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From: **John Carlile M.S., David Bernard, Ph.D, John Clark, Ph.D**

Date: **June 13, 2012**

Re: **Comments and Suggestions to the Expert Panel on the Draft Final Report Entitled "THE EFFECTS OF SALMON FISHERIES ON SOUTHERN RESIDENT KILLER WHALES"**

The comments and critique of the draft final report provided in this communication come from the Alaska fishery scientists who participated in the workshop. Our goal is to suggest ways in which we believe the logic and findings reported in the draft report can be strengthened to make a good report better. For that reason we have kept our praise (and there is plenty that is praiseworthy in the draft) to a minimum and concentrated comment on areas we believe to be weaknesses in the draft.

Our critique roughly follows the organization of the draft report. We say roughly because there are many instances where the same issues are covered in different sections of the draft final report, or where statements in one section are supported by statements in another section. We covered the executive summary and sections 2 through 6. We skipped comment on section 1 because procedural matters are covered in that section of the draft. Our critique is embedded with quoted text from sections of the draft report in italics and with line numbers, or with page and paragraph numbers for text from the executive summary. We begin our critique with general comments.

Our General Comments

Our first comment is to commend the expert panel on a job well done. The draft final report provides some important insights, some of which bear directly on the key questions asked of the expert panel, while others place the issues outlined in the workshop in their proper context. Insights of the former kind are

- predicted changes in Chinook salmon abundance caused by fisheries are unlikely to increase the risk of extinction of the SRKW population (from lines 1328–1336);

- while the average of the observed population growth rate of SRKWs is 1% annually, there is a chance that the core rate is actually < 0% AND at the same time a chance that the core rate is actually above the delisting threshold of 2.3% (from lines 450-451);

Insights that provide perspective on the issue of recovery of SRKWs as a function of fishing for Chinook salmon are

- abundance of SRKWs has not been declining, but has been increasing in the previous four decades (from lines 417–420, 429–430);
- the population growth rate is a weak metric of SRKW population recovery and should be reassessed as a delisting criterion (from lines 1253–1260);
- an actual recovery of the SRKW population without any future reduction in catches of Chinook salmon is inevitable (from lines 184–187);

We find the implications of the last two bulleted insights especially telling. Our interpretation of the draft is that the panel believes the growth-rate criterion used for delisting is biologically inappropriate, and that an abundance-based criterion should be used instead (see lines 1273–1278). We agree. The consequence of such a substitution is that no reduction in catches of Chinook salmon would be needed to reach a biological recovery. Consequences of keeping the growth-rate criterion is the significant chance (an estimated 40% or more chance) of never delisting SRKWs against an arbitrary standard regardless of how large their population becomes, or regardless how great the reductions in catches of Chinook salmon. This issue bears directly on the purpose of the workshop.

Our issue here is not with the insights themselves, but with how they were presented in the draft final report. None of these insights were presented in the executive summary; we believe they should be. Taken in their entirety, these insights represent excellent work on central issues, and as such, should be prominently reported.

The Executive Summary

The executive summary in the draft report is too short. Perhaps the panel decided to use section 2 to summarize their findings, however, a single page of text is not long enough for an executive summary. A summary of findings is needed, but it's the executive summary of a report that's read first, quoted most often, has wider circulation, and is most often reported. For these reasons the executive summary needs to highlight conclusions.

Take for instance the inclusion of the panel's conclusions concerning the population growth rate as a delisting criterion as noted above. In the text beginning the second paragraph of the executive summary on page iii:

> "*During 2011-2012, NOAA Fisheries and DFO commissioned a series of three scientific workshops to rigorously explore the evidence available to determine*
>
> - *to what extent are salmon fisheries affecting recovery of SRKW by reducing the abundance of their available prey; and*

- *what are the consequences to their survival and recovery?*"

(the bulleting is ours). In regards to the first bullet, the panel has evaluated the evidence and answered the question asked:

> "*The panel believes that the existing delisting criterion of 2.3% growth rate is unlikely to be achieved given current circumstances or by reducing Chinook fisheries, but if current trends continue SRKW will eventually increase to a point where a reappraisal of their status would lead to downlisting or delisting.*" lines 184–187

The panel based this judgment of biological recovery on population size—not on the flawed technical criteria given in the biological opinion; they wrote

> "*The weakness of the growth rate metric of SRKW population recovery is evident …* " line 1253

> " *… the choice of 2.3% growth rate as a downlisting criterion should be re-assessed*." line 1259–1260

As per the "*consequences to their survival*" in the second bullet, the panel wrote

> "***Key Question 5.*** *Based on your expert opinion, what level of confidence would you assign to the conclusion that predicted changes in Chinook salmon abundance caused by fisheries increases the risk of extinction of the SRKW population?*
>
> *Confidence - VERY LOW*." lines 1328–1331

If questions asked are put in the executive summary, the panel's answers should be in the executive summary as well. We believe that all the key questions asked the expert panel along with their answers should be summarized in the executive summary. We also suggest that all of the bulleted insights listed above be addressed in the executive summary.

* * *

As short as it is, we suggest that some points already in the executive summary be reconsidered. In the third paragraph on page iii the text reads:

> "*The underlying hypothesis presented by the two agencies is that SRKW are highly dependent on Chinook salmon, that the rate of increase of SRKW is higher in years of high abundance of Chinook, and that reductions in Chinook harvest would lead to an increase in SRKW abundance.*"

Later in the same paragraph:

> *"The panel found that the evidence for high dependence on Chinook salmon during the summer is convincing."*

The use of "*dependent*" in both sentences implies the panel partially accepts the agency hypothesis, at least the first part, concerning the summer diets of SRKWs. We hope this is not

the case because no evidence was presented at the workshops that SRKWs are obligate predators. That SRKWs eat Chinook salmon there can be little doubt, and that Chinook salmon have been an important part of SRKW nutrition is likely. But as pointed out in lines 1304–1306 of the draft report, this evidence is based on observation, not experimentation. Given evidence of the flexibility in the diets of killer whales in general, and the diets of SRKWs specifically, a switch to other prey during times when Chinook salmon are sparse is a reasonable expectation. Also, we could find no reference in the draft report as to evidence for "*dependence*" of Chinook salmon in the winter diet of SRKWs. The draft report is replete with text on the lack of such winter evidence; the panel even went so far as to recommend a satellite monitoring program (lines 749–750) to get this missing information. The issue of winter diets should be addressed in the executive summary, especially the part about not knowing what that diet might be.

Text in the executive summary as to the panel's judgment on the second part of the underlying hypotheses, that is "*reductions in Chinook harvest would lead to an increase in SRKW abundance.*" consists of the following in the last sentence on page iii:

> "*Overall the panel felt that the estimated impact of Chinook catch reductions on SRKW should be regarded as a maximum estimate and that the realized impact would likely be less.*"

This feeling, judgment, or conclusion seems oddly generous considering that just before the quote in the text the panel discounted the importance of estimated reductions presented at the workshops. The discounts were due to the following: current low harvest rates in fisheries, only a subset of stocks exploited in those fisheries being available to SRKWs, maturation schedules of Chinook salmon, and competition from other marine mammals. The last three of these factors guarantee that the "*estimated impact of Chinook catch reductions on SRKW*" will be larger than the actual impact. This is the position the panel takes in section 5 lines 1084–1092. We would also add that if no cause-and-effect relationship exists between the numbers of the two species, catch reductions in fisheries would have zero impact on whale numbers. We suggest that the panel consider giving the more realistic conclusion in the executive summary that the actual impacts of catch reductions on SRKW (numbers) <u>will be</u> significantly less than the estimated impacts presented at the workshop. We bring this topic up again later in more detail.

* * *

The statistical "correlations" that are at the core of arguments for the importance of Chinook salmon to SRKWs are mentioned in the executive summary in the third paragraph on page iii:

> "*There is also a statistical correlation between periods of lower Chinook abundance and higher SRKW mortality rates.*"

but mentioned without qualification. Logistic regression was used in the workshop to detect a correlation between abundance of salmon and the population growth rate of whales. Considerable time and effort were expended presenting the subsequent analyses. However, attempts to reverse the logistic regression to project SRKW growth rate as a function of Chinook salmon abundance in PVA modeling failed to capture the observed variation (process error) in the latter statistic. Hind-casting with the PVA modeling of birth rates with parameters derived from logistic regression was provided in section 6 as an example:

> " *… this figure shows that process error in births is under-estimated because the (+/-) 2 std error intervals do not include about half of actual births and some fall seriously outside the range.*" lines 1217–1218

The implication of such underestimation of process error is that other factors are just, or more important than salmon numbers in potentially determining vital rates of SRKWs. The panel expressed such reservation when it wrote

> "*… the Panel has some concerns that the uncertainty regarding future abundance has been under-represented in the PVA models, in particular involving processes of calving probability, stage-specific survival, and sex ratio at birth."* lines 1204–1206

The essence of these concerns should be included in the executive summary.

* * *

Lastly, we believe that the matter of which Chinook salmon stocks are exploited by SRKWs as handled in the executive summary needs to better reflect the conclusions given later in the draft. In the middle of the third paragraph on page iii:

> "*There are also concerns about whether the index of Chinook abundance accurately reflects the Chinook stocks most important to SRKW.*"

Text in section 5 (lines 914–917, 1055–1062) contains conclusions by the panel that not all Chinook salmon stocks perceived as geographically coincident with SRKWs are available to the whales. Also, the incomplete evidence we have on the diet of SRKWs indicate that of those stocks available to SRKWS in the summer, the whales essentially prey upon Fraser stocks to the near exclusion of others (lines 890-904 in section 5.2). We believe these conclusions should be presented in the executive summary to show the appropriate boundaries of the scientific debate.

The Section 2: Summary

The draft has an important inconsistency, error, and subsequent omission in reporting fluctuations in abundance of SRKWs over the past half century. References in the text about abundance are located in this section and in section 3.1:

> *"Demographic reconstruction showed that the largest known size was likely 96 animals in 1967, leading to the conclusion that the population size has not varied dramatically over the last 45 years"* (lines 191–192).

> "*The abundance of SRKW fluctuated between 60 and 100 individuals during 1975 to 2010.*" line 417

From materials presented at the workshop, the actual range from 1974 through 2010 is 72 to 96 with 72 whales observed in 1974 (presentation by J. Ford, workshop 1). Even with correcting the statistical error in the draft, a 25 percent decline in six years seems a rather dramatic variation to

us. We know the reason for this decline — removals and incidental deaths from capture for the aquaria trade. Given:

> "*Concerns about its* (the SRKW population) *future arise entirely from the current and recent size of the population and the potential impacts of future, unforeseen events on a population that lacks the resilience created by higher abundance.*" lines 430-432

we would expect a discussion of the resilience of this small population given its history, a history that included "pulse fishing" the population. No such discussion occurs in the draft (or a reference to the perturbation). There is a need for this discussion (most likely in Section 3) because the response of this small population to the perturbation bears on its resilience. Perhaps a population of multi-ton individuals is more resilient than expected from their small abundance alone.

* * *

A judgment as to the dependence of Chinook salmon to the summer diet of SRKWs was given in the executive summary:

> *"The panel found that the evidence for high dependence on Chinook salmon during the summer is convincing."*

This judgment is repeated in section 2.2 along with comment on the importance of Chinook salmon to the winter diet of SRKWs:

> "*Diet information from SRKW in the summer indicates a heavy reliance on Chinook salmon. As Chinook abundance declines in the fall the diet data show that chum salmon and other species become more important. There is little winter diet data, but the data that do exist also suggest the importance of Chinook.*" lines 197–200

That "*little winter diet data*" consists of the observation of two Chinook salmon eaten off the coast of Washington by SRKWs. We suggest that the draft reflect that there is for all reasonable purposes no winter information on diet at all, and "*the importance of Chinook salmon*" to the winter diet of SRKWs is essentially speculation.

* * *

In section 2.2 under the subheading "*Reducing harvest would increase availability*" (line 260), the draft report reads:

> "*Recent analyses presented at the workshops explored whether reductions in Chinook harvest would increase food for SRKW and thus SRKW population rates of increase. These analyses have made the simple assumption that a certain number of Chinook foregone from the harvest will result in an equivalent increase in abundance of Chinook for SRKW.*
>
> *There are several reasons this assumption may not be true.*" lines 261–265

The panel erred with the verb "*may*" in the last sentence. Based on different availabilities of stocks to fisheries and to SRKWs, and to exploitation and predation on salmon at different stages of maturity, the one-to-one transfer of reduced catch to increased consumption by whales is demonstrably unrealistic. The verb "is" should be substituted for "*may*" in line 265 and the adverb "*likely*" dropped from line 294. Also, we suggest that lines 311–313 be rewritten to be something like "*The sum of these concerns is that we believe the* realized *benefits of reducing Chinook harvest* would be less than estimated *in NOAA's recent analyses*."

* * *

We agree that with a correlation between dynamic rates of species A and species B, evidence linking the two species is needed to support causation (as paraphrased from section 2.3). However, we don't accept the panel's argument that some support exists in this case. The text reads:

> "*The mechanistic data developed so far certainly provide some support for causation, if only because if there was no evidence for poor condition (possibly due to nutritional stress), or if Chinook were not an important part of SRKW diet, then the support for causation would be weakened.*" lines 335–338

We submit that if there is no evidence of poor condition at all in a biological population, the evidence should be rejected, not the hypothesis. Individuals in <u>all</u> biological populations will display a range of body conditions that on the extreme may be due to stress from any number of causes. This generality makes the presence of individuals with poorer condition expected, and subsequently not evidence for a specific cause at all. In this case involving SRKWs, poor condition must be linked to a restricted diet. We note that for one of the 13 whales with the "peanut-head" syndrome, we know the condition was not caused by nutritional stress. The whale was struck by a boat and demonstrably wounded. And as for the other argument, the one based on the importance of Chinook salmon to whale diets, that importance is confirmed only when a likely mechanism has been determined, not the other way round. The fact that whales eat Chinook salmon is the observation that sets up the hypothesis; it's not the simultaneous confirmation of that hypothesis as well.

We suggest that lines 335–338 beginning with the sentence "*The mechanistic data …* " be dropped along with the sentence beginning in line 341. We also suggest that the text:

> " *... that some individuals are in poor condition and those individuals have high mortality rates is a strong piece of evidence for the causative hypothesis, but currently available from a very small sample size, and poor condition has not been definitively related to nutritional stress* per se." lines 357–360

be reworked. The second part of this compound sentence appears to contradict the judgment that there is strong evidence for the causative hypothesis.

* * *

Obvious by omission in this section is reference to any evidence that would support <u>rejection</u> of causation. One such piece of evidence that comes to mind is the proclivity of SRKWs to share captured Chinook salmon with members of their pod (lines 617–620). The expectation of sharing

over 3 of every 4 salmon caught (presentation by J. Ford in workshop 1) is that a dearth of salmon would produce a general decline in condition across the entire population if the nutritional-stress hypothesis is true, not the observation of a few distressed individuals (the 13 with the peanut-head syndrome). This sharing behavior does much more than "*complicates understanding the effects of prey limitation*" (lines 617–618); it makes causation less likely. At least the scientific consequences of sharing should be discussed in the draft as part of the mechanistic approach.

* * *

Another piece of evidence supporting the rejection of causation can be found in section 3.3. Analysis of the effects of population density on population growth rate produced for this workshop provided results contrary to theory and to observation. The relevant text is

> "*All of these difficulties of interpretation cast doubt on a simple, causal interpretation of the positive correlation between salmon abundance and killer whale vital rates. The absence of a clear signal from density* (of whales) *lends support to the non-causal interpretation that salmon abundance is correlated with an unmeasured limiting factor that is not influenced by population density of killer whales*. " lines 524–528

> " *… the absence of a clear negative feedback from population size to vital rates complicates the mechanistic interpretation of a positive correlation between vital rates and food supply. Classical theory in community ecology predicts that reductions in the number of predators or increases in the number of prey should produce similar responses at the population level. This finding raises doubts about the cause and effect relationship between salmon abundance and killer whale vital rates.*" lines 555–559.

If information that tends to confirm causation is presented in the draft summary (section 2), information that tends to disconfirm causation should be presented there as well. While obviously this important information was presented in the draft, it was not presented in the executive summary or in section 2.

* * *

The panel revisits the issue of rejecting the hypothesis of causation outright or accepting it through confirmation of underlying mechanisms on recommending future work in section 2.4. While we largely agree with their recommendations, we do have some suggestions.

The headings of "*Critical missing pieces*" on line 350 and "*Items Critical to the underlying hypothesis*" on line 356 are inconsistent. Under the former, the panel wrote:

> "*The panel has identified a range of data and analyses that would be high priority to obtain, but were not feasible* (to do so) *during the duration of our review. We classify these into those critical to the evaluation of the link between food supply and rates of increase, and those that that provide supporting mechanistic evidence.*" lines 351–354

The inconsistency is that "(items) *critical to the evaluation of the link*" is not the same as "*Items critical to the underlying hypothesis*." The latter establishes the link that the former evaluates.

By the time we reached the topics (items) themselves, our initial understanding was that those topics labeled as "*critical*" had a greater importance than those labeled as "*supporting*".

After reading the text on the individual topics of investigation, we were more confused. Proposed investigations on "*nutritional stress*" and "*contaminant fingerprinting*" seemed "*critical to the underlying hypothesis*" of causation, but were binned together with modeling investigations that were clearly designed to provide information "*critical to the evaluation of the link*" (the causative relationship). We suggest that section 2.4 needs a better organization.

We also suggest using a term other than "*supporting*". Recommended investigations if undertaken would provide information that would tend to either confirm or disconfirm mechanisms. The implication behind studies that "support" mechanisms is that panel is interested only in confirming the mechanisms. The text in each paragraph does not read that way, and we do not believe that such a one-sided approach is what the panel intends.

The Section 3: Status and Growth Rates of Killer Whales

Most of our suggestions relating to section 3 have been provided above, and much of our supportive material in those earlier (and later suggestions) are in this section. We take this opportunity to repeat some of the salient insights first given here that should be repeated in the executive summary and perhaps in section 2 as well. We start with

> "*While the average of observed population growth rate of SRKWs is 1% annually, there is a chance that the core rate is < 0% and at the same time a chance that the core rate is above the delisting threshold of 2.3%.*" lines 450-451

In regards to this insight, we suggest that Figure 3.1 be given additional color coding relative to the probability that $\lambda > 1.023$. The same suggestion goes for Figure 3.3.

We repeat our suggestion made earlier that discussion in this section should include the topic of what removals from the population of SRKWs by the aquaria trade during the late '60s and early '70s means to resilience of the population. Given that the panel believes

> "*Concerns about its* (the SRKW population) *future arise entirely from the current and recent size of the population and the potential impacts of future, unforeseen events on a population that lacks the resilience created by higher abundance.*" lines 430-432

we would expect a discussion of the resilience of this small population given its history, a history that included "pulse fishing" the population. As we stated earlier, no such discussion occurs in the draft report (or a reference to the perturbation).

The Section 4: Feeding Habits and Energetic Needs of Killer Whales

We agree with the statement in the text in section 4.1 that relates to diet:

> "*Selectivity by Southern Residents on different stocks of Chinook salmon is poorly known, an important uncertainty both in terms of understanding which Chinook stocks*

they rely on, and in terms of energy intake, because the energy density of Chinook varies among stocks." lines 587–589

However, we also feel that some elaboration beyond the description "*poorly known*" is needed. Some stocks of Chinook salmon can be excluded from the whales' winter diet based on what we know of the ranges of those stocks vis-à-vis the range of SRKWs. Also, we do know that when eating Chinook salmon during the summer in Puget Sound, the whales have concentrated their feeding almost exclusively on mature salmon bound for the Fraser River. This distinction also needs to be added to lines 648–651 in section 4.3.

We take small issue with the following text in section 4.3:

" *... further diet studies are unlikely to change the fundamental finding to date that Chinook are the most important component of the SRKW diet. Instead, they should provide data needed to determine whether SRKW can adapt their foraging during times when Chinook are rare to consume alternate prey at rates that do not compromise their fitness.*" lines 671–673

for the obvious reason that we have essentially no information on what the whales eat during the winter. Also, future diet studies can also identify which stocks the whales eat.

* * *

Some confusion occurs in the statement concerning apex predators other than killer whales:

"*It is possible that killer whales consume a larger component of the Puget Sound Chinook stocks because there are fewer other important apex predators compared to other ecosystems.* " lines 715–716

The evidence is that SRKWs hardly consume Puget Sound stocks at all. Genetic stock identification of body parts presented by Hanson in workshop 1 show whales forgo eating Puget Sound stocks in favor of Fraser stocks while present in the sound with the possible exception of feeding in May.

The Section 5: Fisheries and Prey Availability

Some assumptions used to analyze the force of predation by SRKWs on Chinook salmon posited in section 5 under the heading "*Competing risks of death framework*" have some interesting consequences. The panel wrote in the draft

"*Among other things, we showed that, under an assumption that killer whales consume an approximately constant number of Chinook salmon, the force of mortality associated with killer whales (and possibly also the forces of mortality for other pinniped predators) likely increases dramatically as abundance of Chinook salmon decreases.*" lines 1001–1004

This is classic predator-prey theory when the predator is so efficient that its stomach size is the limiting factor to the amount it ingests. Under such circumstances the abundance of prey is not

limiting to how much the predator eats. In our situation the consequence of this satiation would be that no causation would exist between whale numbers, abundance of Chinook salmon, and fisheries regulation.

* * *

Earlier we suggested that the executive summary include the insight that many stocks of Chinook salmon normally perceived as being in the range of SRKWs are not available to the whales. The basis for that suggestion is found in section 5:

> " *… these data suggest, for example, that the far-northern-migrating Chinook stocks (including those from the Northern Oregon Coast, NOC) would likely not be present in substantial numbers within the range of the SRKW during the winter months and they would very clearly not be available during the summer months as they would have no reason to enter the Strait of Juan de Fuca or Puget Sound en route to their spawning streams as maturing fish. We note that NOC north-migrating Chinook were included as part of the "fall" and "north" groupings in Ward's Workshop 2 logistic regression."* lines 1056–1062
>
> *"Many Chinook populations would not be available even during this winter period, however, including immature fish from the northern Oregon coast and many far-north-migrating Columbia River stocks that are probably generally beyond the northern range of the SRKW until they return as mature fish on spawning runs."* lines 914–917
>
> " *… during the summer period it seems fairly clear that a rather limited set of Chinook populations and only the maturing fish from those populations would be directly exposed to predation by the SRKW, and only fisheries that impacted these stocks could affect prey availability during the summer period.*" lines 894–897
>
> " *… stocks of Fraser River Chinook would* a priori *appear to be the most vulnerable to SRKW predation and the most important stocks during the summer months.*" lines 903–904

The last quote pertains to the evidence that SRKWs concentrate their predation on some available stocks (Fraser stocks) instead of other available stocks (Puget Sound stocks) in their summer range.

* * *

Of the bulleted recommendations for further analysis in section 5.4, we concur with the first and third recommendations, concur with the second and fifth with adaptations, and suggest dropping the third recommendation. We suggest that besides winter observations off northern California and Oregon, a more refined contaminant analysis should be referenced in the text here as well. A recommendation for such an approach can be found elsewhere in the draft (lines 373–378). In the fifth recommendation, predictions from the proposed heuristic model should be compared to actual observations. Such a validation would add credibility to the inferences drawn from the exercise.

We suggest that the sensitivity analysis of how SRKWs respond to fishery scenarios not be presented at workshop 3, or the results presented in the final report. Such sensitivity analysis is

based on the presumption of a causal relationship that has yet to be confirmed. Objection to confusing correlation with causation in similar modeling was raised by attendees during presentations at workshop 2. Also, considerable text in this draft report concerns the insufficiency of correlation in confirming causation and the lack of that confirmation in this instance:

" … *the panel cautions that this correlation does not imply causation …*", 5th sentence, 3rd paragraph, page iii

"The history of population dynamics is replete with correlations that have not turned out to be causal." lines 244–245

" *The panel believes the NOAA and DFO scientists have done excellent job of their statistical analysis, but in the end believe considerable caution is warranted in interpreting the results as confirming a causative relationship between Chinook abundance and SRKW survival*." lines 256–258

"*The panel's overall view is that the projected increases in SRKW abundance are both small, and likely overestimated because of our concerns expressed in the issue of correlation versus causation*, … " lines 293–294

"*the panel cautions against overreliance on the correlation implying causation …* ", line 307

"*As discussed in Section 2.2 the panel has reservations about assuming that correlation implies causation.*" line 333–334

"*All of these difficulties of interpretation cast doubt on a simple, causal interpretation of the positive correlation between salmon abundance and killer whale vital rates. The absence of a clear signal from density lends support to the non-causal interpretation that salmon abundance is correlated with an unmeasured limiting factor that is not influenced by population density of killer whales."* lines 524–528

"… the absence of a clear negative feedback from population size to vital rates complicates the mechanistic interpretation of a positive correlation between vital rates and food supply. This finding raises doubts about the cause and effect relationship between salmon abundance and killer whale vital rates." lines 555–559.

" *… SRKW growth and salmon abundance data are observations of uncontrolled events obtained from an unknown sampling design. Such observational study designs pose a high risk of incorrectly assigning causes to correlations, result in relatively weak inferences, and are typically considered more useful for hypothesis generation (Schwarz 1998)."* lines 1168–1171

"*The analyses performed to date on the relationship between salmon abundance and killer whale fecundity and survival have likely extracted as much information as can be gained from the historical data. The results certainly lend credibility to the hypothesis that SRKW growth rates and abundance are related to salmon abundance, but they also raised many questions about specific mechanisms, the chance of spurious correlations,*

alternative hypotheses, data gaps, and expected changes in Chinook availability." lines 1299–1304

This litany of warnings from the panel, along with reporting evidence that tends to disconfirm a causal relationship, is inconsistent with the recommendation of modeling based on a causal relationship not supported by evidence.

The Section 6: Projected Future Status and Recovery

Because much of the material in section 6 has been referenced above, we won't belabor the material here. However, we do suggest that the simulation analysis suggested in this section be reconsidered:

"*The Panel suggests that simulation analyses be performed to determine whether any magnitude of realistic increase in salmon fisheries would have a detectable effect on future killer whale growth rates."* lines 1310–1311

When weighing the need for such simulations, we suggest the panel consider the example they present:

" … *Figure 6-2 (lower left panel) suggests that Chinook abundance would need to increase 25-40% to achieve SRKW population growth rates near 2.3% per year. There have only been 3-5 years out of the past 32 years in which Chinook abundance has been near those levels.*" lines 1314–1317

In light of what has happened in the last 30 years with terminal runs:

" … changes in coast-wide abundance of Chinook populations over the past 30 years, the period of time over which status of SRKW has been closely monitored, has been relatively modest: an approximate 16% decline in total abundance, but with a corresponding substantial 37% increase in terminal abundance (returns to freshwater) due to increased restrictions on marine fishery harvests." lines 885–889

Events indicate that we have had the 40% increase (or nearly so) in the last 30 years that the panel projects would be needed to realize a 2.3% growth rate in SRKWs. Terminal runs to freshwater are by definition the group of fish extant following predation by whales, which means that availability to whales at least from May through September has at least increased by that rate. Yet a nominal growth rate of 2.3% has not been realized.

Our Final Remarks

This concludes our critique. We repeat that our intention is to present thoughts and ideas that we believe would strengthen the final report. We also repeat that the draft final report represents very good work by the expert panel on the difficult issues set before them. We thank the panel and their sponsors for the opportunity to review the draft, and are looking forward to the third workshop in Seattle this fall.