Northwest Fisheries Science Center comments on *THE EFFECTS OF SALMON FISHERIES ON SOUTHERN RESIDENT KILLER WHALES FINAL REPORT OF THE INDEPENDENT SCIENCE PANEL*, May 3 Draft.

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June 26, 2012

The draft report provides a good summary of the scientific issues surrounding the question of the effects of salmon fisheries on the status of the southern resident killer whales. In general, we think the report did a good job of capturing the key issues, and identifying areas of particular uncertainty in an objective and balanced way. Considering the large number of presentations, reports and papers the panel needed to digest to evaluate the questions put to it, we think on the whole the panel did a good job of sorting through everything to come up with their recommendations.

We did identify some areas where we think the report either misinterprets or does not mention some important information, however. These are summarized briefly below, and discussed in more detail in our specific comments. Some of these topics may also be covered more extensively in workshop 3.

* Winter distribution and salmon stock availability – We think the report tends to understate the amount of information available on the winter distribution of the whales. The report is correct that much less is known about the winter than the summer distribution, but there are some important sources of information that we think were overlooked or underemphasized. John Ford documented SRKWs foraging on Chinook salmon in February and March in the Strait of Georgia and in April in the Strait of Juan de Fuca (*Resident killer whale feeding habits: Assessment methods, winter diet, and chum stock ID*). Using data collected from passive acoustic recorders, Brad Hanson documented the SRKWs are present in coastal waters of Washington, Oregon and California (*Using moored passive acoustic recorders to assess seasonal occurrence and movements of Southern Resident Killer Whales in US coastal waters*), and also discussed winter distribution in workshop 1. Ken Balcomb also documented that SRKWs were observed in the winter months in the coastal waters of Washington, Oregon and California, as far south as Monterey Bay (*Winter Distributions*). For the most part, the information in these presentations seems not to have been cited or used in the report. Not mentioning the acoustic recorder data that Brad Hanson presented in workshop 2 seemed to be a particularly notable omission, as in many ways these data are providing exactly the sort of information the panel says is needed.

* Seasonal and spatial overlap of salmon and whales -- We think the report understates the potential spatial/temporal overlap of the whales and various stocks

of Chinook salmon. Immature fish from many Chinook salmon populations likely reside in winter range of SRKW, certainly far more than just the southern Oregon and California populations suggested by the Science Panel. A full list of the Chinook populations in the winter range of SRKWs can be found in a closer review of three recent papers (Trudel et al. 2009, Weitkemp et al. 2010, and Sharma and Quinn 2012) that provide coast-wide assessments of the marine distribution of Chinook salmon based on analyses of recovery data for coded-wire tagged (CWT) juvenile salmon. Additional corroborative information on the marine distribution of Chinook populations comes from genetics studies (e.g., Tucker et al. 2011, 2012). Briefly, during the winter, some juvenile and immature Chinook salmon from northern California, Oregon, coastal Washington, the Columbia River, and Puget Sound occupy waters where SRKWs have been documented to occur. We will summarize this information more comprehensively for workshop 3, as it is an important topic.

* The report raises several important questions about how trends in other salmon predators, such as seals and sea lions, may be affecting the killer whales. To help address this question the panel requested that Eric Ward include model output of pinniped abundance from an Ecopath model as co-factor in the analysis of killer whale growth rates. Based this analysis, the report suggests that there is a statistical link between pinniped abundance and whale population growth rates. However, Eric does not believe these analyses have much, if any, validity, since they are based not on data but rather model outputs of pinniped abundance that do not appear to be consistent with the limited data that are available. In particular, the model output predicted that sea lion abundance has declined over the last decade by 50%, whereas the available data suggests stable or increasing abundance. Although we agree that exploring the effects of trends in other salmon predators is important and should be done, we believe this particular analysis is not helpful in addressing this question.

* In several instances, the report fails to mention or cite some important publications or workshop presentations. This is perfectly understandable given the large amount of information presented and the difficulty of keeping track of particularly the unpublished material. Nonetheless, we think it will be important to correct these omissions in the final report. See specific comments for details.

Specific comments:

p iii
"However the panel cautions that this correlation does not imply causation, but rather both could be caused by another environmental factor. There are also concerns about whether the index of Chinook abundance accurately reflects the Chinook stocks most important to SRKW"

EW: This is really the heart of the matter. With 30+ years of observed births and deaths, we can correlate killer whale demographic rates with different salmon

stocks. But even if we collected 30 more years of these same data, these data alone are not likely to be useful for identifying which stock(s) are most important or limiting unless the stocks remain of equal importance to the whales over the entire time period.

With the 30 years of data that we've observed to date, all of the regression analyses that have been done to date have attempted to identify a single stock, or group of stocks, that best explain the whales' demographics. What these analyses implicitly assume is that the relative importance, or ranking, of the stocks has been constant through time. This is likely an unrealistic assumption, as different salmon stocks have experienced differential changes in habitat, abundance, harvest rates, and run timing, and the killer whales themselves may have changed habitat preferences from one year to the next. This is just a long way of saying that it's pretty unlikely that whatever the most important stock is in 2012 has always been the most important stock. This is at least partially supported by the analyses I did following workshop 2, that showed the <u>total</u> coastwide abundance of Chinook salmon better explains SRKW demographics than any individual stock (or group of stocks identified in workshop 2). In addition, many of the salmon stocks are themselves highly correlated with each other, making it statistically very problematic to infer exactly which stocks are most limiting. In summary, we think that the results of the logistic regression models are supportive of a relationship between killer whale growth rates and broad indicators of Chinook abundance, but are less useful for attempting to identify which particularly stocks are limiting.

Page iv – the panel recommends "(2) use of contaminant data to determine if the indices of Chinook abundance used in the statistical models accurately reflect the Chinook stocks important to SRKW"

We will provide additional information on contaminant and chemical analysis at the third workshop. Such information may be particularly useful in gaining insight into stocks consumed by the whales during times of the year when collection of prey or fecal samples is very difficult. We would like to note, however, that in general we believe that genetic analysis of prey remains or fecal samples will provide finer scale stock resolution than is likely to be possible through contaminant analysis.

Line 131 – BH: It would be useful for the report to note that the order of their recommendations does not necessarily imply priority within the categories, unless the panel intended them to be prioritized.

Line 161 – BH: We agree that photogrammetry is a potentially very useful tool for assessing condition. However, here and elsewhere we think the report perhaps overstates the precision of the data that are currently available. In particular, the information presented in the Durban et al. report does not fully support the idea that photogrammetric data as currently collected will necessarily allow accurate assessment of nutritional status, due to relatively high measurement error. Also, the measurements from aerial photogrammetry did not detect that L67, an animal that

clearly showed classic signs of emaciation, was thinner than other whales. We agree with the panel that better data on the condition and nutritional status of individual whales over time would be very valuable, and that photogrammetric data may provide this, but think that some of the potential limitations should also be noted.

Lines 191-192, 542 "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."

MF,LB: Wiles (2004) obtained a rough estimate of 117 based on adding the known removals to the population size in 1974, so it is possible the population was somewhat larger than 96 in the early 1960s (see Ford presentation in workshop 2). As an aside, it is probably worth noting the population was back up to ~96 animals in 1996, just prior to the ~20% decline from 1997-2001 that prompted the listing.

Line 200 – "It seems illogical that SRKW would forego feeding on other species" – BH: this statement makes the assumption that all species have equal detectability and equal catchability, neither of which is likely true. For example, recent research suggests that killer whales are capable of detecting, localizing and recognizing Chinook salmon through their ability to distinguish Chinook echo structure as different from other salmon (Au et al. 2010).

Line 210 – 212 - "The strongest suggestion of poor nutrition condition is the photogrammetric evidence from Durban et al. 2009 and the peanut-head syndrome" BH: We are not quite sure what evidence the panel is referring to. As previously noted, the aerial photogrammetric did not detect malnutrition in L67's other body metrics although the whale did exhibit the peanut-head syndrome. "13 SRKW in poor condition over the period 1994 through 2008 and all but two of these individuals died" - however, this should be presented in the context of total mortalities - during that same period a total of 59 whales died – so less than 25% exhibited peanut-head syndrome – we suggest the text should be corrected to clarify this point.

LN 290: "more recent logistic regression analyses (requested by the Panel) suggest that growth rates of SRKW are also related to the abundance of other marine mammal predators, which were not included in earlier versions of the statistical models"

Please see the general comment on this topic above.

EW: Marine mammal abundance wasn't included in previous iterations, because we had incomplete time series for most species (no harbor seal surveys after 1999, very few estimates of sea lion abundance after that date). In lieu of real data, the panel

asked me to include Dave Priekshot's estimates of seals and sea lion biomass from Ecopath output but treat it as 'data'. Including harbor seals or sea lions alone didn't improve the model fit, but only when both time series were included did the fit improve (in this situation, the effect of sea lions was negative, and seals positive). However, I would place little if any weight on this analysis. In an email to the entire panel and ESSA on Apr 17, I wrote:

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I'd reiterate to the panel that these should really be taken with a grain of salt, for 2 reasons:

(1) the seals / sea lion estimates aren't based on data, but Ecopath output from Dave Priekshot. His indices of sea lions suggest that the population has been halved over the last decade. We don't have many sea lion abundance estimates, but the ones we do have (from the CA stocks, for example) are totally inconsistent with the population being halved -- more likely, sea lions have been stable (or increasing).

(2) from seal diet samples, we know harbor seals have quite a bit different selectivity than the whales, preferring smaller 2-year old Chinook, and other species (herring, gadids, pink salmon, etc), so they're competing indirectly with whales, by consuming Chinook several years before. So I don't really understand the positive seal effect in these models, other than it's canceling out the sea lion effect...when I try lagged models, the marine mammal models fare a bit worse.
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I'm not disagreeing that sea lions and harbor seals may have an effect on SRKW. But I'd strongly re-iterate to the panel that this entire analysis is suspect since the Ecopath estimates are so different from our understanding of the recent trends for CA sea lions. There are better approaches we might use to examine interactions for the existing data (such as my talk in Workshop 1 on ecosystem approaches).

line 401: (other ecosystem factors)
EW: The panel suggests that we expand the model fitting analysis to include ocean productivity indices. We do not think this make a lot of sense to do, for a couple reasons. First, it's something we have already explored (see the appendix of Ward et al. 2009). Second, the panel acknowledges that some causal factor is affecting both NRKW and SRKW. If our focus is on causal factors, and KW are several trophic levels above plankton, I don't see why we'd want to consider examining correlations with environmental indices (or primary productivity). This seems to be getting us further away from a mechanistic understanding, not closer to it.

Line 243-244: "While it seems unlikely this is pure chance, it is certainly possible that another factor has affected both Chinook abundance and SRKW survival, and there is no causative link."

EW: This statement is technically true, but on line 179, the report said

"sustained periods of increase and decline, shared between both SRKW and NRKWs suggest there is likely a common causal factor"

Might be worth tying these 2 statements together?

MF: In our opinion, it seems unlikely that the survival and fecundity of a top predator such as a killer whale would be mediated by environmental factors other than via their prey. We may be missing something, however, so we would be interested to know if the panel has any specific alternative mechanisms in mind.

Line 262 – BH: suggest substitute "prey" for "food".

Line 273:

EW: for public knowledge, it might be worth parenthetically including the approximate percentages, or ranges of immature fish in the catch

Line 330-331; and 361-364 – BH: See above comment on photogrammetry. This might be one method of obtaining information on seasonal nutritional status, but some evaluation of whether the measurement error is sufficiently low to make this feasible is needed. Also, we think it would be useful for the report to mention that other methods, such the hormone data presented by Sam Wasser (and now published – Ayres et al. 2011), may also be useful for evaluating nutritional status over time.

Line 373 (Contaminant fingerprinting):
EW: What we really need are the changes in contaminants for each individual from one year to the next. Otherwise, for a young male of a given age, we really have no way to quantify how much of his contaminant signature is from his diet, and how much is from his mom.

BH: Please cite Krahn et al. 2007 here.
MF,SO: We will be presenting additional chemical information at the third workshop.

Line 379 (fishing scenarios):
EW: During workshop 2, while the focus of Eric Ward's talk was on the maximum increase (20%), he actually included results for 10 different scenarios in the talk, and indicated that any reduction between 0-20% can be calculated from the results presented. In other words, the results can be used to evaluate any fishing scenario

so long as the scenario can be translated in a specific change in abundance of Chinook salmon available to the whales relative to the status quo.

Line 404 – 405 – BH: We think that Ford and Ellis (2006), and the more recent fecal analysis we presented at both workshops, suggests that there is no serious bias in the prey samples.

line 415
EW: Throughout the workshop process, panel members often have referred to the data as "exceptional" or "amazing" or "highest standards for inference in population ecology" (e.g, line 442). This language tends to imply that the data sets are "perfect", but they have some limitations. In particular, it's certain that an occasional birth is missed when a calf dies before being observed. This could be estimable, but there's also been an increasing trend in search effort over time – more boats on the water, greater public awareness, etc, and we have no way of quantifying that trend in effort. So although it is a great data set, it is not perfect, and some of these limitations should be understood.

line 466:
EW: I think it's important to point out that while these rates are time-varying, they're still replacement of females by females.

Line 482:
EW: J pod has lots of females, and L has few. This distinction is important

line 475:
EW: While selectivity didn't receive much attention, I updated Workshop's 1 analysis, updated the document, and posted it to ESSA's website prior to Workshop 2

Line 575 – BH: Suggest also citing Krahn et al. 2004

Line 581-583 –BH: Not sure what Hanson et al. 2005 refers to. For fall data, they could cite my unpublished report to the panel (see Hanson et al. *Comparing diet composition from prey remains and fecal samples*, from the second workshop).

Line 594 – BH: I also presented stock-specific consumptions estimates for the summer months in workshop 1 (see Hanson et al. workshop 1 report). Suggest adding the following sentence. "NOAA also estimated the stock-specific consumption for Southern resident killer whales in the summer (Hanson et al. presentation workshop 1)."

Line 598-9 – BH: "weight loss can arise from a variety of causes that range from infectious disease to chronic degenerative processes, but the common cause in wildlife is poor nutrition". It would be helpful to have some citations supporting

this statement. Also, we wonder if this generalization would be applicable to a species that prey shares.

Line 652 – BH: "However, it is conceivable that smaller Chinook may not be shared as readily and could be swallowed whole." We also mentioned the possibility in our fall prey report, which could be cited (see Hanson et al. *Prey selection by southern resident killer whales in inland waters of Washington during the fall and early winter*, from the first workshop).

Lines 653-656 – BH: Regarding fecal DNA testing, both Brad Hanson (workshop 1) and Mike Ford (workshop 2) presented NOAA's results on these analyses, so citing these presentations would be appropriate. The fecal DNA quantification results to date are all generally consistent with the prey sampling analysis.

Line 657 – BH: "The small numbers of samples obtained during the winter suggest a greater reliance on chum" – what is this generalization referring to? If it's our work in Puget Sound in Oct - Jan this is not an unreasonably small sample size (n= 60; see report cited above). Both the Hanson and Ford presentation/reports could also be cited here. Note that Hanson's winter samples are almost entirely from inland waters. DFO's winter sampling in coastal waters was predominantly Chinook – see John Ford's presentation on winter samples.

Line 658-9 – BH: In inland waters, we have a fair bit of data on winter diet (see Hanson presentations and reports that were made available to the panel). John Ford also reported a fair bit of data on winter diet. It is true that we have very few samples from southern residents in coastal areas, but the coastal/inland distinction is important to recognize.

Lines 671-674 – BH: "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". In addition to possibly switching prey, the whales have the option of simply going somewhere else in search of their preferred prey. Brad Hanson's presentation at the workshop 2 described some of the rapid, long range migrations the whales take, probably in search of prey. We think it is important recognize this movement option, and to cite Brad's presentation.

Line 692 – BH: It would be helpful if the panel could more clearly explain why they believe that there would be seasonal differences in energy requirements and provide citations supporting those statements. We are unaware of any data indicating seasonally varying energy requirement in killer whales (see paper that Dawn Noren sent to the panel for the second workshop).

Line 693 -4 – BH: "Photogrammetry data could be used to address season changes in body condition, and its possible relationship to season changes in metabolism due

to differences in dive behavior and daily activity budgets " What data on seasonal changes in dive behavior is the panel referring to? Even if they do change their dive behavior, why would this change their energy requirements?

Line 697-702 BH: "A mismatch in seasonal prey availability with seasonal energy requirements can have significant physiological effects on fecundity and susceptibility to disease. Photogrammetry data could also be used to investigate body condition changes in years of high versus low Chinook abundance. A systematic use of photogrammetry to evaluate seasonal and annual changes in individual whale body condition can provide key data to assess the nutritional status of SRKW relative to population recovery. " As noted above, we agree that the photogrammetry data is useful. However, we are not sure that the methods are of sufficient resolution to track seasonal changes in body conditions. Further studies may certainly be useful, but some of the limitations should be noted.

Line 706-707 – BH: While we did not use a ratio per se, the panel did not mention my presentation on a comparison of species and Chinook stock-specific consumption estimates compared to Chinook stock-specific run reconstruction estimates in their summer range. This presentation clearly showed that comparing the number of fish consumed to the total number of fish likely available can be an informative exercise. As we get more information on SRKW outer coast occurrence and Chinook stock distribution this approach will be expandable to this area and season. Williams et al. (2011) also made similar calculations and could also be cited. The basic point here is that while we agree that the small changes in prey/consumption ratios can be difficult to interpret, we are not convinced that all efforts to estimate prey needs in comparison to prey availability are necessarily without merit.

Lines 717-722 – BH: In workshop 1, I presented some estimates of prey consumption that I think are in fact informative. See presentation and report "*Assessing the adequacy of Chinook stocks to meet the energetic needs of Southern resident killer whales in their summer range*".

lines 743-745: "Changes in social behavior may also result from changes in nutritional status. For example, group sizes of killer whales….."
DN: The panel only cites Lusseau *et al.* (2004) which found that group sizes of killer whales might change in response to changes in the availability of prey. Additional references found similar or complimentary results. Parsons *et al.* (2009) found that a long-term reduction in returning stocks of Chinook salmon appeared to reduce social cohesion in Southern Resident killer whales while McCluskey (2006) reported that Southern Resident killer whales increased their movement in the San Juan Islands when Chinook availability was low. These two other studies provide further evidence that the availability of Chinook may impact social and other behaviors, and thus, including these other references are also important as it seems that some may still not be convinced that Chinook availability has the potential to impact several aspects of SRKW biology and behavior.

Line 754-7 – BH: Samples from stranded animals are routinely collected.

lines 762-763: "Satellite tagging of whales in late summer is needed to identify winter foraging areas and calculate activity budgets to better estimate the prey requirements of SRKW."

DN: The satellite tags that are currently used on SRKWs only provide location data without swimming speeds and diving profiles. In fact most tags, particularly satellite tags, do not provide swimming speeds in units that would be beneficial to estimating activity budgets and daily energy expenditure/prey requirements. It's possible that the technology will improve to allow for such measures, but I think it is important to provide a point of reference regarding what is currently feasible with satellite tag data.

BH: We agree with the panel that satellite tagging is a very useful tool, and are actively pursing these studies. However, because the tags only stay on for ~30 days (and often less), we think applying tags in late summer as suggested by the panel is not as useful as tagging in winter.

Line 771 – BH: There are also other methods of assessing nutritional status (e.g, hormones – see Ayres et al. 2012).

lines 764-768: "In addition, increased analysis of foraging behaviors is needed to detect changes in activity budgets (proportion of time spent foraging, socializing, resting, travelling), movement patterns, etc…."

DN: This means that more behavioral and activity budget data will need to be collected in a systematic way. Not all studies have collected the data with the same methods, and a retrospective analysis of all of the components listed by the panel will likely not be possible.

lines 771-775: "Research designed to monitor nutritional status of SRKW should focus on: biopsy sampling to detect seasonal changes in blubber lipid levels; photogrammetry methods used by Durban et al. (2009) and Fearnback et al. (2011) to evaluated different width-length ratios, and longitudinal sampling to investigate seasonal changes in body condition in individuals, and at risk age and sex classes."

DN: It is unlikely that the biopsy samples from SRKWs that are taken (which are not full depth) will show a reduction in lipid content because the inner portion of odontocete blubber is the source used as an energy store. For example, Koopman *et al.* (2002) showed that in starving harbor porpoise, lipids were withdrawn only from the inner layer of thorax blubber during starvation. Therefore, some proportion of the blubber of odontocetes must be considered structural/mechanical rather than an energy reserve. Furthermore, it is possible that some lipid is lost

during the remote biopsy process (Krahn *et al.* 2004), which would make the results of the suggested analysis suspect.

The Durban *et al.* (2009) report actually states that the width-length ratios are problematic due to high variation within individuals (not just across individuals) due to errors during measurements. These issues would need to be addressed before this method could be used as an index of nutritional status. A current utility of the method developed by Durban, Fearnback, and colleagues is that it provides much better estimates of individuals' body lengths which can be used to calculate better estimates of body mass for modeling energetic requirements.

lines 775-776: "Further, analysis of individual calving intervals, and of group sizes and association strengths could be assessed as measures of relative feeding conditions and payoffs to the predators."

DN: The Parsons *et al.* (2009) paper investigated association strengths and social cohesion relative to prey abundance.

Lines 878-881 – BH: "Audience discussions following Bernard's presentation at Workshop 2 suggested that SRKW feed in California waters during winter months, but there were no detailed presentations of winter location observation data from California or Oregon waters" **This is NOT correct**! On day 3 of the workshop, Brad Hanson presented data from NOAA's acoustic recorder efforts that give a much more complete picture of the SRKW's coastal distribution than the panel report describes. In particular, Brad discussed data from 129 detections from WA to California over the last 6 years – the most data compiled on SR coastal distribution to date.

Lines 891-897 – BH: "the SRKW population must be foraging primarily on maturing Chinook salmon that are entering the Strait of Juan de Fuca or Georgia Strait on their return to freshwater streams, primarily those spawning in streams that enter Puget Sound and the Fraser River. Therefore, 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." This is exactly what Hanson et al. (2010) found, so it would be appropriate to cite this paper.

Lines 901-904 – BH: "Given the relatively small size and young age of most Puget Sound Chinook, particularly of hatchery origin, and the apparent inclination of SRKW to prefer larger age 4 and 5 Chinook, 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." - As I discussed in my workshop 1 presentation, we don't really know the mechanism responsible for the prey selectively. It's possible that rather than a "preference" by the whales, they simply have a harder time detecting or capturing smaller fish.

Lines 905-906 – BH: "The extent to which SRKW depend on Chinook salmon during the winter period seems poorly identified as is the geographic and temporal distribution of the SRKW during the winter period." See Hanson presentation from workshop 2 regarding distribution. There is less information than in the summer, but there is more information that is implied by the report.

Lines 914-915 – BH: "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. Chinook from the southern Oregon coast and California would be available only to the unknown extent that winter feeding activities are focused on Oregon and California waters." We think that simply breaking things into "winter" and "summer" is misleading. The whales also spend a lot of time off the coast during the spring (March – May), when a larger variety of migrating stocks are likely to be available. Mature fish returning to these river systems are going to be either resident on the continental shelf or returning on to it, and is in some cases are likely to be available some time in advance of river entry. We will provide more summarized information on this topic for workshop 3.

Line 949-950 – BH: "during the winter months as extensive information would be needed on both the coastal marine distribution of various Chinook stocks during winter months and on the coastal marine distribution of SRKW, both of which appear very poorly understood" – We think the panel should consider and cite Laurie Weitkamp's (2010) recent paper on Chinook distribution. [see also main comments above]

Lines 967-969 – BH: "reflects a positive response to our conjecture that, at least during the summer period, SRKW must be intercepting and primarily consuming maturing/mature Chinook salmon en route to their freshwater spawning grounds" There is no need for conjecture – the information in Hanson et al. 2010 provides data in support of this statement and could be cited.

Line 976-977 – BH: "SRKW appear to prefer large Chinook" – again this could be a detectability issue, not necessarily a preference.

Line 991 – BH: "Comparisons of age composition in SRKW feces" . We do not know how to get fish age from the fecal samples – if the panel has idea on this we would appreciate more information.

Line 996 – BH: "comparison of age composition of Chinook in SRKW samples with test fishery age composition is not appropriate due to size selectivity in the test fisheries themselves" . The panel should clarify which test fishery is being referred

to here. Laurie Weitkamp and I looked at the size composition of Chinook in the Albion test fishery by mesh size and we concluded that the 8 inch mesh, which is predominantly used, did not appear to bias the catch.

Line 1012–1015 – BH: "Given the potential rates of consumption of Chinook salmon by the SRKW generated by Ford during his Workshop 1 presentation25 (67,000 – 81,000 Chinook during the months of July and August, with range of from 342,000 – 410,000 Chinook per year assuming 70% of diet is Chinook" . We suggest also reviewing and citing Hanson's presentation/report for workshop 1 that calculated stock-specific consumption during the summer months. See also Williams et al. (2011).

Lines 1016-1019 BH: "it is easy to imagine that the force of mortality associated with killer whales at low Chinook salmon abundance may be quite large, especially if Chinook abundance is measured by those populations that are actually available to SRKW and of appropriate size/age (selectivity)." Again, please see my report "Assessing the adequacy of Chinook stocks to meet the energetic needs of southern resident killer whales in their summer range" and associated workshop 1 presentation which calculates number of Chinook consumed compared to the stock-specific run sizes.

Lines 1029-1031 – BH: "Preikshot and Perry also suggested (slide 13) that simulated Chinook salmon mortality (force of mortality) in Georgia Strait associated with pinnipeds was well below that of killer whales for the period 1960- 1985, whereas during the period 1990-2010 it was roughly comparable to that associated with killer whales." I think it is important to point out differences between killer whale and pinniped predation. Pinniped take a much wider variety of prey, and consume younger age salmon than do killer whales. This is not to say that pinniped predation is necessarily insignificant, but some nuance is needed here.

Lines 1040-1053 – BH: I presented data on stock-specific abundance during the summer months based on run reconstruction data developed by DFO – see "Assessing the adequacy of Chinook stocks to meet the energetic needs of southern resident killer whales in their summer range" from the first workshop.

Lines 1054-1060 – BH: The panel should review and consider citing Laurie Weitkamp's 2010 paper on CWT recoveries.

Lines 1069-1072 – BH: "primary available stocks would seem to be maturing fish from Puget Sound and The Fraser River…" Please consider citing Hanson et al. (2010), which describes data that directly support this topic.

Lines 1075-1076 – BH: Bernard provided very little information to support his assertions of Chinook stock distribution. We suggest the panel review and cite Laurie Weitkamp's 2010 paper on Chinook CWT recoveries. The whales spend

considerable time during the spring months on the outer coast, where they will have contact with a variety of migrating stocks.

Lines 1079-1080 – BH: "The possibility that SRKW may spend considerable time off California during winter would obviously be of great importance in selection of appropriate stock groupings." Again, we have data on this. Please see my presentation from workshop 3, and review and cite Krahn et al. (2007) regarding contaminant ratios and CA. [note: there will be additional information on this topic at workshop 3].

lines 1099-1102 – the panel recommends that additional photos/information are acquired from marine mammal observers in Newport, OR and Monterey, CA.

DN: This is something that we've been doing since the marine mammal group started at NWFSC in 2003 with mixed results. With the increased effort over the last 9 years, a handful of additional confirmed sightings have been noted, but the influx of data is not great. The acoustics recorders actually provide more data – whales can be identified to the pod level, but the data is not acquired real time since the recorders have to be retrieved. These recorders are providing data on habitat use and movement patterns of SRKWs (to the level of the pod; J, K, or L) during the winter months.

Line 1131 -- "Recent growth rates are near or at recovery goals for some SRKW pods (Section 3.0, Status and Growth Rates)."

EW: I think there's some confusion here. What I think you're referring to are the recent growth rates of females that I calculated (basically an annual lambda, quantifying the expected replacement of females by females) - e.g. Figure 3.2 in your report. The recovery goals include males though, so when we do that, the growth rates become lower, and are not near recovery goals (remember, in doing population projections, I estimated the chance of meeting recovery goals to be small).

Lines 1261:1272:
EW: Just to avoid confusion, I think it's important to note that the posterior distribution of lambda represents (1) the expected growth rate, not the realized growth rate, and (2) only includes females. The recovery criteria are based on the realized growth rate of the whole population.

Lines 1213-1218:
EW: In discussions with the entire panel after workshop 2, I discussed this issue in detail. They asked for, and I sent an updated figure that represented 95% CIs on the data - not the mean, depicted in the figure they describe - but for some reason, the new figure I made for them did not make it into the draft report. I'd recommend using the new figure.

line 1106:
EW: See my comment about line 290 and marine mammal abundance. There are better methods to do this kind of analysis that will allow for gaps in time series.

line 1240:
EW: I've already examined / tested for trends in sex ratio at birth, and the best model (random) continues to be supported.

Line 1278 ".. suggests the population may have always been Rare-Uncommon." It is possible the population has always been small. However, some published genetic analyses (e.g., Ford et al. 2011) have suggested the population may have been much larger in the not too distant past (hundreds of years). This is one area where more genetic data (currently being collected and analyzed) are likely to be quite informative.

line 1300: "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"

EW: I think it's worth re-iterating that while we may continue to collect information about births and deaths, it won't help identify the mechanism responsible for population fluctuations, or stocks that are limiting or important. Although it's counterintuitive – collecting more data may actually increase our uncertainty about the importance of relative stocks.

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