get that trend getting that low but it is definitely true that if you're interested in finding out why a particular stock is declining at some rate compared to another stock should be using those raw values at the Kalman filter productivity parameters.

Q Right. I just raise that because one of the mandates of the Commission is to investigate and make recommendations for the future sustainability of the stock.

DR. PETERMAN: Yes, okay.

Q So to the extent that stocks have the same looking scaled trend, it may not have the same implications for the sustainability of those stocks; is that fair?

DR. PETERMAN: Right. Yeah, absolutely.

Q Thank you.

DR. PETERMAN: Did you want to add anything, Brigitte?

DR. DORNER: Just that the A values are in the log scales so it's not one but zero. The replacement value.

DR. PETERMAN: Oh, yes, that's right, that's right. So in those appendices, the replacement value is zero rather than one because it's a natural log of the number. Technical detail that anyone who goes to use them should know anyway.

Q You gave some evidence earlier today on your conclusions regarding overescapement. And I would just like to bring your attention --

MR. SPIEGELMAN: Mr. Lunn, if we can pull Exhibit 73 up?

Q And I have that document in a white binder for you as well, Dr. Peterman.

DR. PETERMAN: Let's see, which one is that? Is it the transcript from -- yeah, okay. This one?

Q That's the one, yes.

DR. PETERMAN: Pacific Salmon Commission Workshop from June 2010?

Q That's right.

DR. PETERMAN: Yes, okay.

Q And this is a document that you were involved in the preparation of; is that correct?

DR. PETERMAN: That's right. I chaired the workshop

and led the expert panel that put the report together.
Q Okay. If I can just bring us to page 86 of that document?
DR. PETERMAN: Okay.
Q And the last paragraph of section 4.7.5? It's written here that:

> The Panel's opinions about the effect of delayed density dependence on the long-term decline in Fraser sockeye productivity ranged from likely to possible to unlikely as a contributing factor.

Based on the analysis you've done and the report presented today on delayed density dependence, do you think that this new information would change that conclusion at all?
DR. PETERMAN: Well, it would for the Quesnel stock. I would say knowing what we know now we would argue that the delayed density dependence probably played a role in the long-term decline of the productivity of the Quesnel stock but it does not relate to the non-Quesnel Fraser stocks. And I should point out, by the way, that this paragraph that you've pointed out here has two time periods associated with it. The first sentence relates only to the long-term trend so the panel had some considerable disagreement, given the evidence they had at hand about how important delay density dependence was in driving the long-term trends of the Fraser sockeye productivity. But we all agreed in the last sentence there that delayed density dependence was very unlikely to have played a role in the 2009 drop.
Q Thank you. And I'll finish off here with just a couple of questions on the recommendations that you were discussing before the break. And I believe that you indicated that you thought few resources would be required to implement a data sharing scheme. And then you carried on to talk about how the data that does exist is in disparate format and it was difficult for you to make it match up nicely for you analysis; is that fair?
A Yes, that's correct.
Q Now, given the importance of these long-term datasets for analysis like you've done in this

report and other quantitative fish research, can you comment at all on the challenges that may be faced in implementing such a recommendation in terms of going back and keeping the integrity of these long-term datasets and putting them together into a database such as you described?

DR. PETERMAN: Well, I honestly can't speak too much to the specifics of it because I haven't done it and so I don't know what the institutional barriers are. I can't imagine that they would be very difficult to overcome, though, and I think it's a matter of committing some people to some time to do it. I have seen or read about other institutions that have large databases, whether they're for economic or business reasons or Census reasons or whatever. They've got a very standardized procedure for collecting, inputting, checking and distributing data. So I think it's long past time when this should be done.

Q In terms of actually creating the database using existing historical data, though, and perhaps this is a better question for Dr. Dorner. Can you comment on the labour-intensiveness of actually putting that together with the data that's out there currently?

DR. PETERMAN: Brigitte, you want to take it?

DR. DORNER: Well, there would be initially some effort involved getting the old data in there. But I think, you know, in terms of overall effort, that will be offset by considerable savings in the long term because once you have that database it's going to be much easier for the individual agencies to keep it up-to-date than it is right now to try and match up data that aren't matching because they have to do that, too, of course.

Q Right. And has the work that both of you did on this project that's being discussed today, does that go some way to creating such a collection of this data that may be useful moving forward?

DR. PETERMAN: Oh, yes, definitely. For this particular purpose, it's a unique dataset but it's limited in the number of variables that we have obviously. There are three variables: spawners, adults produced and, in a few cases, juvenile abundances.

Q What other variables do you think might be valuably collected in this kind of --

DR. PETERMAN: Well, I think almost anything that biologists have gathered that relate to trying to understand the dynamics that these populations would be worthwhile having in such a database. So this would be information on body size, condition factor, which says how large they are relative to their length and reflect something of their feeding history and health, age structure, obviously, the exact locations of where they were caught and how they were discriminated by stock, which methods were used.

Q This sounds to me like there's a lot of different pieces to this puzzle and there would be a lot of different scientists, who would have to come together to contribute to something like this.

DR. PETERMAN: Yes, that's right. But I don't think it's totally unwieldy. Like I said, it's a matter of commitment and, as Brigitte pointed out, the benefits in the long run would be considerable.

Q Can you comment at all on the need for science professionals in order to get funding to do projects and to have sort of professional development to use the results of their work to publish papers in order to raise their professional profile?

DR. PETERMAN: Well, I guess what you're asking is, how important is it to have access to the data? Because this is essentially what we do in academia and research scientists in all the agencies that I've just discussed also have publications as one measure of their professional development and promotion and differs among agencies and among positions within agencies. But it's certainly the case that having access to data and ability to talk to the people who collected them and who can give you the cautions about using them is really important.

And I would also put, more importantly, though, than the measure of need that you just described, a personal measure, there's a much wider need for decision-makers to have access to very high quality scientific advice when making difficult trade-off decisions. So the more people who have looked at this data, the better. And it's not appropriate to have only one person analyzing a dataset, say, as important as the Fraser sockeye, to give scientific advice to the

decision-makers. I'm not saying that has been the case but I'm just saying that having that openness of the data is very important.

MR. SPIEGELMAN: Okay. Thank you. Those are my questions.

DR. PETERMAN: Thank you.

MR. PROWSE: Cliff Prowse for the Province, Mr. Commissioner. My questions have been covered by my two friends in front of me so I don't have any questions.

MR. LEADEM: Good morning, Mr. Commissioner.

CROSS-EXAMINATION BY MR. LEADEM:

Q Good morning, Dr. Peterman, and good morning, Dr. Dorner. I think you're joining us from Lasqueti Island, are you not?

DR. DORNER: That's right, yeah.

Q I have a couple of questions on a number of topics and let me tell you where I'm going to go and then I'll get there in sequential fashion. The first line of questions will deal with a very significant *caveat* that you gave to all of us here that we're dealing with an ecosystem and so we're dealing with a very complex issue and I want to expand on that or want you to expand on that with us.

DR. PETERMAN: Okay.

Q Then I want to move to the causes or the causes that you've eliminated and the causes that still remain. To the extent that you can comment on those, I'm going to probe you. And then I will move to the recommendations that you make, which my clients are totally in agreement with. And finally, I will end up with outliers and how they can help us understand what is going on and what is not going on. And I should indicate for the record that I represent the Conservation Coalition and that we come from the perspective of conservation first and we're concerned about the fish from that perspective.

So looking at your report, page 13 of your report, I think you give us a very important *caveat* that I want you to expand upon. At the bottom of the page, you say:

An important concept for readers to keep in

mind when considering the evidence presented in this and other scientific reports to the Commission is that ecological systems are dynamic and constantly change across time and space.

And then you go on to say that it is very unlikely that if we're looking for a cause for the 2009 decline, that we're not going to find a single cause. It's more likely that there's going to be an interaction of causes or interaction of stressors or interaction of things. And I wonder if you could just expand on that from an ecological/biological perspective?

DR. PETERMAN: Certainly. Well, I think it's true in all of the ecological literature, not just the literature dealing with fish, that we had these dynamics occurring as a result of several simultaneously operating processes. And the key phrases there are "simultaneously operating processes". It is often the case that one of those processes might dominate the others in affecting the population dynamic. So for instance, you might have, let's take an example with fish that we know as severely overfished, and that's the northern cod on the east coast, so there is a mechanism there that was obviously causing a decrease in productivity and abundance of that stock but simultaneously there was also an environmental change happening so that the ocean was coming cooler, the currents had changed and so that has led to a debate about, well, what was the relative importance of fishing compared to environmental changes, the processes that were affecting the fish, the food supplier, the predators?

And this is a very typical thing that we observe in research on mechanisms causing changes in any kind of organism, is it's often the case that you cannot tease apart these multiple mechanisms just by observation of the natural system. It's not always the case but it's often the case. And that's why scientists try to do experiments or we manipulate some purported cause of the observed change that we've seen in the past. We want to say, okay, if we control it ourselves, know when we start and where we start

and we have multiple replicates then we can have greater confidence that, oh, yes, that mechanism was important or it's not. So I think the caution is simply a reminder of what most ecologists are aware of, that looking for a single cause we might be lucky, we might find that but more than likely, in my opinion at least, and I think Brigitte agrees, it's not likely we're going to find that single smoking gun, so to speak.

Q And the problem with being a proponent of a manipulative experiment is it's just there's questions of costs that are associated with it, as well as if you're trying to examine a hypothesis that might be convoluted, it's very difficult to keep all the factors constant, all the variables constant so that you can then focus upon the one variable that you will change. Isn't that true?

DR. PETERMAN: That's exactly right. And there are all sorts of costs, as you mentioned. I mean, biological costs, financial costs, social costs of any kind of controlled manipulation. So they're not easy to do.

Q I want to now go on to page 65 of your report, right at the very top of the page. This is found under a heading, "State of the Science". And you make a point that the causes for the similarity that appear in the data that you've derived, the causes have not been investigated in this study.

DR. PETERMAN: Right.

Q And yet you go on to make some intriguing suggestions. And I want to just focus upon some of the intriguing suggestions you make there. One is that there's a mechanism that operates in a larger regional spatial scale and you say, "such as climate-driven, oceanographic changes". And I want to just probe you a little bit there because if, in your data, we saw that the Bristol Bay and the western Alaska stocks actually are doing quite well compared to the other southeast Alaska, all the way down to Washington stocks, do we know enough about the migration pattern or what happens in the ocean for the western Alaska stocks to know that there's not mixing, or, do you know if there are mixing of those stocks in the ocean?

DR. PETERMAN: Yes. Okay. So my answer will have a few parts to it.

Q Okay.

DR. PETERMAN: So the first part is, yes, we do know something about the ocean distribution of, say, the Bristol Bay stocks compared to the Fraser stock, well, the B.C. stocks in general. And there is an overlap in space and time in the Gulf of Alaska, such that they share whatever processes are affecting their survival rates: predation, food supply. And we don't know the exact distributions, of course, there are far too few samples or too few years. But it appears from what evidence exists that there is an overlap in the summer and winter months for, say, the Fraser stocks that go to sea as juveniles. They're out there sharing the environment with Bristol Bay stocks by that first winter. Okay?

So the period that's different in terms of the ocean conditions felt by the fish in the Fraser compared to Bristol Bay would be in that first, say, six, eight months of ocean life. So the Fraser are on a bit different track along the coastal continental shelf along the west coast and the Alaskan stocks that are coming down through the Aleutian Islands during that time. And then only in winter, I believe, is when they start overlapping. And then they remain overlapped until they start going back to their home rivers late in the ocean life, which would be during the last year of their life.

Q So does that assist you in hypothesizing that insofar as there's an overlap and we know that the western Alaska stocks are doing reasonably well in comparison to the southeast Alaska's down south stocks. Does that tell you that maybe there's something happening along the migration path of the southeast Alaska stocks and the Fraser River stocks that is affecting them in a way that is not obviously affecting the western Alaska stocks?

DR. PETERMAN: Yes, well, that would be my opinion. And I want to emphasize we did not do any data analysis in this report on that topic.

Q Yes.

DR. PETERMAN: However, based on other research that my colleagues and I have done and that other researchers have done, I would definitely say that that is the case. We found, for example, that the sea surface temperature encountered by the juveniles, as they enter the ocean in their first

summer of life, is a reasonable indicator of the conditions that would affect their survival rates. And what we found is that for all the stocks in this case from the central coast down - this is from the study we published in 2002 so nine years ago - data up to that point showed that for the stocks from the central coast down, when the sea surface temperature is above average, when the smolts hit the ocean, they tend to have lower productivity on average than when the ocean is at a moderate temperature level.

The converse is true for the Alaskan stocks. When the Alaskan stocks enter the ocean that's warm, their productivity seems to be higher or it is higher. I shouldn't say seems to be. The data show it. So the returns-per-spawner are higher for the Alaskan stocks, the central Alaskan and western Alaskan stocks when they enter warm oceans. But the opposite's true for B.C. stocks. So this suggests that whatever indicator is represented by sea surface temperature, which is only an indirect indicator of a lot of different processes operating in the water mass, that's a different set of mechanisms in the south than it is in the north.

Q Okay. I want to go onto a couple of the other things that you say might be responsible. One of them is widespread predation. And we're going to hear from Dr. Trites and other doctors on that topic. I think it's Project Number 8. So we'll be hearing from them in due course as to whether or not that is something that can account for the declines.

DR. PETERMAN: Right.

Q And I gather you won't want to comment on that without hearing from their evidence or knowing what it is that they say?

DR. PETERMAN: Well, that's right. I don't have any evidence directly on the predation hypothesis but I guess again, and just to come back to this general point, this would apply to all the proposed mechanisms causing decrease in productivity of Fraser stocks, you'd have to ask yourself, what is the spatial distribution of the mechanism that someone's evaluating? And have they only looked at the Fraser? Or have they looked at it on the larger scale?

Q Right. Okay. And I'll be sure to ask some of those kinds of questions that you're asking right now when they appear in a couple of weeks' time.
DR. PETERMAN: Okay.
Q You also then go on to say "or pathogen-induced mortality". Are you familiar with some of the work that's being done with respect to the virus and the genomic signature for the viral genomic signature that Dr. Kristi Clark has been done?
DR. PETERMAN: That's Kristi Miller, I believe?
Q Kristi Miller.
DR. PETERMAN: Yeah, yeah, that's right.
THE COMMISSIONER: How are you voting?
MR. LEADEM: I'm not going to tell you that. I might get myself into too much trouble, Mr. Commissioner.
THE COMMISSIONER: You surprise us every day.
DR. PETERMAN: Yes, I've heard Kristi Miller talk on two different occasions about her research and, in fact, one of the times was in the Pacific Salmon Commission workshop in June that we just referred to earlier. And I have not read any of her papers on this topic.
MR. LEADEM:
Q All right. So you can't comment on whether or not that's something that you would think would be valuable to consider in terms of looking at factors that might contribute to the decline?
DR. PETERMAN: Well, I think it would definitely be something worth looking at, absolutely.
Q Right.
DR. PETERMAN: But I do not have any data to evaluate that hypothesis. Again, for these kinds of studies, you're just doing observational studies and not actual manipulations, planned manipulations. You have to look for appropriate comparison groups hoping that one purported mechanism would be present in this group of stocks and not present in the other in order to increase your confidence in your conclusion. So that would apply to this one as well. Are there samples that do or do not show this immune response from fish that haven't shown the kinds of trends that we've shown, and, is that pattern shown, the immune response shown in all the stocks that show the pattern that we show? Those are the kinds of questions that I think everyone needs to ask about

their mechanisms.
MR. LEADEM: Mr. Commissioner, I'm about ready to move onto the recommendations. And when we return from lunch, Dr. Peterman and Dr. Dorner, I want to review some of the recommendations with both of you and to ask you for some clarification on those.
DR. PETERMAN: Okay.
THE COURT: Thank you very much, Mr. Leadem.
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: Mr. Leadem.
MR. LEADEM: Tim Leadem for the record, appearing for the Conservation Coalition.

CROSS-EXAMINATION BY MR. LEADEM, continuing:

Q Drs. Peterman and Dorner, I was thinking I would leave the causes, but I just wanted to come back to the one cause that you could unequivocally overrule -- eliminate, and that was whether or not over-escapement had lead to the decline of the 2009 and preceding years that we've seen exhibited through your data. You weren't alone in that. In fact, two of your reviewers also came to that same conclusion after reading your report; isn't that correct?
DR. PETERMAN: Yes, as I recall, that's right.
Q And I'll just take you there just so that we can put this into the record. If you turn, for example, to page 82 of your report, there's some commentary from a reviewer by the name of David Welch. That's Dr. David Welch; is that right?
DR. PETERMAN: Yes.
Q And he's a well-respected fisheries scientist, is he not?
DR. PETERMAN: Yes.
Q And at page 82, number 1, he says:

> The authors conclude that there is little to no evidence in support of the theory that Fraser River sockeye escapements have been

excessive (a point of view that Prof Walters has been a particular champion of) and have reduced the productivity of the stocks. I am satisfied from Peterman and Dorner's results that this is not the case — and this should allow the Commissioner to strike one possibility off the rather dauntingly long list of possible causes for the Fraser River sockeye decline.

So it would appear that Dr. Welch is in full agreement with you on that point; is that right?

DR. PETERMAN: That's the way I interpret it.

Q And then if we go to page 89, Dr. Sean Cox is a colleague of yours from the Simon Fraser University and a well-expected fisheries biologist, is he not?

DR. PETERMAN: Yes, that's right.

Q And, at number 1 under strengths, he says:

Finally, the report conclusion that over-escapement is probably overrated (sorry, but I paraphrased the conclusion) will hopefully limit what can be a distracting debate.

So I take it from that, that he was also in agreement with you, and that that's how you would interpret that remark as well?

DR. PETERMAN: Yes, that's right.

Q Now, on to the recommendations, and those I would find in your report beginning at page 65. As I indicated to you earlier, my clients are in full agreement with these recommendations so my questions are simply to flesh out in greater detail some of the mechanisms by which these recommendations can be put into place.

I want to start with recommendation 2, because I think that's a really valuable recommendation that you've made to us, Dr. Peterman and Dr. Dorner. You suggest that there be some sort of an organization, more or less an amalgamation of scientists, and you point to, in the very last sentence of the reasoning under recommendation 2:

...an informal international group of scientists working on the topic of "Salmon

Ocean Ecology" --

I wonder if you could tell the Commissioner a little bit more about that group, how often it meets and who are some of the members.

DR. PETERMAN: Certainly. Well, in fact, this group just met last month down in Seattle, and it is not an annual event, but it's every few years and the group is normally composed of agencies' scientists from the U.S. and Canada who are all working on topics related to ecology of salmon in the ocean. So there's been a lot of comparison of notes by people and findings from Oregon, for instance, and the important role of oceanographic factors there with studies up in Alaska and in between.

So it is a very informal group, though, so I don't think there's any special funding they have for coordinated research. It's more people coming together, reporting on what they've been finding, sharing notes and then going back and maybe changing the direction on what they're doing based on what they heard.

Q You would like to see that process formalized, I take it, to a more significant extent than what exists already?

DR. PETERMAN: Yes. I think that would go a great deal towards resolving one of the issues that we address here indirectly, and that is the communication issue. And the coordination issue is the second one, coordination in terms of research activities, research questions that are being posed and data that are being collected to address those questions. So if it could be coordinated across a larger scale, I think it would accelerate the gaining of knowledge that's relevant to the management of these important species.

Q Now, you're familiar, of course, with the United Nations Food and Agriculture Organization. I think you wrote some papers for them, or wrote at least a paper --

DR. PETERMAN: One, yeah, was co-author on the Precautionary Approach document, '95.

Q Yes. That was back in 1991 --

DR. PETERMAN: That's correct.

Q -- you wrote that about the precautionary approach that's applied to capture fisheries and species

introductions.
DR. PETERMAN: That's right.
Q And would that organization be something that perhaps we could link into or that could be -- this could perhaps foster this kind of an organization that we'd been discussing and you've been discussing in recommendation 2?
DR. PETERMAN: No, it didn't strike me that that would be relevant. The FAO tends to deal with more catch statistics among the world's countries, and that's their main mandate, I think.
On more regional scales, I think they would look to the regional organizations. So the regional -- well, do you want me to continue on, on what possible regional organizations there might be here?
Q (Nods yes).
DR. PETERMAN: Okay. So there are a couple of obvious ones. The first one is the NPAFC, the North Pacific Anadromous Fish Commission, and that's composed of all the nations that fish salmon, so from Korea, Japan, Russia, right through U.S. and Canada. That organization has been around for several decades, but they are mainly focused on collating catch statistics, coordinating efforts to enforce the ban on high seas driftnet fishing, for example, and other such activities. They don't really conduct research as an institution.
The research group that might be an appropriate one would be PICES. It's the Pacific version of what's in Europe called International Council for the Exploration of the Sea.
Q Yes.
DR. PETERMAN: But this, it's called the Pacific Science -- PICES, P-I-S -- P-I-C-E-S. I'm trying to think of what it stands for. Actually, there is no direct translation as I recall because it was a take-off on the European organization, ICES. They just added a "P" in front of it to become "P", Pacific version of collaborative scientists from the region.
So that includes scientists from all the nations I just listed plus China, so China, Korea, Japan, Russia, U.S., Canada. And they have had limited success, I think, in establishing large-scale coordinated research programs of the type that Brigitte and I are talking about here. They

certainly have done some, but they obtain funding from their own countries, usually. There might be some large pool of international funds available as well.

Q That gives us a couple of good leads. I take it that scientists generally get together, and when they do, there's a wide dissemination of ideas and it leads to further collaboration amongst the scientists. And that's to be fostered, that free exchange of ideas among scientists. Would you agree with that concept?

DR. PETERMAN: Oh, yes, definitely. Conferences are always a big source of collaboration. Ideas come up and we decide we want to compare notes. For example, this Pacific Salmon Ecology meeting I just was at last month, I developed a couple of contacts with people and my Ph.D. student is going to be visiting their labs this summer for some extensive work.

Q Now, you were instrumental in hosting that workshop that was conducted last June on the --

MR. LEADEM: I think it's Exhibit 73, Mr. Lunn.

Q This was the proceedings from the workshop, the "Synthesis of Evidence from a Workshop on Decline of the Fraser River Sockeye". How did that workshop come into being? How was it organized and who was instrumental in pulling all of that together?

DR. PETERMAN: Well, as I understand it, the Pacific Salmon Commission was the organization that instigated it, so as you know, this is an international group that has members from Canada and the U.S. The Canadian representative, I think, was Paul Sprout, at the time Regional Director General of DFO who, in talking with Larry Rutter, the U.S. representative, decided that having this workshop would be a good idea, try to pull out as much evidence as possible, really, to the decline in Fraser sockeye, in particular the 2090 (sic) then, but also looking back further.

So they collectively contacted me to ask whether I would chair this workshop and help them choose a panel of experts who would help sort through the evidence, and then also pick who the speakers might be to present evidence.

Q Moving on to the recommendations, recommendation 3 talks about sharing of databases and storage and

sharing of databases.
DR. PETERMAN: Mm-hmm.
Q And, Dr. Dorner, I want to turn over to you because I understand that you've had a lot of experience, and your expertise is more in the database and the statistical side. Do I have that right?
DR. DORNER: Well, I have a degree in computer science. I'm not exactly a database expert, but I can probably answer to your questions.
Q Right. I'm wondering if you have some things that you can offer the Commission with respect to what form the database should take and how to go about setting up some framework for that database to protect the data that's in there from being corrupted and from being manipulated.
DR. DORNER: Well, since it's going to be a collaborative database, it should probably be some form of SQL database --
Q Yes.
DR. DORNER: -- probably available online to all the people that would be participating, but beyond that, it would require some considerable research to see what would be the most appropriate system, probably a lot of interaction too with the potential users, what kind of features they would like to see there.
Q And do you, both of you, envisage like a database that's readily accessible to the scientists who are doing research into this field?
DR. DORNER: Well, from our perspective as scientists, that would certainly be the ideal state of affairs. Whether the agencies would be prepared to share all the data is a different question, but from the perspective of science, yes. Shared would be best.
DR. PETERMAN: Yes, I agree with that.
Q And then moving on to recommendation 4, this deals with the need to increase a number of sockeye stocks for which the agencies estimate juvenile abundance either as out-migrating smolts or fall fry. We've heard some evidence about this already. I think Dr. Riddell may have spoken a bit about this in terms of his studies and work on out-migrations from Chilko.
I take it from this that you, as scientists, would like to see more datasets be available to

you from the out-migration so that you would then have more of a dataset array from which you can sample. Is that what you're suggesting here?

DR. PETERMAN: Well, yes, in general terms, yes. I would phrase it a little more specifically. I guess the ideal situation would be to have abundance measures at various life stages of any given population so that when something like this decrease in productivity over a long time happens, you can try to parse out or separate out where, in the life history, most of that change is occurring.

If you only have abundance estimates at the start of the life cycle, the spawners, and the end of the life cycle, adult recruits, you can't tell where in time or in space the processes might have been operating that caused the changes you observe, whether it's an increase in productivity or a decrease. So these juvenile databases are extremely important in that regard. You at least create two life stages where you've got some data.

But they're logistically challenging of course, and expensive, so we understand the reasons why there aren't a lot of them on the coast.

Q You reference the fact that the -- as far as you are aware, the long-term datasets on juveniles in the Skeena River sockeye series was stopped.

DR. PETERMAN: Yes, that's right. I think 2002 was the last year for smolt abundance on the Skeena, if I'm not mistaken, and it was a long series prior to then, several decades.

Q And your final recommendation, recommendation 5, talks about the need for better research, particularly the residents in the marine environment. We discussed this briefly when I was asking you questions about the Western Alaska stocks and the degree to which they may or may not mix with Fraser River sockeye.

To my way of thinking, you know, not much is known about what goes on in the ocean, and we've heard some evidence from Dr. Riddell and others, and I think Dr. Welch, about telemetry will only go so far in terms of the ability to track the animal so that if you are trying to track the migration of smolts, either from Cultus or Chilko, you lose them right after -- once they go to Queen

Charlotte Sound. You're familiar with that research, are you?

DR. PETERMAN: Yes, I am.

Q Do you have any concrete suggestions of what types of research projects should be the focus upon ocean research?

DR. PETERMAN: What's your budget? Seriously, this is the problem. Ocean research is extremely expensive so you have to be very strategic in what it is you're trying to sample and why, and what questions you're trying to answer. So, honestly, I don't want to be, you know, flippant, but it is difficult for me to answer that question.

Clearly, anything that we can do to better understand the nature of the mortality agents that are affecting the fish as they move from the time when they leave, the period when they're estimated in fresh water, to when they come back. It would be added information to what we've got now. I actually didn't just say in the ocean, because I want to emphasize for these nine populations for the Fraser sockeye for instance, where we have juvenile estimates, there is some substantial freshwater periods where some changes might be occurring that are very important to our overall life span estimates of productivity.

But based on a huge amount of research by a lot of other people over the past several decades, it is clear that there are very important processes operating in the ocean that influence salmon and I think we've seen instances, for instance, this climatic shift in the mid-1970s that has been well documented as improving the survivor rate of salmon, not just sockeye, in the north, by the way, in the north, but also pink salmon.

So there's clearly something going on in the ocean, and I think the challenge is figure out a way to do this efficiently and with the funds available.

Q If we go back to your recommendation 2 where you were postulating that it would be good to have a group of scientists who shared information, would the suggestion or the topic for research be something that those group of scientists could come to some agreement with so that if you're limited with respect to funding, and you wanted to

be really refined with respect to what research project you're going to see conducted out, wouldn't you want the best minds of science on salmonids applying their selves to that particular question?

DR. PETERMAN: Yes, absolutely. And, in fact, that's one of the recommendations that we made from this June 2010 workshop that the Pacific Salmon Commission put on, was to have a coordinated effort among all the scientists relevant to salmon survival.

One of the biggest challenges that I've had as a fisheries scientist in the past, and many others have had, is that you'll get one study on, say, predation on salmon in one river, and another study on diseases on salmon in another river, and another study on contaminants in salmon in yet another river, and you can't put them all together. What would be necessary in this case, in particular, is the coordinated effort among all the scientists involved to simultaneously take samples on the same groups of fish over, preferably, several years, and also gather data on the physical oceanographic variables and other physical variables, non-biological variables that are relevant. So both biological and physical variables need to be gathered.

If I can just give you one quick instance of this sort of thing happening, unfortunately it's not in Canada, but -- it's in the U.S. So several years ago a big research program happened in the -- started in the Bering Sea. They now have -- its acronym is called BASIS, B-A-S-I-S. It is an incredibly integrated and coordinated research system in the sense that there are people gathering data on all parts of that Bering Sea ecosystem simultaneously on a regular temporal and spatial grid. So they're going out and sampling the physical variables in the ocean and the biological variables for the food supply, the predators, the competitors, all this simultaneously across a grid of points in the Bering Sea.

Now, this is very expensive, of course, and maybe only the U.S. can do this, I don't know, but it's a matter of priority. So if we really want -- if it turns out that the ocean processes are

driving what we've seen here in terms of decreasing productivity - and that's an "if" - if that's true, then that's where it's really going to take us is a coordinated effort of all scientists looking at all aspects of survival processes for salmon.

Q Thank you. I want to conclude by taking a look at the outliers, because I think that maybe you would start by agreeing with me that we can learn a lot if we focus upon outliers. They may provide valuable clues into what's going right --

DR. PETERMAN: Yes.

Q -- in an era of declining stocks, and the one that you specifically focus upon is the Harrison River stock because that has showed an increase while all the other conservation units have shown decreases. Is that right?

DR. PETERMAN: Yes, that's right. This one really stands out as a wild population. It's unmanipulated and it is showing a completely different time trend in productivity from most of the rest of Fraser stocks.

Q And you pointed out in your paper that they have a different life history strategy and I won't go over that. I think we all know that they tend to migrate out to sea much more quickly, they're not reared in lakes and so forth, and they also have a different migration pathway.

DR. PETERMAN: Sorry, if I could just correct something you just said?

Q Certainly.

DR. PETERMAN: No, no, there's not evidence that the Harrison fish migrate out to sea more quickly.

Q Okay.

DR. PETERMAN: If anything, it's the other way around. From Dick Beamish's studies, I believe the Harrison fish hang around in the Gulf of -- pardon me, the Strait of Georgia --

Q The estuary.

DR. PETERMAN: -- the Strait of Georgia longer than do the other Fraser sockeye smolts.

Q All right. I was referring to, I guess, the Salish Sea. Or they migrate out to the saline environment much more quickly than the other --

DR. PETERMAN: Yes, well, earlier in their life history, that's right. The Harrison fish migrate out in their first year of life as fry, small

silver (phonetic) ground fish, as opposed to staying over winter and rearing up to much larger body size in a lake, which is what most of the rest of the Fraser sockeye do. That's right.

Q And you also mentioned -- thank you for the correction. You also mentioned they had to simulate different migration pathway than some of the other Fraser River sockeye. Rather than exiting through the northern part of the Gulf of Georgia, they actually exit through the Strait of Juan de Fuca and up the west coast of Vancouver Island.

DR. PETERMAN: Yes. And again, as I think I said, that's rather very limited evidence. I think it was one study that noticed that. They really haven't been identified as separate individuals in these other studies to say that was an exceptional year. There's only this one case. Where the Harrison have been shown, oh, these are Harrison fish and, look, they went out the Strait of Juan de Fuca.

Q So that leads me to conjecture, or to start to hypothesize in this way, and maybe you can correct me if I don't have this right, that either there's some stressors in the environment that the Harrison River sockeye are not encountering, and the other Fraser River sockeye are, or there's something else about the Harrison stock that distinguishes it.

Could it be that it may be something even in their genetic make-up, which may be equated with this life history strategy that they've assumed, that separates them? I mean, are we talking about a stressor in the environment that they're avoiding by the life history strategy, or is this something in their genetic make-up that allows them to survive the same stressor? Can we say one way or another?

DR. PETERMAN: Well, I don't think we can say definitely one way or the other, but I suspect that a genetic basis is not likely to be the contributor here to the difference. If anything, I would say something like body size would be. So the Harrison fish, being smaller at the time when they go to sea compared to the other Fraser sockeye, might make them less vulnerable to whatever stressor it is that's causing mortality

for the other fish. That's one more likely possibility than the genetic one.
Brigitte, did you want to add to that at all?
DR. DORNER: No, I think I agree. The other possibility that I would keep in mind is the timing of migration, that perhaps the other fish go through something that hurts them at a different time than the Harrison do, and so the Harrison aren't as much affected.
Q Right. Do we know when the Harrison actually exit the Salish Sea and go out to the Juan de Fuca and head to the Gulf of Alaska?
DR. PETERMAN: Somebody does, but not me, I'm afraid. Do you know that, Brigitte?
Q Okay. Maybe we'll get that from someone else?
DR. DORNER: I do not know.
Q And do you know anything about whether they mix with the other stocks of fish in the Gulf of Alaska? You talked about the overlap earlier.
DR. PETERMAN: Not an overlap -- I don't know about the overlap in the Gulf of Alaska, but I do know that from the studies that Marc Trudel did in the Queen Charlotte Sound, which is just north of the northern end of Vancouver Island, that he sampled the Harrison fish, the Lake Washington fish, west coast of Vancouver Island fish and some of the other Fraser stocks in the same region.
Q Right.
DR. PETERMAN: So they come together at that point in their northward migration, at least.
Q Now, the other outlier that I think was mentioned, not so much in your paper but in some of the commentary, was the Okanagan stocks. Obviously that goes up the Columbia, then into the Okanagan. And you weren't able to, as I understand it, use the dataset from that particular stock because it had not gone back the requisite number of years so you weren't going to be able to compare apples and apples. Is that a fair statement?
DR. PETERMAN: Yes, that's what I understand, so when I put out the request for datasets in this particular case to the appropriate agencies, it came back that, no, they weren't available. So at least it didn't meet our criteria, and our criteria were at least ten years of data for which we have been able to adequately separate the catches from mixed-stock fisheries into the

specific stocks. So it's unfortunate, because I know there is considerable interest in the Columbia pathway for these sockeye.

Q Dr. Dorner, did you have something to add? Sorry, I didn't see your hand raised. I'm not used to looking over to my left here.

DR. DORNER: Yeah. I would just like to add the Okanagan dataset itself is actually quite long, but what they don't have is recent age records, and that makes it impossible for us to actually assemble the recruit data, so we do have return data but not the spawner recruit data that we would need to calculate productivity.

Q I see. But you would agree that the clues provide -- that the Okanagan stuff may provide some clues as well to what's going on with respect to the Fraser River sockeye; is that right?

DR. PETERMAN: Yes, I'm sure another stock would help, especially when farther south. We only have the Lake Washington stock that's farther south than the Fraser. On the other hand, you have to keep in mind that the Columbia will have some considerable manipulation, human-caused manipulations that could affect it as well.

Q Yes, sure.

DR. PETERMAN: Right. But then several other stocks in our group have such manipulations too, not nearly to that extent, though.

Q The other and perhaps final outlier that I would suggest to you might be worth examining is just the 2010 return. Obviously, if you look at the patterns of decline over the last decade or so, it stands out in stark contrast to that. So you would agree with me, then, that examining the 2000 return and trying to discern --

DR. PETERMAN: 2010, you mean?

Q 2010 return.

DR. PETERMAN: Yeah.

Q And trying to discern what factors were present that enabled those fish to have -- not only survive but have historically good productivity would be valuable; is that right?

DR. PETERMAN: Absolutely, yes. I think this is going to be an important contrasting situation, 2009 and 2010 years, and I hope you'll hear about this from the other researchers who are studying the various mechanisms because you'll have to ask yourself,

for any given mechanism, did the salmon exposure to that mechanism change that dramatically in those two years to explain the difference in total returns that we've seen -- or, pardon me, to explain the difference in productivity, the returns per spawner, or I've seen.

MR. LEADEM: Thank you both. Thank you, Dr. Dorner.

DR. PETERMAN: Thank you.

MR. LEADEM: Thank you, Dr. --

THE COMMISSIONER: Mr. Leadem, just before you sit down, I wonder if I -- because you may have something to ask arising out of this, but Dr. Peterman, can I ask you this?

QUESTIONS BY THE COMMISSIONER:

Q Mr. Leadem took you to page 66 in your recommendations 2 and 3.

DR. PETERMAN: Yes.

Q In recommendation 2, you use the language:

> All agencies in Canada and the U.S.A. that manage or conduct research on sockeye salmon...

And then you give your rationale for that. In recommendation 3, you talk about:

> All agencies involved with salmon research...

DR. PETERMAN: Ahh.

THE COMMISSIONER: And over the page, again in recommendation 4, you say "salmon management" and "salmon migration". What I would find helpful is because this Commission is focused on Fraser River sockeye salmon, if you could tell us what your views are under those recommendations with respect to the topics you've raised there, which is the data issue, in terms of are you talking across all the species that should be collected? That's really what I'd like to know from you.

But secondly, whether you can say in order to understand what you're getting at there, which is this kind of an analysis on sockeye, is it necessary to have the same kind of analysis done on all salmon species to make sense of what's going on?

DR. PETERMAN: Okay. Yes, thanks for catching that. I was I was a little -- Brigitte and I were not quite as careful as we should have been in sticking in "sockeye" in front of "salmon" where we should have.

So it's certainly true that for recommendation 2, it does apply to sockeye salmon and all other species for that matter. It would be a good idea to know about what's going on with the other species to the extent that there's some interaction between species.

At some point, I might address the issue that was raised in our Pacific Salmon Commission's workshop in June about interactions with pink salmon, for example. So if we didn't have abundance data on pink salmon, we'd be a little worse off than we are now where we do have some.

So, yes, but I think you're right that, say, for recommendation 3, well-structured databases, because sockeye really, the economically most important species, they're important for this Commission by definition, that these comments could be restricted to the sockeye without much loss in impact.

THE COMMISSIONER: I'm sorry, Mr. Leadem, I didn't know whether you might have anything arising out of that.

MR. LEADEM: No, thank you, Mr. Commissioner.

THE COMMISSIONER: Thank you very much.

MR. HARVEY: I think I'm up next. It's Chris Harvey for the Area G Trollers and the United Fishermen and Allied Workers' Union.

CROSS-EXAMINATION BY MR. HARVEY:

Q Dr. Peterman, your paper is very interesting in its implied search for a long-term sharing mechanism explaining the decline. I say "implied" because you say you don't deal with cause -- causation, but you certainly provide information from which others can draw conclusions relating to causation.

If we look at page 128 of your report --

MR. LUNN: Sorry, Mr. Harvey, did you say 128?

MR. HARVEY: One-twenty -- yes, this is page -- this is Exhibit 748, Report 10, page 128.

Q This graph, more than the earlier ones, has a

group of blue squares towards the bottom left.
DR. PETERMAN: Yes.
Q And I say that's more than the earlier ones.
MR. HARVEY: If you go back to 127, for example, Mr. Lunn.
Q We don't see that to the same degree. But at page 128 we do. The areas, the runs in those areas, apart from the Washington run, are all Canadian runs; is that correct?
DR. PETERMAN: Now, I'm not exactly sure which area you're describing. So the --
Q 128 from north --
DR. PETERMAN: Yes.
Q North coast down.
DR. PETERMAN: Yes. On the left side, from north coast down, they're all Canadian except for the Washington stock --
Q Yes.
DR. PETERMAN: -- that's in the middle there.
Q Yes. Now, we don't have any evidence I don't think, so far, with respect to the style of fisheries management in Washington, but we've heard a little bit about the style of fisheries management in Alaska.
DR. PETERMAN: Mm-hmm.
Q It could be, could it not, that the mechanism that is shared here that accounts for this, is that from the north coast down on this graph, we have fisheries managed according to the policies of the Department of Fisheries and Oceans in Canada. In the upper part of the graph, we have fisheries managed according to different policies. That could be a possible sharing mechanism that one would want to examine apart from Washington, the Washington Lake; is that...?
DR. PETERMAN: Well, you could examine that if you want, but I guess the question that I would ask is what aspect of the management process would lead to a change in the productivity - remember the adults produced per spawner - that would cause this positive correlation among the B.C. stocks that would not also cause it among the Alaskan stocks for instance.
Q Yes.
DR. PETERMAN: Frankly, I can't think of one.
Q You can't think of one, but you did mention a moment ago when one is looking for mechanisms,

other mechanisms, hypotheses, one would want to test them against your results.
DR. PETERMAN: Yes, in general, sure, that's good science.
Q Yes.
DR. PETERMAN: But I guess this question kind of falls in the same category as asking whether en route mortality of the fish going upstream in the Fraser has an explanatory value for the declining trends in productivity, recruits per spawner, and --
Q Yes.
DR. PETERMAN: -- I say in that case, it doesn't because the en route mortality is not included in the dynamics of that life stage that was looked at.

So I'm just thinking out loud here. Brigitte might have a different viewpoint. But I'm trying to think what is it that could be shared by the management actions between when the fish leave the spawning grounds as juveniles and come back as adults that would cause the kind of shared trends. I just can't think of what that would be.
Q No, no, yeah, I'm sorry. Now I understand what your problem is. But you do agree, don't you, that the - and I think you said this - that it may be possible that something has occurred to the out-migrating smolts in their freshwater life stage that affects their rate of survival in the ocean. That's a possibility?
DR. PETERMAN: Yes.
Q Yes, all right. And the freshwater stages of the runs from the north coast down, apart from Washington, are managed by the Department of Fisheries and Oceans, and the freshwater stages of the other stocks are managed under a different regime, obviously. You'd agree with that?
DR. PETERMAN: Well, again, I'm not sure what you mean by managing freshwater stage. I'm sorry, I'm being a little technical here, perhaps. Maybe you don't mean it that way. But fisheries management affects usually the adults, unless you're looking at enhanced populations, in which case we're manipulating the freshwater survival rates through hatcheries or spawning channels.
Q Well, in particular, the analysis of the carrying capacity of the freshwater system, and how that works in with the escapement that the fisheries

manages determine is optimum, those are all -- those matters --
DR. PETERMAN: I see.
Q -- are governed by different regimes in Canada than they are in the U.S.
DR. PETERMAN: Okay, I see what you mean. Yes, you're right.
Q Yes, all right. Now, this graph leads one to suppose that there might be some north/south influences happening here that one should look at, because the blue areas fall more readily in the south and the red areas more readily in the north. That leads me to ask you why you did not include Columbia River data in your analysis?
DR. PETERMAN: Okay. The reason is that the data were not sufficient to address our request to have total adults recruits produced by the spawners that they were estimating. We requested the Columbia data, but we did not receive it.
Q Who did you request it from?
DR. PETERMAN: DFO. So we put in a standard request. There's a long list of stocks for which --
Q Yes.
DR. PETERMAN: -- we want the data, and that was one of them.
Q So how did you get your Washington data?
DR. PETERMAN: I went to Washington Department of Fish and Game, Kyle Addicks.
Q Couldn't you go to the same people to get the Columbia? Well, except I suppose it would be Oregon.
DR. PETERMAN: No, see, the stock we're talking about here is the Okanagan sockeye which migrates down through the Okanagan, obviously --
Q Yes. Yes.
DR. PETERMAN: -- into the U.S., into the Columbia River and then out. So the assessment process for the Okanagan sockeye is the responsibility of DFO.
Q Yes. Well, there's -- the DFO have that data, surely, do they not?
DR. PETERMAN: That's what I thought. Well, yes, they have some data but, again, the question that I asked was, "Do you have data that's sufficient for our purposes?" I described the purposes. All salmon biologists know what a salmon brood table is in terms of having the number of spawners and then the number of adult recruits that are

produced by them. In order to get the adult recruits, as Brigitte said, you need to have age-structured data, and to do the kind of analysis we did, you want at least ten years of data, sequentially.

Q Yes. So --

DR. PETERMAN: So apparently -- excuse me, if I could just add one thing -- one response that came back was, from DFO to our request, was "the following stocks, we do not have sufficient data to meet your criteria for the following stocks" and the Okanagan sockeye was one of them.

Q Well, the return side, the recruit side would come from the U.S. authorities, correct? The number of salmon that arrive from the sea to the river mouth.

DR. PETERMAN: That would be both. Well, they're caught also in Canadian fisheries on the way down the coast, as I understand. But also the U.S., yes, definitely.

Q And but the spawner -- the spawning numbers for Osoyoos Lake and Lake Okanagan would be Canadian data.

DR. PETERMAN: That's correct.

Q Are you saying that that data doesn't exist?

DR. PETERMAN: All I'm saying is what we heard back from DFO is what I said. The Okanagan did not meet our requirements which spelled out that we needed at least ten years of data with appropriate age-structure data.

Q Did you ask them once, or did you persist?

DR. PETERMAN: Well, I know I mentioned it indirectly to people on the phone once more. That was it.

Q Because the -- but are you aware that in the Columbia, there have been what, for the Columbia, was record returns in 2008, 2009 and 2010?

DR. PETERMAN: I certainly heard about the large 2010 run. I was not aware of the 2008 and 2009.

Q All right. If there were record runs returned in 2008, 2009, it would confound the picture presented here on page 128, wouldn't it, in that you would have a -- you may well have a rising trend, an increasing trend in productivity in the Columbia apart from the northern U.S. rivers, one doesn't see.

DR. PETERMAN: Yes. I would say if the description that you gave is correct about the time trend and

abundance for the Okanagan sockeye, then it would be similar to the latter part of the Harrison River sockeye returns, for instance, going in a different direction than the others.

But I should point out that the Okanagan sockeye has a hatchery on it. I can't tell you what percentage of the fish are produced by hatchery as opposed to wild, but I think it's a significant contribution. Furthermore, there've been major manipulations in the waterflow regimes and in various project control activities on the Columbia system. So I don't know how comparable that would be in terms of being not as wild a stock as the Fraser sockeye are.

But it's a fair point. We would be much better off if we had had Columbia data.

Q Yes.

DR. PETERMAN: I would have liked it.

Q Are you aware of this at least, that the recruits or the spawning levels in the Osoyoos-Okanagan system have been determined to be substantially less than the carrying capacity of the Okanagan system?

DR. PETERMAN: I wasn't aware of that, no, but I'm not surprised to hear it.

Q All right. And that is a feature also of the Harrison, is it not?

DR. PETERMAN: I can't speak to that. I don't know about what the spawner abundance is relative to the spawning capacity, or rearing capacity.

Q Perhaps I could refer you to Report number 4.

MS. BAKER: Mr. Commissioner, report number 4 was not on the list of documents Mr. Harvey provided in advance of this hearing, and it has not been tendered in evidence yet in the hearings. So that does pose some problems, I think, for this witness. It's not an exhibit and it's not been -- there's no notice given, yet Mr. Harvey has given us notice of a long list of documents, but not Project 4.

MR. HARVEY: Well, that's correct, but this is not an adversarial hearing, and what I'm attempting to do is put to this witness what will come up in some subsequent paper to see whether he has any disagreement with it, which I think we should hear about surely.

MS. BAKER: I think the process in these hearings has

been that the technical reports have not been entered into evidence until they're tendered with their authors, so there's some hesitation, I would say, to put it forward now when those authors have not yet testified as to that report.

THE COMMISSIONER: Yes, I understand your point, Ms. Baker. I think the difficulty is I think Mr. Harvey has done that once before, perhaps with Dr. Johannes, with a report that he has raised that is not yet in evidence. Am I correct in that, Mr. Harvey?

MR. HARVEY: Yeah.

THE COMMISSIONER: I suggest that Mr. Harvey ask his question. To the extent that Dr. Peterman has not seen this report or not considered it, then I think in fairness to Dr. Peterman, either he need not answer or he may want to take some time to consider his answer.

MR. HARVEY: Yes.

THE COMMISSIONER: I just would like to err on the side here of ensuring that if there is something important here to put to Dr. Peterman, that we not lose the opportunity.

MR. HARVEY: Yes.

THE COMMISSIONER: So I'm going to allow Mr. Harvey -- but, Mr. Harvey, if Dr. Peterman needs some time to consider the point, I would certainly give him that opportunity.

MR. HARVEY: Mr. Commissioner, I think I can make it easier and less troublesome, because in the Peterman 2010 report, there are appendices and the same information is there.

If I could have exhibit -- I'm not sure whether this is Exhibit 573 or 634. It's the Appendix C to the Peterman 2010 report.

DR. PETERMAN: The Pacific Salmon Workshop?

MR. HARVEY: Yes.

MR. LUNN: That's 573.

MR. HARVEY: Five-seventy-three, thank you. And it's Session D, Daniel Selbie. It's at -- it doesn't seem to have a number on it. It's 1, 2 -- it's the fifth page in. There's a graph. That's it. If we could just highlight that graph or enlarge it.

Q This graph shows -- it's entitled "Adult Sockeye Production Relative to Optimum Capacity in Fraser Drainage Lakes." The greyish -- or the bluish

colour, I guess, if we take the first one on the left, is the PR model, so that's based on photosynthetic rate as a measure of predicted optimum escapement, and then the green is the maximum observed spawners. Is that as you understand it, Mr. -- Dr. Peterman?

DR. PETERMAN: Yes, I can see that.

Q Now, for the Cultus, we'll start with the Cultus on the left, because I think this was somewhat of an outlier in your material. The Cultus has, according to this, an optimum escapement capacity - I've been calling it carrying capacity - significantly greater than the maximum number of spawners observed; is that correct? Am I reading this correctly?

DR. PETERMAN: Well, it's -- yes, essentially, so the purple bars, I believe, show the estimate of the number of spawners required to produce the maximum number of smolts.

Q Yes.

DR. PETERMAN: Not the maximum number of adults, but the maximum number of smolts.

Q Yes. And it's based on a habitat assessment of what that particular lake system can support.

DR. PETERMAN: That's right.

Q All right. So the Cultus has that feature, that apparently the stocks put into that system have not reached anywhere near the maximum carrying capacity. The next one is the Harrison. You see it has the same feature.

DR. PETERMAN: Yes.

Q Whereas, while we're here, if we go across, the Chilliwack is the other way around. The Lillooet is the other way around, Seton is the same feature, carrying capacity greater than the escapement. Similarly, the Anderson, the Adams. When we get to the Shuswap it's dramatically the other way around, you see. The maximum of spawners far in excess of the carrying capacity, if I could call it that. I'm interpreting that correctly, am I?

DR. PETERMAN: Yes.

Q Yes. And the Chilko has the same feature and the Quesnel has the same feature. Am I interpreting that correctly?

DR. PETERMAN: Yes, that's right.

Q All right. So if we -- I was asking about the

Harrison, and having looked at that, would you agree that one of the features of the Harrison system is that the maximum number of spawners that have been put into that freshwater system have been far short of what is determined on this assessment to be the carrying capacity?

DR. PETERMAN: Yeah, that's what this graph indicates, although the legend points out it's only brood year 1990.

Q Yes. Yes. All right.

DR. PETERMAN: Yes.

Q So that's the limitation. But you would expect the smolts coming out of the Harrison to be healthy and well-nourished as compared with, say, the smolts coming out of one of the other systems where the escapement has exceeded the carrying capacity. In relative terms, they'd be better nourished and stronger. Can you --

DR. PETERMAN: Well, all else being equal, which it rarely is, unfortunately. Yes, the different lakes have different systems of --

Q Yes.

DR. PETERMAN: -- predators, prey. Furthermore, just a slight correction, the Harrison fish go out as fry, not as smolts.

Q That's right, oh, I see.

DR. PETERMAN: What you said, I understand the gist of what you said, yes.

Q Yes. Yes, I see. They don't go through the smoltification process until, of course, they reach salt water. But the Harrison, you were -- you said that is very important -- would be very important for whoever is looking for causation.

DR. PETERMAN: Yes.

Q Because there are important clues there. It would seem to me that the differences between the Harrison and the other Fraser stocks lie in the freshwater stage, mainly. Would you not agree with that?

DR. PETERMAN: No, I don't. From what little information I have, I don't -- again, I'm just speaking from what I know. Brigitte might know differently, but I don't know much about the freshwater life phase of the Harrison fish. What I do know is what I described earlier about the marine phase. That is, they enter the salt water as fry, they stay in the estuary of the Fraser

River for a few months. They stay in the Strait of --
Q Well, what --
DR. PETERMAN: -- Georgia later than the other stocks and they go out to sea by the Strait of Juan de Fuca apparently.
Q Well, they can't enter salt water as fry, can they? I thought --
DR. PETERMAN: Yes.
A -- the definition of a smolt is a juvenile fish that is capable of -- has made the change from existing fresh water to existing salt water.
DR. PETERMAN: Right, okay. This is a bit of a terminology problem unfortunately. Even biologists have it. So the smoltification process is a physiological process --
Q Yes.
DR. PETERMAN: -- where the fish change over from having a freshwater physiology to a saltwater based physiology. But that happens at a very small size for the Harrison fish in the year in which they hatch. So they go to --
Q Yes.
DR. PETERMAN: -- sea as fry, and I guess when you say they hit saltwater, maybe you call them smolts. We don't, we usually call them fry.
Whereas the other Fraser sockeye stay over in fresh water over one winter and the following spring obviously, and then they change into smolts, go to sea.
Q Yes.
DR. PETERMAN: So this is minor terminology difference. I think it's whether we call them smolts or fry is probably not the key problem here. Just call them early life stage and late life stage if you want, or seaward migrants.
Q Yes.
DR. PETERMAN: Seaward migrants would handle it, actually.
Q All right. They're smaller when they reach salt water than the other Fraser River --
DR. PETERMAN: Yes, that's right.
Q Okay. But the smoltification process in all these fish has to take place when they reach salt water.
DR. PETERMAN: Yes, exactly.
Q But surely the -- well, one distinguishing feature of the Harrison is that if there are stressors in

the other Fraser River stocks, in the freshwater system, the Harrison stocks are less likely to suffer from those stressors.

DR. PETERMAN: You're saying because they're there for a shorter period?

Q Yes.

DR. PETERMAN: That would seem logical, yes.

Q Yes. All right. And the other difference may be what we've seen in this chart that's still up on the board that the carrying capacity of the lake is significantly greater than the escapement. That may or may not be a similarity, depending which stocks one is comparing it with.

DR. PETERMAN: Right. There are several other Fraser stocks that have that same association as you pointed out already.

Q Yes. Now, was I right that the Cultus - 'cause I'm unsure about this - that the Cultus do not show the same declining feature as the other stocks you examined?

DR. PETERMAN: Actually, they do. So the Cultus, if you look -- well, if you care to look on Figure 9 - but I don't want to distract you from your plan of argument - but the Cultus actually do show a declining trend in productivity from the Kalman filter estimates.

Q When did it start?

DR. PETERMAN: About 1980.

Q I see. All right. Barkley Sound, is that -- was that an outlier showing a different trend?

DR. PETERMAN: Well, Barkley Sound, the Great Central and Sproat Lake stocks in other words, showed a different trend than most of the Fraser sockeye in the sense that they did have a decline, a sharp decline in the late '90s and early 2000s, but they had a higher productivity in the late '90s than in the early '90s.

Q Yes. So -- I see.

DR. PETERMAN: But there's some stocks of the Fraser Summer Run, for instance, that have that same pattern.

Q Yes. And I think you said the central coast, that is, the Rivers Inlet, Smith Inlet stocks have that pattern?

DR. PETERMAN: Yes, that's right.

Q All right. With respect to the Rivers and Smith Inlet stocks, the -- what happened with respect to

those stocks is this, is it not - and tell me if this general picture is wrong - that there was an experiment conducted there starting in the early '80s to increase the escapement expecting that it would produce greater returns, increasing the escapement by more than double what it had been, and the higher returns did not come in. What had been a steady fishery was - perhaps the word totally destroyed is too strong - but it was severely impacted and when in the '90s we see this -- by the time we get to the '90s, there was very much reduced escapement in those areas. That might well explain the rising trend in the '90s.
Perhaps we should look at your chart. I think it's at page 51.

DR. PETERMAN: Yes, that's the chart for the recruits per spawner.

Q The central coast is in the upper right-hand quadrant?

DR. PETERMAN: That's correct.

MR. HARVEY: So could we enlarge that?

Q So generally, it shows from where this begins, looks like in the late '60s or thereabouts, relatively steady until the late '80s when it starts to decline.

DR. PETERMAN: That's right. And then the late '80s onward to the mid-'90s, the productivity goes down, and then it starts going back up in the early '90s.

Q Early '90s.

DR. PETERMAN: Right. So it is true that the spawner abundance was increased for both those Smith Inlet -- or, pardon me, Owikeno Lake and Long Lake.

Q Yes.

DR. PETERMAN: In fact, it might be useful just to take a quick look at those data. We have those data at page 12 of Appendix P-1, Mr. Lunn. It could be the next page, I think. Maybe I counted wrong. Keep going, please? Oh, yeah, there they are, there they are, yup. Okay, so the bottom two left-hand --

Q Bottom right?

DR. PETERMAN: -- column, Long Lake and Owikeno Lake. So here the blue triangles are the spawners. So you'll see that in the mid-'90s through to 2000, for Long Lake, the spawner abundance was going down, and the productivity went up. Then the

productivity went down sharply again starting in the early 2000s -- well, a little bit before then actually.

You've got to be a little careful in translating these time periods, though. I have to tell you that these Kalman filter values on the right are smooth, remember. So we can't look at specific years and say something happened in the year "x" and it's exactly reflected right away in the Kalman filter smooth trends. But, in general, what you can see is that the Long Lake spawner abundance has gone down from about mid-1980s through 2000, then it went up for two years, and then back down again.

Owikeno Lake, the opposite. You had a slight bump upwards. So if you just scroll down a little bit, yes, so that like about '97 brood year, the spawners jumped up for the Owikeno Lake, and yet the productivity also went up as you see on the right.

Q On the left, the blue graph is spawners; is that right, the blue line?

DR. PETERMAN: Yes. And on the right, there are these productivities, so the blue triangles happen to be for the Owikeno stock.

Q Yes. Isn't there a consistency between the spawner levels going down in the late '80s, early '90s, and then when we go over to the right, four years later - these are four-year cycles I would take it - the productivity goes up.

DR. PETERMAN: No, not quite. Now, these data are all tuned to the same year in the sense that what you see is the brood year.

Q I see.

DR. PETERMAN: That is, the year of spawning. So the 1990 results on the right show you the Kalman filter estimate of productivity from the 1990 brood year spawners.

Q Yes.

DR. PETERMAN: So actually what you were just describing there is not quite right because the spawner abundance for Owikeno Lake went down from the mid-1980s generally through to the mid-'90s, and yet so did the productivity. You look at the Owikeno blue triangles on the right, the productivity went down during that period as well.

Q That doesn't seem to be consistent with the

evidence we've had on Rivers Inlet, if I could just refer you to that.

DR. PETERMAN: Remember, we're talking about the productivity on the right side here, which is the success rate of spawning, so it's how many recruits are produced per spawner. So that's what jumped up in the late '90s. And it did increase in the case of Owikeno Lake. The number of recruits, which is the red dot line on the bottom graph on the left, it did increase the total recruitment in that period. But then it went down again starting the early 2000s.

Q So can you make any sense of that in connecting it with the large escapements that took place in the '80s, and then we've got a decline in productivity in '92/'93, is that...?

DR. PETERMAN: Right. So I think maybe part of the confusion here is that your expectations of what the productivity on the right might show is solely based on how many spawners there are. And what these data are telling us is that there's something other than just the spawner abundance that's affecting the --

Q Yes.

DR. PETERMAN: -- productivity.

Q Yes. All right. So is this all we can take from the central coast example that whatever was affecting it, apart from spawner abundance, it was affecting it at a different time period than it was affecting the Fraser River stocks?

DR. PETERMAN: Yes, I would think that's right. There was something else going on here in the central coast in the mid-'90s, mid-to-late-'90s that wasn't reflected in most Fraser River stocks. I should say "most" because there are some that showed that upward trend, the Fraser Summers, in fact, three of the stocks -- no, sorry, two of the stocks.

Q Yes. Okay. I'd like to look next, getting back to the Fraser to Exhibit 73 which is the Pacific Salmon Commission Workshop.

MR. HARVEY: Actually, I wonder if that's a convenient time for a break.

THE COMMISSIONER: Certainly, yes.

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

(PROCEEDINGS ADJOURNED FOR AFTERNOON RECESS)
(PROCEEDINGS RECONVENED AT 3:21 P.M.)

THE REGISTRAR: Order. The hearing is now resumed.
MR. HARVEY: Well, I'll proceed, even though we seem to have lost one of our witnesses. All right.

CROSS-EXAMINATION BY MR. HARVEY, continuing:

Q Dr. Peterman, I just want to be sure that I've got -- I understand your -- the thrust of your report with respect to residuals and then your Kalman filter. Am I right that your work seeks to identify a decline in productivity that is inconsistent with the Ricker and Larkin Model predictions?
DR. PETERMAN: Yes, I guess you could phrase it that way. That's right. So we're fitting those models to explain some portion of the data.
Q Yes.
DR. PETERMAN: And then what's leftover is telling us about other things besides spawner abundance --
Q Yes.
DR. PETERMAN: -- that could be causing changes, yes.
Q Because the Ricker and Larkin Model both predict a decline based on excessive escapement levels, correct?
DR. PETERMAN: Except for the Quesnel.
Q I'm sorry, the Ricker and Larkin Models would predict the Quesnel, would they not? Isn't the --
DR. PETERMAN: Oh, yeah, I'm sorry, I -- okay --
Q Yeah.
DR. PETERMAN: I'm sorry, I was one step ahead of you there, I think.
Q Yeah.
DR. PETERMAN: Okay, sorry. Could you just phrase that again?
Q Let me go step by step --
DR. PETERMAN: I'm sorry.
Q -- because it is difficult. Well, let's start with the Ricker Model. The Ricker Model identifies or predicts a decline, but it only looks one year ahead; is that correct?
DR. PETERMAN: Not quite, no. So I heard the word "decline" and I was thinking about our long-term trends --
Q Yes.

DR. PETERMAN: -- that's why I jumped towards it.
Q Yes.
DR. PETERMAN: No, they're --
Q They're short term now.
DR. PETERMAN: Okay, sure, short-term. So what the Ricker Model tries to do is take into account a given year's spawner abundance --
Q Yes.
DR. PETERMAN: -- and ask, does that influence the returns per spawner? The Larkin Model says, not only do you want to ask that question about this -- the effect of this year's spawner abundance, but also the spawner abundance of last year, the year before and the year before that, so in other words, a total of four years of spawner abundance are taken into account in the Larkin Model, and only one year's spawner abundance is taken into account in the Ricker Model.
Q Yes. And both models predict a decline after the top of the curve level of a spawner abundance; is that correct?
DR. PETERMAN: Yes.
Q The Ricker Model would seem to assume, by not looking at other years the way the Larkin Model does, it would seem to assume a speedy recovery of the -- whatever problems the ecosystem experiences as a result of over-escapement, whereas the Larkin Model stretches out the period of recovery; is that one way of looking at it?
DR. PETERMAN: I'm sorry, I don't see it that way. The recovery period is not something either model speaks to. The models are fit to pass data.
Q Yes.
DR. PETERMAN: And so they're simply descriptions of the past data.
Q Doesn't the Larkin Model -- well, don't both -- one of the explanations for both models is that the food web that the sockeye juveniles depend on is driven down by an overabundance of competition?
DR. PETERMAN: That's certainly one mechanism that people have talked about, yes.
Q Yes. And the Larkin Model can accommodate a finding that the food web, once driven down in one year, takes more than one year to recover?
DR. PETERMAN: That's the implicit assumption --
Q Yes.
DR. PETERMAN: -- behind the Larkin Model. Well, I

should say it actually doesn't assume that, it allows it to be reflected in your parameter estimates of productivity, yes.
Q Yes.
DR. PETERMAN: Whereas the Ricker Model does not.
Q Okay. So when you come to identifying long-term declines, you're already assuming a decline based on the Ricker and Larkin Models, but you're looking for something that may be exacerbating the decline; is that the right way to put it?
DR. PETERMAN: Well, yes, I guess you could put it that way, but it's not quite exacerbating in the sense that where these trends on the right-hand side of the screen right now are a good example where over the period, say, 1987 to the mid-1990s, there's some factor other than spawner abundances that is causing those productivities to go down. So we've already taken into account the effect of spawner abundance --
Q Yes.
DR. PETERMAN: -- on recruits per spawner, and in all of these case there's something else, apparently, that's decreasing the survival rate --
Q Yes.
DR. PETERMAN: -- during the life history.
Q But my point was only this, that if you found data indicating that recruits per spawner were going down in a manner consistent with the Larkin Model, you would have one of these graphs that show no change?
DR. PETERMAN: Flat, that's right, like the Quesnel stock, exactly.
Q Yes.
DR. PETERMAN: Yes, that's right.
Q But if it's declining faster than the Larkin Model predicts, then you'd have a downward trend in your graph?
DR. PETERMAN: That's correct.
Q All right. Well, yes, that sounds to me like you're looking to identify something that's exacerbating a downward trend.
DR. PETERMAN: Okay, sure.
Q So if we could turn, now, to Exhibit 73, starting at page 42, I just want to look at the graph - I think we can get rid of the other ones, Mr. Lunn -
MR. LUNN: Thank you.

MR. HARVEY:
Q - and now looking at the graph on page 42, the red line, that shows the total Fraser River sockeye productivity returns per spawner four-year average, and it shows a high point in about 1960 and then a dramatic drop, and then goes along, rising slowly, ups and downs until about 1993 or 4 or thereabouts, then we start quite a dramatic decline; is that right?
DR. PETERMAN: Yes.
Q Now, it's been suggested that perhaps climate change is accounting for that -- for the decline, but climate change doesn't operate in a dramatic fashion like that, does it?
DR. PETERMAN: Well, it could. There are many examples in the oceanographic literature - and by the way, ocean dynamics are frequently driven by climate dynamics - where both the climate systems and the ocean systems show these more or less consistent patterns of maybe relatively small change and then a rapid shift to a new system.
So, for example, I mentioned that 1976-77 regime shift.
Q Yes.
DR. PETERMAN: It's well documented in the climatological literature, as well as in the oceanographic literature, that the system -- both systems, climate and ocean, are relatively similar from year to year in their patterns - of course there'd be some up and down - but that there was a dramatic and rapid shift in the climate forcing of the ocean dynamics in the mid-1970s. And so it shifted to a new level of dynamics.
Q Yes.
DR. PETERMAN: So I wouldn't say that seeing this pattern is explained by that, I'm just saying, in response to the way you phrased the question, it is possible --
Q All right.
DR. PETERMAN: -- that something like a climate system could change rapidly and dramatically.
Q But if there was such a dramatic change such as a regime shift in the early '70s, one would expect to have evidence of it; we'd know about it?
DR. PETERMAN: Yes. Well, there was another shift in 1989, which seemed to go in the direction even further than the '76/'77 regime did in terms of

slightly increasing productivity. It wasn't nearly as large of a shift as in the mid-'70s, but there was still evidence from both the climate signal and the oceanographic processes. So late '80s, early '90s is where you would expect to see some change starting in the ecosystem that's supporting the fish.

Q Is that a change that affects the abundance of food for -- sockeye food in the ocean environment?

DR. PETERMAN: It could be. I haven't seen those data because, unfortunately, one of the longest series of data we had on food supply for salmon in the ocean was Ocean Station Papa, which was terminated in 1980 when they went to satellites for forecasting the weather, rather than using the weather ship.

Q I think you said that the regime shift in the early '70s -- have I got the date right, early '70s?

DR. PETERMAN: Mid-'70s, yes.

Q Mid-'70s, that that changed the abundance of food?

DR. PETERMAN: Yes, that's what we -- well, among other things, yes, but that's definitely an important signal that we saw.

Q Is there any evidence of that occurring in about 1993?

DR. PETERMAN: I can't say. I'm not aware of that literature thoroughly enough to say.

Q No? All right.

DR. PETERMAN: Maybe Brigitte, do you remember anything like that?

DR. DORNER: No, me neither.

Q So if one is looking for something dramatic that happened in about 1993, when -- well, one would have to look -- one would be looking for something other than climate change so far as the evidence we have indicates?

DR. PETERMAN: Well, I wouldn't dispel the climate change, for the reasons I said. And I hope that one of the other researchers who is going to report to the Commission later will be able to address that point directly.

Q All right. On page 44, there's a heading dealing with residuals.

DR. PETERMAN: Mm-hmm.

Q I think this is probably opposite. It starts off under that section:

> In any salmon population, the average ratio of recruits to spawners must eventually decrease as the abundance of parental spawners increases. This reduction is due to a variety of factors affecting either spawners (e.g., competition for spawning sites) or the next generation (e.g., predation rates, disease, or increased competition for food).

So you're not denying that basic feature of population dynamics?

DR. PETERMAN: No, we're not, that's why we stated it as a fact.

Q Yes. Towards the bottom of that page, the bottom three lines, you say:

> Stocks with the lowest correlation between the residual indicator of productivity and the standard non-residual indicator tended to be those in which spawner abundance had increased dramatically in the last 20 years or so (e.g., Quesnel, Stellako, and Pitt). For these stocks, density dependence may have become important,

and that's discussed more below. Can you explain what you mean by the lowest correlation?

DR. PETERMAN: Sure. Yes, I realize that this one was a bit of a source of confusion from looking at some transcripts from a previous hearing. And so these terms were all defined up above, but just to reiterate here, so what was done there in this Pacific Salmon Workshop that we're looking at -- Pacific Salmon Commission Workshop, pardon me, is that we had two different time series of indicators of productivity. One, was just the recruits per spawner and the natural log of it. So just think about it as a transformed version of recruits per spawner.

Q Yes.

DR. PETERMAN: Okay? And that's what we called the standard non-residual indicator, okay? It's the recruits per spawner, the very first measure that I talked about today. The other measure, residual indicator productivity, is the residual from the

best fit Ricker Model. So once you take into account the effect of spawner abundance on productivity, then it's what's leftover. Some years it's up above what you'd expect based on the spawner abundances, some years it's below. So those residuals have a time series, and then the raw numbers of recruits per spawner have a time series. You take the correlation between the two for each of these populations that we had for the Fraser, the 19, and what we found was that the correlation is lowest for those stocks for which there's been a tremendous increase in spawner abundance. And so the logic is a bit contorted here, but what it says is that if you don't take into account the spawner abundance you're going to get a different measure of productivity than you would if you do.

Q Yes.

DR. PETERMAN: So taking into account the productivity -- the spawner abundance is what the residuals are showing. So the fact that the series are less correlated for a spawner -- for stocks where a spawner abundance has gone way up makes logical sense. You're taking that into account by fitting the Ricker Model and then you're just looking at the residuals that are leftover from environmental factors.

Q so the Quesnel, Stellako and Pitt fit the Larkin -- Ricker/Larkin Models better, is that what it comes to, or is it the other way around?

DR. PETERMAN: No, this wasn't referring to the Ricker and Larkin Model at all. In this section we're only talking about the Ricker Model.

Q Oh, Ricker, okay.

DR. PETERMAN: Yes.

Q So these ones, the Quesnel, Stellako and Pitt, fit the Ricker Model better?

DR. PETERMAN: Yes, that's right.

Q In other words, the decreases in productivity can be explained by overabundance --

DR. PETERMAN: Right.

Q -- over-escapement, overabundance of spawners?

DR. PETERMAN: Well, it can be explained more by the full range of abundances, some of which were high, some of which were low.

Q Yes.

DR. PETERMAN: Yes.

Q All right. Page 81, if you could go to page 81, Mr. Lunn.
MR. LUNN: Yes, I'm there.
MR. HARVEY: On yes, we have it there.
Q The bottom paragraph, Consistency with spatial and temporal trends in the Fraser. About four lines up from the bottom there's a sentence that:

> It should be noted here that sockeye salmon from the Columbia River returned in record numbers in 2008 and 2009.

DR. PETERMAN: Mm-hmm.
Q I think that refers to something we discussed earlier.
DR. PETERMAN: Mm-hmm.
Q You're prepared to accept that?
DR. PETERMAN: Yes. That's what we heard at the workshop.
Q Yes. Then page 82, under the heading, Plausibility and realism of proposed mechanism, I want to go to the bottom eight or so lines of that paragraph, beginning, "Moreover, some research suggests,". Enlarge that, Mr. Lunn, about eight lines at the bottom of that top paragraph, the main paragraph. That's it:

> Moreover, some research suggests possible latent effects of poor freshwater experiences on salmon (e.g., stress, disease, poor feeding conditions) that do not manifest themselves until after the fish enter the ocean. Thus, although the plausibility of the proposed freshwater mechanisms is inherently high, no good evidence exists *at this time* to suggest they are a major contributing factor in the recent decline of Fraser River sockeye salmon.

And that leads me to ask what more evidence you need, because you do -- there is sufficient evidence, is there not, of a decline in productivity - it's been identified by Ricker and Larkin, among others, of decline in productivity - that results due -- is a consequence of the freshwater experience?
DR. PETERMAN: Well, certainly there's been a large

amount of research done on density-dependent processes in freshwater. All we were saying in this section that you've just highlighted here is that, yes, we know those mechanisms exist, but there wasn't direct evidence for that on the Fraser sockeye to explain the recent decline. Let me give you one concrete example. The Chilko sockeye abundance -- pardon me, the Chilko smolt abundance was a record high in 2007, so those fish were the ones that contributed to the main returns in 2009. So it was an extremely high abundance of juveniles in the lake, and yet they were average body size. In most other years, in most other lakes, when we've studied the relationship between abundance of juveniles going to sea and their body size, it tends to be that they're smaller when there are a lot of them, for obvious reasons, competition for food.

Q Yes.

DR. PETERMAN: But in this case, the Chilko smolts were an extremely large number, 77 million, over twice what the previous record was, record high, and yet they were average body size. So if there had been something going on in the lake that had caused them to have poor nutrition, for example, you would have expected them to see below average -- show below average body sizes, and they weren't. And yet they came back in an extremely poor survival rate. The smolt to adult survival rate, as I recall, was the lowest on record for the Chilko time series. In fact, it was one quarter of the previous low, 1958.

Q So you're talking about the 2009 recruits?

DR. PETERMAN: Returns. Returns, yes.

Q The Quesnel return was meant to be the major run in 2009, wasn't it?

DR. PETERMAN: I don't recall. It was one of three, I think, yes.

Q The Quesnel fry and smolts, in that generation, were definitely a smaller body size, were they not?

DR. PETERMAN: I haven't seen data on that, I'm sorry, I can't say.

Q If they were of a smaller body size, you would expect them to be less able to deal with the stressors that they encountered during smoltification and early ocean phase of their

life?
DR. PETERMAN: Yes, generally salmon biologists have shown in studies elsewhere, as well as in the Fraser, that smaller body size tends to be associated with poor survival rate from smolts to adult, that's right. But it's highly variable, the effect of size.
Q So you agree that it may hold true for the Quesnel in the 2009 recruit generation, but not necessarily the Chilko?
DR. PETERMAN: Well, like I said, I haven't seen the data on Quesnel, I only --
Q All right.
DR. PETERMAN: -- just heard you describe it now, and if you're correct, then, yes --
Q Yes.
DR. PETERMAN: -- if they were below average size that's consistent with their poor return rate.
Q At page 86, you deal with, under 4.7.5 Conclusions about the likelihood that the hypothesis is correct, and here we're dealing with, I think, the delayed density dependent mortality as an important contributor.
DR. PETERMAN: Mm-hmm.
Q So dealing with that hypothesis, and the section reads that:

> Many biologists who worked on the management of Fraser sockeye through the middle and latter part of last century believed that "cyclic dominance" meant that spawning targets and exploitation rates should differ across cycle lines.

So that was the old style, and that's coming back into fashion now, is it not, as a result of all the modern analysis?
DR. PETERMAN: Well, the modern analysis, do you mean the FRSSI process?
Q Well, I mean the analysis that people like Carl Walters and Brian Riddell and Jim Woodey have done.
DR. PETERMAN: Well, I'm not exactly sure which analyses you're talking about. They've done so many. I'm sorry.
Q Well, I'll leave that.

That is, higher percentage harvest rates in low-abundance years (off-cycle lines) would keep them low to mitigate the delayed effects on productivity of one cycle line on another. The "experiment" of the last 20 years was to see if the "off-cycles" could be built up to the levels of the strong cycles. Whether the experiment has been informative is not yet clear; Carl Walters expressed a need to confirm that his model-fitting results were "real" and not an artefact of the statistical procedures.

If the evidence is as conclusive as Walters suggests, then the experiment has been a success in that it has provided valuable information. However, the attempt to increase abundance of offcycle years may have been a failure at producing more fish, or even the same amount of fish for harvest than would otherwise have been the case.

The Panel's opinions about the effect of delayed density dependence on the long-term decline in Fraser sockeye productivity ranged from **likely** to **possible** to **unlikely** as a contributing factor.

Could I ask where you stand on that continuum?

DR. PETERMAN: Where do I stand on that continuum? Well, depends on the stock. I guess our analyses that I reported on this morning would suggest that delayed density dependence has played a role in the Quesnel for the long-term decline in productivity.

Q Yes.

DR. PETERMAN: But we have not seen evidence of that for the other Fraser Sockeye stocks.

Q So, and I think - I haven't got it in front of me - but I think one of the mandates of this Commission is to find as a fact the reasons for the 2009 failure - I'm paraphrasing. You would support, as a finding of fact, that the high escapement levels in the Quesnel led to the substantial and serious declines of the Quesnel 2009 return; is that --

DR. PETERMAN: No, I'm afraid I can't speak

specifically to 2009.
Q All right. At any rate, you do say that delayed density dependence is a likely cause contributing to the long-term decline of the Quesnel run?
DR. PETERMAN: In the Brigitte Dorner and Peterman report?
Q Well, I thought that's what you just said a minute ago.
DR. PETERMAN: Yes.
Q Sorry. Now, Proposed Research, this paper says:

> The Fraser River Sockeye Spawning Initiative -

-- known as FRSSI, I think --

> - group should attempt to replicate Carl Walters' results for fitting the Larkin and Ricker models.

So is that research suggesting that the FRSSI Model, which sets the upper and lower benchmarks should attempt to -- or take into account Carl Walters' results and should use the Larkin and Ricker Models?
DR. PETERMAN: Yes, as I recall, that's what we said at that workshop as the panel.
Q Yes.
DR. PETERMAN: Mm-hmm.
Q Now, you discussed in your evidence this morning the - now, I forgot whether it's a 2006 or 2004 - paper that asks the question -- this is Exhibit 417, perhaps we could just have that brought up, the question relating to over-escapement, Exhibit 417.
DR. PETERMAN: Oh yeah.
Q Does Over-Escapement Cause Salmon Stock Collapse? Yes, 2004. I find the -- well, you've defined the question raised in the title there, you would define "collapse" as being less recruits -- less than the one to one recruit per spawner? Is that --
DR. PETERMAN: No.
Q No?
DR. PETERMAN: No, I don't think we ever used the word "collapse" in our report.
Q All right.

DR. PETERMAN: Well, we might have, but not in the context of our specific results. I guess all we were trying to do was to come up with two different measures --
Q Yes.
DR. PETERMAN: -- of the effect of spawner abundance in something other than the brood year in productivity. So we did that in two ways. One, was to ask how frequently in the historical data series have we seen the recruits come back at fewer than their spawners that produced them.
Q Yes.
DR. PETERMAN: That's the first part that is directly analogous to what Walters et al did in their report.
Q Yes.
DR. PETERMAN: And then the second measure was to look at this relative fit of the Ricker and Larkin Models and the see what the time trends were that they produced.
Q Yes.
DR. PETERMAN: So to get to the specific point about how our results compare with this Walters et al 2004 paper, we found the same thing that they did, even though they had a slightly different metric, what they said was a measure of over-escapement. We saw very few instances, seven percent of the cases, in which we had fewer recruits returning than spawners.
Q If the question were framed differently, instead of "Does over-escapement cause salmon stock collapse?" if it were, "Does over-escapement cause salmon stock decline in productivity?" you'd answer that as, "Yes," would you?
DR. PETERMAN: Decline in productivity, probably yes, because almost by -- if you believe the Ricker and Larker Models, any increase in spawner abundance will cause the returns per spawner to go down.
Q Yes. Now, I want to -- because you said you still adhere to the conclusions in this paper, I'd like to refer you to evidence that was given here by a panel with two of the -- including two of the authors of this paper, Riddell and Walters, on February 10th of this year, at page 14.
DR. PETERMAN: Okay.
Q So I'll start reading. This is at page 14, towards the bottom. It's a question put to Dr.

Riddell -- well, it's a question put and Dr. Walters answers it:

Q All right. Now, some of you have already spoken to aspects of this next question that I have, particularly as regards the evidence on the 2004 paper. But my question of the panel now is does the panel agree that there is no historic evidence of catastrophic recruitment failure coming about as a result of extremely high escapement. I think that's in large measure what the 2004 paper is speaking to. But am I right in what I say, that no historic evidence of catastrophic recruitment failure from high escapement levels?

DR. WALTERS: No. As we indicated yesterday, there are data more recent than we had that do hint at that possibility for a couple of the stocks, Quesnel, most spectacularly, and Chilko.

Q You say "hint at", but we haven't seen it, have we?

MR. WALTERS: No, we see radical drop in recruitment.

Q Okay.

MR. WALTERS: Following a period of high spawning stock.

Q But have you seen it to the level of it being catastrophic to the stock?

DR. WALTERS: Well, I'd say in the Quesnel case, a drop from in the millions down to in the hundred thousand or so is pretty catastrophic, yes.

Q The 2004 paper, as I read it, says that there's no evidence that over-escapement will cause a stock collapse. Are you changing your view on that?

DR. WALTERS: Yes. As we explained yesterday, for two reasons: newer information --

THE COMMISSIONER: Sorry, can you scroll down, please?
MR. LUNN: Certainly.
THE COMMISSIONER: All the way down?
MR. HARVEY:
Q At line 15:

DR. WALTERS: Yes. As we explained yesterday, for two reasons: newer information and the failure in that 2008 paper -

-- I think he must mean the 2004 paper --

- to have looked at both the Gilhousen work, showing strong cycles back historically, and also the newer data.

So, and perhaps to complete it, I'll read what Dr. Riddell has to say:

DR. RIDDELL: Well, I can finish. I think I said yesterday that at that time I would still support what we wrote. So we're kind of mixing two elements here. If your question was is the paper still sound, well, then I agree with Carl, that we have seen an even greater range in escapements now. We have done more analyses, so I think people now would have a different conclusion to be drawn.

Would I personally say that we're still seeing a high risk of catastrophic loss? I don't think so. But Carl is more familiar with the interline interactions than I am...

Do you disagree with any of that?

DR. PETERMAN: No, that's a rather broad question. Well, no, I guess I don't disagree with it to the extent that I'm, not aware, though, of what, specifically, Carl Walters was talking about when he said there are more recent data that he had to suggest -- his mind changed. So I'm not familiar exactly with which data he was talking about, but, in general, I follow the arguments.

MR. HARVEY: Yes, all right. And finally, I think we can get this in before we break, at page 62 of this transcript -- I'm sorry, it was a passage by Dr. Woodey that I wanted to -- I'm sorry, I think we will have to do that tomorrow. Thank you, Dr. Peterman.

DR. PETERMAN: Okay, thank you.

THE COMMISSIONER: I'm sorry, Ms. Baker, we're adjourned until 10:00 tomorrow morning?

MS. BAKER: That's correct.

THE COMMISSIONER: What are the balance of the time estimates, can you give me some sense?
MS. BAKER: I'm not sure how many more minutes Mr. Harvey will be, but we have, after him, about two and a half hours.
THE COMMISSIONER: Thank you very much.
THE REGISTRAR: The hearing is now adjourned until ten o'clock tomorrow morning.

(PROCEEDINGS ADJOURNED TO THURSDAY, APRIL 21, 2011, AT 10:00 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.

_____
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.

_____
Karen Acaster

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

_____
Diane Rochfort

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

_____

Karen Hefferland