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Review of the Comparative Survival Study Draft 2015 Annual Report

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Review of the Comparative Survival Study (CSS) Draft 2015 Annual Report

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I. Background

The Northwest Power and Conservation Council's 2009 amendments to the Columbia River Basin Fish and Wildlife Program called for a regular system of independent and timely science reviews of the Fish Passage Center's (FPC) analytical products. The 2014 Program continues to call for these reviews, which include evaluations of the Comparative Survival Study's draft annual reports. These ISAB reviews began five years ago with the evaluation of the CSS's draft 2010 Annual Report (ISAB 2010-5), followed by a review of the draft 2011 Annual Report (ISAB 2011-5), the draft 2012 Annual Report (ISAB 2012-7), the draft 2013 Annual Report (ISAB 2013-4), and most recently the draft 2014 Annual Report (ISAB 2014-5). This ISAB review of the draft 2015 CSS Annual Report is the ISAB's sixth review of CSS annual reports in response to the Council's 2009 Program.

These reports are extremely valuable to many stakeholders in the Basin for many different purposes, and the data collection and reporting should be continued.

II. Summary

This ISAB review begins with an overview of the latest report (this section); then moves to suggesting topics for further CSS review (Section III), general comments on each chapter of the 2015 CSS Annual Report (Section IV), and ends with specific queries and suggestions (Section V).

The annual CSS report is a mature product, typically including only updates with the latest year of data and expansion of analyses as more data are acquired. Many of the methods have been reviewed in previous ISAB reports and so now receive only a cursory examination. As more data are acquired, new patterns and questions arise on the interpretation of the results – this is now the primary focus of our reviews. The ISAB appreciates the detailed response of the CSS to suggestions provided in previous reviews. We do not expect that the CSS would necessarily respond immediately to new requests for further analyses.

Chapter 1 is similar to previous years with the 2013 results added. The report now disaggregates results for Snake River wild steelhead and Chinook to the major population group (MPG) level. The results in Chapter 1 appear to show a natural experiment involving the effects of mainstem discharge on salmon survival with the proportion of downstream transportation of juveniles and proportion of spill being roughly constant since 2006, but total flow in the river differing among years. The ISAB suggests that focused analyses (graphs and formal analyses) be conducted on this natural experiment to test statistical relationships between flow and salmon population parameters such as survival, smolt-to-adult-return rate (SAR), and other response variables.

In Chapter 2, an updated Life Cycle Model (LCM) is presented that relates juvenile survival of transported and untransported cohorts of juvenile fish and a PIT-tag based indicator of powerhouse passage. This is useful refinement of the LCM, and the ISAB looks forward to additional refinements such as the inclusion of additional terminal areas and further partitioning of survival. The LCM was used to investigate the long-term impacts of increased salmon productivity and reduced dam effects on the long-term persistence of salmon populations. It would be helpful to reverse this analysis and evaluate what combinations of increased productivity and reduced dam contact might lead to sustainable populations.

Chapter 3 is mainly an update with the latest information on in-river effects on juvenile travel time, instantaneous mortality, and survival. A key finding is that there is large variation in the results among years and among cohorts. The variation among years is understandable; the variation within a year less so. Many figures (e.g., Figure 3.2) show a consistent pattern in fish travel time and survival over cohorts as the year progresses. The discussion lists potential explanations for the effect of "day." Can planned or natural experiments be designed to distinguish among these hypotheses and is it worthwhile to do so? For example, do these relationships provide information on optimal timing of releases for hatchery fish? It is also not clear if these cohort effects continue to the final performance measures (e.g., SARs). Data are clearly too sparse to investigate this question for individual cohorts, but can a more gross separation be used (e.g., a simple split of cohorts into two parts – early vs. late)?

Chapter 4 described overall annual SARs and was updated with new data; details are presented in Appendices. Additionally, the authors investigate relationships between SARs and salmon population productivity (return per spawner) and inter-stock correlations among SARs. These chapters and appendices will continue to expand over time. Is there a better way to present the results than in an ever-expanding set of graphs and sets of tables? As indicated in the report, different SAR objectives will require different accounting locations (e.g., finer geographic locations) and methods (e.g., for persistence at local or basin levels). The current tables and plots are generally well done, but as can be imagined, this chapter could become overwhelming. The ISAB suggests that consideration be given to how to present these results in the future to best serve the various stakeholders. Perhaps an electronic report that can be customized for a particular interest group may be more useful than a static paper document?

The CSS report could then focus on unexpected findings or relationships (such as that between SAR and productivity or inter-cohort relationships).

Chapter 5 is mostly an update on Snake River subyearling fall Chinook. As with Chapter 4, some consideration is needed on the best way to present an ever-increasing amount of data so that the results of the data analyses are useful for stakeholders. This chapter also includes a new power analysis indicating how much additional tagging is needed for fall Chinook above Lower Granite Dam (LGR). Is it feasible to tag the required number of fish? Also, these additional tagged-fish will provide added information on down-river detection rates – does this lead to improvements elsewhere in the CSS domain of study?

We understand that Chapter 6 in the 2014 report (PIT-tag versus CWT survival estimates) is currently in preparation for the next iteration of the CSS report. Rather than report on incomplete analyses, it was removed from this year's report. We look forward to its inclusion next year. Evaluating potential bias in survival rates of PIT-tagged fish is an important topic.

III. Suggested Topics for Further Review

In 2013, we recommended these topics (ISAB 2013-4, Page 1):

1. Hypotheses on mechanisms regulating smolt-to-adult survival rates (SARs)
2. Life-cycle modeling questions and Fish and Wildlife Program SAR objectives
3. Data gaps
4. Rationalization of CSS's Passive Integrated Transponder (PIT)-tagging, and
5. Publication of a synthesis and critical review of CSS results

In 2014, we recommended these topics (ISAB 2014-5, pages 2-3):

1. Hypotheses on mechanisms regulating smolt-to-adult return rates (SARs) [update from 2013 review]
2. Life-cycle modeling questions and Fish and Wildlife Program SAR objectives [update from 2013 review]
3. New PIT/CWT study to further investigate differential survival among these tag types

The CSS group has incorporated many of our suggestions into the current document. For example, the current report has a substantial discussion of correlations among SARs from different regions or effects of transport on SARs (#1 in 2013; #1 in 2014). The life cycle modeling now allows for variation in stream productivity and hydrosystem survival and simulates the correlative impacts of these changes on predicted future population abundances

(#2 in 2013; #2 in 2014). Members of the CSS have now published many peer-reviewed articles synthesizing the results (#5 in 2013). The ISAB appreciates the CSS efforts to respond to our queries which in turn leads to further questions as noted below.

However, some of the recommendations from the ISAB appear to be beyond the scope of the CSS. For example, the ISAB identified several data gaps such as fish body mass metrics (#3 in 2013), but limited resources and questions about which agencies should collect this information have prevented acquisition of these data. The CSS expends considerable effort to coordinate PIT-tagging in the basin with other groups but does not feel that it is the appropriate body for a full rationalization of the PIT-tagging effort (#4 in 2013) along the lines recommended by the IEAB report (2013-1). Resolution of these issues may require higher-level policy discussions among the stakeholders in the Basin. The update of Chapter 6 from last year (#3 in 2014) has been deferred until the next report.

In 2015, we recommend the following four topics for future reports:

1. Use SAR data to examine both intra- and interspecific density dependence during the smolt out migration and early marine periods.

ISAB (2015-1) found relatively little direct testing of density dependence during the smolt outmigration period when many natural and hatchery salmonids may co-mingle as they migrate toward the ocean. Would it be possible to use CSS SAR data to examine both intra- and interspecific density dependence during the smolt out migration and early marine periods? The potential for compensatory density dependence was suggested by bioenergetic estimates of numerous prey consumed by spring/summer Chinook as they migrate from Lower Granite Dam to Bonneville (ISAB 2011-1). Also, there was some evidence of depensatory mortality of smolts in the Snake River caused by foraging birds (see Fig. VII.1 of ISAB 2015-1).

2. Propose actions to improve SARs to pre-1970s levels.

The Chapter 4 Discussion provides a good summary of key information, leading to the conclusion that pre-harvest SARs of ~4-7% are needed to improve productivity to pre-1970s levels. Given the range in observed survivals at sea (S.oa), to what extent might actions in the mainstem Columbia and Snake rivers allow this to occur? What are the key actions predicted to influence survival to reach SARs of 4-7%? For example, can the Life Cycle Model simulation study be run in “reverse” to help evaluate the relative benefit of alternative management actions? These evaluations might support an actual test (like a spill-experiment).

3. Explore additional potential relations between SARs and climate and ocean conditions

The authors should consider further exploration of potential relationships between SARs and indices of climate and ocean conditions that have not been previously evaluated by CSS, e.g., the North Pacific Gyre Oscillation (NPGO; see Kilduff et al. 2015; Miller et al. 2014), NOAA local biological indices (e.g., copepod biodiversity, northern copepod anomalies, biological spring transition, winter ichthyoplankton, juvenile catch-June), and Alaska Marine Ecosystem indicators. Similarly, can methods similar to Chapter 2 be used where years that are "similar" to those expected under future climate change scenarios are used to simulate the predictions for survival, SARS, and other population parameters under future climate scenarios.

4. Consider ways to explore the variability of inter-cohort responses

Finally, the CSS report has studied effects on the "mean" response to various factors. For example, fish travel time reductions improve "mean" survival, but as noted, there is high inter-cohort variability. Perhaps a lower average survival with less inter-cohort variability would be more beneficial, i.e. a more robust response. Are there management actions that could reduce this intra-cohort variability? The current time series is now approaching a length where this could be investigated in the future. Some planning is required to ensure that data collected now are stored in a format that will be suitable for future investigations along these lines.

IV. Comments on the draft CSS 2015 Annual Report by Chapter

Chapter 1. Introduction

This chapter is similar to previous years with the 2013 results added. The report now disaggregates results for Snake River wild Steelhead and Chinook to the MPG level.

It is interesting to note (Figure 1.6) that 2013 had a high spill proportion (around 40%) with a low discharge and is similar to conditions in 2010 and 2007. Since 2006, spill percentages have been relatively constant, the proportion of juvenile salmon transported has been relatively constant, three of the years have been low total flow years, and five of the years have been higher total flow years. This appears to be a natural experiment (but power may be limited with only 8 years of data when spill was mandated) on the impact of total flow on survival. The ISAB suggests that some focused analyses (graphs and formal analyses) be done on this natural experiment to see if the results provide some empirical evidence the association between year-to-year total flow and salmon population parameters such as survival, SAR, and other response variables.

Chapter 2. Life cycle modeling approach to estimating in-river and early ocean survival

This chapter presents an update on Life Cycle Modelling development. The model now (1) increases separation of life stages, including juvenile migrants split into transported and untransported fish, which allows transported and untransported fish to have different survivals once they enter the ocean; (2) adds a PIT-tag based indicator of powerhouse passage, (3) adds an index of the proportion of fish transported and of water transit time through the hydrosystem, and (4) integrates empirical in-river survival and SAR data into the statistical fitting procedures. The ISAB is pleased with the progress so far and looks forward to the evolution of the LCM.

The key findings of the CSS analyses are:

(a) survival is sensitive to hydrosystem operations where the power house contact index appears negatively related to survival in both the hydrosystem and early ocean stages.

(b) some MPGs appear to be in the region of limited capacity for production (assuming Beverton-Holt density dependence).

The CSS used the life cycle models to ask two questions about the potential for increases in long term abundance:

(1) What happens with improvements in the productivity in freshwater spawning and rearing (typically as a result of habitat improvements)? This was modelled by varying the productivity from 50 to 250 smolts/spawner.

(2) What is the impact of changes in survival in the mainstem as a function of hydrosystem operations modeled by using values of 50 to 100% of the historical powerhouse contact rate?

Not unexpectedly, improvements in early life productivity are positively correlated with direct improvements in forecasted abundance, but improvements in productivity run into apparent density dependence effects for several watersheds (Imnaha, Minam, Lostine; p.44 and Figure 2.9). Similarly, changes in powerhouse contact rate are associated with (near linear) changes in abundance because density dependence was not modelled in the main stem. For example, according to the LCM, if the powerhouse contact index was reduced to 50% of historical value, a 40-400% increase (!) in returns is predicted (Figure 2.10). In ISAB (2015-1), it was noted that an increase in survival across the lifecycle would increase the number of returning fish which, assuming all else remains the same, moves the equilibrium abundance to the right on the density-dependence curves. Was this shift in the equilibrium also observed in the simulation studies?

Additional commentary about the Powerhouse Contact Index is recommended. Can it be predicted/measured reliably for a cohort of fish given observed or forecasted spill conditions? Does a cohort with 50% having no dam contact and 50% with 1 dam contact experience the same response as a cohort with 75% having no dam contact and 25% with two dam contacts; i.e., is there a non-linear effect of powerhouse contact index at the individual fish level?

When modelling effects of productivity and improvements to hydrosystem survival, the effects of productivity bump into density dependence in several sites. There is support for this density dependence from Figure 2.8 and from studies outside the model, as described in ISAB 2015-1 and the references within. How do improvements in habitat over time feed into the LCM model given that these improvements alter the density dependence relationship?

The association of hydrosystem improvements and subsequent abundance examined in the model are "theoretical" (i.e., not a function of flow or other factors). Can the statistical models be used to propose hypotheses about how much hydrosystem operations could improve salmon abundance; e.g., what levels of spill and/or fish travel time are predicted to lead to "75% of current impact of hydrosystem operations"? Could these hypotheses be tested in a spill experiment?

Will the life cycle modeling effort eventually be extended to include other species and populations? Interior steelhead (separate models for A and B runs) should be a priority given recent lower than expected returns.

The proposed modifications of the LCM tool to add a time perspective and accommodate climate change hypotheses should be pursued. For climate change hypotheses, additional climate variables related to US West Coast/Columbia River Basin salmon survival should be considered, including the North Pacific Gyre Oscillation (NPGO; http://www.o3d.org/npgo/), and local and remote biological indices from the area of known ocean distribution at each life stage.

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

This chapter is mainly an update from previous years. Most of our comments relate to improvements in presentation and/or justification.

Table 3.1 indicates that Fish Travel Time (FTT) is estimated well for individual cohorts, but there is wide range among cohorts. Are the data sufficiently dense to explore the relationships of other variables to this variability in the responses among cohorts? The data are likely too sparse for individual cohorts, but, for example, could cohorts be divided into low, medium, and high FTT (based on simple partitioning) to see if the variability in FTT then translates into similar variability in SARs?

From Tables 3.1 - 3.2, it appears that the biggest issue controlling precision and coefficients of variation (CVs) in two reaches (McNary Dam-Bonneville Dam, MCN-BON; and Rock Island Dam – McNary Dam, RIS-MCN) is sample size and detection rates. We agree that there are tradeoffs in changing both of these metrics. The ISAB agrees that further work is needed to evaluate where spillway detection would be most beneficial for these two particular reaches but also throughout the system. The Independent Economic Analysis Board (IEAB) conducted a general study investigating tagging rates for various locations (IEAB 2013-1) – their expertise could be helpful in this more focused evaluation.

The overall conclusion is that "improvements to fish travel time, mortality rates and survival may be possible through management actions that reduce water transit time (WTT) and increase spill percentages. There are only two approaches for reducing WTT: reducing reservoir elevations and/or increasing flow rates." Some suggestions are provided regarding an experiment to confirm this statement. The CSS is encouraged to continue to discuss such an experiment with the Basin stakeholders.

Chapter 4. Patterns in Annual Overall SARs

Chapter 4 provides very useful analyses that should be updated each year. The ISAB agrees that it is important to continue the CSS time series for both hatchery and wild salmonids for the reasons noted in the report. The data patterns raise many questions, some of which we raise here.

ISAB (2015-1) found relatively little direct testing of density dependence during the smolt outmigration period. Would it be possible to use CSS SARs data to examine both intra- and interspecific density dependence during the smolt out migration and early marine periods? The potential for compensatory density dependence was suggested by exceptionally high prey consumption requirements by spring/summer Chinook in the mainstem river that might lead to density dependent growth (ISAB 2011-1). Alternatively, there was some evidence of depensatory mortality of smolts in the Snake River caused by foraging birds (see Figure VII.1 of ISAB 2015-1).

Recent analyses indicate SARs from smolts originating at Rocky Reach Dam were about 60% of the SARs for smolts originating at McNary Dam or that about 40% mortality is occurring in the upper river reach. To what extent might dam passage versus movement through the reservoirs account for the 40% mortality? We agree that it is important to continue to evaluate SARs from the upper Columbia River, e.g., Rocky Reach Dam.

The ISAB agrees that evaluation of PIT-tag related mortality and tag loss should be a priority research topic, and we look forward to seeing results of the ongoing evaluation.

The Discussion provides a good summary of key information, leading to the conclusion that pre-harvest SARs of ~4-7% are needed to improve productivity to pre-1970s levels. Is this sufficient to enable a self-sustaining natural population at spawning densities that exceed minimum abundance thresholds shown in Appendix Table B.70? Given the range in observed survivals at sea (S.oa), to what extent might actions in the mainstem allow this range in survival to occur? What are the key actions? For example, can the LCM simulation study be run in “reverse” to evaluate what actions may have higher priority? This could provide justification for an actual test (like a spill-experiment).

Chapter 5. Estimation of SARS, TIRS, and D for Snake River Subyearling Fall Chinook

This chapter is mainly an update from previous years. It updated estimates of the effects of holdovers on SAR and other metrics. It also includes a new power analysis that indicated about 40,000 subyearling fall Chinook need to be tagged in the Snake River above LGD to detect a 50% difference in SAR between transported and in-river groups with an 80% power. Is it feasible to tag the required number of fish? Elsewhere in the document, the importance of spillway detectors was noted. If these were installed, would the power analysis change? If this number of fish are tagged, they will provide additional information on down-river detection rates – does this lead to improvements elsewhere in the CSS domain of study?

Appendix A: (SR), SAR, TIR, and *D* for Snake River Hatchery and Wild Spring/Summer Chinook Salmon, Steelhead, and Sockeye

This appendix is an update from previous years. We have no comments other than some consideration should be given on the best way to present these results for different audiences in the future as the number of tables and size of each table continues to grow.

V. Specific Comments on each Chapter

Chapter 2

A key objective of this chapter is to evaluate "the two different alternatives of how population trends could have been different than empirically observed trends," but the chapter never articulates clearly what the two alternatives are. Are these the two "key aspects of the system" mentioned elsewhere in the chapter?

An index (PTRANS) is mentioned here, and it is described as being reported in the 2014 CSS report. How is this index calculated? Referring to the previous CSS report is rather cumbersome for the reader to find where the calculation of this index is described. While it makes the report more bulky, it may be helpful to include technical excerpts from previous reports in appendices to make the documents more self contained.

The chapter investigates a range of changes in productivity and changes to hydrosystem operations. How were these ranges chosen? Is it feasible to make such changes in the current system? Some justification for these ranges is needed.

Figure 2.4. The effect of transportation on ocean survival is only available indirectly, e.g. the readers need to "manually" compare the posteriors for g_H and g_T. It would be easier to show the posterior directly for the difference or ratio of these two values.

Chapter 3

p. 61. The six environmental factors include a linear and quadratic effect of day. All possible subset regressions were used. Some care is needed as models with a quadratic effect of day but no linear effect of day are not very sensible. They presumably will have very low support as measured by Akaike information criterion (AIC) values and will not be suitable fits to the data; they should not be fit as they are unrealistic biological models.

p. 62, Table 3.4 and Figure 3.5 may need some clarifications. Are the "model forms" *MY*, *MY +Day* just composed of those variables as random effects or are the other variables with high relative importance values (Figures 3.5-3.6) also part of the models? If the answer is "no," it seems that the *MY+Day* models with $R^2 > 0.90$ are all that are needed and the other variables (e.g. *WTT*, *Surface* shown in Figure 3.5 top right) must not contribute to the model despite the relative variable importance = 1.0. The effect of Julian Day is treated as a random effect, but many of the graphs show a near linear relationship with time – is a random effect appropriate in these cases? Equation 3.5 seems to indicate that Julian day occurs both as a random effect and a fixed effect in the model, so there is some ambiguity in exactly what the model *My + Day* actually means (i.e., is *Day* the random or fixed effect?). Some careful editorial review is needed on these pages.

Figures 3.5-3.6 need to define x-axis terms (e.g., Day, Day2, WTT) in the caption and each of the graphs need y-axis labels. Computing variable importance when a linear and quadratic relationship is present is a bit tricky because if the quadratic term is present, the linear term MUST be present.

Chapter 4

p. 82. How was it determined that most differential delayed mortality is expressed during the early marine stage? If this is an assumption rather than direct estimation, then please write the statement as an assumption rather than fact. It would be helpful if the authors could provide a brief summary (or at least citations to published literature) of scientific evidence both for and against this hypothesis.

p. 82. The authors state: *"S.o1* is back-calculated from the age-structured recruits to the Columbia River mouth, assuming 80% annual survival of sub-adults. This is consistent with other cohort-based Chinook modeling studies (e.g., Pacific Salmon Commission 1998), and assigns all ocean survival rate variability to the *S.o1* life stage." The reference list in the report does not include the cited reference (Pacific Salmon Commission 1998). It would be helpful if authors could briefly explain how the PSC's (and other) cohort-based Chinook modeling studies determined that the annual survival rate of sub-adult Chinook in the ocean is constant across post-smolt age groups and equal to 80%. It is true that this is an almost universal assumption in cohort-based modeling studies that has never been thoroughly evaluated.

p. 83. SARs "based on reconstruction for earlier years." Please clarify. Do you mean years prior to 1994 or 1997 or is it separate for the two species? If so, please specify the time period.

p. 83. When did Big Sheep spring/summer Chinook become extirpated? It is worth highlighting this important information.

p. 83/84. In the recruitment equation, how was period (T) determined? Is it simply before and after 1970 as suggested on the top of the next page – if so this should be more clearly defined. There have been many changes to the system over the years – is there any evidence that more than two periods would be needed? The equation also needs some revisions. If T is the period effect (a parameter) it needs to be multiplied by an indicator variable which indicates the period for year *j* and cannot stand on its own.

The period effect (*T*) might also reflect non-stationarity caused by long-term ocean effects.

p. 84. When estimating the pre-harvest SARs for John Day spring Chinook, how was survival rate of adults returning through the fisheries determined? Was this approach also used for the upriver stocks? Was pinniped predation in the lower river considered?

p. 87. Summer Chinook reportedly have higher SAR to Lower Granite Dam adult fish ladder (GRA) than spring Chinook, an interesting finding. Previously, it was reported that SARs to Bonneville Dam adult fish ladder (BOA) were about 26% higher than those based on returns to GRA in response to in-river harvests, dam loss, and straying. Is this 26% value consistent for both spring and summer runs?

Figs 4.1 and 4.5 provide important SAR time series, which the authors relate to dam construction in the Snake River Basin. It would be helpful if the year in which each Snake River dam began to impact the SAR values could be shown on the time series.

p. 89. SARs for wild A-run steelhead was approximately 40% greater than that for B-run steelhead. To what extent might this be caused by the additional year spent at sea by B-run versus A-run steelhead or to factors related to the smolt stage?

p. 92. Why is it not possible to estimate sockeye SARs in 2010 when no PIT-tagged sockeye were transported (Table B.47)? It seems that a SAR could be calculated though it would represent in-river fish only.

p. 93. "SARs of John Day and Yakima River wild spring Chinook averaged (geometric mean of ratio; based on BOA returns) 3.4 times and 2.4 times, respectively, those of Snake River wild spring/summer Chinook (Table B.2), and the wild SARs were correlated (average r = 0.73) between regions during the period 2000–2013." This is an interesting finding that indicates much greater mortality for Chinook smolts migrating through the Snake River. Does this differential mortality disappear if SARs are calculated for BON to BOA for all groups, i.e., survival in the ocean only?

Figure 4.10 caption. How can the release to BON survival be greater than 1.0 (100%)?

Figure 4.15. This graph indicates SAR of subyearling Chinook is equal to or perhaps greater than that of yearling Chinook. This is surprising given that yearlings have already undergone significant mortality during the previous year. It would be good to address this pattern. Is it related to differences in hatchery and wild Chinook SARs? How were the mixed-stock estimates calculated?

Figure 4.15. If hatchery and wild steelhead SARs are not correlated, then it may not be appropriate to combine the two stocks. SAR differences by A-run and B-run and by hatchery wild stocks could affect the time trend depending on how the data were analyzed.

p. 103. S.oa (survival from BON to BON) declined five- or six-fold after 1970 for spring/summer Chinook and steelhead respectively. To what extent has in-river survival changed between these two periods? Maybe this is the subject of a different chapter, but it would be interesting to compare the shift in survival at sea with changes in in-river survival.

p. 105. The value of correlating S.oa with S.01 is not clear given that S.01 is calculated assuming 80% survival each year after the first year at sea, rather than as an independent measure of S.01. We encourage the investigators to explore alternative approaches that more directly estimate year-to-year variation in early marine versus late marine mortality.

For the upper Columbia stocks, the report compares differential SARs of fish from the upper Columbia versus McNary Dam, as a means to highlight mortality of smolts in the upper reaches. For comparison, what is the mortality in the Snake River Basin, i.e. the upper reaches for those fish?

p. 107. The authors state “large declines in life cycle survival rates associated with development and completion of the FCRPS in the 1970s as well as with other environmental change.” Does “environmental change” refer to the 1976-77 climate regime shift? It would be useful if the CSS could provide a more thorough evaluation of potential factors related to the declines in the 1970s. For example, have the authors considered other major factors (beyond FCRPS and environmental changes) that may have contributed to declines in the 1970s? For example, there were many major changes in fishery management/regulatory processes that might also be associated with the decline in the 1970s (e.g., U.S. vs. Oregon 1969, increases in mitigation hatchery production in the Columbia River Basin, start of Alaska hatchery programs in 1971, lack of US-Canada harvest quota agreements in the 1970s, Marine Mammal Protection Act

1972, U.S. vs. Washington 1974, and implementation of the Fishery Conservation and Management Act [FCMA] and 200-mile fishery conservation zone 1976).

Figure 4.20 is interesting but complicated because the Survival Rate Index (SRI) is correcting recruits per spawner (R/S) for spawner density while also adjusting values relative to the pre-1970 period. It might be informative to show intermediate steps and to plot the recruitment curves for the two periods, so one can see the range in spawners and recruitment associated with the two periods.

Given the relationships shown in Fig. 4.20 and 4.21, how much increase in hydrosystem survival is needed to achieve the SAR target of 4-6% or 4-7% that is associated with restoring productivity to the pre-1970s level?

p. 110. We agree that it will be useful to update the relationships between SARs and SRI with data after the 2006 migration year. These data will include values with higher SARs.

Beginning in the caption to Table 5.10 thru 5.12, the authors state “All reach survival estimates are of combined T and R groups.” What are the T and R groups? If these are transported and in-river groups then it makes no sense to combine them as the transported should have a vastly higher survival than the in-river. Appendix A indicates what T and R represent, but readers shouldn’t have to go there to find these definitions. Please include more information in the legend.

Chapter 5.

p. 163 the authors refer reader to Chapter 4 for methods of TIR and D, which is OK. But at the end of the paragraph they say that “…D values were less than one..” it would be good if they provided a sentence as to what D values mean relative to transported or in-river migrants. They do provide this at the bottom of page 163, but the reader has to guess up to that point.

Editorial Comments

The text would be improved by using a more consistent notation referring to between dam movements of fish. Sometimes the document uses “to” (e.g. JDA-to-BOA) and sometimes the “to” is omitted (e.g., JDA-BOA).

Some of the figures seem to have low resolution (e.g., Figure 4.14). Please ensure that all graphs are created at a sufficiently fine resolution (e.g. 300 dpi+) for inclusion in reports.

p. 9. The term “rates” at the end of the first paragraph should be “probabilities”? The CSS made similar changes from the previous draft, but there may be a few places in the report where the old language is still used.

p.10, last sentence. The phrase “the electronics at the dam were used…” needs clarification. There must be a combination of a sensor and some sort of sorting device to route the fish? Perhaps a better choice of wording is needed here.

p. 11. A schematic to show what is described in words about how Group R, Group T and Group CRT fish are determined would be helpful.

Figures 1.2/1.3/1.4/1.5 – These figures are improved from the previous draft. The use of grey scale can lead to difficulties if the resolution of the figures is too low – as noted elsewhere as well.

Please standardize the legend across figures and improve the display when multiple symbols overlap. For example, an open circle is a sockeye release site in Figure 1.4 and 1.5, but is a Chinook release site in Figure 1.2 and 1.3. Some care needs to be taken when multiple symbols are combined. For example, in Figure 1.5, there are many white squares, white circles, and black dot locations which is a sockeye tag site (legend says black square), sockeye release site (legend says black circle), and a steelhead release site (black dot). Does a combined steelhead and sockeye release site have a different symbol?

p. 26, lines 28-30. How is the normalization of the environmental time series done?

Figure 2.1. Add the definition of the abbreviations for the environmental conditions to the figure legend.

Figure 2.3. The figure uses BH (a1,b1), BH (a2,b2), BH(ap,bp). This notation is not used elsewhere. It presumably means a Beverton-Holt relationship as outlined in equation 2.1, but this should be clarified in the legend.

p. 32, lines 6-14 and Equations 2.9-2.12. The parameters mentioned in these lines are not explicitly shown in Figure 2.3 and must be inferred from the definition of x in the Figure, but x in Figure 2.3 only refers to H and T – what happened to R? The left hand side of Equations 2.12 should also have a “hat” on it because it is a function of other estimates.

Table 2.1. Where do the “fixed” values come from for s_2, s_3 and m_1?

p. 35. Some care is needed in describing how the Bayesian Markov Chain Monte Carlo (MCMC) algorithms work. For example, MCMC does not automatically lead to improvements in fit. The

Metropolis-Hastings MCMC approach evaluates the ratio of the likelihood x prior at the current and proposed new values and chooses a move probabilistically. So it is possible that on a specific iteration for the fit to actually be worse. It is true, looking at the entire chain, that the MCMC algorithm tends to move towards parameter values that fit the data better than the initial conditions, but this is not true on a particular iteration. Some careful editing is needed on this page.

Figure 2.4. The legend refers to the productivity and capacity parameter (generally) – give the actual symbols in the legend used in the model (presumably the *a* and *b* parameters)

This figure is very busy, but some units for some of the parameters would be helpful.

p. 39, Lines 1-5. Some of these terms have hats on them or the adjective "predicted" in front of them – consistency in usage may be in order.

p. 44. The units on R-bar are presumably just numbers of fish. Are changes to productivity and capacity simply changes to the *a* and *b* parameters in the model – it would be helpful to clarify these points.

Figure 2.9. Add units to the axis for productivity (smolts/spawner) and abundance.

p. 47, Lines 34-37: The statement here implies that it would be possible to get a perfect fit to the models with the right data. It is unlikely that a perfect fit would be obtained, even if data from the Grande Ronde/Imnaha MPG were available.

Figure 2.11. Add units to the axis for capacity (spawners) and abundance. Some clarification is needed that capacity refers only to single species and not the aggregate of all species using the watershed.

p. 49, line 13. "…survive fish at a higher rate." Please reword.

Figure 2.12. Units are needed for all axes (see earlier comments on previous figures).

p. 52, line 32 (and elsewhere in this chapter). Some careful editing required because the results obtained are "statistical," so it is difficult to infer causality. Another example is, p.47, l.25 where it is asserted that the PITPH "affects" early ocean survival rather than "is related to" or "strongly related to."

p. 57, l.5. What is meant by "production fish"?

p. 57. It may be helpful to add/move the info on the temporal size of the cohorts (e.g. 1-week or 2-week, etc.) to Table 3.1/3.2 to make it easier for the reader to know the temporal size of each cohort.

Table 3.1 and Table 3.2. Shouldn't the captions say "1998-2014" instead of "1998-2013"?

p. 58. In the phrase "observed travel times will be truncated to some degree", would "underestimated" be a clearer substitution for "truncated"?

p. 60, l.34. This link appears to be stale and needs to be updated.

p. 61, l.34. Clarify that "Julian day of release" is related to fish release rather than water release of some sort.

p. 63, l.25. The phrase "due to the loss of individuals with long travel times," may need some rewording. Does "loss" refer to "mortality" or simply "not counted"?

p. 64, l.18. Change wording after S_i hat to "both increased and decreased" rather than "either".

Figures 3.2 – 3.4 need y-axis labels and in general the relations between solid dots and circles and the SEs are difficult to read because of the sizes. It seems that the authors are just interested in the general relations, but, if it is important to see the individual relations, perhaps the report could present larger figures in several more pages or use judicious offsetting of fitted and observed values to make the figures easier to read. Captions should read 1998-2014 – please check all other Figure legends as well.

p. 68, l.38. Are the model coefficients provided somewhere, perhaps in electronic format for the interested reader?

p.73, l.27. (related to p.52, l.32 comment): Suggest changing wording to "We found some evidence that the increased number of dams with surface passage structures in the spillways may be related to reduced mortality rates" to change wording from causal to relational.

p.75, l.3. What reach is referred to as "this reach"? The previous sentences talked about several dams so it's unclear.

p. 76. NPCC 2003 missing in reference list.

p. 82. ISAB/ISRP (2008) missing in reference list.

p. 82. Pacific Salmon Commission (1998) missing in reference list

p. 77. Change "an SAR" to "A SAR"

p. 101. Stiefel et al. (2015) is not in references.

p. 113. What is “S.r”? Is this the same as S_R (in river survival)?

p. 165. “Finally, overall SARs (LGR to GRA) for wild Snake River subyearling Chinook were less than 1% for all years, when jacks were excluded (Table 5.16).” This should be Figure 5.20.

VI. References

Independent Scientific Advisory Board. 2015. Density dependence and its implications for fish management and restoration programs in the Columbia River Basin. ISAB Document 2015-1. Northwest Power and Conservation Council, Portland, Oregon: www.nwcouncil.org/fw/isab/isab2015-1

Independent Scientific Advisory Board. 2011. Columbia River Basin food webs: developing a broader scientific foundation for fish and wildlife restoration. ISAB Document 2011-1. Northwest Power and Conservation Council, Portland, Oregon: www.nwcouncil.org/fw/isab/isab2011-1

Kilduff, D.P., E. Di Lorenzo, L. W. Botsford, and S. L. H. Teo. 2015. Changing central Pacific El Niños reduce stability of North American salmon survival rates. PNAS 112 (35): 10962–10966

Miller J.A., D.J. Teel, W.T. Peterson, A.M. Baptista. 2014. Assessing the relative importance of local and regional processes on the survival of a threatened salmon population. PLoS ONE 9(6): e99814. doi:10.1371/journal.pone.0099814