

February 9, 2011
Cohen Hearings
Testimony of
UBC Professor Carl Walters and Dr. Jim Woodey

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 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 1 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 compensatory, what is called compensatory 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 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 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 1 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 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, 1 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 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 1 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 1 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 re-colonization in some of the areas from which stocks had been lost, but what seems to have actually happened was that re-colonization 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 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 re-colonization 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 may 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 appreciate 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 accomplish 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 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.