ISAB

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

Review of the Comparative Survival Study's Draft 2013 Annual Report

Richard Alldredge
Kurt Fausch
Colin Levings
Katherine Myers
Robert Naiman
Bruce Rieman
Greg Ruggerone
Laurel Saito
Dennis Scarnecchia
Carl Schwarz
Chris Wood

ISAB 2013-4
October 14, 2013

ISAB Review of the Draft 2013 CSS Annual Report

Contents

ISAB Review of the Draft 2013 CSS Annual Report

I. Background

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

II. Summary

This ISAB review begins by suggesting topics for further CSS review, then provides general and specific comments on each chapter of the report, and ends with specific editorial suggestions.

The ISAB suggests five topics for further CSS review:

1. hypotheses on mechanisms regulating smolt-to-adult survivals (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

The CSS is a large-system study that has collected a substantial amount of PIT-tag data from multiple species and stocks over a 17-year period, but to date identification of hypotheses on the causal mechanisms regulating SARs has been limited. The ISAB suggests a comparative approach to identifying hypotheses that may lead to a greater understanding of causal mechanisms. The CSS posed important questions related to stream productivity and hydrosystem survival that were not addressed by the life-cycle model in this report and need to be addressed by the next version of the model.

A detailed reevaluation of SAR objectives (2-6%) is warranted. These objectives should be reevaluated for each species and Evolutionarily Significant Unit (ESU) of salmon and steelhead based on realistic values needed to support robust viable populations. Discrepancies in SARs between PIT-tagged and non-PIT-tagged fish reported in other publications raise two important issues that could be addressed now: (1) what are the implications of correcting biased SAR estimates from PIT tags with respect to performance against recovery and Fish and Wildlife Program objectives, and (2) what proportion of US Endangered Species Act (ESA)-listed populations are being PIT-tagged and what are the implications for imposing this additional mortality? Further work is needed to analyze the relationship between the ratio of transport/in-river SARs and in-river survival. With many years of experience now, the CSS needs to identify critical data gaps. What crucial pieces of information are not addressed by the CSS, and what improvements can be made to provide them? Some examples provided by the ISAB include the lack of habitat-specific estimates of smolt survival in the estuary, information on how age at maturation affects SARs, the contribution of mini-jacks to SARs, and the relationship between SARs and biomass of adult returns of hatchery and wild salmon.

The ISAB recommends a new focus on rationalization of the PIT-tagging program given the very large detection infrastructure already in place and the overlapping objectives of the different tagging studies (see IEAB document 2013-1 and ISAB 2013-3). It may be possible to reduce the numbers of populations and fish that are PIT tagged without significant loss of information, leading to greater program efficiencies at lower cost. The ISAB also recommends that the CSS prepare and submit a manuscript for peer-reviewed publication that synthesizes and critically reviews the results of the CSS study.

Most of the information in the CSS's 2013 report is an annual update of information in previous year's reports. Our summary, therefore, focuses on new information presented in Chapter 2, which develops and describes a simple life-cycle model. In this model, information from multiple populations is used to estimate parameters common to the different populations (ocean survival) while allowing each population to have a different spawner/recruit relationship. The key advantage of this approach is the reduction in the total number of parameters used to describe dynamics of the populations because of the assumption of common ocean survival. Additionally, estimates can be obtained for certain life states for populations that lack direct data. The conceptual basis of the model appears to be sound, although evidence supporting the primary assumption of common ocean survival was not provided in the report. Three versions of the model with different levels of complexity were evaluated. While the ISAB understands that model development is at the initial stage, there are numerous difficulties in the model's description that make it uncertain if the three versions of the model have been implemented correctly. Model equations do not match the flow diagram. No estimates of precision from the models were presented. Only a small amount of model assessment was done. Comparison of models using the Akaike information criterion (AIC) should be included.

An alternate but similar approach would be through the use of state-space models in a Bayesian context. This would allow the incorporation of natural variation in the transition between life stages that is currently ignored in this approach and the ability to incorporate prior information on some parameters in a more natural way rather than, for example, assuming the second year of ocean survival is exactly 0.6. Applying a hierarchical approach to the spawning parameters across the populations would also allow some sharing of information across population when data are sparse. Finally, it would also provide probabilistic forecasts of future population trajectories.

III. Suggested Topics for Further CSS Review

(1) Hypotheses on mechanisms regulating SARS. The CSS is a large-system study with substantial PIT-tag data collected from multiple species over a 17-year period. However, to date, identification and evaluation of hypotheses on the causal mechanisms regulating smolt-to-adult survival (SAR) have been limited. In general, each species/year combination is treated in isolation. Many figures in the 2013 CSS Annual Report show related stocks tracking each other over time, often quite strongly. This of course is not surprising. But has the CSS looked at the opposite side? That is, are there stocks that should be tracking each other but are not? Perhaps this would help to identify hypotheses on causal mechanisms. For example, the 2013 historic return of fall Chinook (1.2 million) to the Columbia River does not track adult returns of spring/summer Chinook and steelhead. The historic 2013 returns of fall Chinook have been attributed by the Columbia River Inter-Tribal Fisheries Commission (CRITFC) and the Northwest Power and Conservation Council (NPCC) to various factors, including "high spring river flows when the fish migrated to the ocean as juveniles two to five years ago, spill of juvenile fish over dams, good ocean conditions, ongoing projects to improve fish passage at dams and the habitat where fish spawn, and improved survival of fish produced in hatcheries" (Columbia Basin Bulletin 9/27/2013). Can CSS data be used to provide a more scientific explanation for the cause(s) of historic returns of fall Chinook salmon in 2013? If PIT tag data are sufficient, they might be used to evaluate why the Hanford Reach Fall Chinook stock (and perhaps some other stocks) have persisted so well over the years versus those trending in a different direction. The Hanford Reach Fall Chinook stock is one of the most robust populations in the Basin. The fish go through many mainstem dams, and the adults support a somewhat robust fishery. What is the survival for this stock through the hydrosystem and in the ocean? What is spawner to smolt survival? Partitioning survival might provide clues as to why this stock is doing relatively well compared to other species and stocks.

Discrepancies in SARs of adult salmon and steelhead as they migrate upstream through the FCRPS should be evaluated. For example, the size of the discrepancies in SARs measured for LGR-BOA (Lower Granite Dam to Bonneville Dam adult fish ladder) versus LGR-Lower Granite Dam adult fish ladder (GRA) seem large enough to have significant implications for population recovery. Differences in geometric means for wild Snake River spring/summer Chinook (25%) and steelhead (36%) suggest average losses during upstream migration from BOA to GRA of 20% and 26%, respectively. An even greater discrepancy (average losses of 43% - 64%) is evident for fall Chinook, although that comparison is not raised explicitly, and it is difficult to evaluate quickly in this report (our numbers based on a partial summary for 2006 to 2009 in Table 5.17). How much of these differences might be attributed to fishing activity or other sources of mortality that could be mitigated? Which dams cause more adult mortality? For Chinook, the estimates of percentage loss seem similar with and without jacks and perhaps that similarity gives us a clue about the likely mechanisms. In any case, it seems that the LGR-BOA SAR data could be used more effectively to explore patterns in upstream survival.

Understanding when, where, and why survivals of hatchery and wild salmon differ is key to achieving Fish and Wildlife Program SAR objectives. Is it reasonable to conclude based on statistical analysis that SAR values are typically higher for wild than hatchery Chinook, whereas juvenile in-river survival (S_R) values are typically the same for wild and hatchery Chinook or perhaps slightly higher in hatchery Chinook (based on S_R values for Snake River Chinook in Figures A.2–A and Tables A.1 – A.3 vs. corresponding SAR values for the group of in-river control PIT-tagged smolts, termed C_0, Figures A.7 and A.8)? This conclusion, if supported by the data, would indicate that wild Chinook typically survive better than hatchery fish below Bonneville and should be reported by CSS. Further investigation seems warranted to find the mechanism(s) that could explain the difference.

(2) Life-cycle modeling questions and SAR objectives. Chapter 2 (life-cycle modeling) poses two questions:

(1) "*What changes in stream productivity* [salmonid productivity in rearing streams] *would be required to achieve population recovery if hydrosystem survival were to remain at the status quo?*"
(2) "*What changes in hydrosystem survival would be required to achieve a 20% increase in population abundance by a particular time in the future?*"

These are important questions, yet they are not addressed in this report. The ISAB encourages the CSS to address these questions using the next version of the life-cycle model.

The ISAB appreciates the discussion of the 2-6% SAR objective in the Introduction of Chapter 4, and we agree that a more detailed look at SAR objectives is warranted. Ideally, SAR objectives should be established for each species and ESU of salmon based on realistic values needed to support robust viable populations. The values should consider differences in SARs for yearling versus subyearling life histories. The life-cycle model presented in Chapter 2 appears to be poised to address this issue, at least for Grande Ronde spring Chinook. It would be worthwhile for CSS to utilize the SAR data in hand to help develop SAR objectives for each species and ESU.

An issue that continues to be unresolved is related to mortality caused by PIT-tagging. Knudsen et al. (2009) found that returns of upper Yakima hatchery spring Chinook salmon marked with PIT tags, as well as control tags (CWT+elastomer tag+adipose fin clip), were 25% lower than control fish without PIT tags. This implies that the non-PIT-tagged group enjoyed a 33% higher survival rate, which is sufficient to explain why SAR estimates from run reconstruction are 19% (Schaller et al. 2007) to 35% (Kennedy et al. 2011) higher than SAR estimates from PIT-tag data. The ISAB is aware of the 4-yr USFWS study to address these concerns with respect to Snake River spring/summer Chinook, but there are issues that could be addressed now by CSS: (1) what are the implications of correcting biased SAR estimates from PIT tags with respect to performance against recovery and Fish and Wildlife Program targets; and (2) what proportion of ESA-listed populations are being PIT-tagged, and what are the implications for imposing this additional mortality, i.e., ~25%, unless the discrepancy is due to tag shedding rather than mortality? After correcting for tag loss, which occurred primarily after release and before adult return, Knudsen et al. (2009) found that SARs of PIT-tagged fish were still 10% lower than those of control fish without PIT tags.

As a final suggestion, further work by CSS is needed to analyze the relationship between the ratio of transport/in-river SARs (TIR) and in-river survival (S_R) (see comments on Appendix A, p. 44-45).

(3) Data gaps. Based on many years of experience, the CSS needs to identify critical data gaps. What crucial pieces of information need to be produced by the CSS or others in the region and what improvements can be made to provide them?

For example, the ISAB has already identified the lack of habitat-specific estimates of smolt survival in the estuary as an important regional data gap (ISAB 2012-7). What data and what approaches to collecting data are needed to address this issue?

Another example is data on age at maturation of Chinook salmon and steelhead, which are often overlooked in the Columbia River Basin, even though it is a highly important trait that affects salmonid productivity and viability. Important future questions might include: how does age at maturation (years spent at sea) affect SAR? Can extensive SAR data be used to better estimate annual mortality at sea, so that we have a better idea about the cost salmon and steelhead incur when spending an additional year in the ocean?

Another gap is data on mini-jacks, which contribute many fish to SARs among fall hatchery Chinook salmon when included in the calculation, such that ~62% of the mature population is represented by jacks and mini-jacks versus larger, older Chinook salmon (Larson et al. 2013).

What percentage of mature hatchery spring Chinook salmon are mini-jacks, and how would inclusion of mini-jacks affect estimates of SARs?

A final example is the lack of information on the biomass of adult returns to the Columbia River Basin in relation to survival. For example, periods of high SARs might be correlated with decreased adult body size, reflecting density-dependent growth of salmon in the ocean. This has important implications for the impacts of hatchery releases on wild salmon survival and recovery in the Columbia River Basin. Is it possible for the CSS to develop a time series of estimates of the annual biomass of adult hatchery and wild salmon returns to the Columbia River Basin?

(4) Rationalization of CSS PIT-tagging. The IEAB Fish Tagging (FT) model is a non-linear mathematical programming model that estimates how many (juvenile) fish should be tagged with what type of tags (CWT, PIT, genetic) to satisfy a set of required outcomes, such as a minimum number of tags of each type detected/recovered at certain locations or indicators (IEAB document 2013-1). At present, the model can be used to evaluate tradeoffs in using different tag types and to search for cost efficiencies in the numbers and geographic locations where fish are tagged. In its recent review of the FT model (ISAB 2013-3), the ISAB recommended: "*Focus on rationalization of the PIT-tagging program given the very large detection infrastructure already in place and the overlapping objectives of the different tagging studies (e.g., estimating in-river and transport survival, evaluating effectiveness of habitat improvements, measuring straying rates, and so forth). This evaluation should also consider tradeoffs between adding more fixed costs to improve detection rates by modification of infrastructure vs. ongoing costs and handling effects of tagging more fish.*" For example, results presented in the 2013 CSS Annual Report indicate similar patterns in SARs for Major Population Groups (MPGs) from the same region of the Columbia River Basin. Thus, the FT model might be used to investigate whether it is possible to reduce the numbers of populations and fish that need to be PIT tagged without significant loss of information. Such an evaluation might result in greater program efficiencies and cost reductions. The CSS/FPC staff could also assist with further improvements to the FT model in developing stock-specific estimates of smolt survival and to validate, modify, and improve assumptions in the FT model about juvenile and adult mortality rates by river reach and dam passage and species- and stock-specific ocean mortality rates, as recommended by ISAB (see IEAB document 2013-1 and ISAB 2013-3).

(5) Publication of a synthesis and critical review of CSS results. Although the CSS investigators have published a number of articles on components of the CSS study, the ISAB recommends that the CSS prepare and submit a manuscript for peer-reviewed publication that synthesizes and critically reviews the results of the overall CSS study (as suggested in ISAB/ISRP 2007-6). The ISAB considers it vitally important to the Fish and Wildlife Program for CSS to summarize and evaluate the current state of understanding on questions addressed by CSS and to make this information available to the scientific community at large, as well as to the general public. A balanced and well-reasoned review article, for example, might help to resolve the current debate over the validity and interpretations of survival estimates in published and unpublished BPA-funded studies (e.g., http://www.nwcouncil.org/media/6877226/1.pdf). The ISAB suggests a retrospective approach to the review that focuses on the original question that the CSS was designed to address, that is, "*can transportation of fish to below Bonneville Dam compensate for the effect of the hydro system on juvenile survival rates of the Snake River spring and summer Chinook salmon during their downstream migration?*" and related hypotheses drawn from the most significant conclusions in the CSS (2007) Ten-year Retrospective Summary Report and other CSS reports and publications. The review should carefully consider the weight of evidence both opposing and supporting the CSS hypotheses, results, and conclusions, using information from the scientific literature and assessing the importance and reliability of the evidence reviewed.

IV. Comments on the draft CSS 2013 Annual Report by Chapter

Chapter 1. Introduction

The introduction to the 2013 CSS Annual Report is identical to the introduction in the 2012 report with the exception of a few updates. While this section is generally well written, the ISAB reiterates its 2012 suggestion for addition of a table with an historical timeline of key objectives and results from past years of CSS work. There is no mention of whether the CSS Oversight Committee explored adding this table, although this action was suggested in the CSS's response to the 2012 ISAB review. The table could also include citations to past reports and publications for more detailed information on past results, which would be useful to those not familiar with the CSS's past work.

Specific Comments and Questions

Page 2, l. 4-6. How is PIT tag loss considered in the analyses? Is it possible that there is a different proportion of PIT tag loss for transported and in-river fish, or could PIT tag loss occur in different proportion for these two groups in the estuary/ocean phase of life? How is this considered in the analysis and the interpretation of results?

P. 2, Figure 1.1. The ISAB continues to have the same problems with this figure (ISAB 2012-7, p. 16). In response to our review, the CSS Oversight Committee said they would consider revising the figure, but apparently decided not to. For readers unfamiliar with salmon life history, the Federal Columbia River Power System (FCRPS) and the factors/actions affecting survival of

Columbia River Basin salmon, the figure is too cryptic. For example, why do the arrows have different colors? Can the significance of the boxes be explained?

P. 3, Figure 1.2. An explanation in the report about why all dams are not outfitted with both juvenile and adult PIT tag detectors would be useful. A few issues are confusing in the legend. What are "CSS Release Sites Basins"? Does marking also occur at the two John Day rotary screw trap sites?

Page 7, l. 35-36. PIT-tag detection probability is given as nearly 100% for returning adults, but no indication of detection probability is given for smolts. The ISAB suggested in its 2012 review that a typical range in detection probabilities for juveniles be included for each monitored dam, but the CSS response was that detection probabilities are highly variable and depend on many factors, so that indicating values would be complicated and likely confusing. However, it seems important to help readers gauge the degree of uncertainty by giving them some idea about the typical value and range of detection probabilities for smolts. Surely a typical range of values could be included briefly here. Much later (Table 5.1) a detection probability of 0.32 is listed for Bonneville; this value seems low enough to justify our request for more information.

Page 8, l. 7-11. It seems that the less fit fish will have been eliminated from the in-river cohort but not the transported fish so there is potential bias in D (the estuary and ocean survival rate of Snake River transported fish relative to fish that migrate in-river through the Federal Columbia River Power System) because the transported cohort is arguably different than the in-river cohort. How is this considered in the analysis and in the interpretation of results?

Page 9, l. 10-14. It is not clear how the uncertainty in the Cormack-Jolly-Seber (CJS) reach survival rates and detection probability parameter estimates for the composite group (Group CRT) is used in the interpretation of key parameters for the component groups T and R.

Page 12, l. 16-20. The ISAB appreciates that including new groups when possible is a valuable endeavor.

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

The CSS's objective in this chapter is to develop a multiple-stock model that links freshwater spawning and rearing (FSR) production and survival to mainstem passage survival and ocean survival. The ultimate goal is to use the model to assess important management scenarios to recover spring Chinook salmon. Three different versions of the model (BH, LC, and LCX) with different levels of complexity were evaluated using data for the Grande Ronde Major Population Group (GRMPG) of spring/summer Chinook salmon. The investigation uses a long time series of SAR data and smolt per spawner data for up to six populations within the GRMPG. The analytical approach is characterized as a first step, indicating the investigators plan additional work with the model. The ISAB encourages the investigators to continue with model development and to explore ideas presented in the report. Nevertheless, there are numerous difficulties in the description of the model that make it uncertain if the three model versions have been implemented correctly in this initial step. Below, the ISAB provides a number of detailed comments on the analysis.

The ISAB advises that an alternate but similar approach would be through the use of state-space models in a Bayesian context. This would allow the incorporation of natural variation in the transition between life stages that is currently ignored in the CSS's approach and the ability to incorporate prior information on some parameters in a more natural way rather than, for example, assuming the second year of ocean survival is exactly 0.6. Applying a hierarchical approach to the spawning parameters across the populations would also allow some sharing of information across population when data are sparse. Finally, it would also provide probabilistic forecasts of future population trajectories.

The Introduction and other sections of Chapter 2 pose two questions:

(1) "*What changes in stream productivity* [salmonid productivity in rearing streams] *would be required to achieve population recovery if hydrosystem survival were to remain at the status quo?*"
(2) "*What changes in hydrosystem survival would be required to achieve a 20% increase in population abundance by a particular time in the future?*"

These are important questions yet they are not addressed in this report. The ISAB encourages the CSS to address these questions using the next version of the model.

The three models used by CSS provide a sensitivity analysis in that the initial model (BH) is a simple Beverton-Holt model and subsequent models increase complexity, including variables that would enable the model to address management scenarios. Graphs are used to show the fit of the models to data, but a more formal statistical evaluation of model fit would be worthwhile in addition to graphs. For example, comparison of the smolts per spawner model fits using the BH and LCX models did not visually seem very different, but it may be very different when looking at residuals.

The key utility of the LCX model is the inclusion of the survival function that involves the PDO, upwelling index, and Powerhouse passage. A form of the relationship was developed in Petrosky and Schaller (2010). It would be worthwhile to show a plot of Chinook survival in relation to these three variables and to describe the amount of survival variability explained by them. Given that the Powerhouse variable is key to management scenarios, are there changes in the hydrosystem over time that are not accounted for by this variable, other than spill which is part of the index? The discussion suggests that the Powerhouse variable (NPH) is the key variable in the survival function, yet no data were presented to show this, including "B3" which is discussed but not shown.

The modeling approach assumes survival at sea is essentially the same among the populations. It would be worthwhile to provide a correlation matrix describing the extent to which population survivals are correlated. How are missing survival values handled when developing a pooled survival estimate in Equation 9?

There is no evidence provided in the report supporting the primary assumptions in the model related to ocean survival, i.e., (1) that GRMPG populations share a common ocean survival, (2) that ocean survival is not density-dependent (capacity parameter set to infinity), (3) that survival in the 1st ocean year is a function of PDO, UPW, and NPH, and (4) that survival in subsequent ocean years is a constant (2nd ocean year = 0.6, 3rd ocean year = 0.7). For example, does evidence from tagging (PIT tags, CWT tags, genetics) support the assumption for a common ocean survival among GRMPG populations? Positive and significant relationships between ocean growth and adult abundance of Columbia River Chinook and coho salmon and steelhead (e.g., Jacobson et al. 2012 and other references cited in their report) are a reflection of limits to ocean carrying capacity of salmon. There is also evidence for density-dependent (feeding competition) effects on growth and survival at both juvenile (1st ocean year) and later ocean stages. An accumulating body of scientific evidence supports the hypothesis that 1st-year ocean growth and survival and, thus, brood-year strength is determined in two stages, called the "Critical Size and Critical Period" hypothesis (Beamish and Mahnken 2001; Farley et al. 2007; Jacobson et al. 2012). The modeling approach described in this chapter addresses only the earliest (ocean entrance) stage, when predation is the primary cause of mortality, versus later stages during the first summer, fall, and winter at sea when mortality is physiologically based. The use of environmental variables that reflect physiological and bioenergetic constraints on growth (e.g., availability of nutrients regulating food supply; ambient water temperatures in ocean regions where juvenile Columbia River Basin salmon are distributed during summer, fall, and winter) might improve model fits and predictive capabilities. Biological indices directly reflecting growth after ocean entry also have a strong relationship to abundance of adult returns (Jacobson et al. 2012).

The ISAB is aware of at least two other life-cycle models under development by NMFS scientists that use Grande Ronde spring Chinook data. Buhle et al. (2013; draft in review by ISAB) incorporate Grande Ronde data into a statistical model that estimates the effect of hatchery supplementation on wild salmon productivity and capacity parameters. Cooney et al. (2013; draft in review by ISAB) are developing a model specific to Grande Ronde spring Chinook while targeting the freshwater phase. It would be worthwhile for CSS to communicate with these other investigators.

The discussion notes the large model residuals for Upper Grande Ronde and Catherine Creek populations during the early period when smolt survival data were not available. As a consequence, model results for these populations relied on pooled survival data. The interpretation for the large residuals was that actual survival in these streams must have been much higher during this early period, which then declined when monitoring began. The investigators should consult (or continue to consult) with people familiar with the Grande Ronde Basin, such as Rich Carmichael and Tom Cooney, to see if there is evidence for a somewhat rapid decline in spawner to smolt productivity during this early time period. What known changes might have caused this decline?

Tables 2.3 and 2.4 show the modeled productivity and capacity estimates for the six populations and three models. The ISAB understands that the habitat condition of these six watersheds varies considerably from disturbed to somewhat pristine. How well do the productivity and capacity estimates correlate with known condition of the habitats? If these estimates do not match expectations, does this mean that the pooling approach has forced all variability onto the spawner to smolt stage, leading to unreasonable estimates? What are the units in these tables? If the units are smolts per spawner at low spawner density and total smolts, how do they compare with observed values?

Two of the models include age structure of Chinook. Is fecundity allowed to vary with age, as it should? We encourage the investigators to further develop models that incorporate age at maturation as this tends to be an important life history characteristic in the Columbia Basin that does not receive the attention that it deserves. Growth is a key factor affecting age at maturation. Variable age at maturation helps reduce risk associated with catastrophic events.

The models and data provide evidence for strong density dependence at the spawner-to-smolt stage. When comparing this relationship among the populations, what does it tell us about the need for habitat restoration? Is the decline in productivity steeper for degraded versus undisturbed habitats, or does the overall relationship shift with respect to habitat condition?

Specific Comments and Questions

P. 21, l. 15-25. The data used in the model fitting are described, but no data are shown in the report. It would be helpful to actually see the brood table for the adults, the covariates (NPH, PDO, UPW), and the juvenile data used in the model fitting. The brood data are assumed to be known exactly, but as indicated on p. 21, data include reconstructions from fishery catches, which are not known exactly. Hence there is some additional error in the brood table that may not be captured by the model, that is, does the process error variance (the $\sigma^2_{R_{p,a}}$ of equation 10) capture all of the uncertainty in the observed data?

P 23. Figure 2.1 indicates that smolts are subject to two survival rates (s0 and s1), but equation (2) only uses s1. This needs to be clarified. The ISAB does not understand the figure. Why are there three spawner boxes with the first two for populations 1 and 2? What are populations 1 and 2, and why are they special?

P. 24. A life cycle model is created starting on page 24 that predicts the expected number of fish in the next stage given the number at the previous stage. No variability is introduced in the number of fish moving to the next stage, e.g., equation (2) indicates that the number of ocean fish is exactly equal to the number of smolts multiplied by the survival rate rather than this being a binomial process. A flow diagram of the model is presented in Figure 2.1, but the diagram does not use the notation from equations (1), (2), etc. It would be helpful to add the defined variables from the equations to the flow diagram.

P. 25, l. 17. Equation (8) needs clarification. It is true that the total number of spawners from a brood year is defined this way, but not all spawners from the brood year are on the spawning grounds in the same year. Consequently, using $S_{p,t}$ for the number of spawners back in equation (1) "mixes" spawners from different calendar years into the same year of spawning. Should not the number of spawners in year *t* be a combination of 3 year-olds from this brood year + 4 year-olds from the previous brood year, + 5 year-olds from two years previous? This is shown at the bottom of Figure 2.1 where recruits are merged from several brood years (the notation is a bit odd as Run3(p, t+3) becomes Recruits (t). The t-index "jumps" here invisibly. Better notation is needed.

P. 26, l. 21-31. The ISAB does not understand the likelihood equations (10), (11), and (12). In equation (10), the product is taken over the index *t,* yet *t* appears on the left term. It should be function of (p,a). Similarly in equation (11), the left term should be a function of (p) only. How are equations (10) and (11) then combined over all (p,a)? Presumably by a direct product, but this is never explicitly stated. Equation (11) is in terms of $J_{p,t}$ but this is never defined in Table 2.1, nor in the model equations (1) to (8). How are these values determined? Similarly in equation (12), neither R_t nor σ^2_R is ever defined in the table of notation nor in the model equations. How was the variance estimated? Were the MLE equations solved explicitly?

No model assessment is reported, e.g. evaluating plots of observed vs. expected results for lack of fit or any substantial outliers, assessing whether an over-dispersion to determine if any adjustment is needed, etc.

Because all of these models are likelihood fits, a table of AIC values should be presented to show the relative support of the three models to the data.

The use of the fitted model to investigate scenarios is a nice way to investigate what type of future average population trajectory can be expected. The report does not present any results arising from this feature of the models.

P. 28, l. 17-20. The ISAB is concerned that in Table 2.4 the smolt capacity terms go to infinity in some models. Also it appears that the smolt productivity (Table 2.3) and capacity are highly negatively correlated so that if estimated capacity declined the estimate productivity increases almost directly. Perhaps a re-parameterization of the model may be more numerically stable. What is the sampling correlation of the two parameters?

P. 28, l. 19-20. How do predictions of capacity in Table 2.4 agree with levels of smolt production in streams, i.e. are the estimated capacities much higher than observed levels of production, etc.? Standard errors should be presented for all estimates to assess how well they have been determined.

P. 30, 33. It is also odd, in Figure 2.3 and 2.6, that the fitted values for recruits in the CC population do not match the large upward trend seen in the 1970s. A similar situation is evident in the GR population. The other populations seem to match the observed data much better. Why is this so? There is some explanation in the Discussion (page 38, l. 7-16), but the ISAB had difficulty following the logic. Perhaps it is a model fitting error where the model converges to a local minimum without changing the initial parameters for the LC and GR populations?

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

Regression models were used to investigate the influence of environmental and operational covariates on the estimates of instantaneous mortality and fish travel time. A statistics-on-statistics approach was used rather than embedding the covariates directly into the CJS mark-recapture models. The ISAB also commented on this issue in our 2012 review, to which the authors responded that perhaps they would include a paragraph in future reports about this topic. AIC was used to investigate the different models, but only the top model in the model set was used for inference rather than the usual model-averaging approach.

Specific Comments and Questions

P. 41, l. 12-19. The ISAB suggests also including fall Chinook salmon in this chapter in the future.

P. 41-42, Study area and definitions. The written description of the three reaches, the species and cohorts used would be easier to digest if a diagram was presented showing the general reach structure (a Y pattern with LGR->MC< and RIS->MCN joining before MCN->BON) and which species and cohort structure used for each reach.

P. 44, l. 13-23. The discussion of the meaning of Z is a bit long and superfluous. The instantaneous mortality rate parameter is well known in the fisheries literature. Simply cite any one of a number of standard reference books.

P. 44, l. 24. Equation 3.3 needs some care. The estimated value of Z applies to all fish that start passage at the start of a reach, including those that do not survive to the end of the reach. However, the mean FTT is based only those fish that survive the entire reach. Consequently, the mean FTT is based on fish that tend to swim faster (because they did survive) and so will “underestimate” the exposure time of the entire cohort of fish. It should be possible to modify the CJS model to incorporate both the travel time and Z directly in the estimation process, or perhaps small simulation in an appendix will show that equation (3) works well enough that any bias is negligible. [Again, the responses to the 2012 report dealt with this issue; perhaps adding a paragraph to future reports is justified.] The modified CJS model would also give the SE of Z directly.

P. 45, l. 1-3: Would it also make sense to estimate the variance using bootstrapping?

P. 45-46, Multi-model inference. The use of information–theoretic techniques for model selection is a standard method for this class of problems. However, this method typically presents the AIC model weights for the top models and does not just pick the “best” model. The appendix needs to be improved to show the model weights for the top models. In the response to the 2012 report, the CSS indicated that all models were more than 3 AIC points lower than the top model. Is this still the case? As well, it is quite common to present the total model weight of models that contain each variable to see the relative importance of each variable (as in the 2012 report). Model averaging should be used for the predictions from the model set. The appendices also don’t present any standard errors for the coefficients.

P. 46, l. 29, equation 3.7: Consider specifying log(Z) or sqrt(Z) as the dependent variable.

P. 48, l. 23-25: This interpretation seems reasonable, but the data are not sufficient to rule out other explanations. The ISAB encourages the CSS team to consider other explanations in addition to spill.

P. 48, l. 26-29: Interpretations of the results in Appendix 3.1 are questionable. WTT has 5 positive and 3 negative coefficients while surface passage has 3 negative out of 10 scenarios, and the interaction of Day and WTT means the interpretation of WTT effect depends on Day.

P. 48, lines 41 to 43: Interpretation of results appears questionable based on Appendix 3.2. WTT has 2 positive and 2 negative coefficients, Spill has 4 positive and 4 negative coefficients, Temp has 7 positive and 1 negative coefficients, and Surf Pass has 1 positive and 2 negative coefficients.

P. 50-51, Figs. 3.1-3.2. Each estimate is treated as an independent observation, even though several of the response values are from the same calendar year. Is a random effect of year needed to address any correlation in the estimates within a year? It is difficult to tell from Figure 3.1 – 3.2 if this could be a problem. Some assessment of this potential problem is needed.

P. 53. Table 3.3 (incorrectly labeled as Table 2.3) shows that the R^2 for predicting Z are generally low, so it is not clear if the “best” models in Appendix 3.2 actually have any predictive power.

P. 53, l. 22-24: See previous comments regarding interpretation of results.

P. 54, l. 4-17: 3.2): See previous comments regarding interpretation of results based on the material presented.

Chapter 4. Patterns in Annual Overall SARs

This Chapter updates the CSS time series of smolt-to-adult return rate (SAR) estimates of previous reports with an additional year of data (final 2010 estimates for steelhead and new 2011 estimates for Chinook salmon). The same methods are used as in previous years. The ISAB advises that annual updating and reporting of the SAR values is important and should continue. In addition, the ISAB appreciates the new reporting in this chapter of SARs for Snake River Basin wild spring/summer Chinook and steelhead at finer geographic and major-population-group (MPG) scales.

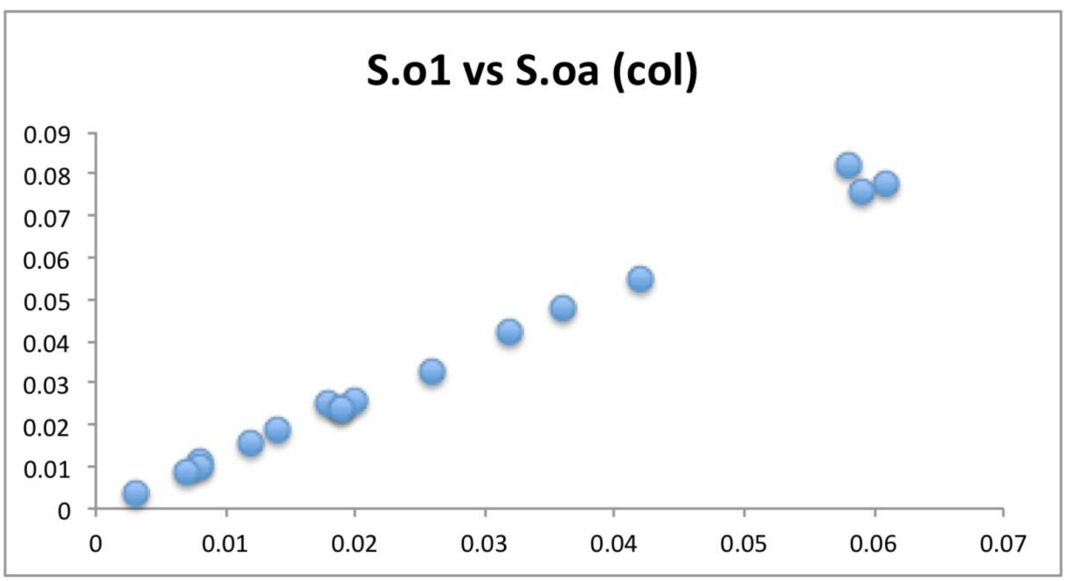
As in last year's report, SAR estimates are compared to the NPCC (2009) 2%-6% SAR objectives, and the conclusions are identical to those in last year's report. The ISAB appreciates the discussion of the SAR objective in the Introduction, and we agree that a more detailed consideration of SAR objectives is warranted. Ideally, SAR objectives should be estimated for each species and ESU of salmon based on realistic values needed to support robust viable populations. The values should consider differences in SAR for yearling versus subyearling life histories. The life cycle model presented in Chapter 2 appears to be poised to address this issue, at least for Grande Ronde spring Chinook. It would be worthwhile for CSS to utilize the SAR data in hand to help develop SAR objectives for each species and ESU.

The report notes the potential to expand some analyses. Some work is being planned to investigate the systematic differences in SARs between run reconstruction (RR) and PIT-tag methods, but no results are reported here. The ISAB encourages this expansion when data allow. The SAR data form the basis for many additional analyses that are needed to inform management of the hydrosystem.

Specific Comments and Questions

P. 60. More discussion or equations are needed to explain the difference between T_0^* and T_0 and to define t_i. T_0^* appears (on page 60) to be calculated identically to T_0 in Equation A.1, but perhaps that is because the t_i are not equivalent to the X_i in Appendix A (i.e., it is not clear).

P. 61, L. 41-43, P. 62, L. 1-17. The investigators back-calculate survival during the first year at sea, while assuming a constant 80% annual survival of sub-adults. As noted in the report, this approach "*assigns all ocean survival rate variability of the S.o1 life stage.*" We interpret this to mean that year-to-year variability in survival after the first year is transferred to S.o1. If so, it is not clear why the metric S.o1 is needed given that it does not necessarily capture the true variability in survival that occurs during the first year at sea. For example, the plot below shows the tight correlation of S.o1 with S.oa (Columbia mouth) based on data in Table 4.52. The problem is that readers less familiar with the methods may think S.o1 does capture variability only associated with this life stage while removing variability during subsequent stages at sea. On page 85, the investigators note that S.o1 and S.oa are perfectly correlated, so why include S.o1?



P. 63, l. 7-11. The influence of jack salmon on SAR values is noted. To what extend has the relative abundance of jack salmon changed over time and therefore influenced the variability in SAR values? To what extent do jacks affect SAR values for hatchery versus wild salmon, given that jacks are more common among hatchery salmon? Larson et al. (2013) found that mini-jacks contribute many fish to SARs among fall hatchery Chinook salmon when included in the calculations. What fraction of total SARs does mini-jacks represent if included?

P. 63, l. 15-18. The SAR of the reintroduced Clearwater Chinook stock is strikingly low compared with that of other stocks. Can you reference any projects that are looking at this low SAR value?

Investigators in the Snake River Basin have reported somewhat low and variable survival of smolts from release locations to the uppermost detection dam. Is there any correlation in upstream survival with survival through the hydrosystem and beyond?

P. 67, l. 10-11. Why is it not possible to calculate a SAR when very few sockeye are transported?

P. 67, l. 15-39. SARs for John Day and Yakima Chinook are clearly much greater than SARs of Snake River Chinook. How do the SAR values compare after standardizing for passage through dams?

P. 71, l. 14-15. It is not clear why calculation of the Snake River sockeye SAR from LGR-to-GRA in 2010 is not possible, given that it was calculated for LGR-to-BOA (shown in Table 4.33).

P. 79, l. 14-17. Fig. 4.11 does not show in-river survival as described in the text – it is Fig. 4.13 that contains these data. But the authors make a good point about the high mortality in the RIS-MCN reach. Survival in the upper Columbia needs to be monitored. The effect of this high mortality on Wenatchee steelhead is noted in the text, but the effect seems to be even greater for Leavenworth Chinook where MCN-BOA SAR is often 1% or less (Fig. 4.11).

P. 83, l. 10. It would be worth indicating that Knudsen et al.'s (2009) result that CWT/elastomer/ad clip-marked fish with PIT tags returned at a 25% lower rate than those without PIT tags implies that the survival rate for fish without PIT tags was 33% higher than for those with PIT tags. Stating the ratio in this way facilitates direct comparison with the 19% (line 5) and 35% (line 15) higher SAR estimates from run reconstruction relative to PIT tags.

P. 85, l. 7-9. Would not the high degree of correlation between S.oa and S.o1 be expected because of the assumption that survival is constant (80%) for each subsequent year at sea? It seems that variability could only come from annual variation in age at maturity. If this is true, then this paragraph seems misleading or uninformative and should be revised or deleted (see above comments related to P. 61, L. 41-43, P. 62, L. 1-17).

P. 86, l. 26-34. The lack of correlation of wild and hatchery steelhead SARs may be related to large differences in life history characteristics. Wild smolts are older (2 or more winters in freshwater) than hatchery smolts (1 winter in freshwater). The apparently lower SAR of B-run steelhead (Clearwater) might be related to more years spent at sea or differences between A- and B-run types in ocean distribution and migration patterns.

P. 87, l. 36-45. The ISAB agrees that more work needs to be done on potential biases associated with PIT tag shedding and mortality, and the authors provide a good discussion of this issue.

P. 88, l. 6-46. As noted above, the usefulness of the S.o1 calculation is questioned because it is perfectly correlated with S.oa and the suggestion that you have an independent estimate of S.o1 may be somewhat misleading. This does not mean that research conducted with S.o1 is not valid, rather the investigators could have simplified the analysis and simply used S.oa.

P. 89, 15-16. Is there benefit in investing in juvenile PIT-detection equipment in some of the dams to address the issue in the 4^{th} bullet point of the conclusions?

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

The ISAB acknowledges that the inclusion of estimates of SARs, TIRs, and D for fall Chinook salmon in the CSS is a work in progress. This chapter first estimates the amount of bias in SARs that could be introduced by holdover juveniles. It then estimates SARs, TIRs, and Ds for subyearling fall Chinook based only on release groups that are thought to have small numbers of holdovers.

Unless one is familiar with how the CSS estimates the SARs, it is not clear how holdovers cause problems. It would be helpful to give an explanation of how holdovers cause problems in the CSS methodology by looking at several scenarios. For example, suppose holdovers did not migrate through the hydrosystem and died before reaching LGR. Then the CJS estimates of survival are unaffected and S1 captures the death rate from release to LGR. Estimates of SAR are unbiased. Then suppose holdovers migrated through the hydrosystem and were not detected (e.g., migrated when the PIT-tag detectors were offline). Then the holdovers are indistinguishable from the previous scenario. Estimates of survival though the hydrosystem are unaffected except for S1, which now is a combination of survival from the release site to LGR + holdover proportion. Estimates of the number of smolts alive at LGR are biased downwards (holdovers are thought to have died). Consequently, when an adult fish returns from the holdover group, it inflates the SAR because the denominator does not include the holdovers. If only some holdovers are detected (e.g., those that stay in the hydrosystem until the next year), then explain the impacts on the various parts of the estimate of SAR. A diagram would be helpful. This comment was also made in the ISAB's 2012 review – perhaps some clarification is needed to prevent it being made in future drafts.

The simulation was generally well described but only in writing. Many readers would find a model description with equations or a figure describing the parts of the model easier to follow. This is especially true for the equation for N_{bias} (p. 119), where it is unclear how HO_u is computed and why it enters into the equation for N_{bias} in the way it does. It should be noted that the simulation is actually completely deterministic and that all ranges for the extent of the bias simply reflect three different survival rates when backcasting the holdover detections at Bonneville to LGR.

The report indicates that there is a model that gives some indication of which release groups have low holdover proportions. For the groups where the holdover proportions are high, is it possible to model some of the movement and thereby estimate the size of bias?

Specific Comments and Questions

P.116, l.35. "*… fish passing during the winter shutdown are not represented in estimates of survival and detection…*" This needs to be clarified. These fish look like "deaths after release" prior to LGR and only affect S1. Other estimates of survival and detection are unaffected. The ISAB agrees that returning fish will contribute to the numerator of the SAR and cause bias.

P.117, l.2. "*However, using the predictions to isolate and remove individual marked fish … proved unsuccessful.*" The ISAB is not sure how this differs from removing entire groups of fish that are thought to have high holdover rates? More details are needed.

P.117, l.34. "*The presence of winter migrants … and survival estimates in the CSS method.*" The ISAB did not understand the last phrase starting with "*and survival estimates.*" If the holdovers were not detected, only S1 is affected (along with C0).

P. 117, l. 34-36: The authors state, "*Since holdover fish could not be easily removed from release groups for SAR estimation using predicted holdover probability, it was important to evaluate the total bias that could occur in SAR estimates if any holdover fish were present in the release groups used.*" It would be useful to have additional explanation for why this is the case. If the explanation is available elsewhere in the report, refer to the location of those details here.

P.117, l.38. "*…CJS derived SAR estimates.*" The CJS model is used only to estimate the C0 value, and so it is not correct to say that the SAR is derived from this model.

P.117, l.37. Does the 21 December date refer to the Bonneville shutdown date, and does the 5 December date refer to the LGR shutdown date? This needs clarification.

P. 119, l. 11-14. The ISAB agrees that this conservative approach is a good idea.

P. 119, l. 22-23. Adding a ("*winter passage*") proportion to the ("*estimated spring passage*") population does not make sense. Is the first value a number (i.e., a population rather than a proportion)? Unfortunately, it is impossible to tell from the imprecise definition of HO_u in the equation that follows. This section remains very unclear (despite ISAB comments about the issue in reviewing the draft 2012 report). The ISAB does not understand the calculation of N_{Bias}, although we presume it is done correctly, and just not explained accurately.

P.120, l.13. Why is the % bias expressed as a fraction of the (incorrect) CSS SAR and not the correct simulated SAR?

p.120, l.20. "*Those fish would have been part of the Co…*" Here the authors need to be careful about the actual Co and the estimated Co (which is used in the SAR). If the holdovers were undetected, then yes they are part of the actual Co, but they would have been treated as "deaths" after release and prior to LGR and would NOT be included in estimated Co and not in the denominator of the SAR.

P.125, l.16. "*… low holdover detection probability*". Perhaps this should read low holdover detection rates. A release group could have a 0 holdover detection probability (migrates during shutdown) but still have substantial bias in the SAR.

P. 140, l. 17-19. "*The confidence intervals on the wild mark groups tended to be much wider than those of the hatchery groups due largely to relatively low numbers of wild fish marked each year relative to hatchery releases."* It would be useful to mention other reasons for the wider confidence intervals.

P. 143, Table 5.26. It seems worth noting that all TIR are greater than 1 and three D values are significantly less than 1.

P.146, l.8. Won't there be bias even if the holdovers were NOT detected in the hydrosystem? Please clarify.

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

This Appendix presents the methodology for the computation of the SR, SAR, TIR, and D parameters along with extensive tables of results. This chapter is an update of similar material from previous years.

In order to understand the equations, a flow diagram would be helpful showing where the various statistics and parameter lie. This was also commented on in the ISAB's 2012 review.

The model fitting of the relationship between log(TIR) and S_R needs additional thought because the TIR implicitly includes S_R. The ISAB suggests that plotting log(TIR) - log(S_R) against time or by itself would be more informative. This should provide some information about possible differential ocean survival.

Specific Comments and Questions

P. 2, l. 33-41. The sentence on lines 38-39 indicates that if point estimates of reach survival exceed 100%, they are considered unreliable and deleted "for the plots." Were these points retained for computing the S_R values and confidence intervals that are presented in the tables? Selectively omitting survival estimates that exceed 100% will bias the distribution of the remaining estimates, and consequently, the median or average of this distribution will tend to underestimate S_R.

The notation d_0 and d_1 – in which the subscripts refer to C_0 and C_1 fish – is confusing because it seems incongruous with the notation d_2, d_3 and d_4 in which the subscripts refer to dams.

P. 9, l. 34. This is not really an expected value since it depends on the random variables $d_{5,0}$, etc.

P. 10, l. 30-32. It is not clear why for 2010, equation A3 is considered to be better than equation A6 "*because of remarkably low detection probabilities at LMN that were probably a result of the noted bias.*" A short explanation would be helpful.

P. 11. In Equation A.10, SAR(Tx_t) is shown to be calculated by summing adult counts at three dams ($AT_{LGR\_t} + AT_{LGS\_t} + AT_{LMN\_t}$). However the AT_i are not defined anywhere; is there a procedure to obtain a cumulative count over the dams that prevents counting the same adult fish more than once?

Is it statistically reasonable to conclude from S_R values for Snake River Chinook in figures A.2 - A.3 (i.e., data in tables A.1 - A.3) versus corresponding SAR values for C_0 fish in figures A.7 and A.8 that SAR values are typically higher for wild than hatchery Chinook whereas S_R values are typically the same for wild and hatchery Chinook (or perhaps slightly higher in hatchery Chinook)? This conclusion (if supported by the data) would indicate that wild Chinook typically survive better than hatchery fish below Bonneville and would be worth reporting here.

P.44-45. Characterizing the relationship between $\log_e$(TIR) and in-river survival (S_R): The sentence on lines 36-37 states that "*the effectiveness of transportation as measured with TIR should be partly dependent on in-river survival.*" More generally, TIR must depend on the trade-off between (i.e., the relative magnitudes of) S_R and D. For Figure A.18, it might be more informative to include a reference line that corresponds to the expected relationship between $\log_e$(TIR) and $\log_e S_R$ when D=0. The plot would then show the amount of variation in TIR that could be attributed to the null hypothesis (S_R) alone and how much remained to be attributed to D (that statistic could also be calculated). A key point, suggested in the text but not the existing figure, is that a linear relationship between $\log_e$(TIR) and $\log_e S_R$ (not S_R) is expected by definition, and need not be estimated empirically by regression. From equation A.15 (i.e., by definition), TIR = D S_T/S_R, so log(TIR) = logD + $\log S_T - \log S_R$ where the $\log S_T$ might reasonably be assumed to be constant at log(0.98). Note that the expected linear relationship requires taking the log of S_R (which is not done in Figure A.18).

V. Editorial Suggestions

Glossary

The ISAB recommends that all acronyms and abbreviations used in the report be included in the glossary. It is very helpful to be able to look at the glossary to be reminded of the definitions. The use of "ESU" was mentioned in the last review, but was not changed in this edition. The authors did explain their age designation methodology (1 salt, 2 salt, and so on) in last year's response to the ISAB review, and the ISAB requests inclusion of these descriptions in the glossary. A number of dam acronyms are missing (e.g., RIS, with no explanation at its first appearance in Table 1.3). This comment was made in last year's review. The response indicated that these acronyms would be (had been?) added to the final report for 2012.

Missing Section

For future reviews of the CSS Annual Report, the ISAB recommends the inclusion of the draft executive summary. The ISAB understands that this might be modified after the CSS response to our review, but a draft executive summary would provide a useful overview of the report's new contents, major results, and conclusions for ISAB reviewers.

Chapter 1

P. 1, l. 22. Is 17th year correct? Last year's report states, "15th year"

Chapter 2

p.19, l.30. How are empirical abundances used to evaluate spatial and temporal variation in salmon survival? Is some rewording needed here?

p.20, l.14. The authors need to clarify what is meant by "intermediate" number of populations and why the number of populations causes problems. Some rewording here?

p.20, l.41. What is the power-house contact rate (NPH)? This does not appear in the glossary, and is never explicitly defined anywhere. Is this simply the number of powerhouses on the outward migration path? Why is it a rate?

p.21. l.22. Why are data only from years that overlapped all populations used? Data from years where some of the populations are measured are also informative and should be included.

p.22, l.14. The capacity parameter is set to infinity for the ocean. But infinity is poorly represented in most computer packages, and so was some large value used? Need to be more specific here.

p.24, equation (1) and following equations. The left side of the equation is the expected number of smolts (or other stages of the population) and should be indicated as so.

p.26, equation (9). It would clarify the text if the beta-terms were indexed by PDO, UPW or NPH rather than 1, 2, and 3.

P. 25, l. 11. Should be third year in ocean; not second year.

p.26, l. 22. Why is there is an additional *a1* parameter when *ap* parameters have already been defined?

p.28, l. 24 "*… within the range of variability of the empirical data…*" Which data? How were empirical data on the ocean survival rates obtained? Perhaps it is meant in the range of the estimates from the other models?

p.28, l. 27. Report the SE for the estimate of the beta-coefficients. What is meant by "*… the NPH effect is stronger in this analysis*"? There are 4 beta parameters, but only 3 estimates are reported.

P. 30. Figure 2.3. Plot the Y axis (here and other plots) on the log-scale to uncompress the scales.

P. 31. Figure 2.4. Make these plots on the log-log scale.

p.32, l. 6. Refers to the BH model, but figures are for the LC model.

p.39, l. 6. How does the logit transform make the effect of NPH stronger?

p.39,l. 12. This paragraph is difficult to follow and needs to be rewritten. It is not clear what the authors are trying to say here.

p.39, l. 41. The authors claim that there are only 13 parameters for the LC model. But there are 6x2=12 parameters alone for the recruit/spawner relationship, plus the ocean survival, maturation, and variance parameters for more than 13 parameters. Please explain how this count was obtained.

Chapter 3

Given that this chapter of the report is updated each year, it is important to be clear about what years are covered in the text, tables, and figures (e.g., if statistical values, or goodness-of-fit values are shown, the year(s) over which these values were calculated should be indicated). Some specific locations where clarification is needed are noted below.

p.43, l.10. "survival rate" implies survival per unit time. The CJS model simply gives you a survival probability for that reach.

p. 43, lines 12, 13, 17: Should these years be 1998-2012? The values in Table 3.1 are slightly different than they were in the 2012 report, which is likely because one more year of data is included in obtaining these values. If true, this means these lines should indicate that one more year of data was used.

p.43, l.19. It would be preferable to use the median c-hat procedure used in MARK and developed by White (2013) to estimate the c-hat value because the methods from Burnham et al. (1987) have been shown to perform poorly.

p. 43, l. 24: Should this be 41% rather than 43% to correspond with Table 3.1?

p. 43, Table 3.1: Here is a case where it might be helpful in the caption to indicate the years over which the CV was calculated. The inclusion of the number of cohorts is appreciated.

p.44, Equation 3.3: An extra minus sign is present in the equation.

p. 46, l.12-13: It appears from the way this paragraph was written that only six environmental factors were evaluated this year as compared to seven last year (see lines 4-7 on this page). However, on lines 12 and 13, the authors still refer to seven variables and 128 possible model combinations. Should these numbers be changed to reflect the different number of variables mentioned earlier in the paragraph?

p. 46, l. 18-22: It is not clear whether the authors did two log transformations (e.g., $\log_{10}$ and ln). The latter could be written as $\log_e$ which the authors do state was something they looked at, but once they mentioned this it becomes ambiguous when they use "log" without a base indicated. Typically, "log" implies "$\log_{10}$". This is not the same transformation as $\log_e$ so it is important to be clear which is being referred to in Equation 3.6.

p.46, l.19. "*dependent variable*" is an old-fashioned terminology. Use "response variable" instead.

p.46, l.27. The authors indicate that a log-transform is needed for Z, but then equation 3.7 does not show any transform? Was a transformation used?

p.53, l.1. Table should be numbered 3.3 rather than 2.3. r2 should be R^2 . It is not clear how the R^2 is computed for survival as it never was used in a regression model directly but is derived using equation 3.8.

p.54, l.34. It is not clear why improvements in the precision of the estimated survival rates are needed. There may be enough residual variation in the regression models that improving precision of estimates of survival has no impact. A small simulation should be done to see if this is a worthwhile activity.

p. 54, last paragraph. Rather than "precision" the authors likely mean "accuracy." Precision refers to the number of decimal places with which one can make an estimate, and how repeatable an estimate is (i.e., getting the same value over and over again). Accuracy refers to how close one is to the true value. We suggest replacing the word "precision" throughout this paragraph with "accuracy."

p.55, l.12. Model with Day^2 but lacking Day in the predictor set should not be fit as they are not sensible. Such models assume a very strict functional form for the effect of Day that is centered around the value of 0 which is simply not sensible. The "all subsets" model selection procedure will need some guidance to not fit these models. Similarly, models with the Day*WTT interaction but lacking the main effect of Day and WTT should also not be fit.

Missing tables. In the CSS response to the 2012 ISAB review, the authors state in several responses that they have provided Tables 3.4-3.7. Why were these tables not included in the 2013 report?

Chapter 4

P. 57, l. 45 and other places throughout the text. The ISAB appreciates (in response to our comment last year) that confusion about fish age has been eliminated by explicitly defining the CSS' convention of using "salt years," which is now used consistently throughout the annual report. As noted above in our editorial suggestions for the glossary, however, the ISAB requests inclusion of an explanation of this age designation terminology in the glossary.

P. 63, Figure 4.1 – The caption should indicate that RR data for Chinook only extend to 1984 (not 1993 as indicated…this point was raised in last year's review too). Also the data point for 2011 seems to be missing (it's expected from reading the caption).

P. 67, l. 14-39. The "*Mid-Columbia River Overall SARs*" section was mistakenly inserted (same section is repeated on p. 72, l. 3-38).

P. 69, Figure 4.5 - the caption and text on page 69, line 9 refers to 14 migration years (1997-2010) but only 13 points are indicated in the figure (2010 seems to be missing).

p. 66. Fig. 4.7 is out of order (precedes Figs. 4.4, 4.5, and 4.6) in the draft. This figure is repeated on p. 71 (more appropriate place).

Tables 4.30 and 4.31 are missing and need to be completed.

P. 76-77, The bottom paragraphs (l. 33-43) and next page (l. 1-6) are same as those at top of page 76, l. 3-19.

Chapter 5

p. 140, l. 4-5 "*SAR estimates by study category for wild subyearling fall Chinook were only available 4 for three of three years and only for the Snake River release groups (Table 5.24).*" The sentence does not make sense.

p. 145, l. 20 – "mentioned" (not "mention")

p. 147, l. 13 – add "in" to "groups IN 2008"

Appendix A

P. 8. Figure A.1 is very helpful, and it might be useful to include it in Chapter 4 as well.

p. 9, l.13. "*… with the m-matrix parameters.*" The values of m_{12} etc. are statistics and not parameters.

p. 10, l. 28. "than" instead of "that"

p. 25. Figure A.8: Update the caption to indicate four (not three CSS hatchery summer Chinook groups).

p. 44. Table A.37: Add bold font for TIR estimate for 2011 (upper CI significantly below 1).

References

Beamish, R.J., and C. Mahnken. 2001. A critical size and period hypothesis to explain natural regulation of salmon abundance and linkage to climate and climate change. Progress in Oceanography 49:423-427.

Buhle, E.R., Scheuerell, M.D., Ford, M.J., Cooney, T.D., and Carmichael, R.W. 2013. Chapter 4.1 Impacts of supplementation on population dynamics of Snake River spring/summer. In R.W. Zabel et al. Life-Cycle models of salmonid populations in the interior Columbia River Basin. June, 28, 2013 draft report in review by ISAB. NOAA, Northwest Fisheries Science Center, Seattle.

Burnham, K.P., D.R. Anderson, G.C. White, C. Brownie, and K.H. Pollock. 1987. Design and analysis methods for fish survival experiments based on release-recapture. American Fisheries Society Monograph 5. Bethesda, MD. 437 pp.

Cooney, T.D., R.W. Carmichael, B.C. Jonasson, E.W. Sedell, and T.L. Hoffnagle. 2013. Chapter 2.1 Grande Ronde spring Chinook population models. In R.W. Zabel et al. Life-Cycle models of salmonid populations in the interior Columbia River Basin. June, 28, 2013 draft report in review by ISAB. NOAA, Northwest Fisheries Science Center, Seattle.

CSS (Comparative Survival Study Oversight Committee and Fish Passage Center). 2011 Annual Report: Comparative Survival Study (CSS) of PIT-tagged Spring/Summer Chinook and Summer Steelhead. BPA Contract #19960200. 2011.

CSS (Comparative Survival Study Oversight Committee and Fish Passage Center). 2012 Annual Report: Comparative Survival Study (CSS) of PIT-tagged Spring/Summer/Fall Chinook, Summer Steelhead, and Sockeye. BPA Contract #19960200. 2012.

Farley, E.V., J.H. Moss, and R.J. Beamish. 2007. A review of the critical size, critical period hypothesis for juvenile Pacific salmon. North Pacific Anadromous Fish Commission Bulletin 4: 311–317.

Independent Economic Analysis Board. 2013. Cost-Effectiveness of Fish Tagging Technologies and Programs in the Columbia River Basin (IEAB document 2013-1).

ISAB. 2013. Review of the IEAB Fish Tagging Cost Effectiveness Model. (ISAB document 2013-3).

ISAB and ISRP. 2007. Review of the CSS Ten-Year Retrospective Summary Report (ISAB/ISRP document 2007-6).

Jacobson, K., B. Peterson, M. Trudel, J. Ferguson, C. Morgan, D. Welch, A. Baptista, B. Beckman, R. Brodeur, E. Casillas, R. Emmett, J. Miller, D. Teel, T. Wainwright, L. Weitkamp, J. Zamon, and K. Fresh. 2012. The marine ecology of juvenile Columbia River Basin salmonids: a synthesis of research 1998-2011. Report of the U.S. National Marine Fisheries Service, National Oceanic and Atmospheric Administration, Fisheries and Oceans Canada, Kintama Research Services, Ltd., and Oregon State University to Northwest Power and Conservation Council, 851 S.W. Sixth Avenue, Suite 1100, Portland, Oregon 97204.

Kennedy, P., T. Copeland, J. Johnson, K.A. Apperson, J. Flinders, and R. Hand. 2011. Idaho 37 natural production monitoring and evaluation project. Prepared for U.S. Department of Energy, 38 Bonneville Power Administration, Division of Fish and Wildlife. Project 1990-055-00. IDFG 39 Report Number 11–23.

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. North American Journal of Fisheries Management 29:658–669.

Larsen, D., B. Beckman, W. Dickhoff, K. Cooper, P. Parkins, D. Fast, C. Strom, and M. Johnston. 2013. High rates of precocious male maturation, “minijacks”, in spring Chinook salmon hatchery programs: Prevalence, causes and potential solutions. http://www.dfw.state.or.us/fish/hatchery/docs/Larsen.pdf. Accessed 2013-09-25.

NPCC (Northwest Power and Conservation Council). 2009. Columbia River Basin Fish and 40 Wildlife Program. Council Document 2009-02. http://www.nwcouncil.org/library/2009/2009-41 02.pdf

Petrosky, C.E., and H.A. Schaller. 2010. Influence of river conditions during seaward migration and ocean conditions on survival rates of Snake River Chinook salmon and steelhead. Ecology of Freshwater Fish 19(4): 520–536.

Schaller H., P. Wilson, S. Haeseker, C. Petrosky, E. Tinus, T. Dalton, R. Woodin, E. Weber, N. 14 Bouwes, T. Berggren, J. McCann, S. Rassk, H. Franzoni, and P. McHugh. 2007. Comparative 15 Survival Study (CSS) of PIT-tagged Spring/Summer Chinook and Summer Steelhead. Ten-year 16 Retrospective Summary Report. BPA Contract # 19960200. Prepared by Fish Passage Center 17 and Comparative Survival Study Oversight Committee representing the Columbia Basin Fish 18 and Wildlife Agencies and Columbia Basin Tribes. 675 pp. http://www.fpc.org/documents/ 19 CSS/FINAL COMPLETE 2010 YEAR CSS REPORT-8-31-07withfrontpage.pdf

White, G. 2013. Median chat. Program MARK help file. Available at http://warnercnr.colostate.edu/~gwhite/mark/markhelp/median_chat.htm Accessed 2013-10-07. There is no primary literature on the use of median chat vs. the bootstrap chat.