Independent Scientific Advisory Board
for the Northwest Power and Conservation Council,
Columbia River Basin Indian Tribes,
and National Marine Fisheries Service
851 SW 6th Avenue, Suite 1100
Portland, Oregon 97204

Memorandum (ISAB 2011-5) **October 14, 2011**

To: ISAB Administrative Oversight Panel
Bruce Measure, Chair, Northwest Power and Conservation Council
Paul Lumley, Executive Director, Columbia River Inter-Tribal Fish Commission
John Stein, Science Director, NOAA-Fisheries Northwest Fisheries Science Center

From: Rich Alldredge, ISAB Chair

Subject: Review of the Comparative Survival Study (CSS) 2011 Draft Annual Report

Background

The Northwest Power and Conservation Council's 2009 amendments to the Columbia River Basin Fish and Wildlife Program call for the continuation of the fish passage related functions currently conducted by the Fish Passage Center. The primary functions are to provide technical assistance and information to fish and wildlife agencies in particular, and to the public in general, on matters related to water management, spill, and other passage measures. The Program also calls for the Fish Passage Center's Oversight Board to ensure that the functions are implemented consistent with the Program. To do this, the Program specifies that the Oversight Board will work with the Center and the Independent Scientific Advisory Board (ISAB) to organize a regular system of independent and timely science reviews of the Center's analytical products.

The Oversight Board, ISAB, and FPC director have established guidelines for this regular review. The guidelines specify that a subgroup of the ISAB will "initiate an examination of the FPC and CSS draft annual reports when these reports are released for public comment. As part of the examination, the subgroup will look at the annual reports to ensure that work products, methodologies, and analyses appropriate for potential science review have been considered." The guidelines also include criteria for identifying FPC analyses/products for ISAB review. These criteria include the introduction of new or novel analyses; new conditions or data that bring old analyses into question; and/or situations where consensus cannot be reached in the region on the science involved in the product.

Summary

The ISAB once again acknowledges the continuing improvement in the organization, clarity, and writing quality of recent annual reports, as exemplified in the 2011 CSS Annual Report. The report contains many very useful tables and figures that enhance understanding of complex results. The ability to address how changes in the river environment affect juvenile salmonid migration rates and survival continues to improve as the dataset includes more years and a wider range of environmental conditions. The long time series in survival rates by species, stock (hatchery/wild), and watershed is appreciated.

The ISAB members who attended the CSS Annual Meeting April 7, 2011 would like to acknowledge the very useful exchange of information that took place.

This ISAB review begins by suggesting a few topics for further review, then provides general comments on the content of the 2011 CSS Annual Report, and finally follows with a few specific editorial suggestions.

1. Suggested Topics for Further Review

If sufficient information is available please:

- Consider the influence of mini-jacks on SARs.
- Consider the effects that differential harvest could have on the interpretation of hydropower, hatchery, and habitat evaluations.
- Consider the extent to which PIT-tag shedding and tag-induced mortality varies with species, size of fish at tagging, tagging personnel, and time after tagging.

2. General Review Comments

The detailed Table of Contents is very useful for guiding the reviewers and readers through the manuscript. Inclusion of a Glossary of Terms, which includes acronyms, in the 2011 report is much appreciated.

Overall, the presentation is well organized and well refined. In general, the introductory material and results reviewed are very good. The discussions of implications of results could benefit from more interpretation and explanation.

An overarching comment, repeated from the ISAB 2010 review, is that connections of the migration and survival results with larger ecological concerns are not apparent. Ecosystem issues are mentioned rarely, or not at all. It would be beneficial to more frequently collaborate with researchers working on studies of other species, food webs, habitat, physiology, contaminants, and disease. Such combined studies might give added insights into mechanisms causing the observed temporal patterns in migration and survival. Although extensive interpretation of these issues is perhaps beyond the scope of the reports, more interpretation would provide useful context for results.

Chapter 1. Introduction

The chapter provides a good, fairly detailed overview of the program history and general methods, including information on how methods and the program have changed over time. There is some redundancy, but some redundancy in a detailed data intensive report is welcome. In this introductory chapter it would be worthwhile to briefly describe what each of the subsequent chapters will cover. For example, potential bias in PIT results is not discussed until Chapter 6.

Page 12, Line 25: Are the three passage routes for smolts also the same for the mainstem Columbia dams? If so, include those along with routes through the Snake River dams. If not, mention how they differ.

Page 13, Lines 7-9: The explanation of why TIR does not provide a direct estimate of delayed mortality is not very clear. This paragraph might be a good place to clarify the use of and any difference between the terms “delayed” and “latent” mortality.

Page 14, Lines 20-21: The first explanation of T_x is not very clear. Please revise.

Page 16, Line 19: Readers unfamiliar with the terms would appreciate a brief definition of “A and B runs.”

Page 16, Lines 30-32: Steelhead may delay emigration for a year or two after tagging. It is good that this behavior is recognized and that size data are used to minimize the problem. However, given the reported protocol, what proportion of the tagged steelhead delay emigration, and how many fish would be staying in the river for another year or so after accounting for subsequent mortality? Could this number be estimated from PIT-tag recoveries among juvenile fish recovered a year or so after tagging?

Use of 90% confidence intervals to balance Type I and Type II errors is mentioned on page 15. Some justification for this level of confidence based on the relative “cost” associated with Type I and II errors should be provided or referenced.

The ISAB encourages continued extension of cooperative tagging efforts of wild Chinook and steelhead in the Upper Columbia basin to aid in providing useful information to fill gaps in current knowledge.

Chapter 2. Annual metrics of juvenile survival, arrival time, and migration rate

This chapter provided a concise and well-organized summary of migration and survival metrics and their results. Overall, the presentation of results was clear, but more interpretation, as identified below, would have given context to the results.

When interpreting results please consider that the very narrow emigration window for sockeye depicted in Figure 2.11, in comparison with the wider window for Chinook and steelhead, may be a result of fundamental differences in life histories. The sockeye may all be smolts, emigrating without doubt to the ocean. Their faster migration rate is also consistent with this idea. In contrast, a somewhat smaller fraction of the other species may be emigrating to the ocean and thus have a more protracted pattern and slower migration rate.

The term emigration rate as used is clear enough, but it is a bit deceptive in that it does not really measure a rate. That is, the fish may move faster or slower at portions within sections, and these differences may have impacts on survival. Would a term such as, time between dams, be more appropriate?

Fig 2.1 and others – y axis label "density" is somewhat misleading so please consider using "numbers recovered" instead.

Page 29, Lines 29-30: The statement, "Most wild Snake River PIT-tagged Chinook passed BON at a later date and had a more protracted emigration than those originating from the John Day River" does not seem consistent with Page 30, Lines 3-5: "The John Day River arrives at Bonneville at a similar time compared to Snake River groups …"

Page 32, Figure 2.8 (and others): Deschutes hatchery Chinook show a real spike compared to others. Is this a function of hatchery release timing? More interpretation would be useful.

Page 33, Figure 2.9: given the differences between hatchery and wild steelhead, is it advisable to lump the data, as has been done in this plot?

Page 36, Note on Fig 2.12: Cath, Imna and Mcca – migration rates of Chinook from these three hatcheries apparently increased progressively over years, but not so for other hatcheries. Some interpretation of this phenomenon should be provided.

Page 37, Line 14: "Snake River groups had an increased emigration rate in the lower reach than in the LGR-MCN reach. In many cases, emigration rates for the Snake River groups were higher than for lower river groups." Was this perhaps because lower river groups were spending more time rearing in the lower river?

Page 39, Line 8: "The steelhead migration rate was higher in the lower river than in the LGR-MCN reach." Does that suggest rearing in that reach?

Chapter 3. Effects of the in-river environment on juvenile travel time, instantaneous mortality rates and survival

One of the major objectives of the CSS is to better understand how changes in the river environment affect juvenile salmonid migration rates and survival. The ability to address this question continues to improve as the dataset includes more years and a wider range of environmental conditions. This chapter updates earlier attempts to model the effects of key environmental variables on travel time and survival.

Chapter 3 includes updated versions of the excellent graphs developed in earlier CSS reports showing fish travel times, instantaneous mortality rates, and survival rates (observed values and values predicted from the selected models) for each stock, each migration reach, and each in-season study interval over all study years. Modeling results are reported in the text, but no summary tables are provided for AIC analyses. Tables reporting AIC values for alternative models would be appropriate here. It would be an interesting challenge to develop a method for graphical presentation of AIC analysis results when a number of analyses (in this case, for multiple stock-migration reach combinations) are reported.

A greater synthesis of findings in Chapter 3, as influenced by Chapter 4, would be useful. For example, an interesting conclusion stemming from Chapter 4 analyses is that transportation has not benefitted, or is detrimental to, salmon when inriver survival exceeds 55%. The obvious question is, what conditions lead to inriver survival greater than 55%? While there is some discussion of key survival factors in Chapter 3, it would be interesting to use the data and quantify the levels of key factors needed to achieve 55% survival. For example, what is the interaction between percent spill and river flow needed to achieve 55% and how does this vary as the migration season progresses?

Another table summarizing results by reach and species showing independent variables included in the best fit model with sign of the coefficient and level of significance for each term is strongly recommended. The table might include alternative models in the case where AIC values did not differ much from the selected model.

Page 60, Line 15: Was the bootstrap method considered for estimating variance?

Page 61, Lines 13-16: Derivation of the Box-Cox method is based on variance heteroscedasticity so the method was first designed to correct violation of the equal variance assumption. Nevertheless, application of the method often selects transformations that improve normality as well so it is sometimes identified as a method to improve the validity of the normality assumption.

Page 61, Line 18: Were models containing interactions of independent variables considered? If so, how did they perform? If not, please justify.

Page 62, Line 18: Why was reduced FTT so large for subyearling Chinook but not other species?

Page 63, Line 8: Usually hatcheries release relatively large smolts, in part because they want the fish to emigrate quickly. But here, hatchery steelhead took 2 days longer to migrate than wild steelhead. Was size of hatchery steelhead, presumably age-1, smaller than wild steelhead, which are typically older than age-1?

In Table 3.1, r2 values are presented as proportions of variation explained in relationships characterizing yearling and subyearling Chinook, steelhead and sockeye but r2 values are meaningful only for linear models including an intercept term. Do all models contain an intercept term? Specifically, is the model for survival a linear model with an intercept term?

Page 70, Lines 24-26: “We see the only way to resolve the remaining questions is to invest in more PIT-tagging efforts for reducing the uncertainty in the lower reach.” It would be useful to mention other ways of reducing uncertainty that have been considered in the region and elsewhere and explain why these ways are not suitable.

Chapter 4. Annual SAR by Study Category, TIR, and D for Snake River Hatchery and Wild Spring/Summer Chinook Salmon and Steelhead: Patterns and Significance

This chapter is generally well written and the inclusion of caveats, such as on page 96, is appreciated.

Page 73, Line 38: PIT-tagged study groups should mimic the experience of non-tagged fish they represent. However, as discussed below in this review, new evidence by Knudsen et al. (2009) suggests that PIT-tagged fish may experience higher mortality than CWT fish or untagged fish. More research is needed to quantify differential survival and the degree it varies with size and species of fish.

Page 75: The table with symbol definitions was very useful in understanding the equations throughout the next few pages. However, it does not contain many of the symbols that occur later in the discussion on adults (pages 81-83). An expansion, or second table, would be useful.

In the table:
It would be useful to define how A codes 0 or 1 and show, perhaps with brackets, how X_{1A2} comprises components X_{102} and X_{112}, whereas X_{1AA2} comprises X_{1002} … X_{1112}

d_0 is defined as the “***site-specific*** removal at dams…” but isn’t it the “***total*** removal at dams…”?

Page 77, Lines 20-23: Please clarify that this procedure applies only to 2001.

Page 77, Lines 25-32: It would be worthwhile to provide a table showing the estimated percentage of the migration that went undetected each year (C_o).

Page 79, Figure 4.1: The figure is very helpful, and should probably appear earlier in the section. It would help to show, perhaps by encircling with a dotted line, how CRT comprises the components T and R.

Page 81, Lines 13-14: Please indicate what proportion of returns are mini-jacks, and therefore have been excluded from the analysis.

Page 81, Line 15: Mini-jacks can only be identified among those maturing salmon that begin to migrate upriver through a dam with PIT detection (or by blood samples but they do not do this at dams). Mini-jacks are excluded from PIT-based SARs. Is there possible bias associated with mini-jacks that are collected for transportation versus those left in the river to migrate? Some recent data suggest that mini-jacks may exceed 50% of male spring Chinook in some hatcheries; therefore, differential detection of mini-jacks among transported versus inriver fish could cause significant bias.

Equation 4.7: The terms AT_{LGR}, AT_{LGS} and AT_{LMN} are not defined in the table, so it is not obvious whether these terms refer to adult detections at each dam (in which case, can adults detected at more than one dam be counted more than once?) or to locations of smolt collection for adults that were later counted as returns (in which case, where were the adults counted?).

Page 97: A discussion of what factors might contribute to the reported differences in TIR and D among the various hatcheries would be useful. Is it related to hatchery practices, river environment below the hatchery, or something else? Is there evidence to suggest why transportation is better for summer versus spring Chinook? Addressing these questions is an important step now that a number of years of data have been gathered.

Page 108, Lines 1-3: Sockeye salmon, which typically rear in calm environments, such as lakes, are much more susceptible to handling stress caused by tagging operations compared with Chinook, coho, and steelhead. Yearling sockeye are typically smaller than yearling of these species and may be more vulnerable to somewhat large PIT tags. Given the new evidence provided by Knudsen et al. (2009), discussed below in this review, on the possible effects of PIT tags on survival of Chinook salmon, it would be worthwhile to investigate whether PIT tags cause increased mortality in sockeye salmon and the degree to which differential mortality may vary with fish size, tagging crew, and year.

Page 108, Table 4.25: How do the SAR values for tagged sockeye compare with SAR values based on total smolt counts and subsequent adult returns, that is, run reconstruction estimates?

Page 110, Lines 9-11: The statement “a variety of operational and environmental factors during the smolt outmigration would only have affected the in-river SARs and thereby the resultant TIRs in 2006 to 2008” prompts readers to expect a conclusion about how TIRs changed after 2006. Can such a conclusion be added?

Chapter 5. Adult passage success rates between dams, D, and the expression of delayed effects

Quantifying the efficacy of transportation is one of the foundational goals of the CSS. This year the CSS was requested to add success rate by adult return year because this rate is of interest to managers so data for return years 2003-2010 is presented in this report. The CSS study data are designed to apply to management questions, including hydropower operations, hatchery evaluations, and habitat evaluations, but the effect of harvest is not emphasized. Differential harvest effects could be confounding influences that affect interpretation of hydropower, hatchery, and habitat evaluations.

Results from Table 5.2 show results indicating transportation had a significant negative effect on adult success in 13 cases, a significant positive effect in one case and no significant effect in 55 cases. A discussion of what factors influence these differing results would be useful including why there is no statistically significant result in 80% (55/69) of the cases presented in the table.

Page 118, Line 12: It is not clear why a constant tagging rate is a primary assumption of this approach. Please explain.

Page 126, Lines 8-14: The results show some cases have a positive regression coefficient for temperature and other cases have a negative coefficient for temperature. A discussion of this apparent inconsistency should be provided.

Page 132, Lines 10-27: The interpretation in the first paragraph, where the adult success rate differential is 92%, is different than in the second paragraph, where the adult success rate is 93%. This apparent inconsistency in interpretation should be resolved.

Page 133, Conclusions: Are the estimated differences in straying rate sufficient to account for the estimated differences in success rate?

If the adult fish that were transported are straying more than in-river migrants, as indicated, are they also more likely to be vulnerable to fisheries during their upriver movement? Are there any data to evaluate this possibility? How reliable is the harvest data from the tribal fisheries between BON and MCN, to what extent is the catch in Zone 6 and Zones 1-5 below Bonneville sampled for PIT tagged salmonids, and can such data be used to contribute to this comparison? For example, Figure 5.5 shows that most of the strays were last detected at the lower dams, suggesting that they may remain in these areas, and perhaps be caught at a higher frequency in tribal fisheries

Page 140, Tables 5.3-5.6: Include +/- signs for regression coefficients in the models as well as p-values to indicate the direction and level of significance for each variable in the model.

Chapter 6. Patterns in Annual Overall SARs

This is a good, important, and well-written summary chapter. The long time series in survival rates by species, stock (hatchery/wild), and watershed is appreciated. This chapter discusses some critical potential bias issues associated with PIT-tag survival rates (and RR survival) that were raised in previous chapters, so it may be worthwhile for the Introduction to provide a more complete explanation of Chapter coverage.

Page 146, line 38: "Estimates of *S.oa* and *S.o1* can then be used to evaluate ocean and smolt migration factors that may influence ocean survival as called for in the Fish and Wildlife Program (NPCC 2009)." It is unfortunate that apparently analyses cannot estimate survival from BON to the seaward end of the estuary (Astoria). There is no discussion of, or reference to, studies that have investigated that aspect (especially McComas et al. 2008)

Page 147: The description of wild Snake River Chinook SARs does not mention the race of Chinook salmon. Are these SARs based on total smolt production and total adult returns of all races (spring, summer, fall)? If so, please mention this, as it is important when comparing wild and hatchery survival rates. If not, please explain how race of wild Chinook smolts is identified in the estimates based on run reconstruction and PIT tags.

Page 148, Line 14: Does reliable evidence exist to explain why SARs of summer Chinook tend to be higher than spring Chinook?

Page 150, Line 11 -. Snake River wild steelhead and wild Chinook SARs were highly correlated (0.72) during the 1964-2008 period when aligned by smolt migration year. Does this suggest an estuary/ocean effect or a common effect in freshwater that propagates through to ocean/estuary? Are there data to support an interpretation?

Page 153, Line 4: Please report the geometric mean of Yakima wild Chinook, if it is available, so that reported values will be consistent.

Page 153, Line 17: The correlation of SARs between regions should be discussed in more detail to better understand the implications.

Page 156, Line 9: The statements such as this identifying new analyses that CSS plans to do in the upcoming year are appreciated.

P. 161. The comparison of SARs based on PIT tags and run reconstruction (RR) is informative. The two studies suggest that SARs based on RR are 19% or 38% higher (geometric means) than those developed from PIT tags. In a given year, this difference was as much as 57% (CSS Figure 6.11). The discussion of sources of bias (e.g., tag loss and increased mortality) is good, but the authors conclude that much remains unknown. Given this conclusion, the CSS report should highlight a recommendation to further investigate factors causing bias in PIT-tag metrics. Potential PIT-tag bias is a critical topic for the Fish and Wildlife Program and additional research is needed to determine the extent to which PIT-tag shedding and tag-induced mortality varies with species and size of fish during tagging, tagging personnel, and time after tagging.

It is noteworthy that the RR approach to SARs probably includes mini-jacks in the mortality category because mini-jacks are not identified at release and mini-jacks are not counted in the adult returns. In contrast, SARs based on PIT tags reportedly exclude mini-jacks that are detected as they migrate back up the adult ladders (P. 81). Exclusion of tagged mini-jacks would cause SARs based on PIT tags to be higher than RR estimates. Therefore, the reported PIT tag bias (low SARs) may be even greater than that estimated by the previous two investigations and as shown in Figure 6.11 of the CSS report. The influence of mini-jacks on SARs needs more attention and research.

The CSS evaluated untagged fish releases associated with the PIT fish releases (p. 14). Could high variability in mini-jacks produced by each hatchery (or in the wild) influence extrapolations of PIT-tagged fish to untagged fish?

Page 163, Line 24: "Similar factors during the smolt migration and estuary and ocean life stages appear to influence survival rates of Snake River wild and hatchery spring/summer Chinook populations, based on our evaluation of trends in SARs for the wild and hatchery groupings." This is an interesting finding. Do data exist to show how the differences in habitat use shown by wild and hatchery fish in freshwater may or may not influence SARs?

Page 165, line 15 "In addition, evidence of positive covariation in mortality rates between the freshwater and subsequent marine-adult life stage for each species, suggests that factors affecting mortality in freshwater partially affect mortality during the marine-adult life stage (Haeseker et al.)." A discussion of these factors would be useful.

Chapter 7. Patterns of variation in age-at-maturity for PIT tagged spring/summer Chinook salmon in the Columbia River Basin

This chapter examines three questions: (1) Is there is a relationship between smolt-to-adult survival (SARs) and age-at-maturity, (2) Do juveniles transported from Snake River dams differ in age-at-maturity from those that migrate in-river, and (3) Are there common patterns of interannual variation in age-at-maturity for stocks originating from different locations in the Columbia Basin?

Chapter 7 is concise and states results clearly. As elsewhere in the report, details on the results of statistical analyses (sample sizes, F-values, and probability values) are not provided in the text or in appendices. An ANOVA table to support Figure 7.1, perhaps in an online appendix, would be useful. The very brief discussion is unclear with regard to the possible application of these results to management. The leap from the Results (where significant among-stock and among-year differences in age-at-maturity are reported) to the Discussion's focus on jacking-rate differences between stocks needs considerably more explanation to fill in the intermediate steps. Mention of some of the various environmental and biological factors that could produce some degree of synchrony in age-at-maturity of multiple stocks would be of interest here.

The observation that "there is evidence that some annual factor is influencing mean age-at-maturity across the Columbia River Basin Chinook salmon stocks" is an important finding. Does CSS have further plans to investigate factors influencing age-at-maturity?

Page 183, Lines 39-46: Evidence should be presented that statistical assumptions such as normality, model linearity, and homogeneity of variance were checked and found valid.

Chapter 8. Snake River Fall Chinook

Pages 194-195: More detail on how "holdovers" would affect the estimation of SARs and TIRs would be useful. The paragraph addressing this point (page 195, lines 21-25) should come earlier and the explanation should be edited for clarity.

Page 196, Lines 21-24: It would be worth noting that this procedure leads to an inherent bias in the calculation of H_p in that all fish coded 1 (detected as holding over) must have held over, but fish coded 0 may also have held over but gone undetected. In other words, H_p will be biased low to some extent. Presumably the error distribution in equation 8.1 is assumed to be Normal, but how might that distribution and the resulting statistics for comparing the fit of different models be affected by the possible underestimation bias?

Page 197, Line 28 (and tables 8.4 and 8.5): Only two models are mentioned and compared in the results, but it is stated or implied in the methods (lines 38-43, page 196) that ***four*** models were considered and compared.

The reason for the rather elaborate modeling effort is not clear. Although it is of value to confirm that late-release and smaller fish have a higher probability of residualizing for a year, application of the procedure does not seem to have been done in the "demonstration" with 2006 migration data. This description is given on page 195, "Once holdover probabilities can be modeled, then those groups and/or individual fish within groups with relatively high probability of being detected as holdover can be removed from consideration for use in estimating SARs using the CSS methodology." Holdover fish were simply identified from PTAGIS data and removed from the database. More explanation of the utility of the modeling effort would be beneficial.

Page 207, Discussion: It would be helpful to include some discussion of how holdover rate has been affected, or would be predicted to be affected, by the significant trend towards earlier release of fall Chinook from hatcheries (Figure 8.7). The modeling results predict that earlier release would reduce the holdover rate if size-at-release were constant. Has size-at-release remained constant, and if not, how would the predicted holdover rate be affected by the actual trade off between size and release date?

3. Editorial Comments

Pages 19-20
Tables 1.2 and 1.3 refer to groups marked in 2011 and *2010*, respectively. Is this an error?

The legend for Figure 2.11, "Timing of PIT-tag detections at Lower Granite dam for sockeye during 2009-2010 (<u>top two panels</u>) and BON (<u>bottom two panels</u>). Plots are for Oxbow and Sawtooth hatcheries" is not correct in describing content of the top and bottom panels.

Page 23, line 27 "We divided the hydrosystem into two reaches for summarizing survival and migration rate: LGR-MCN and MCN-BON. We used Cormack-Jolly-Seber (CJS) methods to estimate survival rates through the two reaches based on detections at the dams and in a PIT-tag trawl (TWX) operating below BON (Cormack 1964, Jolly 1965, Seber 1965, Burnham et al. 1987)." In effect, isn't this three reaches?

Page 37, Lines 15-16: Delete the first "faster."

Page 57, Line 4: "thee" should be "three."

Page 58, Lines 34-43: Survival results could be presented in a more understandable manner in a table.

Page 76: Equation 4.2 refers to R, but it presumably should be R_1 as in the table?

Page 77, Line 28: Should ">6%" be "<6%"?

Page 82, Lines 22-23: The explanation in the text is potentially confusing. Please consider, "D is the ratio of SARs of transported smolts ($SAR(T_0)$) to SARs of in-river migrants ($SAR(C_0)$ …"

Page 96, Table 4.12: Values for "combined A-run" appear not to have been computed yet and are shown as zeros. These are presumably placeholders.

Page 112, Figure 4.10: It would be helpful to label points with years, at least for extreme points, because 2001 appears to have a lot of leverage in the regressions for both species.

Page 116, Line 9: Specify which estimate. "…but the estimate of detection efficiency will not."

Page 119, Equation 5.5: LHS refers to $NT_{2\text{-salt}}$, $NC0_{2\text{-salt}}$ and $NC1_{2\text{-salt}}$ but RHS refers to PROP(3-salt). Is this an error?

Page 124, Figure 5.2 and Page 125, Figure 5.3: The legends refer to a line at 0, but 0 is not included in the scale of interest.

Page 126, Lines 1-2: Insert commas in the list of factors for clarity.

Page 127, Lines 15-18: The meaning of the sentence is not clear. Please edit for clarity.

Page 134, Table 5.1, Title Line 5: Change "is" to "are"

Page 127, Line 6: "Figures 5.3-5.4" should read "Figures 5.4-5.5". For context, would be helpful to indicate the overall proportion of juveniles that were transported in the year(s) resulting in the high proportion (88%) of transported fish in the 31 adult strays.

Page 132, Line 16: "…in these years." Clarify which years.

Page 159, Line 10: Delete "changes from"

Page 162, Line 32: Does "at the Columbia River mouth" refer to the NOAA PIT trawl location, the Astoria Bridge or further seaward?

Page 173 Table 6.13: presumably "LGR-to-BOA SARs…" should read "LGR-to-**GRA** SARs", consistent with the pattern in previous and subsequent tables.

Pages 199-202, Figures 8.8 - 8.10: Suggest labelling the y-axes in these figures to improve comprehension.

Page 200, Line 9: typo - delete “did”

Page 203, Line 5: “Figure 8.9” should read “Figure 8.11”

Page 204, Figure 8.11: The clarity of this very useful figure would be improved if dates on x-axes were better aligned across successive frames.

Page 205, Line 20: Please quantify “essentially zero” holdovers

Page 205, Line 26: Suggested change “…a total of 55 smolts were detected as holdovers.” to “…a total of 55 adults that had previously been detected as holdovers.”

Pages 206, Lines 6-9 and Figures 8.12 and 8.13: Comparison to spring/summer Chinook is interesting. It would be useful to show the estimated overall SARs for hatchery and wild spring/summer Chinook in the figures.

Page 209, Glossary: AP should be defined and included in the Glossary of Terms.

Page 209, Glossary: The description of delayed mortality ends in mid-sentence.

References Cited

Haeseker, S.L., J.M. McCann, J.E. Tuomikoski, and B. Chockley. 2011 (Accepted/In Press). Assessing freshwater and marine environmental influences on life-stage-specific survival rates of Snake River spring/summer Chinook salmon and steelhead. Transactions of the American Fisheries Society.

Knudsen, C., M. Johnston, S. Schroder, W. Bosch, D. Fast and C. Strom. 2009. Effects of Passive Integrated Transponder Tags on Smolt-to-Adult Recruit Survival, Growth, and Behavior of Hatchery Spring Chinook Salmon. NAJFM 29:658–669.

McComas, R. L., G. A. McMichael, J. A. Vucelick, et al. 2008. “A Study to Estimate Salmonid Survival through the Columbia River Estuary using Acoustic Tags, 2006.” Report by Fish Ecology Division, Northwest Fisheries Science Center and Pacific Northwest National Laboratory (PNNL). Portland, OR: US Army Corps of Engineers and Seattle, WA: NOAA, National Marine Fisheries Service. www.nwfsc.noaa.gov/assets/26/6914_05082009_161212_Acoustic-Tag-2006-Accessible.pdf.