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

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L'Honorable juge /
The Honourable Justice
Bruce Cohen

Commissaire

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Errata for the Transcript of Hearings on February 9, 2011

Page	Line	Error	Correction
30	45	Adams River *13:03:29	Adams River South
39	8	Cultus *13:27:34	Cultus data

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Boris Tyzuk, Q.C.	Province of British Columbia ("BCPROV")
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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")
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APPEARANCES / COMPARUTIONS, cont'd.

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

APPEARANCES / COMPARUTIONS, cont'd.

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

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THE REGISTRAR: Order. The hearing is now resumed.
MS. BAKER: Thank you. Good morning, Mr. Commissioner. Today we have a new panel of witnesses, two of whom you have met before, two of whom are new, and we've got one, as you can see, on the video feed from Florida. So we have Dr. Carl Walters in Florida on the screen, looming over us here larger than life, and we have Mr. Ken Wilson, who you met the last two days, Dr. Jim Woodey in the centre on the panel, and Dr. Brian Riddell. So for Dr. Riddell and Ken Wilson, their oaths would remain, but the two new witnesses will need to be sworn.

KEN WILSON, Recalled.

BRIAN RIDDELL, Recalled.

JAMES WOODEY, Affirmed.

CARL WALTERS, Affirmed.

THE REGISTRAR: Could you state your name, please.
DR. WOODEY: James C. Woodey.
THE REGISTRAR: Thank you. Dr. Walters, your name?
DR. WALTERS: Carl John Walters.
THE REGISTRAR: Thank you. Counsel.
MS. BAKER: Thank you. And as we discussed yesterday, I have a few questions for Dr. Woodey that spill over from yesterday's hearing, but I'll just incorporate those into our overall presentation this morning. So I think I will go through the backgrounds of Dr. Woodey and Dr. Walters just at the outset and then we'll move to the questions for Dr. Woodey alone.

EXAMINATION IN CHIEF BY MS. BAKER:

Q So I'll start with you, Dr. Woodey. First of all, your bio was provided to the Commission, and that's at Tab 11 of the binder before you, and it should be coming up on your screen. Just by way of background, you obtained your Ph.D. in 1971 on

sockeye salmon; is that right?
DR. WOODEY: That's correct.
Q And you worked with the International Pacific Salmon Fisheries Commission from 1971 to 1985?
DR. WOODEY: Yes.
Q And you've stayed with the -- well, with the new Pacific Salmon Commission after the transition up until 2002; is that right?
DR. WOODEY: That's correct.
Q And you were the Chief Biologist and Head of the Fisheries Management Division for the PSC during that time?
DR. WOODEY: That's correct.
Q And in your work as Chief Biologist at the PSC in relation to Fraser River sockeye, you worked with the Fraser River Panel, you were involved in monitoring programs, and you designed fishery management strategies to achieve Treaty objectives; is that fair?
DR. WOODEY: Yes, that is correct. The work evolved over time, but all parts of the fisheries management was under the control of the IPSFC, and I was the Chief of the Fisheries Management Section for that last few years, and then with the PSC.
Q And this is your biography that we now have on the screen before you?
DR. WOODEY: Yes.
MS. BAKER: I'd like that marked, please, as the next exhibit.
THE REGISTRAR: Exhibit 414.

EXHIBIT 414: *Curriculum vitae* of Dr. James C. Woodey

MS. BAKER:
Q You have been retired since 2002, but you've continued to work as a consultant and you've continued to be involved in research involving cyclic dominance and population dynamics of Fraser River sockeye; is that right?
DR. WOODEY: Yes. I was more involved in the early years after retirement, and in the last few years it's been at a lower rate of involvement.
Q Thank you. I'll just move now to Dr. Walters. Dr. Walters, you have a long history also in salmon biology, correct?

DR. WALTERS: That's right.
Q And your c.v. has been provided to the Commission, as well, and that should be found at Tab 2 of the materials. And I don't know if you can see it on your screen, but if not, I'll just try and highlight some points from this lengthy resume. You have been a Professor at the University of British Columbia, since 1969; is that right?
DR. WALTERS: Yes.
Q And your work at the university is in Applied Ecology and Population Dynamics; is that right?
DR. WALTERS: Yes.
Q And also dealing with Fisheries Population Dynamics, et cetera?
DR. WALTERS: That's right.
Q You have been involved in a number of professional activities which are set out in your c.v. at pages 3 and 4, and I don't think I'm going to go through them orally, just to confirm that they are there on your c.v. Your main research at the university is in theories of harvesting and natural resource management; is that right?
DR. WALTERS: That's right.
Q And you have authored many, many publications in the area which are set out in your c.v., the last -- well, there's a whole publications record, which is how many pages long here, 13 pages long, which is set out at the back of your c.v.; is that right?
DR. WALTERS: That's right.
MS. BAKER: Okay. I'd like that c.v. marked, please, as the next exhibit.
THE REGISTRAR: Exhibit 415.

EXHIBIT 415: *Curriculum vitae* of Dr. Carl Walters

MS. BAKER:
Q Thank you. Now, I'll turn back to Dr. Woodey. Dr. Woodey, before the Pacific Salmon Commission was established in 1985, the IPSFC was responsible for setting annual escapement targets; is that right?
DR. WOODEY: That's correct.
Q And as Chief of the Fisheries Management Division of the IPSFC, was that something you were responsible for?

DR. WOODEY: Yes, during the time that I was the Head of the Fisheries Management Section, but I was involved in developing escapement targets as the Assistant to the Assistant Director of the IPSFC from the time that I was first employed with the IPSFC in 1971. So I was involved for a period of more in the order of 14 years, as opposed to just being responsible for three or four years.

Q And we have been provided with a copy of a forecast document and an escapement target document prepared by the Salmon Commission -- actually, I think, well, this is in 1985. I'm not sure actually if this was under the Pacific Salmon Commission or the IPSFC, but if you can turn to Tab 13 of the materials, you'll see a 1985 Fraser River Sockeye Forecast document. And would this be under the old Commission or the present Salmon Commission?

DR. WOODEY: That would be actually produced in 1984 under the IPSFC.

Q Okay. Just prior to the transfer over.

DR. WOODEY: Yes, a year-plus prior to the transfer.

Q And the escapement targets are set out in that document at page 58 in a table. Can you just explain what this table shows us.

DR. WOODEY: The table is the combination of the pre-season forecast, given for each stock, forecast of four-year-old returns, in the second column, the five-year-old, that would be what are termed "5/2s", the "3" ocean fish going out in the first year and "5/3s", the "2" ocean fish, well, after two years in freshwater. Those would sum to a total forecast, returned forecast. And then the net escapement goal would be developed independently of that forecast. And that all of the stocks, the major stocks and many of the minor stocks in the Fraser system were forecasted each year, and this table would have been found in each of the pre-season forecast documents.

Q Okay. Currently, forecasting is done under a separate document from escapement targets, which are produced and contained through another process. This one document did both those things, it contained both the forecast and the escapement targets; is that right?

DR. WOODEY: That's correct. The escapement targets were done separately, but as part and parcel of

the overall presentation of information to the IPSFC, the Commissioners, so that they had both the forecast in hand and the proposed net escapement goal.

Q All right. How did you set the annual escapement goals for the different stocks?

DR. WOODEY: The escapement goals were set in a number of different ways. When I first became involved in 1971/'72, the numbers of years of data that we had to work with were very limited to roughly 20 years, because the racial analysis program that identified the catches by stock and thus produce a total return each year by stock was begun in 1952. And therefore when I began working on this in the early '70s, we had roughly 20 years of data. And when we're looking at dominant line returns, we would look at the data for the dominant lines separately from off-lines. So we would end up with relatively few data points, something in the order of five or six data points, and that made the estimation of net escapement goals somewhat problematic because there wasn't a lot of information. Now, of course, there's over 50 years of data available, and therefore even on a single dominant line there's adequate numbers of data points to be much more accurate in the setting of escapement targets.

But the basic technique was a combination of, by 1985, of running stock recruitment analyses on the information that we had at hand, and if those stock recruitment estimates provided optimum escapement goals, then that would have been used. More than often it would be a combination of the estimates from the stock recruitment relationships and historical data that might be available that influenced our thinking on the setting of those goals.

There are situations that in the time period that some stocks were still in the rebuilding phase and had not reached that point where currently you could look back at that long term of data and understand what the productivity of the different lines, cycle lines, were or would be, and thus provide better estimates of optimal escapement. So it was not an unscientific technique, but all different pieces of information had to be brought into play to provide net

escapement goals.
Q The table that we see in front of us has an escapement goal set for every stock listed, which looks like there's about 18 or so. Would the stocks be managed in that way on an individual stock basis, or would the escapement goals be aggregated in some way?
DR. WOODEY: Escapement goals were aggregated. We, in fact I think it would be more proper to say, that we were targeting the management toward certain major stocks each year, the Early Stuarts, certainly because it is a stock which comes in fairly independently of the other stocks. In 1985 it was a dominant line year for Quesnel stocks, Horsefly, Mitchell, and that stock grouping would have been the primary focus of management during the season. And then in other years, particularly years that had strong late run returns, Adams, Shuswap stocks, they would have been the target of management.

In some cases the escapement goals that are provided are essentially taken off the harvest rate, which would have been needed to achieve the escapement goal for the major stocks. So if we're looking at a 70 percent harvest rate for Quesnel stocks, we would set the goal for co-migrating stocks, minor co-migrating stocks, essentially by that harvest rate. So they may not have been optimum harvest or optimum escapements for the smaller stocks, but they were practical estimates of what could be achieved.
Q And was the escapement goal designed to reflect dominant cycles and subdominant cycles and off-cycle years, or was it a goal that was set kind of on an average across all cycle lines?
DR. WOODEY: The escapement targets would be set by cycle line with the dominant line being unique, some dominant line generally separated from the others, and then the off-cycle lines for cyclical stocks would be, if you will, relegated to going along for the ride, type of thing, with the harvest rates of the major stocks for that year being the dominant goals.
Q I think it's 18 named stocks there, how are the stocks which are not, the "Miscellaneous", I guess, is how you've described them here, how did you do the calculations for those stocks, or why

were they not broken out in the same way?
DR. WOODEY: In regard to the --
Q Escapement goals.
DR. WOODEY: -- escapement goals. Generally speaking the escapement goals for the minor stocks and the off-cycle lines of major stocks, such as Adams, would have been set primarily by through the estimates of what was likely to be the harvest rates for the dominant stocks that year. And so, as I say, if we were looking at a 70 percent harvest rate for Quesnel, the minor stocks that essentially were completely overlapped in their timing in the marine areas and freshwater migration would have been assigned close to that escapement rate, and thus an escapement target, which reflected that escapement or that harvest rate.
Q If I could get you to just give me some definitions, just to make sure we're all on the same page in the next couple of questions. Can you define what a "fixed escapement policy" is. What does that mean?
DR. WOODEY: A fixed escapement policy would be where regardless of the forecast for a particular stock, the escapement goal would remain the same, perhaps as estimated through a stock recruitment relationship. In other words, it tended to give us results where the particularly dominant escapements on large stocks were relatively well close together in all of the dominant line years. And so am I -- maybe I got off on...
Q No, no, I just want if we use that term "fixed escapement goal", or "policy", I just want to make sure we understand what you're talking about when we use that term.
DR. WOODEY: Yes.
Q Okay. Then the next question is, the same kind of question, a definitional question, what's a "fixed harvest rate policy", as compared to a fixed escapement policy?
DR. WOODEY: The fixed harvest rate policy would be to set the harvest rate or set fisheries which would produce an anticipated harvest rate and fish that at that level, regardless of the abundance of the stocks coming back. So the variation between cycle years would not influence the fishery management that would produce in small return

years, smaller escapements, in large return years, larger escapements, on that line. So you'd get more variation in the escapement levels for an individual stock over time.

Q What's the IPSFC management of Fraser River sockeye during your tenure? Was it based on a fixed escapement policy, or a fixed harvest rate policy, or something else?

DR. WOODEY: It was generally more configured to be a fixed escapement policy with the larger stocks, the stocks that were the focus of management being managed to achieve escapements for those stocks that were similar on each of the recurring dominant lines and such.

Q Did the IPSFC ever manage sockeye based on a fixed harvest rate policy?

DR. WOODEY: No, we have not.

Q Once the setting of escapement goals moved to Canada, a new method for setting those goals was developed, and it's been described now as the FRSSI model, the Fraser River Sockeye Spawning Initiative. Well, first there was a rebuilding strategy and then eventually it became the FRSSI model. I just wanted to know if that was something that you were involved in the development of. I think you might have been leaving the PSC right around the time FRSSI started to be developed, so was that something that you were involved in?

DR. WOODEY: I was not involved in any of the development of the model. I was a participant in the process that the FRSSI model people brought together to get feedback on the work that they had been involved in, development of the model.

Q And did you have any criticisms of the first iteration of the FRSSI model?

DR. WOODEY: Yes, I did. I had concerns about the model, the stock recruitment models that they were utilizing in the FRSSI model, and that was because the stock recruitment model, the Ricker model that they had decided to use, had a tendency with low harvest rates of building the offline stocks, offline abundances of escapements, and in cyclic dominant stocks producing by their simulation modelling a more even production. And that was from my point of view a misleading and erroneous approach to the stock recruit modelling.

Q There was, we heard yesterday from Mr. Cass, that in 2006 there was a workshop held to address the topic of cyclic dominance. Were you part of that workshop?
DR. WOODEY: Yes, I was.
Q And were the criticisms that you had of the FRSSI model addressed following that workshop?
DR. WOODEY: Yes, they were. I would have to say that the approach that I had taken and had written about was a slightly different approach than what the group as a whole decided would be the appropriate way of approaching it, and that was to have the FRSSI model use what's called a Larkin model for all stocks on all lines. So that would pick up the delay density dependence, that's a characteristic of cyclic dominant stocks.
MS. BAKER: Thank you. Before I leave the topic of escapement planning, I should have this 1985 Fraser River Sockeye Forecast document marked as the next exhibit.
THE REGISTRAR: Exhibit 416.

EXHIBIT 416: 1985 Fraser River Sockeye Forecast (IPSFC)

MS. BAKER:
Q Now, Dr. Woodey, just to lead off on this notion of cyclic dominance, I think it might be helpful just to get again a definition from you, what does that term "cyclic dominance" refer to? Can you just give us some help on that.
DR. WOODEY: Cyclic dominance in Fraser River sockeye is a natural, from my point of view, a natural reproduction pattern that was found to be in place in the early years of contact in the early 1800s, and for most stocks became -- or I should say most cyclic dominant stocks, became a pattern that when we began managing fish, actively managing fish in the '40s and '50s, was recognized as being the state of nature and was the accepted norm. And management from that point to the time that the IPSFC was disbanded and in the early years of the PSC, was recognized to be the norm.
Cyclic dominance involves one large return year, the dominant line year; generally a subdominant line year, generally that being the year following the dominant year, and then two

years where the abundance is somewhat lower from less than one percent of the dominant year abundance to a few percent of the dominant year abundance.

Cyclic dominance appears to be a biologically-driven phenomenon, rather than a fishery-driven phenomenon, although Dr. Walters and a few others in the audience were involved in writing papers regarding cyclic dominance that were originally, or thought by some, to be the consequence of the harvest strategy, harvest management strategy. And it appears to me in retrospect that the real conflict between approaches occurred in the returns in the late '60s, early '70s, where a happenstance of dominant year failures and strong subdominant year returns, gave the appearance that there might be a change of dominance, and that the fisheries, IPSFC, harvested down the subdominant year to retain the dominant year pattern. And that fishery harvesting, that high harvest rate that occurred in those years, for 1967 and 1971, that from a mathematical modelling point of view tended to suggest that the harvest plan, the management of the fisheries, was the cause of the cyclic dominance, wherein going back we can see that marine survivals were high on those subdominant year lines for, in this case, Adams sockeye.

And it's right now we're undergoing the same type of situation with the Horsefly, the dominant line of the Horsefly or Quesnel system stocks, is the 2009 line, and a low recruitment on that line and thus low escapement in 2009 and the high marine survival rate on the stocks in 2010, has caused the subdominant line run of the Quesnel stocks to be larger than the dominant line run. So it's the same type of thing that we saw in the late '60s, early '70s on the Shuswap stocks.

Q Thank you. Are all of the stocks on the Fraser system stocks that show this pattern of cyclic dominance?

DR. WOODEY: No, there's several stocks that are cyclic that show cyclic dominance, all of these stocks are located in the Upper Fraser. And the reason for that is the more stable stream environments, spawning stream environments, likely have given stability to fry production and on the individual

lines, the big lines, and we get a very proportion of the recruitment as four-year-olds. And so those two things go together, that is, a high proportion of four-year-olds and stability of the system generates a condition that cyclic dominance occurs.

In the Lower watershed, the stocks actually take a survival strategy, producing much higher proportions of "5s", five-year-old return fish, to spread the risk over more years - or maybe Carl, Dr. Walters, can provide a better terminology - but it provides an insulation against catastrophic loss due to high flows, and so on, in the streams that are on the Coast, which are very unstable from a standpoint of heavy rainfall events and such.

Q So can you give us the names of -- would you like to add something, Dr. Walters? Is that what I see your finger in the air about?

DR. WALTERS: If Jim doesn't mind, do you?

Q No, that's fine. If you want to add in, that's great.

DR. WALTERS: Let me add a point here about a little historical point. Right around 1985 when you were asking Jim to do those forecasts, we were doing simulation experiments at UBC to try to figure out how big the errors would be in estimating the best spawning stock size given note of the few years of data, like for each cycle line. And we discovered to our horror that the statistical methods that Jim and we had been using are grossly biased when you do it by cycle line. The statistical model will always tell you to keep the escapement near where it currently is. It will always tell you to maintain cyclic dominance, even if in fact it's not optimum to do so.

That led to a series of analyses where we used the Larkin and Ricker models to aggregate across the cycle lines, and we concluded that it was possible that the cyclic dominance had been caused by fishing. We didn't assert that it was. We said it was possible that it had been.

So we recommended deliberate experiments to rebuild the off-cycle lines and that led to considerable and bitter controversy. Jim was at the time rightly very sceptical about those experiments, but as I understand it, they

proceeded anyway. But it was not done on the notion that the Ricker model was right. It was done on the possibility that there had been severe bias in the productivity estimates, because we know that the statistical method would cause those biases.

Q Okay. Just to clarify, the experiment to rebuild the off-cycle years, is that the strategy that we've heard about called the Rebuilding Strategy?

DR. WALTERS: That's right. The one that seems to be failing, and that if we had paid closer attention to Jim, and if we'd paid closer attention to other long-term analyses done by Pacific Salmon Commission staff, like Gilhousen, we probably would not have recommended.

Q Okay, thank you. Back to Dr. Woodey. Just to clarify, if you can help us with some names of the stocks that are cyclically dominant that we would be hearing about and ones that are not, just to help us when we're looking at charts and things and the stocks are laid out. If I understand it right, the Shuswap, Quesnel, and some of the Stuart stocks are cyclically dominant; is that right?

DR. WOODEY: That's correct. There are other more minor stocks in the system that do show cyclic dominance, as well, but within the Shuswap there are the Late run Adams, Lower Shuswap stocks, and also Seymour and Scotch Creek, which are Summer run fish, which show cyclic dominance. So the tendency is that within one individual watershed, most if not all of the stocks follow the same pattern of recruitment, cyclic dominance.

Q And now you -- we had a bit of a discussion around whether harvest rate or harvest strategies could have created the cyclic dominance effect and it sounds like that maybe has moved to one side. Right now is there consensus in the scientific community as to what the mechanism is for cyclic dominance?

DR. WOODEY: I'm sure there's some debate still going on. Our view of the world is that cyclic dominance is a freshwater phenomenon, and it's driven by the impact of one cohort or brood year offspring, juvenile sockeye, and their, from my point of view, consumption of the food resources in the year that they're in the lake, and the

residual effect of that cropping on subsequent cohorts of juveniles.

And when I say cohort, it's just the juveniles from the dominant line spawning affecting the food resources and that impacts the subdominant line juveniles, and then the subdominant line, or in some cases, a two-year lag of impacts, dominant line juveniles impacting the growth and the survival of subdominant and first offline. So it's that delayed density dependence within the freshwater environment that drives cyclic dominance.

Q And the food, the nutrition factor that you've just described is one of the hypotheses. I take it there's a few other hypotheses, including disease transfer and predation, or some other biological hypotheses as to how this kind of cyclic dominance is created; is that right?

DR. WOODEY: Yes, that's correct. Dr. Walters can pick up the thread on some of these. The predation model issue, from my point of view, which was, I should say, which was the prevailing point of view for many years as developed by Ward and Larkin, Dr. Fred Ward and Dr. Peter Larkin, in a publication in 1965, I believe, examining the Shuswap run, Shuswap stocks. They concluded that predation mortality was driving it, that is the predators in the system ate a lot of juvenile sockeye from the dominant line year, grew well, had high fecundity and such, and their offspring, the trout that were preying on the juvenile sockeye, produced a lot of offspring that grew to a size that they could prey on the subdominant or generally offline year juveniles, and thus add to the mortality rate.

And some of the work that I've done, and that will be added to the next paper we have, indicates to us that the cycling of the predators is not the issue. It's just the predation rate, and the depensatory, what is called depensatory predation, where when there are few juvenile sockeye in the lake, the predation rate goes up to a point that it offsets the compensatory mortalities that you generally find in sockeye. So where you would expect the production rate on the offline years to be better because the lower density would give higher success of spawning, and/or egg survival

and such, to my way of thinking, the mortality in the lake from the predators, it's just overcoming that compensatory advantage and thus resulting in fewer juvenile smolts going out, per adult, on the offline years, and maintaining thus the cyclical pattern, keeping a lid on the production on the offline years.

Q Thank you. And did you have something to add, Dr. Riddell?

DR. RIDDELL: Well, I may say you're touching on a couple of really important points that maybe the panel needs to try and help some level of agreement on. Your first question was whether we think that there's a biological basis to cyclic dominance now. I think what Carl referred to earlier in terms of the interaction between lines, and Jim's comment, I would say that most people now, or my opinion would be that most people believe that there is a biological basis. We're still trying to understand what it is. But I think that the notion that it's maintained by fishing is not accepted.

Now, the other point, Jim started off the discussion about the productivity between years, and he subsequently went to disease and predation. Well, last week we talked in Stock Assessment about our Fraser Lakes program conducted by the Department. We do know that it's not as simple as just food production between years, because we do have data showing that the recovery of the lake is certainly sufficient to produce food far in excess of what would be required by the small number of fish in the subdominant cycles. So the reduction in the spawners is far, far greater than would be required by the productivity available within those lakes. So it's something more, or an interaction of all these things together.

Q Okay. I guess, yes, Dr. Walters.

DR. WALTERS: Yes. I think a key kind of overview statement needs to be said about cyclic dominance, is that what it's about is the sockeye interacting with the ecosystem. I agree, I've gone through the zooplankton data myself, and I agree with Brian Riddell, that Jim cannot be right about it being only that part of the food web that's interacting with the sockeye.

A key point here is that our models like the

Larkin model and others that we've tried to produce to explain cyclic dominance should predict that if we stop fishing we'll go back to something like the populations were in the late 1800s, that is, there should be a single very strong dominant line, filling basically what we now understand to be the carrying capacity to all the nursery lakes, and then three very low lines follow on that, and it should be synchronous across stocks, because it was back then. None of our models predict that as a recovery endpoint. None of them predict the right response to not fishing any more, and that means that there's something fundamentally missing from all the models.

We certainly are missing whatever it is that links across populations to cause synchrony, and we very likely are missing top-down effects associated with -- Jim's right, that trout predation is not the answer either. There's something like parasites or diseases that we're missing entirely in our analyses. Another key point is that there's very few -- very, very few people have actually worked on this issue, surprisingly few, considering how important it is, and there's very few papers published about it, very little real speculation, very little fieldwork.

Q Okay. Did you have something to add, Mr. Wilson?

MR. WILSON: No.

Q We've sort of moved into the questions I had for the panel, so this is working very well. One of the next topics I wanted to just make sure we had a handle on was this idea of a maximum sustained yield. Again we'll start with Dr. Woodey. If you could just give us a definition of what that is so we know what we're talking about if that comes up.

DR. WOODEY: Yes, Mr. Commissioner, the concept of maximum sustained yield has been around for quite a long time. Dr. Walters is kind of the...

MS. BAKER: There's a battery change happening, Dr. Walters, on a mike.

Q Okay. I think we're back in business.

DR. WOODEY: Mr. Commissioner, Dr. Walters is kind of the expert in it. He's written books on it.

Q Okay. So maybe I'll pass it over to him.

DR. WOODEY: So in order to avoid embarrassing myself, I should let Dr. Walters answer it.

Q Right.
DR. WOODEY: Or Dr. Riddell. But if I can give you my view, maximum sustained yield is that average harvest that maximize or the maximization of the average harvest on a particular line in cyclic dominant stocks. And for example, we actually are looking for the escapement goal that will produce those maximum yields, and that tends to be defined by mathematical process of estimation. In using the Ricker model, it's relatively simple. The estimates of that maximum sustained yield point or escapement in other models is more difficult.
But the other point I'd make to be sure everyone is understand maximum sustained yield point or escapement is not the point of escapement which produces the largest run. It's the difference between the necessary escapement level in the return year and the return itself, that is the yield, and maximizing that yield is not the point of maximum return.
Q Did you want to add anything, Dr. Walters?
DR. WALTERS: Yeah. No, I think Jim's done a really good job. The key point is that MSY is not a simple single deterministic number that we calculate for model equations. As Jim said, it's the average yield, or average overall of the variability that we expect to occur out there, associated with the spawning stock that produces the largest average surplus of new recruits over those needed to replace the spawning stock.
It was discovered in the early 1970s that, in general, maximum average yield is a better word than sustained yield. Maximum average yield for long periods of time is obtained by following a fixed escapement policy, not a fixed harvest rate policy, and not any other more complex rule.
So when you say you're managing with an escapement policy, as you mentioned to Jim, you're essentially trying to do an MSY or maximum average yield management.
Q Okay. A couple of other preliminary questions. Dr. Woodey, I understand that there's two kinds of spawning systems in the Fraser watershed. One is a spawning ground limited system, and one is a lake limited system. Is that correct?
DR. WOODEY: Mr. Commissioner, the terminology here is something that I've been thinking about and

essentially rationalizing in my own mind for a number of years. There are systems in the watershed that we could call small stream/large lake systems, for example, Francois Lake in the Upper watershed and Nadina River, and that's not a small river, but a small quality spawning area. There's a spawning channel on it now to increase fry production and such, but you're putting relatively few fry annually into a large lake, and as such you don't get much density response in the system. So in a sense the juveniles, whether relatively few or the maximum number of juveniles going into the system, you don't get much response in terms of size. There's not any real sharp drop-off in size of juveniles. So that's what I call a spawning ground-limited system. The other stocks that are in the watershed or systems might be Chilko, which shows a relatively modest amount of decline in juvenile size over the range of abundance.

Then there are stocks or systems that are what we may call lake-limited systems, where the spawning area is good quality and large, and the lake where physically may be large, but may not be highly productive. And thus when you have high densities of juveniles going into the lake, the size that you have, the size that they attain is relative to the abundance of adults in the spawning population, and you can get severe drop-off in juvenile size in those systems. And that's been kind of the situation that we've had in the Quesnel system, and I'd point to it as being the characteristic lake-limited system in the watershed.

DR. WALTERS: Can I add a point here?

Q Yes.

DR. WALTERS: It's a warning, really, to be very careful about trying to talk about habitat limits on these populations, and that's because there's a tendency when you talk about -- when you try to use them to establish population size reference points, this population can be that big, that population can be so big, as reference points for measuring where the stocks are and how badly they've been impacted by harvesting. The reason that's very dangerous goes back again to the early history of the populations, early before the

fishery got going. Well, we know the stocks were exhibiting violent cycles, and those are not predicted by habitat capacity. In fact, the best estimate we have for total smolt rearing capacity for the Fraser is about somewhere around 400 million smolts from the recent Wild Salmon Policy analyses almost exactly predict the peak cyclic populations observed in the late 1800s of around 40 million fish. They indicate in that these fish were only successful at filling their habitats in one out of four years. And the other three years they were at numbers far, far below the habitat capacities indicated by spawning or lake rearing. So I don't think that habitat capacity measures or arguments are either useful or relevant to management of the sockeye. They're potentially very misleading.

Q Dr. Riddell, have you got any response to the two points you've just heard?

DR. RIDDELL: No, I think that I agree with the way Jim has defined the habitats. But I think that our thinking now is much more consistent with what Carl has just said, that I like his terminology he used, that you look at this within the context of the ecosystem, because if it's a biologically based cyclic dominance, yet we don't know the actual mechanism, it's clearly not as simple as habitat space and production. There is some other ecological mechanism functioning that we need to really investigate yet.

Q Mr. Wilson anything to add?

MR. WILSON: Yeah, I agree.

MS. BAKER: Okay.

THE COMMISSIONER: Ms. Baker, I wonder if I could just ask the panel, including Dr. Walters, just so I understand what you are addressing, you have been going back to pre-contact behaviour of the resource and post-contact behaviour of the resource. But when you say "ecosystem", does the science have adequate or sufficient knowledge of the changes in ecosystem both pre- and post-contact to be satisfied that your conclusions are in fact driven by the right parameters. Do the models reflect changes in the ecosystem to the extent that you fully understand the elements that you've just been describing with respect to habitat and the other factors around whether it's

a biological cause or a post-contact impact cause, or whether it's a harvest management issue.

MS. BAKER:

Q Dr. Walters?

DR. WALTERS: I think we can say pretty definitely that the stocks were exhibiting violent cycles before the fisheries became large enough to cause those cycles. That's one of the really important findings from the Gilhousen work, that there was a cyclic pattern established by the early 1890s. Not enough fish had been removed from the stocks at that time to cause the cycle.

Now, the issue of whether we can use that pattern to predict where the stocks would go under very low harvest rates today, the issue is really about whether the habitat structure out there or the stock dynamics have changed enough to make that early history irrelevant or not a good predictor. And I don't see that there has been such changes. I don't see that the habitat is less productive than it was. That I don't see that the stocks are less productive than they were. So I see no reason not to use the stock dynamics seen at the start of the fishery as a pretty good model for what we would see under very restrictive management.

Q Thank you. Dr. Riddell?

DR. RIDDELL: Well, I think Jim referred to even earlier papers, and Carl referred to, there's documents in the early 1800s with Hudson's Bay Company and relations with the Interior First Nations, or Tribes at the time, that there were years of abundance and there were years of scarcity. And I think that's important, so that the cyclic dominance goes quite a long way back. There's certainly consideration that some of the First Nation fisheries through the 1800s were substantial, but that did not stop or actually control the cyclic dominant cycle. So I think that that supports the notion that we have come to that it's largely biologically based.

And I think really the reference, Mr. Commissioner, to the models is one of saying that we can investigate these interactions. We don't have the knowledge of the biological interaction that's functioning yet. So I don't know that we could say that the exact same biological

interaction is limiting us today as it did before. But certainly the observation that they're similar and that the habitats still have about the same capacity supports that cyclic dominance is a biological feature that's been with us for probably as long as we know, two or three centuries.

Q And, Dr. Woodey?

DR. WOODEY: The part of your question, Mr. Commissioner, was relevance to the models. And the models that we have can only, with the current set of environmental conditions in the lakes, the productivities of lakes that we currently see, we cannot go back and estimate what may have been occurring in those systems 100, 200 years ago, simply because we haven't collected any data, from my point of view anyway. But if there have been declines in productivity, then we're capturing those in our models that we're currently using.

The question then becomes one of is the productivity, the ecological productivity of the system different than it was back then, and of course there's different views on that issue.

MR. WILSON: Excuse me.

Q Sorry, yes, Mr. Wilson.

MR. WILSON: I would like to make a point. You know, we go back to pre-contact and have a discussion about what salmon populations might have been like. I think we can most of us agree that populations on average were larger and escapements at some times were very substantial. We have, you know, 40 million fish perhaps, but I've got a quote here from Dr. Ricker that peak abundance in Fraser sockeye might be as high as 160 million. That was quoted by Northcote and Atagi. In those years, cyclic or not, you would anticipate massive escapements moving into the Fraser, and I don't think it's reasonable to assume that those were not important.

Prior to contact and intensive fishing, it's likely that very large escapements were common. And those escapements have an impact on the freshwater ecosystem that may be quite profound. There's very rich literature looking at paleoecological data, sediment cores, the importance of marine-derived nutrients to both the productivity and carrying capacity of freshwater

ecosystems, benefits to streamside vegetation, aquatic vegetation, the bears, the birds, the general argument around ecosystem services. Now, whether or not nutrient delivery plays a role in cyclic dominance, I can't say. But it is likely that nutrients arrived in very large amounts periodically prior to the onset of fishing. I think it's important to consider that in the broader picture. Thank you.

Q Thank you. And, Dr. Walters, you had another additional comment.

DR. WALTERS: Oh, let me just, lest you buy any of what you just heard, let me point out that the most violently cyclic dominant stock is the Shuswap and the nutrients don't go into the lake.

Q And can you just explain why that is? Why does the (indiscernible - overlapping speakers).

DR. WALTERS: Because the Adams River, where they spawn, is right at the outlet of the lake. Those nutrients go downstream. If they fertilized an ecosystem, it will be a downstream ecosystem filled with enemies of sockeye during their migration. This business about lake fertility and enhanced production because of lake fertility, can be happening, and it certainly is happening, but it's already measured in the stock recruitment data. So in Quesnel, when we see higher production out of the dominant cycle line, when we observe that, we are observing it under nutrient-enriched conditions on that cycle in. So it's double-counting to pretend that there's some extra benefits there that we wouldn't see.

And also if that natural system was exhibiting the violent cycles documented by Gilhousen and others, with one year of plenty and three years very poor in between, it's really hard to imagine that sockeye had a large and sustained impact on much of the rest of the ecosystem. it must have been a really nice to eat them when they were around, but they could not have been sustaining a much healthier or larger ecosystem if they were such a rare component of that ecosystem's diet, if you like.

MS. BAKER: Mr. Commissioner, it's 11:20. Should we take a break now?

THE COMMISSIONER: Yes. Now, for Dr. Walter's sake, are you going to keep him online, or...

MS. BAKER: I think we keep the link, but he can go and walk around. So we'll have a 15-minute break, come back in 15 minutes.
THE COMMISSIONER: Thank you very much.
MS. BAKER: Okay. Dr. Walters, you heard that?
DR. WALTERS: Gotcha.
MS. BAKER: Thank you.
DR. WALTERS: Fifteen minutes.
THE REGISTRAR: The hearing will now recess for 15 minutes.

(PROCEEDINGS ADJOURNED FOR MORNING RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: The hearing is now resumed.
MS. BAKER: Thank you.

EXAMINATION IN CHIEF BY MS. BAKER, continuing:

Q I have a question that is a pretty important definitional term for the purposes of this panel, so I'm going to ask each of the witnesses to answer it, and I'll start with Mr. Wilson and I'll move across the table and then end with you Dr. Walters.

So the question is we're here on this panel, we've called it an over-escapement panel. What does over-escapement mean, and I'm going to start with you, Mr. Wilson.

MR. WILSON: Thank you. As I was saying earlier, prior to contact I think there's significant evidence that salmon populations may have been substantially larger in the Fraser than they are now, and I think that these large and perhaps cyclic returns were associated with very significant nutrient inputs into fresh water on a fairly regular basis.

I think these ecosystems, in all likelihood, adapted to this periodic significant influx of nutrition. It supported lake productivity, stream productivity, and while we can have a debate about exactly how those nutrients were used, they were used.

Over-escapement really can only be understood if we call it by its proper name, and I think in this case, it's under-fishing. We're not harvesting all the fish that have been identified

as surplus to the escapement goal using the kinds of management models and processes we currently use. But it shouldn't be construed as biologically harmful in any way. I think it was a natural part of the process, a natural part of the ecosystem. If you look this year at the very large returns to the Shuswap, we saw a redistribution of spawning effort and large numbers of spawners in lots of places in the Shuswap where traditionally we haven't seen large returns.

So if you imagine a world where these very large escapements were commonplace, I suspect we saw a different distribution of spawners, very large escapements from freshwater areas, lots of carcasses and nutrients that benefited, in all likelihood, large sections of the Fraser watershed.

So I guess, in sum, I'm simply suggesting that under-fishing -- I know when I started with the Department as a biologist, it was really the only thing a management biologist could do to get himself in serious trouble was to under-fish. If you exceeded the escapement goal, you could get yourself in trouble if it was a significant overage.

So it's really a human yield argument, not a biological or ecological argument. I don't think there's much evidence to suggest that there's any harm being done to the natural world by what are clearly natural events, large escapements, periodic or otherwise. It is a yield argument and it's about how many fish we decide to kill. I think it's fair to say that we don't harvest salmon for the benefit of salmon. We harvest salmon for the benefit of humans. Over-escapement is exactly that. It's failing to take advantage of the entire surplus as identified by people like us.

MS. BAKER: Dr. Woodey?

DR. WOODEY: Over-escapement has had a negative connotation in the industry and I'd say in the biological community locally since the large run and escapement of Adams River sockeye in 1958. That, at the time, generated something in the order of three-and-a-half to four million fish on the spawning grounds, and the returns from that

spawning were some of the smallest on record. That has never been explained carefully, clearly. There were attempts to actually limit the numbers of fish that entered the lower Adams River to ensure that they were not -- that later spawners not digging up the reds (sic) of earlier spawners, things of this nature.

From a management point of view, over-escapement is the level of actual escapement that reaches spawning grounds. That's, in my context, more than double the MSY point, so it would be larger than what we call the "p max" or the maximum -- the escapement level that produces maximum returns on average. Some stocks show fairly significant declining limbs of the Ricker or a Ricker curve that's fit to the existing data that suggests that in the Fraser watershed, over-escapement can actually lead to a substantially lower total recruitment from that spawning population and thus it's not an insignificant issue from the standpoint of future returns and harvest.

So we're looking at over-escapement as being a negative issue as it pertains to harvest in the future.

MS. BAKER: Thank you. And, Dr. Riddell, what is over-escapement?

DR. RIDDELL: Well, for the Commission, maybe we'll try and put this fairly succinctly. I think really what people are referring to is a significant reduction in the return per spawner, which we call the productivity when you have very large numbers of spawners on a particular lake, in a lake system.

I think both the speakers before are correct. You put this in a yield context. This is about production and we very commonly discussed production within the context of the Ricker stock recruitment curve. This is the dome-shaped curve where you relate the number of spawners to the subsequent number of progeny that return from that spawning year.

There is a line in that relationship that is equal to the -- "a" progeny returning per "a" spawner, and we call that the replacement line. So if you were to pick a point where people become very concerned about over-escapement, it's very

likely to the right of the intersection of the recruitment curve and the replacement line.

Because even in the absence of fishing, even with that lost yield, it's implied by that, you would still have a population that will decline in the future. That would be what the expectation would be if you had very large numbers of spawners.

Now, Jim has just made a very important point in all of this, I think, is that many times, escapements that subsequently occur in a year will be on a particular point that might be called "MSY". But it's only the very large escapements that should be probably at least twice the target escapement that I think people would really become concerned about the so-called over-escapement where you would be projecting or predicting significant loss of recruits per spawner. I'll leave it at that.

Q And, Dr. Walters, what is over-escapement?

DR. WALTERS: When Brian and I were asked to write about this for the Pacific Fisheries Resource Conservation Council, we pointed out that there are two definitions. One, the Alaskan definition, I think people call it today, and that's allowing escapement surplus to those needed to produce the maximum average yield. The second definition was a catastrophic collapse in recruitment of very high spawning stock sizes.

We argued based on the evidence we had then that there was little risk of that in the Fraser. But subsequent to writing that report, two things have come to light. One of them is additional data collected during a period -- recruitments from high spawning stocks during the late 1990s and early 2000s. Another was Gilhausen reconstruction of abundances in the late 1880s.

The newer data do provide stronger evidence of over-escapement in the terms of the big decrease in recruitment, most spectacularly for the Chilko stock. Taken together with the Gilhausen reconstruction, I think we have to now admit substantially higher risk of severe stock declines and severe cyclic population behaviours under reduced harvest rates.

MS. BAKER: The point that was just raised by Dr. Walters where he says that there can be an impact,

and pointed out Chilko as an example. So my question is: Does escapement beyond this MSY point that's been referred to in answer to the first question, does escapement beyond that point actually negatively impact the productivity of all Fraser River stocks? Is that what you're saying, Dr. Walters? I'll start with you.

DR. WALTERS: It certainly has impacted productivity of particular stocks like the Chilko, the Adams and the Quesnel. The data are pretty clear that the highest recruitments to those stocks have been produced at intermediate spawning stock levels, not at the highest point stock levels.

I think what Gilhausen and the early data warn us is that we also need to think about the possibility that these effects have transmitted across stocks, that the old mechanisms that cause synchrony in the cycles across the stocks may be reasserted. They may be in fact reasserting as we speak. It may be that some of what we've seen in the last four years, the very low production and suddenly a very high production across several stocks like the Chilko and Adams, it's indicative that the system is trying to return to that earlier synchronized mode where all the stocks are showing high in one year, all the major stocks at least, and then very, very low returns in between.

I don't think anyone wants to see that world again. It's certainly not a world that would be good for any of today's fishing interests, that boom and bust or feast and famine world with only one good year out of four.

Q Mr. Wilson, can I ask you to respond? There's different points of view from where we started with you that have been articulated. What's your response to them, and also to the question that I just ended up with, whether there is an impact on productivity and, if there is, whether it's spread across all stocks?

MR. WILSON: Well, clearly there's an impact on productivity at the very high -- if productivity is measured as returns per spawner. It certainly impacts at very high spawner abundance. And I'm not arguing the point that managing escapements is important to maintaining human yield.

What I am suggesting is that if we go back to the time pre-contact, when harvests were low,

populations were large and periodically very large escapements, much larger than we've seen recently certainly, may have been commonplace. This isn't a problem for salmon. The salmon have adapted and the systems have adapted to this natural periodic influx of nutrients. I'm saying that from a salmon's perspective, it's not a bad thing necessarily. We don't understand all the consequences of these sorts of large escapements.

If you broaden your frame of reference beyond human harvest and the abundance of salmon alone, I'm suggesting that it's an entirely natural thing that these ecosystems have adapted to, and we're now changing the world because we're trying to redirect and have redirected for the last 100 years or so up to 80 percent of that nutrient for human use. I'm not saying that's a bad thing, I'm just saying that it's one thing to say that it affects future yield to humans and another to suggest that there's biological harm or ecological harm that results from periodic large escapements, whatever the consequences of that escapement might be for future production.

Q If I could just pick up on something that we heard from Mr. Lapointe when he was here earlier in these hearings. He said that whenever you have extremes in an ecosystem, an extremely high abundance or an extremely low abundance, there will be impacts. And he said it's not benign or neutral to have a large escapement because it affects not -- and even if you leave the human element to one side, it will affect other species in the ecosystem. For example, he gave an example of where kokanee could be severely affected because there would be a high number of juvenile predators which were sockeye. So you could radically diminish other animals living in that system through high escapements. Do you have any response to that?

MR. WILSON: Well, only to reiterate the point that this is a human perspective. The ecosystem is adapted and quite capable of using all the nutrients that come in, in one way or another. There's no wasted resources.

It does affect future yield, and it may affect total productivity. But it's still part of a natural process that occurred prior to contact.

The salmon were here when we arrived and I suppose, by most accounts, we're in reasonably good health. So the suggestion that they need us around to kill them in order for them to maintain healthy populations levels, I just don't understand the logic.

Q So would you agree with Mr. Lapointe that there could be ecosystem impacts, non-human impacts though, impacts on other species from a very large number of salmon on a system.

MR. WILSON: Well, sure. Obviously, any particular event of that magnitude, millions and millions of spawners arriving all at once, dying and disappearing into the lake, will benefit many, many organisms, and may be a disbenefit to others. I mean, ecosystems are highly dynamic and they're under a constant -- they're in a constant state of change.

I'm simply suggesting that this whole issue of over-escapement is seen through the lens of human interest, and that from an ecological perspective, it's very difficult to make the argument that large escapements are necessarily bad.

Q Dr. Woodey?

MR. WILSON: Dr. Walters is waving his hand.

MS. BAKER: Oh, can you hold your thought for a minute, Dr. Walters, and I'll ask Dr. Woodey and Dr. Riddell to answer and then we can come back to you.

DR. RIDDELL: He's frozen anyhow.

MS. BAKER: He's frozen anyhow. Dr. Woodey?

DR. WOODEY: The concept of over-escapement, particularly in the Quesnel system in the last ten years has raised a number of issues that pertain to the management of the fisheries, and we won't get into the cause of the over-escapement, but it's something that's got to be part and parcel of the overall analysis here.

But the over-escapement in 2001 and 2002 gave, in the Quesnel system, at least double if not more fish on the spawning grounds than what our MSY estimates of escapement would be. So we're talking three-and-a-half million and three million in those two years as opposed to more MSY levels of escapement of a million-and-a-half to two million on the dominant line, and probably

more of the million to a million-and-a-quarter on the subdominant line. So we're two to three times, and the Quesnel system is a -- Quesnel Lake, the juvenile size dropped precipitously -- well, dropped on the dominant line juveniles and precipitously on the subdominant line juveniles.

So we're getting a crash in the system Part of that carrying over -- now, that was returns in 2005 and 2006. We hit the 2009 situation with the dominant line essentially has decreased to an escapement level in 2009 partly on this very low marine survival of only 150,000. So we went from three-and-a-half million escapement to 150,000 in two cycles, eight years.

The rebuilding of the Quesnel system, if that's an objective that's adopted by DFO in order to -- let me step back and say that in the 20-year period prior to that, the Quesnel system was the largest producer of sockeye in the watershed. So we've essentially lost, for the time being, the largest producer which has got to be viewed as part and parcel of the lower productivity of Fraser stocks in this last ten-year period.

So the rebuilding of those stocks in the Quesnel system will take time, and it will also require that a lower harvest rate continue for some time on those years that the dominant/subdominant line return. So there's consequences in the management of the fishery that are totally independent of ecological and ecosystem issues. But it's also bringing us back in the productivity of the system and production per catch, et cetera, for all user groups, back to a time well before the higher productivity that we've seen in the 20 or 30 years prior to now.

Q Thank you. Dr. Riddell?

DR. RIDDELL: I was just going back to your first question. If you're talking about the effect across all populations and would it affect productivity, well, I think the answer that we would all give is yes. But it has to be taken in a broader context, because the Ricker stock recruitment curve alone predicts that you'll have a lower productivity as the population gets past a certain point in terms of numbers of spawners.

So the real issue is one of what we're talking about before and how you define over-

escapement. Carl referred to two contexts which I would agree. One is the potential loss in yield per number of spawners there, but the other is this notion of long-term viability of the populations. Carl then went on and talked about Chilko as an example of that, and it's one that I've been looking at recently because the other thing that Carl didn't refer to is that it's a unique situation, the Chilko, where we have the smolts enumerated. The smolts are being quite productive. We're getting some of the best smolt production in recent years, and yet we're not seeing the marine survival.

So I think this other issue that we really have to be aware of now for the future is what are these common factors between the populations within the Fraser and where is that actually happening? How is that functioning?

But I'm not sure that it's simply on the spawning grounds in Chilko. The other data doesn't seem to support that.

Q And finally back you, Dr. Walters?

DR. WALTERS: Yeah, well, let me make two things. First, the biodiversity in the Fraser sockeye system is devastated by the Hell's Gate disaster and logging dams and some other things back around the turn of the century. We had hoped that increasing escapements would enhance recolonization in some of the areas from which stocks had been lost, but what seems to have actually happened was that recolonization occurred most rapidly during the periods of very high exploitation, '50 to 1980, around in that time.

I think the big escapement this year to the Adams did see a lot of fish dispersing out to other areas, but it also taught us that that isn't necessarily good at all from the standpoint of biodiversity. In most of the spawning streams around the Shuswap, fish need to spawn in early summer. They're part of the Early Summer run complex. They need to spawn early because those streams are cold and they need to have longer egg development times in them.

Just a few stocks like the Adams River *13:03:29 is the best spawn timing later. But when a large number of those Adams fish spread out into the streams where fish need to spawn earlier,

they are in competition with those earlier spawning fish and they -- if they mate with those earlier spawning fish, they'll produce offspring that are less fit.

So, in fact, some of this dispersal that people talk about and recolonization and increase in biodiversity, increased escapement maybe have just the opposite effect of what we would hope.

The other thing is that one of the lesson, as Brian point out in the Chilko, the Chilko escapement went up dramatically in 2000, and we had a period of high escapements that didn't produce higher returns, but returns remained normal for a few years, and then there was a huge drop in survival. That really feels like the high escapements and high smolt -- high rearing densities and the like, stimulated something to develop in the lake that is now killing Chilko smolts after they leave the lake, at very high rates. Our best candidates for such a "something" is parasites and diseases.

I got a grad student to go through and look at a large number of Chilko smolts collected over the years at the Chilko fence, and she found really high parasite loads in those smolts, higher than had been found in other stocks. It's quite possible that high escapements, combined with fertilization of Chilko Lake, led to a dramatic increase in parasite loads being carried by those fish, and that that's what's killing them at such higher rates now as you've heard about from Scott Hinch's tagging study and so on.

We really need some serious basic research on mortality agents in the freshwater system, and how those may be carried later in the lives to cause mortality after they leave the fresh water.

Q Thank you. I take it that kind of work is not being done currently by the Department of Fisheries and Oceans?

DR. WALTERS: Not that I'm aware of.

DR. RIDDELL: Well, I can add a bit to it and I'm no longer really all that in touch with exactly what they're doing, but there is work going on, on fish health. There is sampling that goes on. Dave Patterson probably referred to some of this work. Is it a dedicated research program? I don't think so at this time. I think it's more of a sampling

program. As we've talked about in the past, we are continuing to do the work on the sonic tagging of Chilko smolts.

It turns out to be an excellent choice, but I had to admit it was more about a matter of convenience because they have very large smolts that you can actually put sonic tags in. So it's an ideal opportunity of chance, I guess.

Q Thank you. The next question I have, it's a slightly different complexion. We talked a little bit earlier about delayed density dependence effects. Dr. Woodey talked a little bit about that. Does that effect apply to all stocks? Is that something that occurs in all stocks? Sorry, I'll start with you, Dr. Woodey.

DR. WOODEY: To my view of the world and looking at the data that we have, not all stock show this delayed density dependence, and that may be simply because of some of the things that I was mentioning earlier as Dr. Walters and Dr. Riddell have commented upon, and that is there are stocks that are small spawning stocks in big lakes, and they don't show this density effect on the growth. When you mathematically look at the productivity of the stocks, there's no evidence of carryover of the effects onto subdominant and off your -- that doesn't necessarily mean that there's no biological effects that are being expressed sufficiently to give you an impact, but it's not measurable from the data that we have.

Some of the more dramatic delayed density dependence that we've found are in the Quesnel stocks, and I was mentioning this. Two large year escapements, the subdominant juveniles, even though they were theoretically pure juveniles, 'cause they were purer adults than the dominant year, 2002 brood, their size dropped considerably.

So that seems to me to be the key diagnostic for delayed density dependence, that if you see a pattern in the data that shows that there is an impact of the large dominant year on the subsequent subdominant, or even in later years.

Q Thank you. Dr. Walters?

DR. WALTERS: When we first fit these Larkin models, the delayed density dependence models within the late 1980s, and the models fit better than the Ricker model, and we thought, ah, but they predict

such crazy violent dynamics, it can't be right.

Then over the years as data have accumulated, and most spectacularly last year when the Grant et al population analysis for the Wild Salmon Policy came out, it was really surprising to see most of the stock showing fairly convincing statistical evidence of delayed density dependence, that is, the Ricker model fit substantially better than the Larkin model and certainly predicting much more of the decline in survival since 1990, than does the Ricker model.

So what I --

Q Sorry, can I just interrupt for one second?

DR. WALTERS: -- (indiscernible - overlapping speakers) not only mounting statistical support for the existence of delayed density dependence, but that also is showing up in a lot of stocks for which we wouldn't have expected it.

Q I just wanted to clarify. I think you might have reversed the names of two models there. It's the Larkin one that shows the delayed density effect, or did I get that wrong?

DR. WALTERS: Yeah. Just to explain there, the Larkin model is a statistical model where we put in terms in a statistical relationship for possible delayed density effects and then let the statistics tell us whether or not those terms are likely to be different from zero, likely to be statistically significant we say.

In the early days, it was only a few stocks that showed statistically significant evidence of density dependent -- actually, none of them. But now, as I said, the FRSSI modelling analysis and the Wild Salmon Policy analysis show it for most stocks. It's possible that this is an artefact of confounding between the effects of population density and other things that are causing declining survival, coincident with high spawning stocks. But it's getting harder and harder to explain the patterns away as statistical artefacts of that kind.

Q Thank you. Dr. Riddell?

DR. RIDDELL: I don't think I have anything to add to this.

Q Okay. Mr. Wilson?

MR. WILSON: I have no comment.

THE COURT: Thank you.

THE COMMISSIONER: Ms. Baker, can I interrupt just briefly just to ask this question?

The panel members, fortunately I think for us, span the history from the pre-Pacific Salmon Commission to the current Pacific Salmon Commission and the management of the fishery throughout that period of time. In the FRSSI model that Mr. Wilson and other members of the panel spoke about yesterday, the terminology used in one of the documents that I read was developing an optimal escapement strategy.

I just want to make sure I understand from all of your perspectives, going back from those early years, perhaps pre-1985 to the current time, whether the research that's being done and the understanding of the models, or pre the models, focused on conservation as the optimal escapement strategy, or whether it shifted from conservation to harvest as an optimal escapement strategy or whether there's a hybrid or a balance between those two, and how all those -- how's the research -- what is the fundamental underpinning of the research? Is it around conservation of all of the stocks in the watershed, or in a mixed-stock fishery, does it take a different shift in terms of the optimal escapement strategy?

I'm just trying to understand where you're all coming from in terms of the conservation element and the harvest element which I think, Mr. Wilson, gave a good kind of photograph there of pre-harvest to post-harvest. Where is the emphasis? What underpins the strategies in the Larkin model, and where are you now placing the emphasis in terms of the answers you're giving to Ms. Baker when it comes to this description of something called over-escapement, which I think Mr. Wilson said was really under-fishing.

I'm just having a little bit of difficulty following the underpinning of your answers.

MS. BAKER:

Q Why don't we start with Mr. Wilson and we'll go across the panel.

MR. WILSON: I think I understand the general argument that yield is greatest at some particular average escapement that minimizes competitive effects and is more in tune with the average capacity of the environment. I think there are some really

important things that are external to these models that need to be considered. We're not simply trying to maximize harvest, in my view, when we manage salmon. We're trying to conserve the resource, we're trying to keep small stocks in reasonable levels of abundance, we're trying to address the harvest needs of First Nations, which are unique, and there a whole range of social and even spiritual values that have to be addressed in the management of salmon.

These MSY models take a very particular view of the world. If you look at the data on which they're based, it's highly variable. As Dr. Woodey pointed out, in many cases they show little indication of declining productivity at large escapements, at least over the range of escapements that we've observed. In other cases, there's a very clear relationship, and Chilko might be an example.

So when you're harvesting a large stock that shows this effect, you might want to harvest it fairly hard. Unfortunately, it's commingled in the fisheries with large numbers of stocks that may not show those effects, and it m ay not benefit from being harvested, if you want to put in that respect.

So it is a compromise. It's not that conservation comes first and we deal with conservation and then harvest comes second. We're compromising constantly in harvesting the yield from the strong stocks, trying to protect the weak stocks and trying to grapple with values that are clearly external to our models but important to people.

Q And Dr. Woodey?

DR. WOODEY: Mr. Commissioner, the history of the management of Fraser River sockeye goes back certainly -- when I say management of the fishery, I'm talking about managing the times and places that fishermen are allowed to harvest sockeye. The treaty between Canada and the U.S. that established the IPSFC was signed in 1937. The staff was established in 1938, but part of the agreement that U.S. had started, it was to collect data for eight years before taking management responsibility in 1946.

At that point in time, most of the stocks,

except for the Shuswap/Adams stock, most of the other stocks were in a depleted situation because there had not been much conservation efforts. The IPSFC closed the fishery in the first half of the year completely for four years to try to rebuild those stocks, conservation, rebuilding and so on, and built the fishways at Hell's Gate and Bridge River rapids and so on, and started the collection of data.

Then there was a noticeable shift in objectives after that to harvesting, but the development of optimal escapements was very tenuous for many years simply because there was very little data that could be used that if the stock hadn't grown to a point of reaching that MSY point, or optimal yield point, and therefore they were being harvested, perhaps from a retrospective view, harvested too intensely in some cases.

But through that whole period of time up until the PSC treaty was signed in 1985 and the Fraser River Panel took responsibility in 1986, the smaller stocks and off-year abundances and many of the larger stocks increased substantially. I wrote a paper in about 1990 looking back at that process, and it was really convincing that there had been, really, a rebuilding of the stocks and that conservation was in fact the first objective for most cases, but that when you're starting getting the stocks rebuilt, then harvest became a vital part of that overall management strategy.

But when we and DFO - I'm not speaking for DFO - but have been managing the fishery in the more modern times, from the '80s on, whenever there's been a lower recruitment of adults because of marine conditions or whatever, the first thing to go is catch. I remember the difficulty of closing the fisheries in 1995 and '99 and then the Fraser River Panel had to do that in '99, and then during the '90s and some of those years.

What we haven't discussed in part of that whole issue is, of course, the changing environmental conditions that the fish were facing. You've heard about some of that I'm sure. But the demands have always been to try to ensure that escapement, viable spawners reaching the spawning grounds as a primary objective, and sometimes that means that there isn't much, if

any, catch taking place. Fishermen are being held hostage, in a sense, to the reproduction dynamics of the fish.

Q Dr. Riddell?

DR. RIDDELL: Mr. Commissioner, if you didn't appreciates it, you've asked a huge question. That really was pretty much the whole essence rolled up.

I think your use of the evolution is a good analogy 'cause our understanding is always evolving, which we would certainly hope with the science programs. But I think that your contrast of harvest to conservation is actually not really what we try to accomplished anymore. There's no question there's been a change over time from a primary harvest and largely in the commercial fisheries, because of the abundance of fish, and now of course there is a stronger concern about biodiversity around the world. In particular, we have concerns about conservation for some populations in the Fraser River. So there has been a significant change in how these things are actually used.

Our understanding of what we're talking about in over-escapement really has to be considered in terms of the population dynamics within a population, because it does relate to the habitat capacities and characteristics of a particular lake and so on. That understanding has really changed quickly, and this, I think, is the main point that -- I saw Carl's presentation at SFU a few months back now, which was a very nice representation of how our thinking about population dynamics and appropriate models has changed through time.

I think that it's fair to say that the Larkin model that we're referring to now, which really is an interline expansion of the Ricker model. With that, that understanding has evolved really only in the last maybe couple of years. Carl has done some work and Carl's referred to the work that the Science Branch has been doing.

What we're really confronted with now is that the Wild Salmon Policy is now the basic salmon management framework. That has four principles. It doesn't say that it's conservation only. It has four principles that are conservation, respect

for First Nation rights, sustainable fishing and transparency. The transparency element is to include users in making decisions and understanding what the decisions are based on.

So, really, I think now it's a much more complicated world unfortunately because, in the broadest context, our goal is maximum -- well, optimal benefit really is what we're talking about. It's not one objective anymore. We try to maximize production for fisheries, and at the same time, there are requirements to meet First Nation needs in the river. There's requirements to sustain the conservation units and we need to involve user groups more. Later in the presentation, hopefully we'll talk about how to do this better in the future.

Q Thank you. And Dr. Walters?

DR. WALTERS: Yes, all of our analyses are based on the presumption of sustainability. We would not have a model put harvest -- allow a lot of harvest if it meant a large loss of production in the future.

The controversies come over three issues. One of them is whether to stabilize the exploitation rates in order to stabilize fishing opportunities, which sometimes results in under-escapements, and sometimes in over-escapements and reduces the yield a bit. That's being done in some fisheries. It certainly is better for industry although it loses biological yield.

Another fundamental issue concerns the new fitting of the Larkin models which indicates much lower escapement goals than most of us are comfortable with. For example, we've almost entirely avoided fishing the Early Stuart for the last several years. With escapement goals in the order of 100,000 to 160,000 fish, the Larkin model says we should only be allowing about 30,000 spawners a year, and that productivity will increase substantially, the fish will do well.

Then the third problem is protection of weak stocks that are harvested together with the big ones. Our classic example of that is the Cultus. The Cultus problem isn't a recent problem. Cultus stock started to decline in 1970, and have been declining every since then. The basic reason for that was the development of the Weaver Creek spawning channel that dumps very productive stock

into Harrison Lake, and was discovered in the '70s to be capable of producing yields of about 300,000 sockeye a year, every year, a four-year cycle, in the Late run.

So fishing targeted at it on the Late runs, the high exploitation rates to take that enhanced production, did in the Cultus. By 1980, we were looking at the Cultus *13:27:34 and saying if you keep fishing at these high rates, that Weaver can withstand, to get those 300,000 Weaver fish on average a year, you will drive the Cultus stock extinct.

Well, it's very clear that it couldn't take the exploitation rates and was collapsing. At that time, my recollection is that there was an explicit shrugging of the shoulders decision to write off Cultus. I suspect that from an economic, pure economic point of view, if we were to look at the value of the 300,000 Weaver fish that we can catch each year, and compare it to the 50,000 that we could ever catch, with luck, from Cultus, that that write-off was not a bad decision.

But that's the kind of trade-off we're facing in the biodiversity part of your question, is whether it's worth trying to protect these small stocks, the small and unproductive stocks.

DR. RIDDELL: Can I make a point?

Q Yeah.

DR. RIDDELL: Carl, I read that comment you just made about a conscious decision to not protect Cultus in the early '90s in one of the other papers I read. I have to admit I have no recollection of any such discussion and that, so before we leave that as a matter of record, is there any way -- I really have no recollection of any such discussion. And, at the time that we're talking about, it would not have been an easy discussion in any way.

So I'm really concerned --

DR. WALTERS: No, no, it was more a shoulder-shrugging in a couple of meetings. The context for that was when policy and planning, Al Wood and others, were looking at the whole business of the impact of salmonid enhancement in general on wild stock. The Cultus case was held up as a really good example of where having an enhanced stock being

fished together with the wild stock could very well result in disappearance of the wild stock.

In that case, I remember sitting around tables where people shrugged their shoulders and said, well, we can't give up those 300,000 fish just to protect a potential catch of 50,000.

I don't know. Jim, can you speak to this in terms of the Commission's decisions about what to do when the Cultus started to decline in the '70s?

DR. WOODEY: Yes, Mr. Commissioner, there's no doubt that the IPSFC, which built the spawning channel on Weaver Creek and was desirous of harvesting those fish, particularly on the non-Adams years, so there'd be two years of dominant/subdominant Adams, and Adams would drive the management the other two years, and the Weaver stocks often were large enough to drive the management of the fishery.

At some point in time -- well, a few years back, the Schubert circulated a memo that I wrote back in 1980 or so, or '70s, that addressed that whole issue. But it was a situation that the Cultus actually hit a low point in what would be the early '80s. So it hadn't plateau'd out. It wasn't going extinct, but some lines were very low in abundance.

The real problem with Cultus now is not harvest. It's the early upstream migration and mortality of Late run sockeye which, in my thinking, is the elephant in the room. With your inquiry, that is the thing that has dominated the management that has caused over-escapement on Summer run stocks, and yet has been so pervasive to reduce the productivity of Late run stocks, that it has got to be seen as being part and parcel of the real problem here.

When I talk about that, I get blank looks from a lot of people because they haven't been involved.

The beginning, the story is, briefly, beginning in the mid-90s, the Late run sockeye started migrating into the river earlier than they had been in the past. Normal behaviour would be that they'd arrive in the Strait of Georgia from the first week -- after the first week of August and the first week of September. They'd delay there for three to six weeks, then they'd migrate

up the river to their spawning grounds and spawn.
But beginning in the mid-90s, the upstream migration, and every year - and I'll say this publicly - every year from 1995 to today, Late run sockeye have migration behaviour pattern which is atypical compared to the 50 years prior to that, that we have records. The managing of the consequences of that early upstream migration and mortality of those fish has been the key management issue that has consequently affected Cultus, in particular.
I don't know how much you've --

MS. BAKER: Well, I think we might touch --

DR. WOODEY: -- heard about all of that, but some of what we're seeing now is a consequence of those issues, and should be discussed very clearly.

MS. BAKER: It's 12:33. We were going to talk about that issue that you've just raised later in my questions, so maybe we'll come back to that after the lunch break.
So we'll be breaking for an hour-and-a-half, Dr. Riddell. We'll be back at two o'clock our time. Sorry, Dr. Walters.

THE REGISTRAR: The hearing is now adjourned till two o'clock. I believe that will be five o'clock your time, Dr. Walters.

DR. WALTERS: Yes. I think I'll shut off my phone and I'll ring back in at...

(PROCEEDINGS ADJOURNED FOR NOON RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: Hearing is resumed.

MS. BAKER: Thank you.

EXAMINATION IN CHIEF BY MS. BAKER, continuing:

Q Dr. Riddell, we've been using some terms in today's testimony, weak -- small stock and weak stocks. Are small stocks all weak stocks or is there a distinction to be made there?

DR. RIDDELL: I think that's an important distinction, that we tend to talk about weak stock management and small stocks don't necessarily have to be unproductive, and really the issue is if it's very small, it's at risk of a number of random events, whether it's fishing-related or it's a habitat

event or something, so small is at greater risk. A small stock, though, in particular a lake, if it's depressed for some reason but has the capacity, it can still be fairly productive and sustain reasonable harvest rates. It's a matter of how fast it would recover. So what you're really most concerned about is a small population in a relatively unproductive habitat, and that is at substantially greater risk than something that is just small but is also productive. All right? So weak does not imply that it's necessarily small and unproductive. It could be small and productive.

Q Sorry, a small and productive would be considered a weak stock?

DR. RIDDELL: Well, it's still numerically small, so it still has a risk element.

Q Okay.

DR. RIDDELL: Some of the populations are quite small, maybe in a few thousand animals and so an event poorly timed or a large fishery when they happen to be present, right, for no reason other than all the population was there, it could be severely damaged. So small is at risk generally. A small unproductive stock could just have cumulative effects over time.

Q Okay.

DR. RIDDELL: Right.

Q These questions are in the first instance directed to Dr. Riddell and Dr. Walters and then I'll ask the other witnesses to add their thoughts. In 2004 a technical paper was prepared on behalf of the Pacific Fisheries Resource Conservation Council and that is in the materials Tab 1 CAN002587. This document has been referred to periodically in testimony so far. It was authored by Dr. Walters is the primary author with Mr. -- or Dr. LeBlond and then Dr. Riddell, as well. So I guess the -- because it has been referred to already in the hearings a bit, I'm just going to cut to the chase a bit on this and ask the authors why this paper was written and what the outcomes, what were the conclusions that were reached in this paper?

And I don't know which of the two of you would like to start on that question.

DR. RIDDELL: Well, maybe since I'm here, I can start.

Q Sure.
DR. RIDDELL: Carl can respond. This paper has come up in a number of contexts and I think we need to actually sort of put it in perspective, as well, because this was written for a very specific question posed by the Minister of Fisheries. The council we're referring to is an advisory council to the minister of the Federal Department of Fisheries and Oceans and it followed from the 2002 Adams River return. That actually is described very briefly in the document on page 15 paragraph 2, but the event of that year was that the run was returning at really above expected numbers; however, there were environmental conditions in the river that the in-season managers were expecting at least a 50 percent in-river mortality and when the season came to an end and the fish were in the river, we had a good escapement upriver and the en route mortality did not occur, and that the environment changed quite quickly. We had very good passage. I think mortality at the time - I went back about nine percent estimated, and so you had very substantial numbers of fish outside of the Adams and Shuswap Lake again.

In 2002 there were protests there. The Pacific Fisheries Council actually was at the Adams River for a couple of days of the protest and so after that, we got the request from the minister's office really posed by their advisory committee. And the question was responding to industry concerns does over-spawning lead to stock collapse. And so I think we need to keep in mind it was a very specific request. Because it was a response directly to the minister's office, it was deliberately written. It's not a particularly technical document. It was technical in the background but with minimal sort of detailed analysis involved and it really was about long-term viability of the stocks. So we really weren't talking about -- we weren't asked to comment on harvest policy and we weren't asked to really comment on appropriateness of fisheries policy or anything else. It was a very specific question.

The conclusions, I think, are very simple in the sense that we did not find in 2003 when a lot

of the work was done, that with the data from, what was it, 1952 to 2002 information from 19 of the Fraser production stocks for Fraser sockeye - and that data is from the Pacific Salmon Commission at the time - River's Inlet sockeye and Babine sockeye and two pink salmon populations on the coast. And based on that at the time we didn't see any evidence that the stocks following large escapements collapsed in the sense of reduced long-term viability. There's no question that there's evidence in the paper about significant reductions in productivity. This is more of an efficiency of production argument. And there is also no evidence as we went through the detailed records about pre-spawn mortality and disease incidence.

Those occurrences in the Fraser did assist them, which is very comprehensive for those. There wasn't any evidence that that was related to the density of the escapement on the spawning grounds either. There had been years of very high mortalities associated with disease but they were not associated strongly with the abundance of fish on the spawning grounds. It was much more to do with the environmental conditions and that is very consistent with a detailed paper written by Dr. Gilhousen at the PSC, as well.

Q All right. Thank you. Dr. Walters, would you add anything to that description?

DR. WALTERS: No, that's a -- Brian's done a great job of summarizing it. The only thing I would add is that in the light of the information that's been gathered since then and particularly in the light of the Gilhousen paper that we simply overlooked, we didn't know it existed, I think we'd be a little more cautious in saying that the down sides of over-escapement are minor.

Q All right. So that -- my next question was going to be whether the conclusions that were reached in 2004 are still valid today and maybe we'll just have you comment on that, Mr. -- or Dr. Walters.

DR. WALTERS: Yeah. Right near the introduction to that paper we make a reference to a Ricker 1987 paper that Ken Wilson mentioned this morning and it had estimates of over a hundred million sockeye for the system back in the 1800s. Gilhousen took Ricker to task and I went back and read through

the original Ricker paper and redid the analysis and that hundred million number is just crazy. It's just wrong. There's no way that there were ever anywhere near that many fish in the system during the period of historical records.

We would have cited in Gilhousen instead and we would have pointed out that once off a SOC may get away with -- you may get away with a high escapement without any long-term impact, but if it's done repeatedly, it could lead to the kind of reorganization of stock structure that would produce this very violent cyclic pattern that was evident in the late 1800s.

Q Dr. Riddell?

DR. RIDDELL: Well, I mean, I think if we look at the paper now then as we'd discussed this morning, the thinking has evolved a bit. We do have even larger escapements to compare, so I don't think there's any question that we would say a little -- our conclusions now might be a little different if we included all that data. There were a few --

DR. WALTERS: And to be honest, you know, we also made a mistake in completely overlooking the issue of delayed density dependence, but we only looked for the immediate effects by plotting recruitment against spawning numbers of a high spawning number immediately on the progeny from those spawners. We didn't even look for the possibility of delayed effects on subsequent spawning runs. I think at that time the statistical evidence that's been piling up in favour of existence in strength delayed effects just wasn't there.

DR. RIDDELL: Mm-hmm.

Q Sorry.

DR. RIDDELL: No, I would agree, but I mean at that time we didn't go back and do that assessment. And a couple of other papers since then, too, I mean, I think at the time an important point is that we may not have had the contrast to really see some of these effects yet. We only really had a couple of really large years of escapements at the time.

We actually pointed this out in the introduction because we were thinking at one point well, how powerful would our analysis be to really look at long-term viability. We had years of data, but many of those years of data had been

fished at fairly high rates, as we've been -- as we've discussed and the populations have sustained themselves, but we had not had many years of very, very high levels of escapement. So there was a limited sort of contrast there whereas now we have more data.

The other thing I'd point out in the paper, I should have commented in the beginning, it's one of the few places where we updated the Fraser Lakes information on the abundance and size of the fry produced in Quesnel and Shuswap Lakes from the Fraser Lake surveys. That data is actually surprisingly difficult to get - readily available if you just phone and ask for it, but it's not widely published. And so I've actually got an update of that material too, if you wish to see that later.

Q I should mark this paper as the next exhibit.

THE REGISTRAR: Exhibit number 417.

EXHIBIT 417: Pacific Fisheries Resource Conservation Council, "Does Over-Escapement Cause Salmon Stock Collapse: April 2004 paper

MS. BAKER:

Q Sorry. And, Dr. Walters, were you just going to add something else?

DR. WALTERS: No, I'm fine.

Q Okay. And Dr. Woodey, do you have anything you want to contribute here in terms of whether there is -- whether over-escapement can cause a collapse of a stock?

DR. WOODEY: No, I don't believe I do.

Q Okay. And Mr. Wilson?

MR. WILSON: Well, I just observe --

DR. WALTERS: Maybe I would add one point here and it's in relation to Jim's work on the Quesnel system. Something else that we didn't have available at the time of the over-escapement report was the fairly dramatic decreases in body size and survival rate of the Quesnel stock as it is built up. The stock started -- its off-cycle lines are down just a few thousand fish, started to grow geometrically back in the 1980s and by the late '90s were up not huge, but much larger than they were initially. And there was a severe decline in body size of smolts and severe decline in survival

rates. An interesting feature of that is that that decline carried through to the off-cycle lines, as well as the on-cycle lines.

In the older data the big runs produced relatively small smolts, obvious competition effects, but as declines progressed, even the little tiny runs for which there was plenty of food according to the plankton data and so on, those little tiny runs started to show suppressed growth and survival, as well. And that indicates some really severe density-related or ecosystem-related carryover effects of some kind going on in the system.

Q Thank you.

DR. RIDDELL: Can I just point out that what Carl's talking about is actually in the document, right? So we can add a couple of data points, but if you go to the Appendix 1, the last page of the document really, really it's only the Quesnel 2002 that we've talked about. 2001 is exactly on the regression line relating the female escapement to body size and you can now add a number of data points to that and the trend is identical. 2002 is the only year --

DR. WALTERS: Except we didn't have the data showing the suppression in the off-cycles.

DR. RIDDELL: This is the data that was available for all cycles in the document. If it's not in the document, it doesn't exist right now.

DR. WALTERS: Ah. Well, Jim's going to publish it.

DR. RIDDELL: Well, it's becoming much more topical and I think that there's no question that we need to get that information out to the Cohen Commission, because you can't bring this up now to the current time and you'll see that the regressions actually are almost identical, right? So it speaks to the sort of resilience of the lake systems, if you want. But the idea that the smolts are -- not smolts, but the Fall fry are getting consistently smaller is not true actually, as we add the new data through the line.

Q Dr. Woodey?

DR. WOODEY: Yes. And just in relation to the graphs that are shown in the last -- currently up on the screen, two problems exist. One is that these regressions are deficient in the sense that they include juvenile Kokanee in the samples that are

collected in the lakes. Juvenile Kokanee are smaller than the juvenile sockeye and when you have relative -- you have an off-cycle line sockeye, the juvenile Kokanee could easily dominate in the regression. So what you're doing then in the statistically in the regression is you're flattening out the regression line by the fact that you're getting small Kokanee on the left-hand column, near zero effective female sockeye. And so that's the first problem.

And the second problem is that when you're mixing all of the data, dominant lines, subdominant line, et cetera, what I've found is that you actually have separate regressions fit to the dominant line and the subdominant line and that the dominant line juveniles for given numbers of parent spawners are larger than the subdominant line juveniles. And that's your delayed density dependence and it's not being characterized properly in either of these regressions because you have the -- primarily in the Quesnel. But the Kokanee problem exists in both Quesnel and Shuswap.

DR. WALTERS: Could I make a comment for the commissioner about that very technical thing you just heard? The bottom line of this stuff, sir, is that we are dealing with highly fragmentary data that is questionable all over the place in its interpretation. We do not have found long-term monitoring programs. We're relying entirely on historical accidents of population change to provide the data for us, rather than any kind of real experiments. And there's no foreseeable end to that ambiguity as far as I'm concerned. We will continue to be confused for a very long time unless we go out and to very much larger scale management experiments to deliberately push some of these populations around in abundance a lot more than I think anyone's willing to do for management.

Q Mr. Wilson, I think you started to answer the question and then you got cut off. So...

MR. WILSON: I just wanted to make the general commonsense argument that when we arrived here and began our commercial fisheries, we likely found salmon stocks that were in pretty good health and more abundant than they are today by far. It

seems to me that if over-escapement was such a serious problem, that problem would have been manifest when we arrived here, we would have expected to find salmon stocks in trouble because of the very large escapements that almost certainly occurred periodically prior to the onset of significant commercial harvest.

Q Dr. Woodey, you had a comment?

DR. WOODEY: Yes. Just going back to what Carl was saying, there's two sources of information relative to the size of juveniles. The first is the actual juvenile sampling, and as I say, the main problem there is the need to separate the fish which are from Kokanee spawners, that is Kokanee being landlocked sockeye and which are small as adults and have small egg sizes and small fry.

Separation of the Kokanee is first problem or first issue, but to get at the actual growth in the lake, there is a second source of information and that is the scales of the adults coming back and so that's what I used in the paper that we wrote regarding the cyclic dominance. The scale measurements from adults that return to spawn and those are all sockeye so we know that they're sockeye and we can use those data for going back many generations, further than the juvenile samples go back. So we do have some data here that we can use.

Q If I can just bring you back to the question that I asked, and I wonder if you could just address it -- oh, sorry, Dr. Walters?

DR. WALTERS: Yeah, I'm sorry. I have heard Ken Wilson say three times now, I believe, that there used to be lots more fish when -- there were lots more fish when white men arrived. That is -- that statement is simply not true. When the fishery purse peaked up about 1890, not enough had been removed before then to have depleted the stock substantially and the stocks were not larger than about 40 million, what we see -- what we predict to be the peak capacity of the system. And they were already cyclic.

It is not true that there were hundreds of millions of sockeye and that everything was healthy until we got here and fished them. In fact, I think Jim Woodey has pointed out that on

average the run sizes of the '90s are pretty close to the run sizes of the late 1800s, run sizes before the fishery had had an opportunity to deplete the stocks.

Q And what's -- what's the information that you use to make that statement?

DR. WALTERS: That's based primarily on the Gilhousen abundance reconstruction, and that is based on -- as soon as information became available in about 1892, which is pretty much as soon as the fishery became large enough to have serious impacts, Gilhousen was able to obtain fishing effort data, how many boats were fishing and when they fished specifically through the year. And he was able to calculate the harvest rates exerted by each boat, by each unit of fishing effort. Given those harvest rates, he could back-calculate what proportions of the runs were in the catch and therefore how large the runs had to have been.

Q And Mr. Wilson, where does your information come from?

MR. WILSON: Well, I'm looking at a reconstructed run. The source cited here was Pacific Salmon Commission. I'd have to sort it out exactly, but it's the same reconstructed database, I believe, that goes back to 1893. It's true that --

DR. WALTERS: That's Gilhousen database basically.

MR. WILSON: Yes. It's true that the stocks were strongly cyclic, but every four years we saw an escapement that was comparable with the 2010 returns. So going back to eighteen ninety --

DR. WALTERS: That's because all the stocks were on the same cycle line. If you --

MR. WILSON: Fair enough.

DR. WALTERS: -- remove the recent Adams stock and Horsefly and so on all to the same cycle line, you'll find that the peak is very similar.

MR. WILSON: I'm just making the point that with these very large escapements occurring every four years, you might have expected to see some stock collapse, but there doesn't appear to be any evidence of that on the data prior to Hell's Gate.

DR. WALTERS: You don't expect to see what?

MR. WILSON: You would expect to see some stocks collapsing when you're seeing escapements of, you know, very large escapements and runs on the 40 million range.

DR. WALTERS: You don't think that having three out of four years having very low returns represents anything like collapse?
MR. WILSON: No.
DR. WALTERS: Consistently three out of four being low?
MR. WILSON: Well, it may be that cyclic dominance is the natural state of affairs in Fraser sockeye. I'm simply making the point that very large escapements occurred every four years. Now, it may be a problem for fishermen and it may be a problem from the standpoint of taking yield, but it's unlikely to be a problem for the fish themselves. I don't see the stock collapse as a biological problem. I don't think that's what we're talking about here.

We're talking about an arrangement that may not suit even commercial fisheries on an annual basis, but we saw strong, healthy runs, cyclic or not, and there's no evidence that those runs are in any sort of decline. The dominant cycle seems to be strong and returning at between 16 and 39 million, based on Gilhousen's data. So it seems to me that if these large escapements were causing such difficulty, that some of those difficulties should have been evident during that time period and should be shown in the data.

DR. WALTERS: They were evident in the three out of four years when poor returns occurred.
Q All right. I think we've got that point. The question I would like to ask now about this paper, the 2004 paper, is we've heard about how it was -- how the work was done and why it was done. Is it -- does it remain today a valid tool for managers to use in assessing impacts of over-escapement on stocks? Is it valid today to use the results of that work to assess whether a stock collapse may result from an over-escapement? And I'll start -- I'll ask Dr. Riddell that question.
DR. RIDDELL: Well, is the paper still valid? I'm not sure that it's the correct comparison, I guess, in the sense that I would still draw the same conclusion about stock collapse, but we are going to add different cautionary notes as Carl alluded to earlier, because we have more data, we've seen larger escapements and we do now think that there are different models, much stronger inferences about the interaction between lines, certainly

increased information about rate of loss of productivity and high spawning escapement goals, but as we've said in the beginning, that's largely expected to some extent.

Are we seeing collapse where we're not getting recoveries of the stocks? No. We're still not seeing that unless the notion of the delayed density dependence really continues to escalate. There is clearly a concern in terms of the trends in return in Chilko sockeye are perplexing, although they did much better last year in 2010 again, so it remains to be seen what happens after that.

You know, in reading through it again carefully in terms of preparation for today, at the time I don't think I would have really changed anything. It needs to be read for the specific question that was asked. As I say, it's not about fishing policy. But I think we would add more assessment -- we probably wouldn't even do as very -- what I said as sort of a simple presentation now because the Larkin analysis obviously requires more consideration of the interlying interactions.

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

DR. WALTERS: Yeah. What Brian really said is what -- and I'd agree, is we'd be a lot more careful today --

Q Okay.

DR. WALTERS: -- about our conclusions. We wouldn't be quite -- we wouldn't be near as strong in saying what we said.

Q Okay. Thanks. I'd like to move to another variation on this theme. Since management of Fraser River sockeye moved from the prior strategy that was used by the IPFFC to the focus on rebuilding and conservation under the rebuilding strategy in 1987 and then through FRSSI, has that change in management strategy resulted in any negative impacts to Fraser River sockeye stocks? And I'll start with you, Mr. Wilson.

MR. WILSON: Well, by negative impacts, are we talking about negative biological impacts on the -- you know, the ecology and -- of Fraser River sockeye or are we talking about changing yield, the number of fish available for human use?

Q Well, why don't you answer both of those questions for me?

MR. WILSON: I think there's likely to be an impact of higher escapements on the number of fish available for harvest, particularly in mixed stock fisheries. I'm far from convinced that there's any long-term negative impacts on the populations themselves that result from high escapement. In fact, I've seen no evidence of it at all.

Q All right. Dr. Woodey?

DR. WOODEY: Could you rephrase the question?

Q Yes. What I asked was whether there's been -- we talked about this a little bit earlier too, that there was a change in management escapement policy and management strategies from the old Salmon Commission to the rebuilding strategy that was implemented by DFO in '87 and through to FRSSI today and since mid-2000s. Has that change in management strategy had any negative impact to Fraser River sockeye stocks?

DR. WOODEY: The change from the IPSFC to the early years of DFO management was relatively -- management patterns relatively similar, so we're not talking about differences immediately based on the 1987 stock rebuilding. But the harvest rates in some of those years were still fairly high and it was because there was essentially a desire to keep the large stocks at approximately maximum sustained yield point. There was concern with the rebuilding of some of the other either smaller stocks or off-cycles of large stocks, but we're not talking about the change to what is called for in the FRSSI model, which would be a much lower maximum harvest rate.

Q Okay. Did either of those strategies have a negative impact on the stocks in your view?

DR. WOODEY: In some cases there had been a positive response. I think the response of the Quesnel runs in the late '80s, early '90s and so on would have occurred regardless of any change in the pattern of management. The subdominant runs were building up and built up further in that time period. Some of the issues if you look at the data and don't question them on Chilko, those large runs that started occurring in the early '90s or large escapements were the result of some fertilization in some of those years. There were good responses to fertilization in some of the early '90s.

Q Dr. Riddell?
DR. RIDDELL: Well, I think if you look at the basic question in terms of what's happened over time, you would have seen a build-up, so there would have been a positive response through the initial escapement rebuilding program following, I guess that was the late '80s, that discussion, and then on through the '90s. When you've got to some of the very large escapements in recent years, as we've talked about several times already, there is evidence now of these interlying interactions that could be seen as being negative. They will reduce the production that we're getting back and so there may have well been a cost in terms of potential production of Fraser sockeye.

Have they done damage to the stock? See, I don't think that you can say that in the sense that you've had lots of spawning capacity. We obviously have had good years, return when we've got good marine survival, but you may through some periods have certainly been able to produce more fish if the density dependence starts to compound after the escapement goals. But I think any talk about negative impact on the stocks is probably not true.

Q Dr. Walters?
DR. WALTERS: I'd agree with what Brian said. I don't think we can identify any serious negative impact on the stocks, unless it turns out that the system is reorganizing itself on a large scale across multiple stocks to go back into a more violently cyclic regime like the late 1800s.
Q And --
DR. WALTERS: If that's occurring, there could be deleterious effects on a number of stocks.
Q And how would we know if that's actually happening, if that hypothesis is valid?
DR. WALTERS: Well, if it -- the worst fear is if next year is a real bust, and then we see two more years of real bust after that and then a real big run back, if we see the pattern of from 2007 to 2010 repeated again.
Q All right. Next question is again on a different subtopic here. Do we -- we've talked a lot about carrying capacity of lakes and I would like to ask the panel if we know enough now about the carrying capacity of lake systems to fully understand the

impacts of large escapements on these stocks. Like, do we have enough information now to do the work needed to better understand some of these impacts?

I'll start with Mr. Wilson.

MR. WILSON: I don't believe so.

Q Okay. Mr. Woodey -- Dr. Woodey?

DR. WOODEY: We know a lot about certain of the lake systems, certain Quesnel and Shuswap in particular. We need much more information on some of the other lake systems in the watershed to ensure that the decisions that are made in regard to escapement policy are taken with a good, clear understanding of what we're dealing with in terms of the productive capacity of lakes and the interactions between cycle lines, delayed density dependence issue.

We have systems, lake systems, that are likely undergoing fairly major changes as a result of the way the escapement has occurred or escapement levels have changed in the sense of the food capacity to rear fish. And I really think that the lake survey program is -- needs to be revitalized and it's been reduced by their financial capacity to cover more lakes, so it's -- right now the only lakes in the system that are consistently done are Quesnel and Shuswap and we do need more information on some of the other systems.

Q Thank you. Dr. Riddell?

DR. RIDDELL: Well, I think I agree with Ken, but I wouldn't be quite as emphatic in terms -- we know enough now to ask better questions. We know to focus on -- we really can't address this freshwater issue without looking at the fish health - some of the comments Carl's made about parasites, for example. We do need to look at some of the other lakes in terms of their dynamics. I think in previous sessions we've talked about what's going on in the Stuart Lakes. Well, we're not doing any work up there any more. So clearly to restore that lake - and Carl referred to it this morning - the statistic it looks like we should fish them harder. I expect you'd have a hard time convincing many people that that's where we should go in Stuart Lakes right now. So to justify that, you clearly want to do

some work in the fresh water.

And I think the big question is maybe we know enough to be a bit dangerous to think we know too much, but we really need to do the ecosystem studies in each year. Again, we talked about this in previous sessions. Right now we tend to sample in only one or two years when the dominant escapements have been there. To really look at the interactions you need to actually do the study each year and that has not been done for a number of years. So I think I agree with Ken that we don't know enough. We have learned a lot by what we're doing but we still need to do more work, I think, to tie this down.

Q Thank you. And Dr. Walters?

DR. WALTERS: I'd agree with what everyone else has said.

Q Thank you. Given time is precious here, if you think you've already answered this question, please just tell me so, but do the TAM rules that are created using the FRSSI model, which has a 60 percent harvest rate ceiling, does that create a risk of over-escapement? And if you've already answered this when I asked the question about whether the policy, the harvest policies of rebuilding and FRSSI had any impact, then just let me know. But if there's a different complexion to this question which gives rise to a different answer, please answer it. So do the TAM rules with the 60 percent harvest rate ceiling that come out of the FRSSI model present a risk of over-escapement?

Mr. Wilson?

MR. WILSON: Well, from the yield perspective, I guess the biggest concern I have is that we put a cap of 60 percent on this aggregate with a buffer to protect diversity in smaller stocks. But what we don't allow ourselves the luxury of doing is harvesting fish in large numbers in terminal areas.

So if you're convinced that a particular escapement is hazardous to the health of a particular salmon stock or CU, then certainly you might want to harvest it down in terminal areas. But part of the issue that's been driving this whole process is the health of smaller stocks and the protection of stocks that aren't producing as

well. So the price we're paying for protecting weak stocks is, if you like, under-fishing on the strong ones. By changing the structure of our fisheries, we could certainly address that problem, gain benefits in terminal areas and still custom tailor the harvest strategy for an individual stock.

That's not going to be easy and it's going to require some changes in our fisheries, but at least many of the concerns that have been expressed here could be addressed to some degree through that style of management.

Q Thank you. And Dr. Woodey?

DR. WOODEY: I don't have any comment.

Q Dr. Riddell?

DR. RIDDELL: I think Ken has addressed it quite well. I mean, obviously 60 percent sounds modest compared to past history, but it's substantially more on average than you've seen in the past probably five or six years anyhow. The question in my mind though is you really need to look at what are the triggers that would allow you to get to 60 percent. Because there could be triggers that very infrequently allow you to get there. So I think you have to look at the whole agreement before I could really comment on whether it's going to contribute to over-spawning. I just don't know unless you really look at how the 60 percent would be triggered.

Q All right. Dr. Walters?

DR. WALTERS: Well, as I mentioned earlier, the most recent Larkin model analyses suggests we might ought to be fishing harder if we don't get continued poor marine survival rate like we've had. I think Ken's bang on in saying that the 60 percent is kind of a compromise and intended to protect weak stocks and allow reasonable harvests and so on. Like, that -- it's a poor man's compromise compared to a more fundamental change in fisheries management that permitted more selective harvesting of a stock, wherever that might take place, up in the river or wherever. As long as we're going to be operating our fisheries primarily as large offshore mid-stock fisheries, we're going to always be facing this nasty trade-off.

We're always going to be facing whether it's

worth saving a stock that produces 50,000 fish like the Cultus, if letting it go we could catch 300,000 fish from the Weaver. Those are hopeless decisions. It's a hopeless trade-off. It's not surprising that the management system has come out with a kind of a compromise, it's lose/lose for everyone.

Q Well, that answer leads me into my next question, which is whether protection of weak stocks like Cultus should -- so how do you balance the protection needs for weak stocks like Cultus against risks of over-escapement or productivity impacts on stocks that may have that risk where there's a high level of spawners released onto the spawning grounds? So how do we do that balancing? And maybe go to you, Mr. Wilson, to start.

MR. WILSON: Well, the whole issue of the value of biodiversity comes into this answer. I guess our observation over the last few years has been that some stocks that we felt were potentially quite minor stocks, such as the Harrison, have become very large and now produce substantial yield and benefits to commercial fisheries. It's my understanding that the world is in a state of rapid change and always will be and salmon populations are in the business of adapting to that change and trying to survive. Biodiversity is the raw material that salmon populations use to adapt to change. Conditions change to favour one stock and to the disadvantage of another and the stocks that produce the yield will change from time to time. But it requires a knowledge of the future that we don't have in order to make a decision a priority about which biodiversity we can afford to sacrifice in the interests of yield and which needs to be protected. So our approach has always been to try to maintain and even improve on what we have. If we can do that, then we're going to be able to win on both counts.

Q Dr. Woodey? And if you can respond to the issue raised by Mr. Wilson, as well, that would be helpful.

DR. WOODEY: From our perspective in harvesting strong stocks and protecting weak stocks, there's trade-offs certainly and one cannot expect that you can optimize escapement levels on all stocks at the same time and there needs to be an understanding

that you don't manage the whole fishery for Cultus Lake sockeye and in fact, Cultus Lake was doing reasonably well in the '90s prior to the early -- the onset of early upstream migration of late run sockeye and that has thrown a whole -- the whole thing out of whack, a monkey wrench. And all of these low exploitation rates are -- not all, excuse me, Brian, but low exploitation rates in the '90s are largely -- well, a combination of poor marine survival, but then even when there are full numbers of fish, the DFO's policy on late run sockeye management has constrained harvest and led to some of these over-escapements that we've talked about.

Q Do you have a response to Mr. Wilson's point that we need to maintain all of the biodiversity we see in this system, that that is kind of protection for the future in a changing world?

DR. WOODEY: In terms of biodiversity as it pertains to sockeye stocks, the -- maintaining all of the cycle, the four cycle lines at high levels or -- is not a high priority from my point of view, because naturally I think the evidence shows that there's been years or periods of time when off-cycle lines have not produced well, but there's a biological reason for that if we're going to maintain cyclic -- cycle line interactions exist and the cyclic dominance pattern is maintained. Where your biodiversity comes in is the fact that you've got major stocks or major returns on at least one, if not two, two lines. You're not going to lose those stocks if one of the off-lines is over-exploited by the fact that a different stock has a dominant run on that line and is harvested at a high rate.

Q So you're saying where you have a mixed stock happening at the same time, and one of those is on an off-cycle, one of them is on an on-cycle, if you harvest at the on-cycle rate, you're not going to impact that off-cycle on the weaker stock? Is that what you're saying?

DR. WOODEY: I'm just saying that, yes, you would impact that line escapement but if it is dominant on a different line, you've still got a major stock and, you know, good health in the system. The sockeye we say are primarily four-year-old fish at maturity, but a portion of their

recruitment is at age five, and those five-year-olds get sloughed off from the dominant line to the subdominant and comprise in, say, for Quesnel have comprised over 50 percent of the escapement on a subdominant line and then their offspring come back as a high proportion of fives.

So there's a direct relationship between the proportion of fives in the escapement and the proportion of fives in the recruitment. So fives tend to produce more fives and so you -- if you get a hole in one line due to over-harvest, there will be a rebuilding of that from five-year-olds that are coming from other lines.

Q Dr. Riddell?

DR. RIDDELL: Well, as Ken pointed out in the beginning of this, it really is a matter of trade-offs and how we set the objectives. And we've also talked about inland fisheries, where you have opportunities in some of these larger stocks, but, you know, fundamentally I suppose this gets back to the Wild Salmon Policy discussion again, and it should be pointed out, I think, that the Wild Salmon Policy does not require every conservation unit to be at the same level of status. And so you really can, through Strategy 4, that I guess we'll talk about later, this really is a matter where the people affected should be involved in the assessment and discussion and come to a best solution. You are going to have trade-offs in these values. Just the way Carl says, if it's out in the open and you have less certainty in your information, the trade-off is going to be even bigger. But this is the sort of thing that we anticipated under Wild Salmon Policy and how you actually meet your objectives for biodiversity as well as sustain fisheries and add to that increasing request for inland fishing.

So this is really a matter of finding a way to get a process in place that can actually undertake these trade-offs and make the best decision within an open environment.

Q And do we have a process now that allows that to be done?

DR. RIDDELL: Well, I mean, this is the discussion about Strategy 4 of the Wild Salmon Policy. We have processes involved. Do we have analytical processes where people can really work with some

of the data? I mean, Carl's the expert in doing trade-off assessments and he's done some nice work with -- in the Skeena drainage for what we called the Independent Science Panel. We did do a structured decision analysis for how you can serve Cultus Lake Coho, but that's an example of the process but it was really a slightly different question in terms of what's the best combination of recovery tools at hand - enhancement versus predator removal and that sort of thing. You can do the same type of analyses looking at the contrast between the productivities of the different conservation units.

But the other thing -- I mean, as Jim has just said, there are opportunities between lines. When you're talking about conservation of genetic material you're looking at a multi-year thing because you're talking about the genetically affected population, not just the census number in a year, so there is a little cross-year mixing that does go on.

Q Dr. Walters?

DR. WALTERS: Yeah, I guess -- I have to wear two hats in answering that question. As a biologist, I abhor the idea of losing these unique evolved genomes, like Cultus Lake. It's a very unique creature. But on the other hand, if I tried to empathize with or put myself in the place of a commercial fisherman and over the last few years he has to worry that he's lost something like half of his income, which is a very large price to ask anyone to pay to ensure the future of the Cultus Lake, it would be very much like you're going to your stockbroker and having your stockbroker tell you that you had to keep every stock you've ever owned. I think you'd get rid of your stockbroker, wouldn't you?

There is a very fundamental conflict of interest here between those of us who prize diversity for its own sake versus those who are having to foot the bill for the things we prize. We cannot objectively say that maintenance of biological diversity is necessary for sustained fishery. We can very likely sustain fisheries forever on the Fraser just on the basis of its large stocks, and the (indiscernible) stocks that, like the Cultus, never will have the potential to

replace the loss if any of those large stocks collapse.

Q What about this -- what about the notion that we have had described or this theory that we need to maintain all the small stocks in order to create insurance for stock collapse, to preserve biodiversity for future changing environments? We don't know now which small stocks could be the future of the system. Isn't it too risky to --

DR. WALTERS: Nonsense.

Q -- say, preserve the big stocks?

DR. WALTERS: In the first place, let me ask you if you would be willing to pay half of your income for an insurance policy? Because that's what we're asking our commercial fishermen to do today.

On the second hand -- and the second thing, most of those small stocks could never be large. You heard testimony in the Fall from two biologists, Holt and Hyatt who spoke about the Cultus having the potential to be as large as the large stocks of today. Well, that's absolute nonsense. Cultus Lake at a little over six square kilometres could never produce the kind of numbers of sockeye that a big lake like Shuswap or Quesnel at 400 square kilometres is capable of producing. Our fishery and our economic future depends on those large stocks and their health, not on the little cute stocks that biologists like me love to look at.

Q What about the idea --

DR. WALTERS: We should not lie by pretending that they have some future economic value.

Q Is Harrison an example of a small stock that has become big and economically viable?

DR. WALTERS: Which has?

Q Harrison?

DR. WALTERS: It's become big. That doesn't mean it's become economically viable. It's been able to become big because harvest rates were low. It has not produced large catches. It's an example of a large unproductive stock. And you talked about small productive and small unproductive, well, there's also large productive and large unproductive. It's not capable, as far as we can tell from its productivity, of sustaining high yields out into the future. It did not sustain them in the past.

Q What about the idea that Mr. Wilson put forward of fishing in the river and then being able to conserve stock like Cultus that's at the entrance of the river almost, but you could then harvest further upriver?
DR. WALTERS: Yeah, if only our commercial fisheries had developed just above Chilliwack, it would have made a huge difference.
Q But would --
DR. WALTERS: Commercial fishermen would argue, too, that they have no right to fish there, that that would mean a complete reallocation to the native fisheries. They would also argue that the product quality declines considerably and prices and economic value decline considerably as you move up into the river.
Q But leaving those economic arguments to one side, as a -- in terms of the biology, would fishing further up the river be one way of allowing the preservation or conservation constraint on a stock like Cultus while at the same time fishing harder on those runs that you would say need to be fished harder?
DR. WALTERS: Sure. We could put in a couple of really large fish wheels just up -- fish traps and wheel systems just upstream of Chilliwack and take all of our catch for the Fraser while being very, very selective with stock ID methods and so on like that and hand the fish out to people up in the basin or sell them on behalf of the public or whatever. There's -- we could certainly reorganize the fisheries in major ways like you've said.

I think the issue here is one of basic rights to the fish. Have the commercial fishermen established basic property rights to those fish? Should the public take away those rights, transfer them to the First Nations fisheries that operate up in the river? To whom should the benefits go?
Q But those --
DR. WALTERS: We have a --
Q Those stocks -- Dr. Walters --
DR. WALTERS: -- pile of biologists -- we are not the people to be asking these questions.
Q Well, I was just going to make that point with you. We were asking you on the biology side today and some of those issues you just raised are more

of a policy type of discussion, I suggest.

Before we leave -- I'm just about -- the last question I wanted to leave with the panel is whether there are any recommendations that you would like to leave with the commissioner to consider in the inquiry. So I'll just -- I'll start with Mr. Wilson?

MR. WILSON: Well, Mr. Commissioner, I don't think we're going to get to a place where compromise and balance aren't important components of the management of Fraser sockeye. I don't expect that the world will be managed to maximize biodiversity and I don't believe that we'll preserve every stock. But I think we need to take a balanced view of both the stock structure, the genetic differences between stocks, the importance of specific often small stocks to First Nations and their rights and their harvest, and the interests of people that may consider salmon to be valuable for reasons other than their food value. So we're not going to get past compromise.

I guess my concern here is that we seem to be trying to answer what is clearly a complex sociological problem that has to do with balancing all our various values. We're trying to reduce that down to a technical question that scientists can give us an answer to, so that we can go away and make our decision without dealing with very complex problem of balancing everyone's interests. And I think that's what we're struggling with here.

Thank you.

Q Thank you. Dr. Woodey?

DR. WOODEY: Mr. Commissioner, my view is that in some ways very much like Ken's, there's got to be trade-offs in the way the management of the fishery balances the biodiversity issues and harvest. But I guess I'll harp again on the issue of why we're sitting here in some degree and that is that the issue of early migration of Late Run sockeye is the biggest problem that we're facing and is -- has in some cases direct relation to the over-escapement that we've seen that has led to a substantial reduction of the Quesnel stocks and possibly other stocks. And at the same time, harvestable -- there's been -- some of that has been just as a result of the policy that DFO

adopted in 2001 and somewhere along the line I expect that you'll have heard that I made a recommendation about how to address the issue of the early upstream migration and harvesting Summer Run stocks, et cetera.

That recommendation was for, I gather, a number of reasons not adopted, but there needs to be a clearer understanding that until we answer and work around that whole issue, much of what we could optimize here, you know, on paper isn't going to be practical because we've got a policy that restrains harvest rate when -- on Late Run sockeye to some much lower level, 30 percent I believe was the target, maybe 35 percent in 2010 on catch of Adams sockeye and so very major numbers of Adams sockeye went upstream unharvested and other stocks had equally large escapements and we will be asking the question four years from now whether the 2010 run over-escapements were, you know, what the consequences of that were. We need more research on getting to the basic reason for Late Run behaviour and we need more investigation into options other than what the Department of Fisheries and Oceans has adopted as its policy because of the impacts it's been having on the escapements on Summer Run stocks.

So this early migration is the elephant in the room, as I said, and it's not going to go away when the inquiry is through with its report.

Q Now, Dr. Woodey, I'm not sure if we were clear with you on that issue that you've just described. We did raise this with Mike Lapointe when he was here, but I wonder if I could just summarize and you can correct me if I have it wrong. But the issue was that there was a higher harvest rate available on Summer Run stocks in that year than there was available for the Late Run stocks, correct? Is that right?

DR. WOODEY: That's correct.

Q Okay. And that because the Late Run stocks migrated at a different time, they were -- there was an overlap; is that the issue?

DR. WOODEY: Yes. They had been coming into the river early and having high mortality rate that then led to the Department of Fisheries and Oceans developing a policy of lowering the overall harvest rate on Late Run sockeye to compensate for

anticipated early upstream migration and mortality.

Q So that your issue isn't -- that was the -- you agree that that was a good decision, I take it, to reduce the harvest on the Late Runs because of the anticipated en route mortality?

DR. WOODEY: The decision, in my opinion, was not correct in that we can predict that the early upstream migrants, Late Run migrants are going to suffer very high rates of mortality and therefore, to allow them to go upstream and allow the Summer Runs that were mixed with them to go upstream is a loss of yield and an over-escapement on the Summer Runs and at the same time, not doing much in the way of solving the problem on Late Runs.

Q And why didn't it solve the problem on Late Runs?

DR. WOODEY: Well, because those early migrants, which were protecting, are going to die en route or on the spawning grounds. And so we spent a pretty substantial amount of money on that whole problem, $3 million or something of that nature in research. We still don' have the answer of why they're doing that, but we can say that fish that enter the river, Late Run sockeye that enter the river prior to about the end of August are going to have an elevated mortality rate and therefore, policies that protect those fish are wrongheaded. If you wanted to do the best job of protecting the Late Run fish that are misbehaving you would shut down fisheries in the marine areas and get as many into the river as you can and then harvest those early upstream migrants because they're going to have this very high mortality rate and you could then harvest the Summer Run excesses. That doesn't mean, Brian, that we're going to harvest -- we would harvest every Late Run fish migrating in the river in August, but the evolution, in my opinion, the evolution of behaviour in the fish in different stocks in the watershed has been driven by the parasite that's killing them and allowing them to go upstream is not really going to solve the problem.

Q Now, I --

DR. WOODEY: It's not an adaptive thing.

Q I understood from Mr. Lapointe's evidence that there was an opposing view which was that although many of those early migratory Late Run stocks

would die, there may be some that wouldn't and they would be very valuable from an ecological point of view, those few that did survive and came in early, they may be very, very important from a long-term biodiversity point of view. What's your response to that argument?

DR. WOODEY: My response basically is that in 2001 when we first had the workshop to try to develop a research program identifying the causes of the early upstream migration, I presented a chart that showed Fraser sockeye with one exception spend 50 days or less in fresh water between entering and spawning and for stocks in the southern B.C., Washington, including Columbia River stocks, coastal stocks, spend 100 days to 190 days in fresh water without dying.

So why do the Fraser fish have very low -- short times in fresh water? Well, to my way of thinking, it's logical to expect that that has been driven by the parasite that has infected virtually every fish when they come into the river and thus, the -- trying to protect early migrants is really wrongheaded in the sense that there is going to be a major loss of those fish where -- so you're not getting yield out of those stocks nor are you getting yield out of the summer run stocks that are passing through unfished at the same time. So it's a -- it's the big problem as far as I'm concerned, and I looked at that, this whole issue, kind of through that lens.

Q Thank you. Dr. Riddell?

DR. RIDDELL: Well, I mean, I hate to come back to science as a past scientist, 'cause that seems to be what we always recommend, but I just don't think that we can afford not to address improved monitoring and continuing the science work because just simply not putting the money in is transferring those costs to other people, whether it's First Nations in-river that are not allowed to fish or whether it's the commercial fisheries outside that have been substantially curtailed. Other people are bearing some very substantial costs by us not doing sufficient monitoring and science.

I would say that while we don't have a direct answer for the Late Run sockeye, we've made some significant inroads in understanding the

physiology of the animal and what's causing some of the mortality. That's not to say that we necessarily are going to be able to fix this thing. What I'm concerned about really is looking at the whole picture and a full context of in-season management. We've had problems with sustaining test fisheries. We're getting less information there. The runs are more variable, so if they're variable, you need more information from test fisheries, not less. Right? So the whole information system is going in the wrong direction.

We need to do continued work on the mortality in-river. It's got to be extraordinarily difficult for managers to sit in pressure-packed discussions in the peak of the season and you have some estimate that might be that you're going to lose 50 to 80 percent of the fish moving up the river. Right? And we've seen years where that's been true and we've seen years where that simply hasn't even happened. Right? So it's an extraordinarily difficult question to address in-season. We probably can improve some of our forecasting for short timeframes like that, but you're always going to have errors.

And so that really, after spending -- or really looking at what's a good investment of funds and how much, I think the next part has really got to be getting people involved and a better sort of planning process. The exercises that I was involved with before leaving are yes, you have multiple stakeholders and you're going to have discussions about expected returns, but they become so protracted that you really don't have discussions about what's an appropriate response when things don't happen the way you plan them to? Because that's what really happens.

We make lots and lots of plans about what will happen as the fish come in and then they don't do it the same way. And so you're immediately in a very uncertain environment. You've got to make really hasty corrections. And to be perfectly honest, my opinion is that in-season, they do a phenomenal job. I mean, you really only have to look at 2009/2010. Two totally different responses and I don't think anybody would say that they were incorrectly

assessed. Yes, you probably could have fished harder in the return in 2010, but again, you were looking at in-season mortality projections.

So, I mean, we talk about W.E. Ricker before. One of the last papers he wrote, his advice was that fisheries managers should expect to be surprised. Expect the unexpected, basically. And so it's a tough environment and I think we are passing costs on to other people if we're not going to do the fundamental work to really examine things like Carl's now finding evidence out for delayed density dependence. Well, we don't know the mechanism. But I would say that the scientists involved now with the tools that they have are narrowing down what those mechanisms might be and maybe we can do a better job in a fairly short period.

But the serious concern I hear is constantly less funds, less funds, less funds. So you're more dependent on external funds and that's highly insecure, as well.

Q Thank you. Dr. Walters?

DR. WALTERS: I just want to reinforce what Brian's saying, that we are entering a period of the next five years it is literally anyone's guess what will happen. We could be entering a Coho world where the declining trends in marine survival and survival rates in all life stages turn out to be not associated with delayed density dependence, but something more like has happened with Coho and we may end up with our fisheries shut down, as we have with Coho for the last more than a decade. Or we could end up seeing a dramatic rebound. We could see strong release and a lot of strong delay density effects because of low stock sizes in the last few years. Things could turn really good.

The critical thing is not to have a management system that is -- has become incapable of responding to those alternatives and not to have a management system where the decision-making is dominated by any particular narrow concern, like saving this stock or saving that stock, as it has in recent years.

And I think one more thing, a point I would make, is that if the commission is going to have anything to say about where the critical research needs are on sockeye salmon, I hope we've

convinced you that probably the most critical needs and the biggest investment needs to be in the fresh water, not out in the ocean. Where the ocean is -- these variations in ocean mortality seem to be symptomatic of problems that arise in fresh water.

DR. RIDDELL: Can I disagree?

Q I guess.

DR. RIDDELL: Partly. Really quickly. I don't disagree with the fresh water and ocean, but I really caution -- we keep taking this jump from the river to the open ocean and there's a little body of water called the Strait of Georgia that is very workable and I think has much more to contain in terms of understanding what's going on. So I really caution against jumping from river to ocean. I think river, yes, let's progress out from near shore to offshore and a lot of this story, I think, is going to be in the lower river and estuary Strait of Georgia.

Q Thank you.

DR. RIDDELL: Carl is smiling, so he probably disagrees again.

Q Maybe he's agreeing.

DR. WALTERS: This is totally self-serving, what you just heard.

DR. RIDDELL: They already know that.

MS. BAKER: Mr. Commissioner, those are my questions for this panel, so if we're taking an afternoon break, this would probably be a good time to do it, and then we'll start with Mr. Leadem.

THE COMMISSIONER: Okay.

THE REGISTRAR: Hearing will now recess for ten minutes.

(PROCEEDINGS ADJOURNED FOR AFTERNOON RECESS)
(PROCEEDINGS RECONVENED)

THE REGISTRAR: Order. The hearing is now resumed.

MR. LEADEM: For the record, Mr. Commissioner, Leadem, initial T., appearing as counsel for the Conservation Coalition.

CROSS-EXAMINATION BY MR. LEADEM:

Q Dr. Walters, for your benefit, you probably can't see, but there's a whole room full of lawyers who

are in basically a run upstream to ask you a line of questions, and I guess I'm considered to be part of the Early Stuart run, because I'm the first one up.

And in my questions to you gentlemen, I'm a little bit intimidated taking this podium, because I'm in somewhat awe of the collective wisdom in this room and across the Continental Divide, and so I intend to ask these questions of you, looking for solutions rather than trying to raise some issues and problems that may already exist between you.

And I want to begin by starting with a concept of cyclic dominance, not that I understand it, but it appears to be, by the evidence I've heard so far, that cyclic dominance is basically here to stay. And so the first question is: Could or should fisheries managers try to iron out the peaks and valleys so that we can arrive at more of a level aspect of return year after year? And maybe I'll start with you, Dr. Riddell; should we be in the business of trying to iron it all out?

DR. RIDDELL: You started with me because I looked most confused? No, I think the answer to that is, "No." That's what we started with. That was one of the primary hypotheses in the sockeye restorations in the late '80s and development of testing, whether we could substantially increase the escapements and improve production.

But one of the primary hypotheses behind that is that there -- there was actually a paper, written by David Welch and Don Noakes, looking at the economic return from what you could accomplish if you recovered the off cycles. And, of course, it's a massive number. And Jim Woodey was one of the primary people that probably identified that this is unlikely to work.

So I think the answer, as we tried to clarify this morning, is I don't think you're going to accomplish that. You can probably manage to maximize production, but you should let the fish choose how they're going to actually restore production. And if Carl is right and they're going back to a really, really strong year and three off years - I'm not sure I agree that it's going to be that cut and dry - but we may very

well have a dominant year, a subdominant year, and then you've got a smaller cycle and then one really small cycle.

But the consequence of that to the fisheries, really, is that you've got to look at that across all 19 Fraser production units, and that's from 38 conservation units. And so let the fish choose. Choose the right sort of management process. And some of that, now, is the FRSSI and the harvest rate approach, and they'll tell us where they're going to go by system.

Q Does anyone else want to weigh in on that question? Dr. Woodey?

DR. WOODEY: Yes, my response to evening out the cyclic dominant stocks is that the theory behind them is that the cyclic dominance large run smaller off/off type thing has the effect of controlling the predation mortality rates through trout and other species, preying on the juveniles in the lakes; that is, cyclic dominance is a lake-driven phenomenon, it's not an ocean issue.

And if that is, in fact, correct and I don't think we have data to the extent needed to make that conclusion, we should not attempt to even out the runs or even out the lines on the cyclic dominant stocks. If after all those data are collected, there may be some room on some stocks to try that.

But if the theory is right, you even out the lines, more food available each year for the predators, the predator population will increase and mortality rates will increase over all lines. So that's, I think, a bad, from what we know right now, a bad theory. And I don't know, someone who may have read the Alaskan paper - I haven't had the chance - can answer how the Alaskan scientists look at that issue.

Q Not having read the Alaskan paper, myself, I don't know if any of you want to comment on the Alaskan situation and how they try to deal with cyclic dominance. Dr. Walters?

DR. WALTERS: Around the Pacific Rim there have been four major cases where cyclic dominant stocks have, either deliberately or inadvertently, had their cyclic dominance break down. The first of those was the Kvichak stock in Alaska, in Bristol Bay. It was the world's largest salmon stock.

And as a more or less deliberate experiment, they broke up its very violent cycle. It is now listed as a stock of concern in Alaska, meaning that its productivity has dropped dramatically, abundance has been relatively low, and it's just starting to recover.

The second case was Rivers Inlet, here in B.C., where we deliberately shut down the fishery and tried to rebuild the stock to test for having the benefits of higher spawning runs. Shortly after that experiment started, Rivers collapsed and it still hasn't recovered.

Then we have the Late Stuart and Quesnel stocks on the Fraser, strongly cyclic dominant, but the cycle has broken down over the last 15 years, and in both cases the mean productivities of those stocks have dropped dramatically.

So I think the evidence is pretty clear that it isn't a good idea, that there's something in the biology of these cyclic stocks that makes them, as Jim says, predators or whatever, that makes them very unproductive when they're not in a cyclic mode.

Q Moving onto another line of questions that I have, we've heard some evidence over the last week or so with respect to modelling, a lot of computer modelling, and it leads me to ask this question of this panel: Should we be placing our faith in models to lead us out of some troubled waters? Should we trust models? Is that going to provide us with the answers or the solutions to some of these issues?

DR. RIDDELL: Well, I'll start --

DR. WALTERS: I'd be happy to weigh in on that one, if you'd like.

DR. RIDDELL: Do you want the last word, as usual?

DR. WALTERS: I'll just say something real simple here, is: This is not avoidable.

DR. RIDDELL: Yes.

DR. WALTERS: You know, we have to make quantitative policy decisions, we have to set quantitative goals, we have to set policies out there that have quantitative impacts. Any method that we use to do that involves some kind of model, logically, necessarily. The issue is not whether to use the model, it's which models to use. And while we certainly do not trust those computer models, I

trust them a lot more than I trust somebody sticking their hand up in the air and pretending to be able to intuit the answer to these questions, with a middle model that has goodness knows what crazy hidden assumptions.

Q Dr. Riddell?

DR. RIDDELL: Well, my answer would be very similar. I mean, I think it's unavoidable, but I don't think that you castigate all models; it's how people use models. Models are described as a representation. We should use models as a way of representing what we understand, seeing whether the data supports that, and that, to me, is a critical step in this to see whether or not we have the right data, do we have -- are we missing key pieces of information, and use the models to make predictions of our understanding and then see whether or not this -- are we really gaining, are we understanding what's going on.

So I don't think that we pretend to understand all predictions and models. Models are getting increasingly complicated because of our computing power, ecosystem-based models are very complicated, but people have made huge strides in how you actually model that type of an environment. So yes, I think we'll continue to use models. It's one of the few ways that can really test our understanding of complex systems, and we need to be cautious about how we use them, not -- we certainly aren't going to be able to avoid them.

Q I'm wondering how fisheries were managed before computer models, and I'm thinking in terms of the way that you approached the seine fisheries back during the '70s and '80s, Dr. Woodey? I mean, obviously you did not have the benefit of these complicated and complex computer models, yet somehow fishery managers made some choices, and what was the prevailing guidance that you fell back upon in those days?

DR. WOODEY: The Pacific Salmon Commission had a fairly narrow mandate that was the commercial fisheries and the -- I should say, excuse me, the IPFSC, and when I came aboard we were trying to monitor the stocks in ways that were quantitative, but there wasn't much real good quantitative evaluation. I spent a fair amount of time at the SFU computing

centre in the early '70s, looking at production dynamics, stock separation, methodology, discriminate function analysis, which is just another -- just a computer-driven way of analyzing stock composition in the fishery samples of what proportion of the fish were of each stock and so on, and then we developed hydroacoustics estimations of upstream migration.

So we went from relatively little technological in put to a fairly intense technological process, and that then allowed us to evaluate both -- get information on abundances, the fish were coming through Juan de Fuca and Johnstone Straits, the migration upstream, and putting that all together with harvests and so on, and we could monitor the run. And decision-making in that period became a day-to-day thing, particularly after the PSC was formed, and for a lot of years, then, it's that kind of hands-on management, using computers and models, but ensuring that there was some intelligence with them.

And just to go back to the original question, the models were not evil or bad in themselves, but you want to be sure that you have the right team to work with models. You need the really hotshot modellers, but you need the biological input to rationalize what's going in and coming out of those models. So it's teamwork that really goes a long way in these modelling issues.

Q I saw that you had your hand up, Dr. Walters. Did you want to weigh in on this again?

DR. WALTERS: Yes. We've done retrospective looks at the big complex management systems up and down the coast, from Bristol Bay and Skeena and Fraser and so on. If you just look at the harvest rates achieved over time in those management systems, what you realize after a minute is that basically what they've done is to stabilize the harvest rates, even though they claim to be doing all sorts of more complicated things and they do respond, occasionally, to extreme events like we've had since the mid '90s.

But most of the time, the way they've managed, in the old days and in the new days, is to do what you did last year, that results in a fairly stable exploit. You have fisheries in

roughly the same times and places and you'll end up with fairly stable exploitation rates.

So that, actually, is a simple way to get around a lot of the complex modelling, is to pretend to be doing all sorts of escapement goals and so on, but then to keep relevant this stable and simple exploitation regime that's in place that have proven to be sustainable.

Q I want to move onto an area of how we conduct our fishery, and I want to see if we can tackle the issue of foregone fishing opportunities, which is another way of saying, I gather, over-escapement. And the way that we have aligned the fishery in four run timing groups with aggregated stocks with some weak conservation units being mixed up with some very robust runs of fish, strikes me as being very problematic, and I think some of you have certainly alluded to this already in giving your evidence.

And so I'm wondering if we can address the issue by trying to segregate out the conservation units from the robust units. I'm not necessarily thinking that terminal fisheries is the panacea, but I'm wondering if there's some approach that your good minds have turned your attention to that you can give some advice to the Commissioner on this topic, because it seems to be a real niggling point that the commercial fishing is sometimes foregone because of the concern for conservation and some of these weaker stocks, be it Cultus or some other stock, and I'm wondering if you have some solutions in mind that can address some of these weighty problems.

DR. RIDDELL: I don't think so. I mean, that's the question on people's mind for years. We joke about it, but, I mean, the IPFSC, again, was kind of leading on the coast. The scale pattern analysis was Ken Henry, right, early 1950s. So recognizing immediately that whether you call them threatened or endangered now, there were always some populations that needed some other level of conservation. We've put a lot of effort, in the science branch, by development of DNA techniques so that we can very, very rapidly get the best possible information.

I'd say the limitation, now, on the quality of the stock ID is, again, the test fisheries. In

the past, we probably forget that with fisheries functioning at a level to sustain as 75-77 percent harvest rate, there was lots of fish to sample, all right? Now, where you have a couple of test fisheries, it's quite possible that you're sampling a small portion of the run. Well, you are.

And so I don't think you can avoid the issue. You certainly can't break it down in any finer time scales. We're certainly finding that, in more recent years, as people are more and more concerned about climate change and effects on ocean waters, the returns are becoming more variable, not less variable, and so there's greater overlap, probably, between the stocks.

So I think the stock mixture by the timing groups is about as good as you're going to get. The mixture within the time groups is reality, I'm afraid. And then it's a matter of where you actually harvest and how big an effect you have.

We talk about changing where some of the fisheries occur to reduce impacts. I mean, when you really look at that, there's not even a great deal of opportunity to significantly change that opportunity. We've got to get up to where the major tributaries separate. So I'm not sure there's an easy way out of that one. I think we've done a very good job in getting down to rapid turnaround of very good information. I'm actually more concerned about the repeatability of the sampling and the DNA, the stock ID, than I am about the tools that we have available.

Q It just struck me that basically the run timing groups and the way that the fishery is managed, you've got to admit are all human constructs, and that what we're endeavouring to do is to really tackle a biological issue that is admittedly very complex. I mean, we've got lifecycle issues, we've got a species that, for much of its lifecycle, it's a very black box in terms of where it goes and what it does, and I'm wondering whether or not, because we, as humans, decide that we're going to harvest these species, whether we can do it in a way that is less -- poses less of a problem to the fish, poses less of a problem, and works through some of these issues where we get to basically have our fish and eat it, too, that we

can conserve them and still have them for harvesting purposes. And if you people can't come up with solutions, I don't know how we, as lawyers, are expected to come up with solutions, so --

DR. RIDDELL: We don't expect that.

Q Well, maybe my problem is that I'm aiming too high, because I think we're all here trying to find solutions. And we obviously have a problem that you have defined, and I've tried to define for you, in some sense, and that is that we have various sectors that have a very live and robust interest in fish, and we have a commercial interest in fish and we have a First Nations spiritual interest in fish, and we have a conservation interest in fish, and I'm wondering if the model we've chosen - and I'm not talking, now, Dr. Walters, about computer modelling, I'm just talking about the process - I'm wondering about the process that we've chosen, it is actually the right process that allows everyone to sort of have what they see as being a value in the salmon at the same time.

DR. RIDDELL: Well, I feel I should let somebody else go ahead.

Q I don't know whether I'm having any takers, and usually Dr. Walters has his hand up by now, and I don't see it.

DR. RIDDELL: Well, I'll maybe just add to the -- I guess the premise to my comment would be that you're still fishing where we've always fished and we have made changes to that, as a matter of fact, because the effort is down, as we all know. But, I mean, you can limit the harvest rate outside, and if you're prepared to move the fisheries, then you can reduce the impact on certain conservation units. But if you're going to be fishing in roughly the same pattern, with just modest moves in-river, I'm afraid the conflict that you're identifying is part of reality.

Q Dr. Woodey, you wanted to re-jig some of the runs, as I understand it, some time ago. Maybe that's the wrong expression, but you wanted to move, for example, Scotch/Seymour from Early Summer to Summer runs, and you've made some recommendations about reconfiguring some of the runs, some of the run timing groups, and some of the actual aspects

of the 19 stocks that have been identified. Do you still think that that's something that ought to be examined and looked at?

DR. WOODEY: Mr. Commissioner, this goes back to about 1987, or something of this nature, in response to a question from the Fraser River Panel regarding the stock units that we used, the Early Stuart, the Early Summers, the Mid Summers and Late runs, we were asked to evaluate the best sets of stocks to include in each of those components, because we were managing the Summer run fish, whether that included Chilko, Quesnel, Late Stuart, Stellako, et cetera, and then we were separating that from the management of the Early Summer stocks and so on, and so I made a recommendation, which I thought, at the time, would simply give a better definition of these stock groups, based on their average timing.

That was not adopted by the panel, and I can't give you a clearer answer as to why they did not feel it was necessary or desirable.

DR. RIDDELL: If I could just comment on that. I mean, to say you could move a couple of the stocks around, I don't see how it, fundamentally, really addresses the major restraints in the ocean fisheries. I think you're still faced with the same sort of problem of recognizing the stocks, assessing how hard you can fish, what the net effect is. A more recent discussion of the same topic, wasn't it the Harrison Lates, in terms of whether the -- was the Harrison Fall really a Summer stock or a Late run? And that actually did have some consequence, but really wasn't going to solve the major stock problems in the Upper River.

Q I see Dr. Walters wants to comment.

DR. WALTERS: Yeah, I mean, one of the most fundamental ideas of fisheries management is you can't eat your cake and have it. You can't have a very large spawning stock size and have a high harvest at the same time. Your harvesting is going to reduce the abundance of the spawning fish, and so you're always dealing with the trade-off and the balance there.

I have to agree with something Ken Wilson said earlier, is that if we must maintain all stocks for legal or other reasons, then we have no real choice but to move to spatial management, to

move the fisheries substantially.

But let me point out one thing, is that people talk as though there were lots and lots of Cultus stocks and lots and lots of Raft River stocks and so on. We're not really talking about a large number of populations that are at significant risk; we're talking about a small number of them. And at least a couple of those are in the early timing parts of the runs where we already are hoarding them and can't afford them more protection without substantial impact on the fisheries. The number of stocks at risk in the summer and fall timing components of the Fraser run is actually quite small. Cultus is the main one.

So I think a bit of careful thinking and triage can go a long way towards reducing the apparent complexity and severity of the problem.

Q Thank you for that, Dr. Walters. I want to move onto another question, and Dr. Riddell, you've been here a couple of times and in part you've answered this question, but I don't mind hearing from you again.

The 2010 returns, I think that a lot of us who watched the fishery were just amazed, after so many years of decline, that we saw such an abundant return last summer, and it certainly defied all the forecasting and all the modelling that went into it and into predicting what was going to happen, and I'm wondering if we have any scientific rationale for why that occurred, or any rationale from any population dynamics study that would allow us to address that issue as an example of something that went right as opposed to things that often go wrong.

Do we know, as scientists, and all of you are in the position of being scientists that advised decision-makers and advised various aspects of stakeholders, do we know what went right in 2010, why there was such a great return? Do we have any reasonable hypotheses that are developing out there?

DR. RIDDELL: As you've said, you've heard from me, so maybe I should let others go and then I'll give my speech.

Q All right. Dr. Woodey, you've been around salmon most of your life. You've dedicated your life to

salmon. Do you have any explanation for why they came back in such record numbers last year?

DR. WOODEY: Not really. But we have seen situations that individual stocks and all stocks across the board have had unusually high survival rates in the ocean in the past, so say, for example, Chilko has a mean survival rate of about nine percent; that is, nine percent of the smolts leaving Chilko Lake, on average, return as adults to the fishery, et cetera. But there has been situations where it's been up in the 25 percent. And same with Quesnel. So it's not highly unusual.

I related an interesting comment to Ms. Baker at one of her meetings, and if you'll pardon me, I'll explain what went on in this. On July 7th, I took my cat to the vet. Now, well, the vet has a cabin on Broughton Island, and his neighbour is Billy Proctor. Billy Proctor is a fisherman, he's like 80 years old, he's fished all of his life, he's lived there all his life, and he told my vet that -- well, my vet said 40 million; Billy Proctor told me 24 million, because he saw so many juveniles there in that area that year, that he knew there would be a big run.

So there may be things going on in the ocean, but you don't have a large return if you don't have a good number of juveniles making it to the top end of Johnstone Strait. So that's an interesting little sidelight, one of which then said to me, "Maybe we should put him on staff," you know.

Q Maybe we should invite him as a witness to the Commission. Mr. Wilson, do you have any views on this?

MR. WILSON: Not really. I have no clue why we saw such a wonderful return in 2010.

Q And Dr. Walters?

DR. WALTERS: Fisheries scientists have been singularly unsuccessful when these big returns -- big recruitments occur at explaining them and any kind of fish talk, whether it's cods or salmon, or anything else, but a key point Jim Woodey made is that this really was not that unusual an event. It's wrong to call it a record, in any sense.

We say that the Adams run came back with about a plus one point three recruitment only; it's survival rate was about one point three

standard deviations above average. Something like that happens in the order of once every 20 to 25 years. What's unusual is when that positive survival anomaly happens in conjunction with a dominant return, so you have just the right million or so female spawners producing it.

This very nearly the same size run last happened on the Adams in 1958. It was just a couple million fish smaller than this one. But you shouldn't think of it as being a really extraordinary outcome at all. It's just things were added up right; the right number of fish with a reasonably good survival.

MR. LEADEM: Perhaps, on that note, perhaps, Mr. Commissioner, noting the time, we can end on that note. I'm virtually finished with my questioning. I would like to review my notes overnight and then we can come back tomorrow. If I have no more questions, I'll turn it over to Mr. Taylor.

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

THE REGISTRAR: The hearing is now adjourned for the day and will resume at ten o'clock tomorrow morning.

(PROCEEDINGS ADJOURNED AT 4:03 P.M. TO THURSDAY, FEBRUARY 10, 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.

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.

Susan Osborne

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

Karen Hefferland